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MICROCREDIT MADE TO MEASURE: EXPERIMENTAL EVIDENCE FROM RURAL MOROCCO

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Abstract

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JEL Classification: C93, D14, G21, L26, O12, O16

Keywords: Microcredit, Tailored loans, Grace period, Loan contract design, Rct, Morocco

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Microcredit Made to Measure: Experimental Evidence from Rural Morocco

July 23, 2026

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Abstract

Does matching loan repayments to expected cash flows raise take-up and the welfare impact of microcredit? We test this in a randomized controlled trial with Morocco's largest microfinance institution. The experiment introduces two new individual-liability loan products: a contract with a five-month principal grace period and a tailored contract with installments matched to expected revenues. Over 3,000 applicants are randomly assigned to an offer of one of the two new contracts or the standard loan. The new contracts attract distinct borrowers: agricultural households take up the grace-period loan, while entrepreneurs operating smaller, more capital-constrained businesses choose the tailored schedule. Most tailored-loan borrowers front-load their repayments, suggesting that demand for flexibility reflects cost minimization, not only liquidity management. The tailored loan raises sales and profits via non-agricultural business expansion and labor reallocation to self-employment. The grace-period loan shows similar but statistically insignificant effects. Neither product worsens repayment performance. We find no effects on investment or consumption. In a second stage, we randomize contract-specific information campaigns across 440 villages. Despite shifting stated preferences toward the advertised products, the campaigns do not raise take-up, consistent with supply-side frictions and a challenging macroeconomic environment.

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1. Introduction

Recent experimental evidence has cast doubt on claims that giving poor households access to microcredit systematically improves their living standards (Banerjee et al., 2015). Two salient facts emerge from this literature. First, relatively few people take up microcredit when it is offered to them.[1] Second, conditional on loan take-up, treatment effects are small (Meager, 2019) and concentrated among sub-populations such as existing entrepreneurs (Banerjee et al., 2019).

Several explanations have been proposed for the low take-up of microcredit. One possibility is a scarcity of creditworthy borrowers and profitable projects, with low-quality potential borrowers rationally self-selecting out of the credit market (Beaman et al., 2023). Another possibility is that credit constraints are not binding for a significant part of the population. A third explanation is that the structure of microcredit contracts discourages households from borrowing, even if their projects are profitable and they could in principle service debt (Johnston Jr and Morduch, 2008). A fourth is that low demand reflects not the design of the products but limited awareness of them. The first two explanations concern the borrower and the return to capital; the latter two concern the product: its structure and whether borrowers know about it. We focus on the latter two.

A recent literature has begun to test whether changes to microcredit contract design can raise take-up and impact. Field et al. (2013) show that a two-month grace period increases business investment and long-run profits in urban India, and Attanasio et al. (2019) document that liability structure shapes selection and use of credit in Mongolia. Other papers examine ex-post repayment flexibility: Battaglia et al. (2024) and Barboni and Agarwal (2026) study contracts that allow borrowers to postpone two or three monthly repayments, respectively, during the loan cycle. In both settings (India and Bangladesh,

[1] In previous RCTs, the take-up of microcredit in treatment areas was 31% in Ethiopia (Tarozzi et al., 2015); 18% in India (Banerjee et al., 2015); 19% in Mexico (Angelucci et al., 2015); and 17% in Morocco (Crépon et al., 2015). Take-up was higher in Mongolia (57% for group loans and 50% for individual loans) where credit was rolled out among women who had indicated an interest in borrowing (Attanasio et al., 2015).

respectively) repayment flexibility raises business activity at the intensive margin without harming repayment performance. Taken together, these results suggest that the canonical microcredit product, with its rigid linear schedule starting immediately after disbursement, may be poorly matched to the cyclical and seasonal income flows of poor households in rural areas of low-income countries.

Against this backdrop, we evaluate two distinct product innovations that relax the repayment schedule rigidity of microcredit within a single setting (rural Morocco). Underlying the experiment are two hypotheses. First, we hypothesize that standard microcredit contracts lack a sufficiently long grace period. Entrepreneurs may have difficulties repaying loans immediately after disbursement because it takes time for investments to generate revenue. Second, we hypothesize that the rigid and linear repayment schedule of the standard microcredit contract is not (sufficiently) relaxed by a generic grace period that is identical for all borrowers and that reduces liquidity demands early in the loan cycle. Instead, a tailored repayment schedule is required that matches the expected revenue flows of individual borrowers more closely throughout the loan cycle as a whole.

We partnered with Al Amana, Morocco's largest microfinance institution, to test these hypotheses by randomly relaxing one of two constraints. To test the first hypothesis, we offer microcredit with a significantly longer grace period: five months instead of one. This may make projects with high expected returns, but delayed initial cash flows, more attractive to borrowers. Moreover, the grace period treatment arm plays the role of a benchmark compared to a tailor-made arm that aligns loan repayments with borrowers' cash flows. Importantly, the products were designed to equalize the lender's expected net present value of repayments for a given principal amount. They therefore did not offer a lower contractual price, although their different amortization profiles mechanically affected maturity and total nominal interest payments. Differences across treatment arms should consequently be interpreted as responses to the repayment structure and its associated timing and cost profile, rather than to an explicit interest-rate discount.

A key conceptual distinction is at what stage flexibility is introduced: ex ante (i.e., at the design stage) or ex post (i.e., at the repayment stage). We test the second hypothesis by offering microcredit with a repayment schedule tailored *ex ante* to match the expected revenues of individual borrowers over the loan cycle. Such loans may make projects with high expected returns but highly seasonal revenues (for instance due to natural or religious cycles) more attractive to borrowers. Our approach is therefore to create and evaluate loan contracts that are *made to measure*, fitting the borrower's expected cash flows ex ante, rather than contracts that insure against unexpected idiosyncratic shocks ex post (the approach taken by Battaglia et al. (2024) and Barboni and Agarwal (2026), who allow borrowers to postpone repayments during the loan cycle). Ex-post flexibility may be better suited to insuring against unanticipated shocks, but our implementing partner, with some justification, feared it could erode repayment discipline. Our design is also motivated by an almost universal complaint from focus groups with (potential) clients: that the standard contract's cookie-cutter linear schedule fits poorly with expected but seasonal entrepreneurial income.

Both hypotheses concern the design of the product itself. Our second focus is the fourth explanation above: that low demand reflects limited awareness of what is on offer, standard or flexible. To weigh both (i.e., that the products are poorly matched to borrowers' cash flows, and that borrowers are unaware of what is available) the experiment proceeds in two stages. In Part A, we randomize individuals who demonstrate a desire to take out a standard microcredit loan (by visiting a branch and applying for such a loan) and who are eligible for a standard loan. We randomize these individuals into either of the two new loan contracts or into the standard loan offer (the control group). By restricting our experiment to individuals willing to take a standard loan, we can evaluate the impact of the new loan contracts on borrower welfare while netting out potential selection effects. Part B of our study then focuses on changes in borrower selection and repayment performance once those new loans become available to the entire population (and not only to individuals

visiting a branch to apply for a standard loan). Here we randomize loan-type specific information campaigns to stimulate take up at the village level. Our aim is to understand whether offering grace-period loans and tailored loans influences take-up and whether the pool of clients selected into these two new contract types is different from those who select into a standard loan. This two-pronged approach helps us to clarify the causal link from offering new loan contracts to borrowing and repayment behavior and to evaluate the effect of these new contracts on borrower welfare in the population that was initially interested in the standard loan offer.

Using this two-stage design, we find that take-up of both flexible contracts is substantial: 29% of applicants offered the grace-period loan and 32% of those offered the tailored loan sign a contract of the assigned type, with most of the remainder taking a standard loan. The aggregate probability of signing any loan is virtually identical across arms (81–84%), so the new products substitute across contract types rather than expand the borrower base among applicants who had already self-selected into the credit market. Conditional on this take-up pattern, the tailored loan generates economically meaningful effects on household business activity. Intent-to-treat estimates measured at least eighteen months after randomization show that, relative to the standard-loan offer, the tailored loan offer raises annual sales by 7,897 MAD (40% of the control mean) and profits by 4,895 MAD (51%), with the gains concentrated in non-agricultural enterprises and in the upper tail of the outcome distribution. The grace-period contract produces qualitatively similar but statistically insignificant effects, indicating that *ex-ante* customization of installment levels across the loan cycle (rather than deferral of principal repayment per se) may be a more effective margin for the borrowers it attracts.

Three additional results from Part A help interpret these findings. First, the profit gains under the tailored loan are accompanied by a reallocation of household labor from casual wage employment toward self-employment, but not by detectable increases in fixed-asset investment or consumption. This is consistent with treated households deploying

working capital into existing non-agricultural activities rather than scaling up lumpy investment or smoothing consumption. Second, neither product worsens repayment performance: the probability of closing a contract more than thirty days late, the number of unpaid installments, and the deviation from the scheduled end date are all statistically indistinguishable from the control mean. Third, the two contracts draw distinct borrower profiles among compliers. Grace-period compliers are agricultural households operating larger, asset-rich businesses whose revenues arrive late in the production cycle. Tailored-loan compliers (more often female and engaged in crafts) operate smaller, more capital-constrained businesses. Within the tailored-loan arm, a majority of borrowers (197 of 309 contracts with complete schedule data) elect a *front-loaded* profile, repaying a large share of the principal in the first third of the cycle. Front-loading minimizes the effective borrowing cost at the expense of early-cycle liquidity, suggesting that cost minimization may be a motive alongside liquidity management.

In Part B, the randomized loan-type-specific information campaigns (across 320 out of 440 study villages) substantially shift *stated* preferences toward the advertised products: relative to a campaign promoting the standard contract, information on the grace-period loan raises the share of households expressing a preference for this contract by roughly 65 percentage points. Likewise, the information campaign on the tailored loan raises the share preferring the tailored loan by approximately 63 percentage points. Yet, these shifts in stated demand do not pass through to actual borrowing. Estimated effects on both the number of loan contracts signed and the total amount disbursed at the village level are small, statistically insignificant, and span both signs across products. The contrast between large preference effects and null take-up effects points to binding supply-side frictions (such as loan-officer screening and branch-level eligibility constraints) compounded by a difficult macroeconomic environment shaped by drought and the aftermath of COVID-19. Together these limit the pass-through from product awareness to credit allocation.

Related literature. This paper informs four strands of literature: experimental evaluations

of microcredit; contract-design innovations in microfinance; seasonal liquidity constraints in developing-country agriculture; and borrower selection into credit contracts.

First, we contribute to the experimental literature on microcredit. A series of randomized evaluations across six countries found that expanding access to standard microcredit has, on average, modest effects on household welfare (Banerjee et al., 2015). Take-up was often below 30% and average treatment effects of expanded microcredit access were generally modest and concentrated among pre-existing entrepreneurs (Banerjee et al., 2015; Angelucci et al., 2015; Tarozzi et al., 2015; Crépon et al., 2015; Attanasio et al., 2015). A Bayesian hierarchical analysis confirms that mean treatment effects are close to zero, with wide posterior uncertainty about the scope for positive impacts (Meager, 2019). Banerjee et al. (2019) show that longer-run effects are similarly limited for most borrowers, though a subset with high baseline returns may benefit substantially.

These findings admit several interpretations. Beaman et al. (2023) argue that low take-up reflects rational self-selection: in their Mali experiment, marginal applicants had near-zero returns to capital, so expanding access drew in borrowers for whom credit was unprofitable. Johnston Jr and Morduch (2008) advance an alternative: many creditworthy households are deterred not by low returns but by features of the contract itself. If the rigid, linear repayment schedule of the canonical product is a poor fit for borrowers' cash-flow profiles, then contract design rather than credit access per se may bind on both take-up and impact. Our experiment is motivated by this possibility.

Second, this hypothesis has motivated a growing literature on microcredit contract design. These studies examine whether relaxing repayment schedule rigidity can improve take-up, investment, and welfare. An early contribution studies repayment *frequency*: Field and Pande (2008) show that moving borrowers from weekly to monthly installments did not raise delinquency, an early indication that schedule design can be relaxed without compromising repayment. A key contribution on grace periods is Field et al. (2013), who randomize a two-month grace period against immediate repayment among borrowers

in urban India. The grace period raised investment and profits but also default rates, evidence that the standard contract discourages high-return investments with delayed payoffs. A long-run follow-up documents that this initial business growth translated into intergenerational human-capital gains over an eleven-year horizon (Agte et al., 2024). Field et al. (2013) attribute these effects to a lumpy-investment channel: borrowers accumulate a larger lump sum and invest in indivisible, high-return assets, a mechanism also central to the asset-based microfinance literature (Bari et al., 2024). Relative to that work, our grace period is substantially longer and embedded in a rural setting, where seasonal cash-flow constraints are arguably more pronounced, and, as we show below, it did not raise default.

A parallel strand studies *ex-post* flexibility: contracts that let borrowers defer installments during the cycle at their own discretion. Battaglia et al. (2024) randomize an option to skip up to two monthly payments in Bangladesh and find that flexibility raises profits and reduces default, operating primarily through insurance: borrowers absorb shocks and undertake riskier, more profitable investment. Barboni and Agarwal (2026) study a three-month repayment holiday in India offered at a two-percentage-point premium and find improved business outcomes without deterioration in repayment performance, consistent with positive selection into the costlier product. Brune et al. (2026), by contrast, find that three repayment "passes" for first-time borrowers in Colombia shifted investment but left profits unchanged and raised default sharply (suggesting that repayment flexibility may be less effective for inexperienced borrowers). Aragón et al. (2020) test a revolving credit line for Indian street vendors and find a 7% profit gain over a standard term loan, growing as borrowers learn to exploit it.

Two features distinguish our tailored loan from this work. First, where prior studies provide *ex-post* flexibility (i.e., borrowers defer repayment after disbursement in response to realized shocks) our tailored loan provides *ex-ante* customization: the schedule is set before disbursement to match the borrower's anticipated revenue cycle, dividing the repayment period into three equal segments with potentially different installments, subject to a floor

equal to the monthly interest due.[2] This targets a distinct friction—predictable seasonal income variation, which our focus-group borrowers named the single most important shortcoming of the standard contract—rather than the unpredictable shocks that motivate ex-post flexibility. Second, we compare both innovations within one experiment, against a common control group, among self-selected applicants. This within-study comparison is new: existing evidence is fragmented across countries, lenders, and populations, making it hard to assess whether the *form* of flexibility matters beyond its mere availability.

Third, our rural setting, where many borrowers engage in agriculture or seasonally dependent activities, connects to work on seasonal liquidity constraints. Fink et al. (2020) show that lean-season loans in Zambia raised agricultural output and on-farm labor, with effects concentrated among poorer households. Burke et al. (2019) show that harvest-time loans let Kenyan farmers avoid distress sales of grain at low post-harvest prices, raising revenues by allowing them to exploit predictable seasonal price swings. Weber and Musshoff (2017) find that grace periods for Madagascar crop farmers help bridge low-return periods but at the cost of modestly higher delinquency. Timing thus matters: the value of liquidity depends on when repayment obligations fall relative to seasonal income realizations. This is precisely the friction our two new contracts aim to address.

Fourth, the scalability of contract innovations hinges on who selects into flexible products and whether selection helps or harms portfolio quality. Barboni (2017) shows, theoretically and experimentally, that higher-revenue borrowers are more likely to take up flexible contracts, especially when priced at a premium, giving conditions under which menu design mitigates adverse selection. Attanasio et al. (2019) find that in Mongolia the liability structure shapes both take-up and group composition, with individual-liability loans attracting riskier but potentially higher-return entrepreneurs. Because our applicants have

[2]Our contract tailors the *timing* of fixed obligations to the borrower's expected revenue cycle, but obligations remain fixed once set. It is therefore distinct from revenue-contingent or equity-like contracts, which tie repayment directly to realized income (Fischer, 2013; De Mel et al., 2019; Cordaro et al., 2025). Such performance-contingent designs offer a more direct link between repayments and income, but raise the costly-state-verification, adverse-selection, and moral-hazard concerns that an ex-ante fixed schedule avoids.

already revealed a willingness to borrow before being randomized into contract types, we characterize selection on a different margin: which borrowers prefer a grace period, which prefer a tailored schedule, and which revert to the standard product. We show that the two flexible contracts attract distinct profiles (agricultural households into the grace-period arm, entrepreneurs running smaller, more capital-constrained businesses into the tailored arm) speaking directly to how lenders should target contract innovations.

We proceed as follows. Section 2 describes the experimental design. Section 3 presents the data and sample, after which Section 4 introduces the empirical strategy. Section 5 reports the results, and Section 6 discusses them. Section 7 concludes.

2. Experimental design

The experiment consisted of two complementary parts, implemented with Al Amana, Morocco's largest microfinance institution and one of the largest in the Middle East and North Africa.[3] The study took place in rural areas of central and northern Morocco, where household incomes are concentrated in agriculture and seasonally dependent activities, and where the study period coincided with successive drought years and the aftermath of COVID-19. Part A randomized individual loan applicants across 40 rural branches into one of three contract types (standard, grace period, or tailored) to estimate the welfare effects of the new loan products on borrowers. Part B randomized loan-type-specific information campaigns across 440 villages in the catchment areas of 20 rural branches to estimate the effects on loan demand. Figure C.1 provides a schematic overview of the research design; Figure 1 shows the implementation timeline. Appendix A documents material deviations from the pre-registered Stage 1 report and explains the rationale for each change.

[3] Al Amana was founded in 1997 and is a non-profit microcredit association dedicated to financial inclusion and the promotion of income-generating activities. It operates an extensive national network of close to 600 service points (comprising urban and rural branches as well as mobile units serving remote areas) and offers a diversified portfolio of microfinance products.

2.1. Part A: Individual-level randomization

Part A assessed the impact of two made-to-measure loan products on borrower welfare. The experiment targeted individuals, either first-time or renewing applicants, who visited one of the 40 participating rural branches to apply for a standard individual-liability loan. Only applicants who met Al Amana's standard eligibility criteria and expressed a genuine interest in borrowing were included. This focus on self-selected applicants who would plausibly have taken a standard loan allows us to evaluate the causal effect of the new contract types while netting out selection effects that would arise if the products were offered to the general population. Eligible applicants were randomly assigned to receive one of three loan offers:

- *Grace-period loan (Salaf El Karaoui = SEK)*: Borrowers pay only monthly interest during a five-month grace period, after which they repay both interest and principal in constant monthly installments for the remainder of the loan. This represents a five-fold increase in the grace period relative to the standard loan.[4]
- *Tailored loan (Salaf El Karaoui Plus = SEK+)*: The repayment schedule is split into three periods of equal length. The borrower and loan officer jointly determine the monthly installment in each period to match the borrower's expected cash-flow cycle. Installments can vary substantially across periods (e.g., low–medium–high or medium–low–high), subject to a floor equal to the monthly interest due. The loan officer uses a dedicated spreadsheet tool to simulate repayment schedules during the loan application process.
- *Standard loan (control)*: Al Amana's existing individual-liability loan with equal monthly installments throughout the loan cycle.

[4]The five-month duration reflects a balance between agronomic considerations and the lender's operational constraints. It was informed by consultations with agricultural experts and a market scan of comparable MFIs, which together pointed to the length of a typical production cycle, and was capped following discussions with Al Amana management, who were concerned that longer grace periods would weaken the contact between loan officers and clients.

2.1.1. Implementation and timeline

All 40 participating rural branches were selected from Al Amana's network of 301 rural branches based on three criteria: a minimum of two permanent staff members (excluding the account manager), adequate recent loan-disbursement performance (at least 65% of monthly objectives met over the preceding two years), and portfolio-at-risk below 10%. Branches were further screened for their ability to implement the experimental protocol.

The intervention was rolled out in a staggered fashion. The first wave of baseline surveys and randomization (Group 1) ran from April 2018 through November 2019. The onset of Covid-19 suspended field operations in March 2020, delaying Group 1 endline surveys and postponing the launch of the second wave; operations resumed in March 2021 with the Group 2 baseline, which continued through March 2022. Endline household surveys were administered at the respondent's home, targeting an interval of approximately 18 months after baseline (an interval that was lengthened for Group 1 by the pandemic suspension). Group 1 endline surveys took place between June-October 2021, and Group 2 endline surveys in December 2023 – March 2024. In total, 3,068 individuals were randomized across the 40 branches. Figure 1 provides the complete timeline.

2.1.2. Randomization

Upon arrival at a participating branch, an eligible applicant first completed a baseline survey administered by an independently hired surveyor embedded in the branch (Group 1) or by the branch's account manager (Group 2). After the baseline interview, the branch's account manager registered the applicant in a pre-programmed randomization tool (an Apache OpenOffice spreadsheet installed on the account manager's computer). The tool randomly assigned the applicant to one of the three treatment arms and included validation checks to prevent the allocation from being overridden or modified after the fact. The loan officer was then discreetly informed of the assigned loan type and instructed to present

and promote the assigned product to the applicant.

Compliance with the randomization was imperfect by design: participants assigned to a made-to-measure product could decline it in favor of the standard contract or choose not to borrow at all, while control-group applicants were never informed of the new products' existence. In the tailored-loan arm, some borrowers who were offered the tailored product (SEK+) selected a repayment schedule with essentially flat installments across the three periods, making their contract functionally equivalent to a standard loan. We discuss how we handle these cases when presenting take-up results in Appendix B.1.

We expected spillovers between individual applicants to be minimal in Part A. Branches typically received only two to three loan applicants per day, limiting direct interaction among participants. Loan officers were instructed to track whether applicants proactively asked about loan products other than the one assigned to them; such inquiries were essentially absent throughout the experiment.

2.2. Part B: Village-level randomization

Part B evaluated demand for the new loan products in the broader population beyond those who would visit a branch unprompted. We selected 20 rural Al Amana branches for Part B, seven of which also participated in Part A and thirteen of which were new. For each branch, we identified surrounding villages with a significant number of potential borrowers based on Al Amana's administrative data and in consultation with branch staff, yielding a total of 440 villages. The number of villages per branch ranged from 11 to 55, reflecting variation in the density of potential borrowers across catchment areas. Figure C.2 shows the geographic distribution of the participating Part B branches and study villages, together with their randomized treatment assignments.

Within each branch, villages were randomly assigned in a fixed ratio to one of four information arms: pure control (3/11), standard information (2/11), SEK information (3/11), and SEK+ information (3/11). Within the SEK and SEK+ arms, villages were further cross-

randomized into a norms-shifting sub-arm (1/11 of villages, 40 per product) and a non-norms sub-arm, yielding six conditions in total. The norms-shifting sub-arm added to the loan-specific informational video with a set of three short client testimonials in which borrowers described their personal experiences with microcredit. These testimonials were selected for their social proximity to the target audience, with the aim of shifting descriptive norms around borrowing by demonstrating that people in comparable rural circumstances had already taken out and successfully repaid Al Amana loans. For the primary analysis, we pool the norms and non-norms sub-arms within each product, giving four groups:

- *Control* (120 villages): No additional promotional activity beyond Al Amana's standard outreach.
- *Standard information* (80 villages): Information campaign promoting Al Amana's standard loan.
- *SEK information* (120 villages): Information campaign promoting the grace-period loan.
- *SEK+ information* (120 villages): Information campaign promoting the tailored loan.

In the three information-campaign groups, a promotional team contracted for the study, and trained on Al Amana's loan products, conducted door-to-door visits to provide information about the loan type assigned to the village.[5] Promotional visits took place approximately every two weeks. The inclusion of the pure-control group (no visits) alongside the standard-information group (visits promoting the existing product) allowed us to disentangle the effect of the new loan offers from the effect of the promotional campaigns themselves. Importantly, what was randomized was only the information provided during

[5]After the initial household visit, consenting participants received up to three follow-up messages via WhatsApp (or SMS for those without WhatsApp access) sent approximately one week, five weeks, and two months after the visit. Each message reiterated the loan product information presented during the visit, provided the address of the nearest Al Amana branch, and included brief survey questions on whether the participant had visited the branch or taken out a loan.

the campaigns; any individual who visited a branch spontaneously could request any available loan type regardless of their village's assignment.

After a pilot of promotional visits in June 2023, village selection and randomization were finalized, and the roll-out of the information campaigns ran from late October 2023 to early February 2024.

3. Data

3.1. Individual-level intervention (Part A)

Our analysis draws on two main data sources: household surveys administered to Part A participants at baseline and endline, and administrative records from Al Amana's management information system (MIS) covering loan contracts, repayment schedules, and borrower characteristics. We describe each in turn.

3.1.1. Survey data

After providing informed consent to participate in the study and for their survey data to be collected and used for research, each applicant completed the baseline interview. During the randomization step, the account manager registered each participant in the pre-programmed randomization tool. Surveyors separately collected contact details to facilitate endline follow-up.

The baseline was administered in two waves corresponding to the staggered roll-out of the intervention. The Group 1 baseline survey ran from April 2018 through late 2019 and was administered by Axétudes, a professional survey firm contracted through Innovations for Poverty Action (IPA), using computer-assisted personal interviews (CAPI).[6] The average interview lasted 46 minutes. The instrument comprised the following sections: a household

[6]The survey instrument was programmed in French. Surveyors administered the questions in the Moroccan Arabic dialect (Darija) or in one of the local Amazigh dialects (Tashelhit, Tamazight, or Tarifit), depending on the respondent's mother tongue. All surveyors were fluent in French, Darija, and the relevant local dialect.

roster recording the demographic characteristics of each member, household economic activities, salaried employment, casual work, and other income sources, as well as an income-seasonality exercise in which respondents allocated their total annual income across the four quarters of the year; household assets, farm animals, and farm trees; and use of financial products and services, as well as a module on subjective expectations about future entrepreneurial profits.[7]

The Group 2 baseline survey ran from March 2021 through March 2022, after operations resumed following the Covid-19 interruption. Due to budget constraints, the Group 2 instrument was substantially shorter than the Group 1 survey and was administered on paper by trained Al Amana branch officers. It retained a simplified demographic section recording the respondent's gender, age, education, marital status, household size, and basic characteristics of the spouse and household head, but did not include a full household roster. The self-employment module collected only an activity-type checklist and total revenues in bracketed categories. Income from other sources was recorded as a simple yes/no indicator for each, with total household income again captured in brackets. The financial-services section was reduced to the number of outstanding formal and informal loans, without the loan-by-loan detail or the subjective-expectations module of Group 1. The asset inventory, the employee sub-modules, and the income-seasonality exercise were omitted entirely. These differences in baseline data coverage across groups mean that the set of candidate control variables for PDS-LASSO selection is restricted to those available in both survey instruments (see Section 4). In total, 3,068 individuals completed the (pre-randomization) baseline survey across the 40 participating branches; 2,667 were re-interviewed at endline roughly 18 months later.

Endline surveys were administered at respondents' homes approximately 18 months after their baseline interview, following the staggered phase-in of the intervention. Group 1 endline surveys took place between June 2021 and October 2021; Group 2 endline surveys

[7]We measure annual sales, expenses, and profits from household own-account activities using the Aggregating, Anchoring, and Adjusting (AAA) elicitation method of Anderson et al. (2021).

took place in 2024. The endline instrument was more comprehensive than the baseline and included detailed modules on household composition, personal and professional assets, self-employment activities (with separate profit elicitation modules for agriculture, livestock, and non-agricultural enterprises), wage and casual labor income, food and non-food consumption, formal and informal credit, financial situation, perceived stress, life satisfaction, and shocks. We discuss attrition in Section 3.3.

3.1.2. Administrative data

Al Amana provided administrative records for survey respondents. These records contain information on loan contracts and repayment schedules, including the loan amount, duration, disbursement date, contractual installment amounts and due dates, and the actual repayment history. We further received administrative data from Al Amana's loan management system, including the loan eligibility assessment completed prior to admitting each client. These records cover client demographics, business characteristics, and financial indicators from the pre-loan assessment (sales, profits, net revenue, working capital, stock value, business and household assets, personal contribution, and repayment capacity), in addition to the contract terms themselves.

These administrative records allow us to construct precise measures of contract characteristics, such as the degree of tailoring in the repayment schedule, whether a grace period was effectively implemented, and the timing and extent of any repayment delays, without relying on self-reports. The pre-loan assessments of client characteristics and financials enable us to characterize the profile of clients who select into each contract type and to further explore compliance patterns.

A second round of administrative data was extracted in October 2024 for the Part B village-level intervention. This extraction covered all loan disbursements in the catchment areas of the 20 Part B branches during the observation period, enabling us to measure village-level take-up, loan amounts, repayment performance, and borrower characteristics.

3.2. Village-level intervention (Part B)

Part B relied primarily on administrative data rather than household surveys. In addition to the administrative records described above, we collected two supplementary data sources.

First, the promotional teams recorded detailed information on each door-to-door information session conducted in the 320 Part B villages that were visited. For each session, the teams documented the number of attendees, the duration of the visit, and participants' baseline engagement with credit, including whether they already held a loan, whether they intended to borrow within the next three or six months, and which of the three loan products (SEK, SEK+, or standard) they preferred. Villages received a median of 20 sessions, with an average of 29 total attendees across all sessions per village. These data allow us to measure the reach and intensity of the information campaigns and to verify that they were implemented with comparable effort across treatment arms.

Second, we conducted phone follow-up surveys with 1,432 loan applicants from Part B branches following the promotion period. The goal was to collect borrower characteristics and loan preferences not captured in the administrative records. The survey covered demographics, economic activities across 13 sectors, employment status, asset ownership, current and desired loan products, and self-reported wealth. We also recorded the village of origin of each respondent, allowing us to match individual applicant characteristics back to the village-level randomization and thus to characterise the pool of clients drawn to each loan product by treatment arm. Of the 1,432 respondents successfully contacted, 1,251 (87%) consented to participate. Of the latter, 458 (about a third) could be matched to a study village in a treatment or control arm. Of those 458 respondents, 174 (38%) signed at least one loan contract after the promotion campaigns began.[8]

[8]This phone follow-up survey was added after the Stage 1 report and served two purposes. First, it collected demographic and activity characteristics not consistently available in the administrative records. Second, it provided the prospect and client-to-village crosswalk used to link Al Amana contracts to study villages. Consequently, village-level contract counts include only borrowers who could be matched through the phone-survey roster and should be interpreted as lower bounds on total loan take-up.

3.3. Balance and attrition

We verify that randomization was successful in both parts of the study. Table 1 reports baseline balance for Part A across the SEK, SEK+, and standard arms, controlling for branch fixed effects. Omnibus F-tests fail to reject joint equality of covariates across the individual treatment arms. For the pooled any-treatment indicator, the omnibus p-value is 0.089 in the full sample and 0.052 in the endline sample, driven by scattered differences in sector composition and informal borrowing. Among roughly 30 covariates, only a handful are individually significant (including cereal and vegetable growing in the SEK arm; informal borrowing, commerce, and salaried labor in the SEK+ arm; and pension receipt in both) consistent with chance under successful randomization. All shared baseline covariates enter the PDS-LASSO candidate set used in the main regression specifications, so any predictive imbalance is absorbed by the controls.

Tables C.19 and C.20 report balance for Group 1 on the shared and extended covariate sets, respectively (the latter include business outcomes, subjective expectations, asset holdings, and enterprise characteristics). Omnibus p-values across all panels are well above conventional thresholds, and the extended Group 1 variables show no individually significant imbalances. One covariate warrants attention: the share of SEK+ respondents engaged in commerce is 8 percentage points higher than in the control group among Group 1 participants (Table C.19, Panel C). Because commerce engagement strongly predicts business outcomes, we verify that it enters the PDS-LASSO-selected covariate set for all business and income regressions. Residual imbalance on this variable is therefore absorbed in the main specifications.

Attrition at endline is low and broadly balanced: 13% of the sample was not re-interviewed, with no significant differential attrition across treatment arms (Table 1, Panel E). Among Group 1 participants, the SEK+ arm shows a marginally higher attrition rate of 4.4 percentage points relative to the control group ($p < 0.10$; Table C.19, Panel E), which we note as a caveat when interpreting SEK+ effects within that subgroup. Omnibus F-tests on the

endline-restricted sample do not suggest that attrition is systematically related to the pre-randomization covariate vector in any arm.

We now turn to balance for the village-level intervention (Part B). Table 11 verifies the balance of the village-level randomization. Villages contain on average 97 households, are located approximately 9.7 km from the nearest Al Amana branch, and 44% are classified as dispersed settlements (i.e., consist of scattered individual dwellings rather than grouped housing). Means are similar across all six experimental arms. Out of 45 pairwise comparisons, only two are statistically significant. Relative to the control group, branch distance is 2.25 km greater in the SEK+ information-plus-norms arm, and the share of dispersed villages is 13 percentage points higher in the Standard information-only arm. This is consistent with what would be expected by chance at conventional significance levels and confirms that the village-level randomization achieved adequate balance on observable pre-treatment characteristics.

4. Empirical strategy

4.1. Individual-level intervention

For Part A, we estimate intention-to-treat (ITT) effects of being offered either (i) a five-month grace-period loan (SEK) or (ii) a tailored three-bracket repayment loan (SEK+), relative to the standard loan offer. The observational unit is the household in the endline survey. Our main specification is:

$$Y_{ib} = \alpha + \beta_1 T_{ib}^{\text{SEK}} + \beta_2 T_{ib}^{\text{SEK+}} + \gamma X_{ib}' + \mu_b + \varepsilon_{ib}, \tag{1}$$

where Y_{ib} is an outcome for household i serviced by branch b; T_{ib}^{SEK} and $T_{ib}^{\text{SEK+}}$ are treatment dummies; X_{ib}' is a vector of baseline covariates selected by post-double-selection (PDS) LASSO (Belloni et al., 2014); and μ_b are branch fixed effects. We estimate equation (1) by

ordinary least squares, with heteroskedasticity-robust standard errors.

Because the Group 1 and Group 2 baseline instruments differ in scope — the Group 1 survey collected detailed business outcomes, subjective expectations, assets, and enterprise characteristics not administered in Group 2 — we run the PDS-LASSO selection on the pooled sample, using only variables available in both instruments. Variables collected only in Group 1 enter the Group-1-specific analysis (Table C.10) but not the main specifications. Appendix B provides detailed definitions of the main Part A outcomes.

Because many outcomes are examined, we guard against false positives using a standard template applied to each outcome family (i.e., table). Specifically, we report: (a) sharpened false-discovery-rate q-values following Anderson (2008); (b) randomization-inference p-values based on 1,000 permutations following Young (2019); and (c) Westfall–Young stepdown-adjusted p-values (Westfall and Young, 1993) for the model-wise null (that all treatment coefficients in a given equation are zero), the coefficient-wise null and the experiment-wise null (that all treatment coefficients across the table are zero).

4.2. Village-level intervention

Part B evaluates how loan-type-specific information campaigns affect village-level take-up and borrower composition. Around each of 20 rural branches, villages were randomly assigned to one of four arms: a campaign promoting the SEK contract, the SEK+ contract, the standard contract, or a pure control (no campaign). In a subset of treatment villages, the campaign was cross-randomized with a norms-shifting module; our primary village-level specifications pool across this dimension, and we report the norms results separately in Appendix C (Tables C.22 and C.23). The main specification is:

$$Y_{jb} = \alpha + \beta_1 T_{jb}^{\text{SEK}} + \beta_2 T_{jb}^{\text{SEK+}} + \beta_3 T_{jb}^{\text{STD}} + X'_{jb}\gamma + \mu_b + \varepsilon_{jb}, \tag{2}$$

where Y_{jb} is a village-level outcome for village j in the catchment area of branch b (for instance, the number of signed loan contracts or the number of prospects identified during promotional visits). T_{jb}^{SEK}, $T_{jb}^{\text{SEK+}}$, and T_{jb}^{STD} are indicators for the three information-campaign arms, with the pure-control group (no campaign) as the omitted category. X'_{jb} is a vector of pre-specified village-level covariates: a dispersed-settlement indicator, distance to the nearest *Al Amana* agency (km), and approximate village population (number of households). μ_b are branch fixed effects and standard errors are heteroskedasticity-robust. We estimate (2) by OLS, absorbing the branch fixed effects. Unlike Part A, we do not apply PDS-LASSO for covariate selection: the village-level sample is modest (N=440) and the set of candidate pre-treatment covariates is limited.

5. Results

5.1. Individual-level intervention

5.1.1. Loan take-up and ITT effects

Loan take-up. Individuals visiting the branch to apply for a standard loan (whether new or former clients) were randomly assigned to one of three offers: SEK (grace-period), SEK+ (tailored), or the standard loan (our control group). After receiving the assigned offer, individuals remained free to decline it and choose a different loan product, or opt not to take any loan. The take-up rates for each loan type, which reflect individuals' revealed preferences on the loan choice set [9], are reported in Table 2.[10] In the SEK arm, 29% of respondents signed this grace-period contract (Column 1), while 52% opted for a standard loan instead (Column 3). In the SEK+ arm, 32% signed a tailored contract (Column 1) and 50% reverted to a standard loan (Column 3). Cross-contamination between the two

[9] Al Amana's credit officers may also have influenced take-up by screening out prospective clients with insufficient repayment capacity.

[10] For this specific equation, we estimate the regression without a constant, so that each coefficient represents the within-arm mean of the corresponding take-up indicator.

made-to-measure products is minimal: only about 1% of respondents in either treatment arm signed a contract of the other made-to-measure type. In the standard arm, take-up of the standard loan is 83% (Column 3), with negligible take-up of SEK or SEK+ contracts.

Overall take-up of any loan (Column 4) is high and balanced across the three treatment arms: 81% in the SEK arm, 83% in the SEK+ arm, and 84% in the standard arm. The similarity of these rates shows that the offer of a made-to-measure loan does not discourage individuals who would otherwise take a standard loan from borrowing altogether (consistent with the monotonicity assumption in an encouragement design). Overall, these results indicate that demand for these made-to-measure products is far from universal and that clients interested in these loans (when they are offered to them) represent only a subset of those visiting the branch for a standard loan.

Entrepreneurial activity. We now turn our attention to the economic effects of the access to made-to-measure loans on the own-account activities of the household (in agriculture, livestock, or managing a small business). Table 3 reports ITT estimates on the sales, expenditures and profits of these activities at endline. It is worth noting that, at that time, only about a third of control-group households reported any positive sales (35%) or profits (31%), reflecting that a substantial share of the sample included in the experiment did not operate an own-account activity. This is a sharp decline from what we observed at baseline, when 69% of households reported total own-account sales above 10,000 Dh over the preceding twelve months.

Being offered the grace-period loan (SEK) increases annual sales (Column 1) by 4,786 MAD and profits (Column 3) by 2,896 MAD, though neither estimate is significant at conventional levels. In contrast, being offered the tailored loan (SEK+) leads to significantly larger effects: sales (Column 1) increase by 7,897 MAD (40% of the control mean) and profits (Column 3) by 4,895 MAD (51%).[11] The effects on expenses (Column 2) are positive but

[11]The Westfall–Young joint test (Column 4) rejects the null of zero effects across both treatment arms for sales and profits, while the experiment-wise test across all six coefficients yields a p-value of 0.075. Sharpened FDR q-values for the SEK+ arm remain below 0.05 for both sales and profits.

smaller in magnitude, suggesting that the increase in business scale is partly driven by activities with higher revenue-to-cost ratios.

To decompose the effect on own-account activities by sector, Appendix Table C.1 re-estimates the main specification separately for crop farming, livestock, and non-agricultural activity. The SEK+ treatment effect is concentrated almost entirely in off-farm enterprise and is driven by high-frequency sales which rise by 6,320 MAD and associated expenses by 2,687 MAD, while crop and livestock coefficients are small and statistically indistinguishable from zero.

In Appendix Table C.6, we show in exploratory extensive-margin analyses that the different loan offers did not affect significantly the probability to operate an own-account activity. All results shown in Table 3 are thus primarily driven by impacts on the intensive margin.[12] Access to the SEK+ loan contract thus appears to allow households to scale up activities they already operate (predominantly off-farm ones) rather than induce entry into new lines of business.

Figure C.3 examines the distributional heterogeneity behind the average effects displayed in Table 3 using inverse-probability-weighted quantile regressions. These are estimated over quantiles 0.65–0.975, which corresponds to the portion of the distribution where households report positive business activity. For the SEK arm (Panel A), point estimates are close to zero through about the 90th percentile for all three outcomes and rise only at the very top of the distribution, with confidence intervals that widen sharply and include zero throughout. The SEK+ arm (Panel B) exhibits a stronger pattern, with 95% confidence bands for the sales and expense coefficients excluding zero over larger portions of the tail. The profit panel for SEK+ is noisier, but point estimates turn clearly positive above the 90th percentile. Taken together, the figure indicates that the mean ITT effects reported in Table 3 are driven predominantly by households in the upper tail of the business-outcome distribution, and that this concentration is more pronounced and more

[12] Table C.7 shows no detectable shift in household participation in any of the twelve disaggregated activities, except for a 3.2 percentage-point increase in arboriculture under SEK.

precisely estimated for the SEK+ arm.

Household income. Beyond the effect on income from own-account activities, Table 4 examines the impact on total household income. This is decomposed into salary income, occasional wages, other (non-wage) income, and profits from own-account activities (previously reported in Table 3). Average annual income in the control group is 34,855 MAD at endline, of which salaried and occasional wages together account for 62%, profits from own account activities 28%, and other income 10%. The tailored loan (SEK+) increases total income by 3,127 MAD (9% of the control mean), though this is not statistically significant. The gain is driven by the increase in own-account profits, partially offset by (non-significant) declines in occasional wages and a significant reduction in other income. Salary income is essentially unchanged. The grace-period loan (SEK) shows a qualitatively similar but weaker pattern with a (non-significant) increase in profits and a fall in occasional wages (and no discernible effect on total income). The decomposition reveals that the offset to higher profits for the SEK+ loans comes primarily from occasional wage income rather than salaried employment, suggesting that SEK+ borrowers substitute away from casual day labor toward self-employment, consistent with the pattern documented by Crépon et al. (2015) for the same Moroccan context. The significant decline in other income under SEK+ (a heterogeneous category including pensions, remittances, gifts, and asset sales) may reflect a reduced reliance on informal transfers and social safety nets as own-account profits rise (Cox, 1987; Cox et al., 2004).

Time allocation. Household time-allocation estimates (Table 5) show patterns consistent with these results: the decline in occasional wage income is mirrored by a reduction in casual labor days, while the increase in own-account profits is accompanied by a modest rise in off-farm work time. The results suggest that borrowers offered tailored loans substitute away from casual day labor toward self-employment.

Investments. In Table 6, we assess whether the effects on profits (or lack thereof) are

driven by differential investment in productive assets. Equipment investment is rare in this population: fewer than 1% of control households report any purchase of general, agricultural, livestock, or non-agricultural equipment. Animal purchases are more common (7% of control households) and account for nearly all of the 6,670 MAD average total investment in the control group. We find no evidence that either loan offer increases investment.

Since the endline survey was administered more than 18 months after loan disbursement, purchases made shortly after receiving the loan fall outside the 12-month investment recall window. We therefore complement the investment analysis by examining treatment effects on business asset ownership at the extensive margin, assessing whether either loan offer affects the probability of owning any assets across activity categories, as well as the probability of owning animals (Table C.5). For business and livestock assets, neither the SEK nor the SEK+ offer significantly affects the probability of ownership in most categories: coefficients are small and statistically insignificant for general, agricultural, livestock, and total business equipment, as well as for animals.

An exception is non-agricultural business equipment, where the SEK+ offer raises the likelihood of ownership by 3.1 percentage points, a 42% increase relative to the control mean of 7.3%. This pattern is consistent with SEK+ borrowers acquiring small-scale tools early in the loan cycle that appear in the ownership stock at endline but are no longer captured as recent investments. This effect survives at the joint Westfall–Young level (p = 0.041), though the row-wise correction across all outcomes yields a p-value of 0.106. These results, although modest, help explain the sectoral decomposition of business outcomes, which showed that SEK+ effects are concentrated in off-farm enterprise.

Overall, the absence of meaningful effects on investments and asset ownership may reflect that profit gains are channeled into working capital rather than fixed asset accumulation. It also suggests that the flexibility offered by tailored or grace-period loans does not lead clients to invest significantly more in lumpy assets than standard loans do.

Subjective profit expectations. Appendix Table C.8 reports the preregistered analysis of re-

spondents' expectations about future profits. Across crop farming, livestock breeding, and off-farm activities, neither loan offer changes expected profits under extremely successful or extremely unsuccessful scenarios, or the perceived probability that profits over the next 12 months exceed the last observed level. These estimates should be interpreted cautiously. The questions were administered only to households operating in the relevant sector at endline, resulting in smaller, sector-specific samples, and coverage of the off-farm module was particularly limited among early Group 1 interviews because it was not administered consistently during the initial survey rollout. Appendix A provides further details on these implementation and measurement limitations.

Household consumption. In Table 7, we display the ITT estimates on household consumption. Average annual consumption in the control group is 43,405 MAD, food representing 57%, non-food expenditures 40%, with durables accounting for less than 1%. Neither loan offer has a detectable effect on any consumption component. The absence of consumption effects despite significant profit gains (Table 3) is consistent with the broader microcredit literature, where treatment effects on business outcomes often do not translate into higher consumption (Banerjee et al., 2015). A possible explanation is that clients reinvest the profit gains into working capital.

Loan characteristics and repayment. After examining the effects of the two new loan types on clients' economic outcomes, we now turn our attention to their impact on loan contract characteristics and repayment performance. Table 8 first examines whether the offer of made-to-measure loan products affected the characteristics of the contracts that borrowers ultimately signed. The sample comprises 2,219 study participants who took out at least one loan with Al Amana during the study period, with outcomes measured entirely from administrative records. Neither treatment arm significantly affected the number of contracts signed or the gross loan amount disbursed. In contrast, both treatments significantly extended loan duration. The SEK offer increased duration by 0.62 months and the SEK+ offer by 0.89 months, relative to a control mean of 19.48 months.

The most pronounced result concerns borrowing costs. The SEK treatment increased the interest-to-principal ratio by 1.44 percentage points compared to a control mean of 16.53 %. The SEK+ treatment, by contrast, shows a smaller and insignificant increase of 0.48 percentage points. Taken together, the results indicate that these product innovations operated primarily through the terms of borrowing, duration and cost structure, rather than through its scale. This is consistent with the design intent: the SEK and SEK+ products modify *how* borrowers repay rather than *how much* they borrow.

As an exploratory descriptive analysis of realised SEK+ schedules, Table C.3 (Columns 4–6) further decomposes the repayment schedule for the SEK+ product, as borrowers had to choose where to place low, medium, or high installments within the schedule. The vast majority of SEK+ borrowers selected a high first-period repayment level (197 of 309 contracts with available schedules), suggesting a preference for front-loaded repayment and lower borrowing cost. This is consistent with earlier results on investment behavior. These borrowers do not use the flexibility to purchase lumpy assets and instead expand activities that generate immediate returns. Borrowers who elected a low first-period level took out larger loans (MAD 11,186 versus 7,918 for high), chose longer maturities (22.4 versus 18.8 months), and faced substantially higher effective borrowing costs (22.4 versus 15.2%). Indeed, the low-level SEK+ contracts closely resemble SEK grace-period contracts along every dimension, which is unsurprising: both designs defer early principal repayment, generating similar amortization profiles.

Table 9 examines whether the offer of made-to-measure loan products affected repayment performance for the subset of respondents (around 80%) who borrowed with Al Amana during the study period. The probability of completing repayment 30 or more days past the scheduled end date is low across all arms and unaffected by either treatment. Similarly, the number of unpaid installments shows no significant treatment effects; nearly half of control borrowers experienced at least one unpaid installment during the loan cycle, indicating that temporary payment delays are common in this setting regardless of

contract type. These results are reassuring from the lender's perspective: relaxing the rigidity of the repayment schedule generated no deterioration in on-time contract completion or installment regularity.

The days-versus-schedule measure, however, reveals a finer-grained difference. Both treatment arms show point estimates suggesting earlier completion (that is, prepayment): –14.65 days for SEK and –14.20 days for SEK+, relative to a control mean of –78.76 days. Borrowers in all arms typically complete their contracts ahead of schedule, but those offered made-to-measure products do so by roughly two additional weeks. While modest relative to the large early-completion margin in the control group, this shift is consistent across both treatment arms. Taken together, the results provide no evidence that the made-to-measure products, despite their more flexible repayment structures, worsened loan performance, and point toward marginally faster contract completion.

5.1.2. Complier characteristics and selection patterns

Take-up of the flexible loans was similar in both the SEK and SEK+ treatment arms (Section 5.1.1). However, we find positive and significant effects only for the SEK+ loans, although we cannot reject equality between the two treatments. This difference does not appear to reflect investment strategies or repayment performance, so we instead examine whether selection into the loans differs across arms.

Tables 10 and C.4 characterize the compliers of the two treatments, using the baseline survey and administrative data, respectively. SEK compliers are more likely to engage in crop farming (49.7 versus 33.0% among non-compliers), cereal and vegetable growing, and poultry- and livestock-related activities. The administrative data corroborate this: SEK compliers are 9 percentage points more likely to be classified as working in the agricultural sector by loan officers, report roughly 23,000 MAD more in household assets, and show substantially higher monthly sales and purchases than SEK non-compliers.

SEK+ compliers present a very different profile. They are more likely to be female

(30.4 versus 22.4%), less likely to be the household head, and more likely to operate crafts activities (around 16.6 versus 9% in both survey and administrative data). Financially, SEK+ compliers exhibit lower measured repayment capacity (1,468 versus 1,604 MAD per month), lower business assets, lower company equity, and lower fixed assets than SEK+ non-compliers, and their stated loan use (at the start of the contract) tilts from merchandise purchase toward fixed-asset investment (22 versus 13%). The two complier groups thus correspond to distinct margins: the grace-period contract (SEK) attracts agricultural households whose cash flows are thin early in the cycle relative to harvest revenue, while the tailored schedule (SEK+) attracts a more capital-constrained, investment-oriented clientele for whom flexibility over installment size relaxes a binding cash-flow constraint.

A natural concern is whether the sub-sample of borrowers who never sign any contract ("never-takers") is itself balanced across treatment arms. Table C.2 reports mean differences across arms for this group. Most characteristics are individually balanced at conventional levels, but the omnibus F-tests indicate some imbalance: $p = 0.051$ for SEK versus control and $p = 0.002$ for the pooled "any treatment" comparison, driven largely by a younger age profile and somewhat higher remittance income among never-takers in the treated arms. Because our main specification is intent-to-treat, this imbalance does not bias the headline estimates, but it cautions against interpreting the complier-level decomposition above as a structural object.[13]

5.1.3. Heterogeneity analysis

Appendix Table C.9 interacts the treatment dummies with respondent gender and with baseline activity indicators for crop farming, livestock breeding, and off-farm activities (as planned in our pre-registered report). The off-farm pattern persists (Columns 7 and 8): the SEK+ $\times$ Off-farm interaction is positive and marginally significant for both sales (12,104 MAD) and profits (7,570 MAD), implying that the households driving the aggregate

[13]The LATE-style reading implicitly conditions on a never-taker population whose composition is not perfectly symmetric across arms.

result are those already engaged in a non-agricultural enterprise at baseline. The SEK results are similar in magnitude and pattern: the SEK $\times$ Off-farm interaction is marginally significant for sales (10,619 MAD), though not for profits.

The gender-split analysis shows that SEK+ raises sales by 10,189 MAD and profits by 6,909 MAD for the omitted (male-respondent) category, significant at the 5 and 1% levels respectively, while the SEK+ $\times$ Female interaction is negative and significant for profits (–7,887 MAD). The implied effect of SEK+ for female-respondent households is essentially zero. Again, the SEK results are qualitatively similar but not significant.

Appendix Table C.10 reports the remaining preregistered heterogeneity analyses for Group 1. SEK+ effects are larger among households with more uneven quarterly income before baseline, which is consistent if this variation is persistent and predictable with tailored schedules being more valuable when cash-flow timing is uneven. By contrast, effects decline with the coefficient of variation of expected profits, which may capture forward-looking uncertainty that a repayment schedule fixed ex ante cannot insure against. The positive interaction with the absolute profit range may partly reflect business scale rather than uncertainty. Given the smaller samples and differing constructions of these measures, we view the results as suggestive rather than definitive.

As a preregistered exploratory subgroup analysis, Appendix Tables C.17 and C.18 re-estimate the main specification separately on Group 1 and Group 2. The point estimates are larger in Group 1, which was administered the longer CAPI baseline, than in Group 2. In Group 2, point estimates retain the expected sign—SEK+ households report sales roughly 5,900 MAD and profits roughly 3,000 MAD higher than the control mean of 9,961 MAD—but standard errors are substantially larger and none of the coefficients reach conventional levels. Part of this attenuation likely reflects the fact that Group 2 was recruited and followed over a later window, with a larger share of their loan cycle and endline measurement falling in the post-COVID-19 period. This was a markedly more difficult business environment for rural Moroccan households, characterized by weaker demand, disrupted

supply chains, and successive drought years that depressed agricultural incomes. This attenuation is consistent with treatment take-up documented in Appendix Table C.16: compliance with the tailored loan is notably higher in Group 1 (41%) than in Group 2 (25%), while compliance with the grace-period loan is more stable across groups (31 versus 28%). Take-up of any loan remains balanced across treatment arms within both groups, so the group differential reflects substitution across products rather than differential entry into borrowing. The survey group with stronger compliance is also the one with larger ITT effects on household business activity.

5.1.4. Robustness

We probe the headline business results along three dimensions: replacing the PDS-LASSO specification with plain OLS, varying the winsorization threshold, and substituting the Aggregating, Anchoring, and Adjusting (AAA) (Anderson et al., 2021) aggregates with their self-reported counterparts. Across all three exercises the SEK+ point estimates remain positive and economically large, with the sole exception of the fully unwinsorized specification, where outlier influence renders the coefficients insignificant.

First, Appendix Table C.15 replaces the PDS-LASSO specification with an OLS regression conditioning only on branch fixed effects. The SEK+ coefficients are essentially unchanged in magnitude (9,138 MAD on sales, 3,638 MAD on expenses, and 5,500 MAD on profits) but are more precisely estimated. The near-identical point estimates indicate that the LASSO-selected covariates do not absorb treatment-correlated variation, and we retain PDS-LASSO as the preferred specification on preregistration grounds.

Second, Tables C.11 and C.12 re-estimate the main equation after winsorizing the AAA outcomes at the upper 2% or 1%, instead of the 5% in the main specification. Under the 2% rule, the SEK+ effects on sales (9,151 MAD), expenses (4,125 MAD) and profits (5,020 MAD) remain significant at the 10% level. Under the 1% rule, magnitudes are similar but standard

errors widen and only the expense coefficient clears conventional thresholds.[14]

Third, Appendix Table C.14 replaces the AAA aggregates with their self-reported counterparts elicited via direct survey items. Control means are roughly half the size of the AAA values (11,644 versus 19,948 MAD for sales; 5,058 versus 9,678 MAD for profits), in line with the well-documented tendency of direct recall questions to under-report enterprise activity in rural settings (De Mel et al., 2009). Treatment effects are correspondingly smaller in absolute terms but proportional to the control mean: SEK+ raises self-reported sales by 3,712 MAD, or roughly 32% of the control mean, against an analogous 40% in the main specification. None of the self-reported coefficients are individually significant, but the sign and relative magnitude pattern lines up with the AAA estimates, consistent with a measurement-error interpretation of the precision gap.

5.2. Village-level intervention

We first report implementation checks and exploratory manipulation-check outcomes before presenting the preregistered contract-take-up outcomes. Table C.21 reports results from the village-level promotional campaigns. Across 440 villages surrounding 20 rural branches, villages were randomly assigned to one of four information arms: pure control, standard loan information, SEK information, or SEK+ information. Within the SEK and SEK+ arms, villages were further cross-randomized into a norms-shifting sub-arm and a non-norms sub-arm. For the primary analysis we pool these, giving four groups. A total of 5,805 door-to-door promotion sessions were carried out. Campaign reach measured in terms of household visits show that the information campaign was well implemented (with an average of 18 households visited per village) and balanced across treatment arms.

[14] As a limiting case, Table C.13 reports the fully unwinsorized estimates. Without any outlier treatment, the coefficients are imprecise and statistically insignificant in both arms and the SEK+ sales effect falls to 769 MAD and the SEK sales coefficient turns negative. In this case a handful of extreme observations dominate both the point estimates and their standard errors (the control-group sales mean more than doubles, to 42,947 MAD). This sensitivity mirrors the tail-concentrated pattern in Figure C.3: the mean effect is carried by the upper tail of the outcome distribution, so restricting a small number of influential observations is what allows it to be estimated with reasonable precision. We therefore retain 5% winsorization as the main specification.

Villages assigned to the SEK and SEK+ information arms had somewhat higher shares of prior Al Amana borrowers among those visited, but the share of respondents expressing demand for a new loan within three months was balanced across arms (Column 5).

We first examine stated loan-type preferences using survey questions administered to campaign participants immediately after the promotional session, restricting the sample to villages assigned to one of the three information arms. Results show that all four SEK and SEK+ sub-arms reduce the share of respondents preferring a standard loan by approximately 61 percentage points relative to the control mean of 62.3%, and increase preference for the advertised product by a similar magnitude (Columns 6–8). Adding a norms-shifting component to the information campaigns does not meaningfully alter the magnitude of these preference shifts for either product.

Table 12 examines whether the village-level information campaigns translated into actual loan demand. The unit of observation is the village, and the outcomes combine the phone-survey crosswalk with Al Amana's administrative records. We report the number of prospective borrowers identified, the number and type of contracts signed, and the total gross amount disbursed. These measures cover only consenting respondents who could be linked to a study village and therefore constitute lower bounds on village-level credit activity.[15] In pure control villages, on average 0.76 prospects per village (Column 1) arrived following promotion campaigns, and 0.38 loans were signed (Column 2) over the observation window from the campaign roll-out through the October 2024 administrative extraction, though activity was even sparser: only 32% of control villages produced any prospects and 18% signed any contracts. None of the three information treatments generates a statistically significant increase in the number of prospects identified or total contracts signed. Point estimates on prospects are positive but small (0.25-0.55 additional prospects) and only for SEK villages marginally statistically significant. These counts

[15]The number of prospective applicants in Column (1) was not specified as an outcome in the PAP. We report it as a non-pre-registered intermediate measure of implementation and movement through the borrowing funnel, capturing whether the information campaigns generated interest among prospective borrowers before any loan contract was signed.

are restricted to phone-survey respondents who consented and could be matched to a study village, and therefore represent a lower bound on actual take-up rather than the full disbursement rate. The null extends to aggregate lending volume. All three estimates are imprecise and statistically indistinguishable from zero. Thus, the campaigns do not detectably affect either the number of matched contracts or the total amount of credit disbursed to matched borrowers.

Columns (3)–(5) decompose signed contracts by product type. No information treatment succeeds in increasing the number of contracts signed. In the pure control group, it is worth noting that demand from the studied villages is very low.[16] Most contracts signed in all arms are conventional Standard loans, and no treatment arm shifts the composition toward the advertised tailored products: no SEK+ contracts were signed by a borrower from a study village, and SEK contract counts remain negligible across all arms. The absence of an effect on take-up is consistent with field reports that loan officers were not actively promoting the tailored products at the branch level (unlike in Part A) despite having been trained on the new loans.

Table 13 examines whether the information treatments shifted the composition of clients who ultimately signed a loan along observable traits. The unit of observation remains the village, and outcomes are village-level counts of clients with each demographic or activity characteristic, constructed from the phone follow-up survey of signed borrowers. Consistent with the null effects on overall take-up reported in Table 12, none of the three information arms produces an economically or statistically meaningful change in the demographic composition of clients. Point estimates on new clients, female clients, clients above the sample median age, and clients with at least primary education are small relative to their control group means (Columns 1-4). The only coefficient that reaches conventional significance is a 0.09 reduction in new clients in SEK+ villages (Column 1), which tracks

[16] Al Amana's administrative data show that the median branch disbursed roughly 120-170 loans per year, with a gradual decline visible after 2019, indicating that low take-up of the standard contract is a system-wide pattern across Al Amana's rural operations rather than a feature specific to the study villages.

the slightly lower overall contract counts in that arm rather than a compositional shift. Columns (5)-(7) examine the principal economic activity declared by clients at the time of the phone survey, and Columns (8)-(10) examine the activity for which they expressed interest in borrowing. Across both sets of outcomes, coefficients on all three treatment arms are small, uniformly insignificant, and not systematically signed, with the only exception being a 0.09 reduction in crop-farming clients in Standard villages. Taken together with the take-up results, this suggests that the information and norms-shifting campaigns neither expanded the pool of borrowers nor redirected it toward specific demographic or sectoral margins.

6. Discussion

Our two-part experiment was motivated by the low demand for microcredit in rural Morocco (Crépon et al., 2015). Among the many possible explanations (see introduction), we focused on two: that potential borrowers lack information about the existence and benefits of microcredit, and that the standard products on offer are poorly matched to the cash-flow patterns of typical economic activities. We tested whether better information can unlock untapped demand for standard microcredit or for more flexible loans, and measured the impacts of (ex ante) flexibility on those who take it up.

The village intervention (Part B) shows that more information on standard or tailored loans shifts stated preferences but has a near-zero effect on actual take-up, for neither product. Taking this at face value, we infer that demand for microcredit in this population is inherently low and that repayment-schedule rigidity is not the primary constraint on borrowing (particularly since the campaign was well implemented, as the shift in stated preferences shows). That said, a more intensive (and hence costlier) intervention might be needed to translate updated beliefs into behavioral change. Our null should therefore be read as an upper bound on the demand response achievable by scalable, low-cost information campaigns: even a well-implemented campaign of this kind moves stated

preferences but not take-up.

The lack of effect may also reflect the targeting of our intervention: Part B addressed a general population in which the share of interested individuals may be low to begin with and for whom the added value of loan flexibility is marginal. Part A overcomes this concern by conditioning on revealed willingness to borrow. When we randomly offer either the grace-period (SEK) or the tailored-repayment (SEK+) loan to applicants who had already sought out a standard loan, take-up rates reach around 30%. Demand is far from universal, but this confirms that among those with a demonstrated interest in credit, a meaningful share prefer better fitting repayment schedules.

The SEK+ loan has significant effects on self-employment revenue and profits, while the SEK effects are more muted. We should not overinterpret this difference (we cannot reject equality of the two treatment effects), but it likely reflects that, despite similar take-up of around 30%, the two products draw markedly different compliers. SEK+ compliers are poorer, hold fewer assets, and are less involved in agriculture and livestock but more engaged in crafts, relative both to non-compliers in their assigned group and to SEK compliers. SEK compliers, by contrast, are wealthier, hold more assets, and are more agricultural.

A further interesting pattern is that the compliers of the SEK+ loans are predominantly choosing higher installments early in the repayment schedule, suggesting a preference for front-loaded repayment and lower borrowing cost. This raises the question of what types of economic activities or investment are compatible with immediate returns. The decomposition by activity and the heterogeneity analysis both point to non-agricultural activities as the main driver of our results: effects are concentrated among clients who owned an off-farm activity at baseline, particularly those with high-frequency sales. Overall, these results suggest that flexibility is used to improve repayment conditions rather than to invest in large, lumpy assets or in highly seasonal activities such as agriculture, which would otherwise not be feasible with standard loans.

Our results also speak to a more basic question: do the tailored loans create additional surplus or merely redistribute existing surplus between lender and borrower? Two features point to creation. First, the lender's position is largely unchanged: the SEK+ loan offer does not significantly alter the number of contracts signed, the gross amount disbursed, or the effective borrowing cost, and it does not worsen repayment, so the welfare gain is unlikely to be a transfer of rents. Second, the profit gains are more than a reshuffling across income sources: the rise in own-account profits is only partly offset by lower occasional-wage and other income, leaving a positive (if imprecisely estimated) effect on total household income (Table 4).

The gains, moreover, come from running existing activities more efficiently rather than from making new projects feasible. The SEK+ loan offer does not significantly raise the probability of operating an own-account business (Table C.6), shift household participation across the twelve disaggregated activities (Table C.7), or increase fixed-asset accumulation (Table 6). The effects instead concentrate on the intensive margin of activities borrowers already operate, predominantly off-farm enterprises with high-frequency sales. By aligning fixed obligations with the borrower's expected revenue cycle, the tailored contract relaxes the within-cycle cash-flow constraint the standard schedule imposes, freeing working capital for quick-turnover activities and drawing household labor from casual wage work toward self-employment (Tables 4 and 5). The broader lesson is that a richer menu of contracts adds value not by bringing in new borrowers, which it does not, but by giving existing ones a repayment schedule that better fits their cash flows.

7. Conclusion

This paper has asked whether the standard microcredit contract (a rigid, immediate-repayment, linear-amortization loan) is the right product for the rural poor, or whether the modest welfare effects documented in previous microcredit RCTs partly reflect a mismatch between contract structure and borrowers' cash-flow profiles — and, separately, whether

low take-up reflects limited awareness of what is on offer. Partnering with Morocco's largest microfinance institution, we tested two ex ante design changes intended to align repayment with expected revenue: a five-month grace period (the SEK loan) and a tailored amortization schedule with three borrower-chosen installment levels (the SEK+ loan).

The patterns discussed in Section 6 speak to two strands of the literature. They reinforce the view of Banerjee et al. (2015) and Meager (2019) that the small average impact of microcredit access reflects a standard contract that suits some borrowers but not others. They also extend the literature on flexible repayment (Battaglia et al., 2024; Barboni and Agarwal, 2026) by showing that ex ante customization (without ex post discretion to defer installments) can generate meaningful effects for borrowers whose cash flows are not linear. Combined with the absence of effects on lumpy investment and household consumption, the picture is one of working-capital expansion among (some) borrowers previously constrained by the timing, rather than the level, of loan obligations. Because these gains accrue without additional lending and without any deterioration in repayment, they are better read as surplus created by a closer match between repayment structure and borrowers' revenue cycles than as surplus redistributed from the lender.

Two implications follow for practitioners. First, tailored repayment schedules appear to be a complement to the standard loan, even though they partially substitute for it at the borrower level: because borrowers sort into the contract that suits them, offering the tailored option alongside the standard one raises welfare without fully displacing the standard product. Second, village-level information campaigns shifted stated preferences but not behavior, suggesting that demand expansion in rural microcredit will depend on loan-officer outreach, incentives, and trust-building rather than awareness alone.

We see several avenues for follow-up research. First, the contrast between shifts in stated preferences and null effects on take-up suggests a need for direct investigation of the mechanisms (e.g., perceived risk, gender norms, or loan-officer incentives) that constrain rural microcredit at the extensive margin. Second, pairing ex ante customization

with ex post flexibility would test whether these two design margins are substitutes or complements. Third, longer follow-ups would reveal whether the working-capital expansion documented here translates into consumption, asset accumulation, or labor-market mobility, or dissipates after the first loan cycles.

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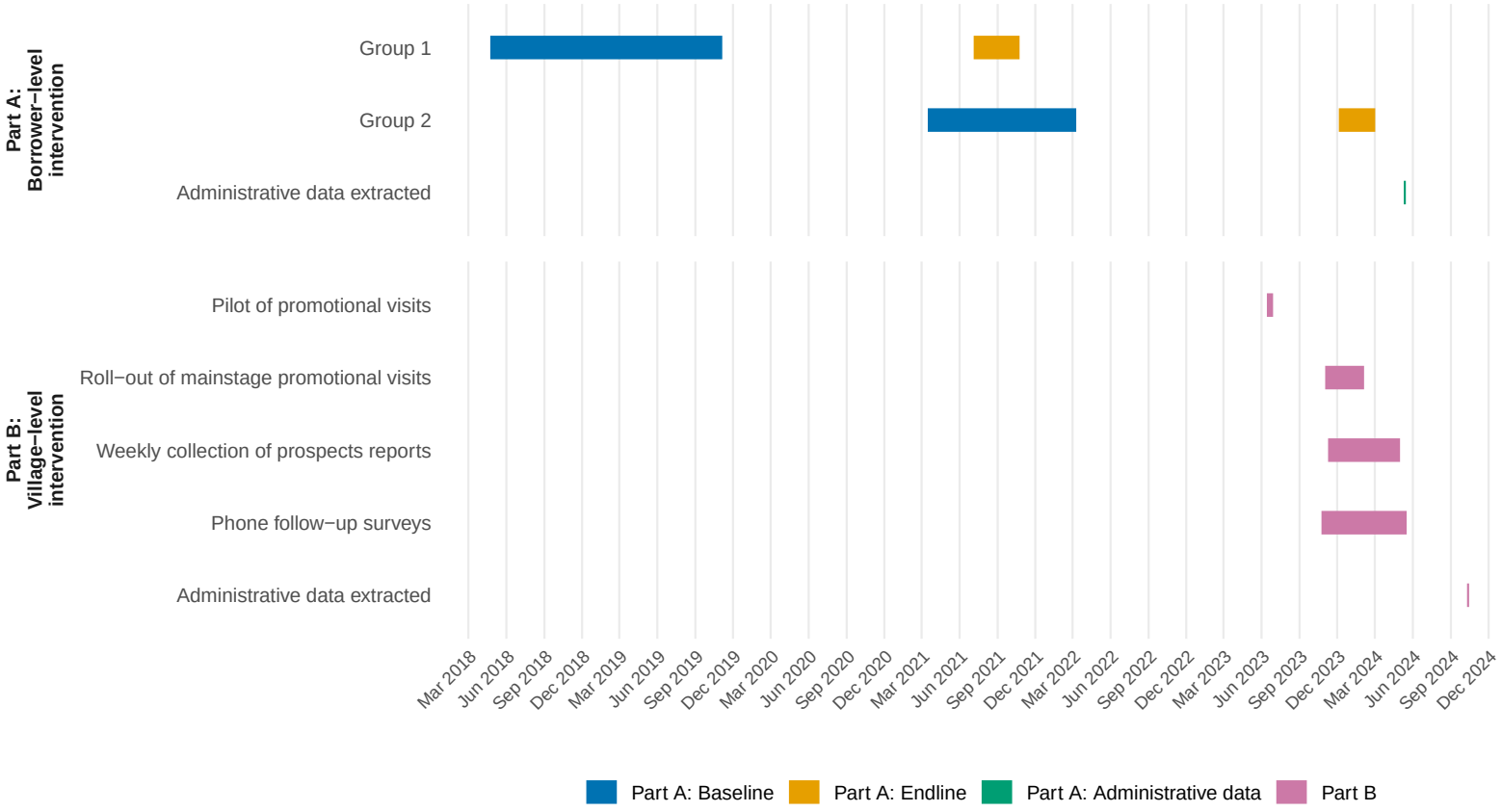
Tables and Figures

FIGURE 1. Timeline for study implementation



Notes: Source: Baseline and endline survey interview dates as well as roll-out and administrative data.

Part A: Individual-level intervention

TABLE 1. Balance at baseline

	Total	Control	Treatment effect vs control					
	(1)	(2)	(3) SEK		(4) SEK+		(5) Any	
	N	Mean	β	SE	β	SE	β	SE
Panel A: Respondent and household characteristics								
Respondent is female	3,020	0.264	0.023	(0.019)	0.001	(0.019)	0.012	(0.017)
Respondent age	3,014	42.465	-0.409	(0.550)	-0.372	(0.557)	-0.390	(0.484)
Respondent has any education	3,015	0.617	0.018	(0.021)	0.010	(0.021)	0.014	(0.018)
Respondent is household head	2,986	0.720	-0.031	(0.020)	-0.007	(0.020)	-0.019	(0.017)
# household members	3,019	4.970	-0.174*	(0.103)	-0.034	(0.103)	-0.104	(0.094)
Panel B: Access to credit								
Loan from formal institution	3,027	0.195	0.018	(0.017)	0.009	(0.017)	0.014	(0.015)
Loan from informal source	3,008	0.046	0.011	(0.010)	0.022**	(0.010)	0.016*	(0.008)
Panel C: Self-employment activities								
# activities	3,056	2.317	0.027	(0.065)	-0.014	(0.065)	0.007	(0.057)
Does crop-farming								
Cereal growing	3,042	0.254	0.035**	(0.017)	0.020	(0.017)	0.028*	(0.015)
Arboriculture	3,031	0.198	0.004	(0.016)	0.009	(0.016)	0.007	(0.014)
Vegetable growing	3,035	0.139	0.030**	(0.014)	-0.003	(0.014)	0.013	(0.012)
Does livestock breeding								
Breeding of cattle, sheep, goats	3,044	0.427	0.002	(0.020)	0.009	(0.020)	0.005	(0.018)
Breeding of poultry, rabbits	3,043	0.297	-0.003	(0.018)	-0.027	(0.018)	-0.015	(0.015)
Production derived from livestock	3,038	0.158	0.004	(0.015)	-0.014	(0.014)	-0.005	(0.013)
Does off-farm activity								
Commerce	3,042	0.315	0.003	(0.020)	0.036*	(0.020)	0.020	(0.018)
Crafts	3,035	0.133	-0.014	(0.014)	-0.006	(0.014)	-0.010	(0.012)
Fishing	3,032	0.053	0.003	(0.010)	0.002	(0.010)	0.003	(0.008)
Construction	3,039	0.146	-0.015	(0.015)	-0.023	(0.015)	-0.019	(0.013)
Transport	3,035	0.044	0.013	(0.010)	0.004	(0.009)	0.009	(0.008)
Services	3,036	0.127	-0.025*	(0.014)	-0.020	(0.014)	-0.022*	(0.012)
Other	2,370	0.047	-0.007	(0.010)	-0.001	(0.010)	-0.004	(0.009)
Panel D: Has income from								
Salaried labor	3,032	0.140	-0.008	(0.015)	-0.026*	(0.015)	-0.017	(0.013)
Casual labor	3,027	0.248	-0.019	(0.016)	-0.019	(0.016)	-0.019	(0.014)
Pension	3,014	0.038	0.015*	(0.009)	0.018*	(0.009)	0.017**	(0.008)
Remittances	3,025	0.179	0.016	(0.017)	0.013	(0.017)	0.014	(0.015)
Donations	3,025	0.059	-0.006	(0.009)	-0.009	(0.009)	-0.008	(0.008)
Asset sales	3,020	0.032	-0.002	(0.008)	-0.009	(0.007)	-0.006	(0.007)
Other	2,294	0.019	0.014*	(0.008)	-0.002	(0.007)	0.006	(0.006)
Panel E: Attrition								
Not surveyed at endline	3,068	0.130	0.001	(0.015)	0.008	(0.015)	0.005	(0.013)
Omnibus F-test across all variables			1.22 (p=0.199)		0.84 (p=0.702)		1.38 (p=0.089)	
Omnibus F-test (endline sample only)			1.26 (p=0.165)		1.02 (p=0.429)		1.48 (p=0.052)	

Notes: Source: Baseline surveys. Unit of observation: Respondent level where indicated and otherwise household level. Table reports baseline balance across three treatment arms with SEK N = 1,035, SEK+ N = 1,038, and Control/Standard N = 995 out of N = 3,068. Column (1) shows the total number of non-missing observations for each variable. Column (2) reports the control-group mean. Columns (3)–(5) report OLS differences in means (β) for SEK, SEK+, and Any treatment (= SEK or SEK+), estimated from regressions of the outcome on treatment dummies with branch fixed effects. Standard errors robust to heteroskedasticity are shown in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. The "Omnibus F-test across all variables" tests joint equality of all listed covariates across arms by regressing each treatment indicator on the full set of balance variables (plus branch fixed effects) and reporting the testparm F-statistic and p-value. The second omnibus row ("endline sample only") repeats this joint test restricting the sample to units observed at endline and using Panels A–D variables only.

TABLE 2. Take-up of loan contracts

	(1)	(2)	(3)	(4)	(5)
	SEK	SEK+	Standard	Any	None
Treatment: SEK	0.29***	0.01***	0.52***	0.81***	0.19***
	(0.01)	(0.00)	(0.02)	(0.01)	(0.01)
Treatment: SEK+	0.01***	0.32***	0.50***	0.83***	0.17***
	(0.00)	(0.01)	(0.02)	(0.01)	(0.01)
Control: Standard	0.01***	0.00*	0.83***	0.84***	0.16***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	3,068	3,068	3,068	3,068	3,068

Notes: Data source: Al Amana administrative records. Observation unit: individual study participant. Each column reports a binary indicator equal to one if the respondent's first loan contract of the indicated type at Al Amana fell within the individual study period, defined conservatively as the interval from 90 days before the study start date through the end of the study window. The pre-baseline window accommodates contracts disbursed shortly before the baseline interview date was recorded; in practice fewer than 4% of take-ups fall in this pre-baseline window, nearly all within a week of the baseline survey. Contracts of individuals who attrited before the window closed are retained. "SEK" denotes the grace-period loan (*Produit Salaf El Karaoui*); "SEK+" denotes the tailored loan (*Produit Salaf El Karaoui plus*); "Standard" includes all other individual enterprise loans (*Produit Individuel Entreprise*, *Produit Moumtaz*, and *Produit Idmaj*). Contracts nominally classified as SEK+ were reclassified as Standard when the recorded repayment schedule exhibited negligible variation across installment periods (maximum-to-minimum installment ratio below 1.05) and sufficient schedule data were available (more than one third of scheduled installments recorded); see Appendix B.1 for details. Each coefficient is the within-group mean of the outcome, estimated by regressing the take-up indicator on three treatment-arm binaries without a constant. Heteroskedasticity-robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3. Sales, expenditures and profits of own-account household activities

	HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	4,786	1,792	2,896	
	(3,029)	(1,544)	(1,929)	0.251
	[0.072]		[0.072]	
	{0.139}	{0.299}	{0.159}	
Treatment: SEK+	7,897**	3,049*	4,895**	
	(3,219)	(1,640)	(1,990)	0.040
	[0.030]		[0.030]	
	{0.020}	{0.057}	{0.020}	
Westfall–Young p (joint)	0.037	0.105	0.035	0.075
Mean (control)	19,948.31	10,270.26	9,678.04	
% > 0 (control)	35.33	41.80	30.95	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. All outcomes are annual totals in Moroccan Dirham (MAD). Sales (column 1) and expenses (column 2) aggregate revenues and costs, respectively, across crop-farming, livestock and non-agricultural own-account activities, computed using the AAA method (Aggregating, Anchoring and Adjusting; Anderson et al. (2021)). Agricultural and livestock components are measured over the 12 months prior to the survey; non-agricultural components sum final adjusted values across up to three activities and three sale types (contract, infrequent, frequent), measured over the past 30 days and annualized. Before aggregation, each sales and expense component is right-tail winsorized separately, replacing values above the 95th percentile of its strictly positive observations with that percentile. Profits equal the resulting total sales minus total expenses and are not winsorized directly; no lower-tail winsorization is applied. See Appendix B.2 for full variable definitions. Non-zero share is the percentage of control-group households reporting a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young (2019); 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), computed across the sales and profits coefficients (both treatment arms), in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all six treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)"). *,p<0.10; **,p<0.05; ***,p<0.01.

TABLE 4. Total household income

	Wages				WY-p val.	
	Salary	Occasional	Other income	Profits	Total income	
	(1)	(2)	(3)	(4)	(5)	
Treatment: SEK	-131	-728	-190	2,896	1,955	
	(851)	(565)	(363)	(1,929)	(1,988)	0.511
	[0.589]	[0.358]	[0.491]	[0.317]	[0.431]	
	{0.875}	{0.188}	{0.582}	{0.158}	{0.354}	
Treatment: SEK+	-79	-823	-758**	4,895**	3,127	
	(852)	(572)	(352)	(1,990)	(2,046)	0.084
	[0.589]	[0.317]	[0.164]	[0.162]	[0.317]	
	{0.929}	{0.143}	{0.033}	{0.019}	{0.129}	
Westfall–Young p (joint)	0.984	0.242	0.062	0.035	0.222	0.148
Mean (control)	12,491	9,111	3,575	9,678	34,855	
Non-zero % (control)	40.53	50.35	38.80	30.95	97.34	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. All outcomes are annual totals in Moroccan Dirham (MAD). Salary income (column 1) sums earnings from salaried employment across all household members. Occasional wage income (column 2) sums casual labor income, imputed for each member as days worked × the midpoint of self-reported minimum and maximum daily wages. Other income (column 3) sums income from non-wage, non-casual sources including pensions, remittances, gifts, asset sales, and miscellaneous income across all members and sources. Profits (column 4) equal total sales minus total expenses from own-account activities, computed using the AAA method, and reproduce the estimates reported in Table 3. Total income (column 5) is the sum of columns 1–4. Salary income, occasional wage income, and other income are each right-tail winsorized at the 95th percentile of their strictly positive observations. Profits reproduce the outcome in Table 3: the underlying sales and expense components are right-tail winsorized separately at the 95th percentile before aggregation, after which profits are calculated as total sales minus total expenses; profits are not winsorized directly and no lower-tail winsorization is applied. Missing values are set to zero before aggregation. Total income is the sum of the four resulting components and is not additionally winsorized. See Appendix B.4 for full variable definitions. Non-zero share is the percentage of control-group households with a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young, 2019; 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), computed across all coefficients in the table, in square brackets. Column 6 reports Westfall–Young stepdown-adjusted p-values: for each treatment row, the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all five equations; the "Westfall–Young p (joint)" row reports, in columns 1–5, the joint p-value that both treatment coefficients are zero within a given equation, and, in column 6, the experiment-wise p-value testing the null that all 10 treatment coefficients across the five equations are jointly zero. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

TABLE 5. Time use, household

	HH own-account activities				Wage labor		
	Crops	Livestock	Off-farm	Total	Salaried	Casual	
	(1)	(2)	(3)	(4)	(5)	(6)	
Treatment: SEK	-0.004	0.021	0.100*	0.125	0.031	-0.193	
	(0.033)	(0.068)	(0.055)	(0.102)	(0.182)	(0.124)	0.409
	[1.000]	[1.000]	[0.494]	[0.564]	[1.000]	[0.494]	
	{0.907}	{0.734}	{0.094}	{0.230}	{0.869}	{0.102}	
Treatment: SEK+	0.019	-0.032	0.089	0.094	-0.124	-0.280**	
	(0.037)	(0.066)	(0.056)	(0.105)	(0.177)	(0.124)	0.090
	[1.000]	[1.000]	[0.494]	[0.998]	[1.000]	[0.400]	
	{0.569}	{0.641}	{0.120}	{0.370}	{0.496}	{0.018}	
Westfall–Young p (joint)	0.795	0.853	0.164	0.381	0.714	0.033	0.167
Mean (control)	0.15	0.41	0.22	0.79	2.67	1.99	
Non-zero % (control)	8.31	9.58	5.20	18.24	38.80	50.46	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. All outcomes are in days per week. Columns 1–3 report time spent on own-account activities: crop-farming and livestock are 12-month totals divided by 52; off-farm non-agricultural days are measured over the past 30 days (values exceeding 31 are treated as 12-month responses and corrected accordingly) and converted to weekly. Column 4 is the sum of columns 1–3 and does not include wage labor. Salaried work (column 5) is days worked in the past 7 days, summed across household members. Casual work (column 6) is 12-month totals divided by 52. Because the source variables use different recall periods, these outcomes do not implement the PAP's common seven-day time-use measure exactly and comparisons across columns should be interpreted cautiously. Missing values are set to zero; variables are not winsorized. See Appendix B.3 for full variable definitions including the off-farm correction procedure. *Non-zero share* is the percentage of control-group households with a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young (2019); 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), computed across all coefficients in the table, in square brackets. Column 7 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value and (ii) the experiment-wise p-value testing the null that all 12 treatment coefficients across the six equations are jointly zero. *,p<0.10; **,p<0.05; ***,p<0.01.

TABLE 6. Household investment

	Household investment					WY-p val.	
	General	Agricultural	Breeding	Non-agri.	Animals	Total	
	(1)	(2)	(3)	(4)	(5)	(6)	
Treatment: SEK	344	363	-1	6	-5,253	-4,540	
	(366)	(261)	(1)	(4)	(5,532)	(5,550)	0.527
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.321}	{0.458}	{0.522}	{0.131}	{0.614}	{0.645}	
Treatment: SEK+	-69	588	0	-0	-5,005	-4,486	
	(247)	(422)	(2)	(1)	(5,330)	(5,352)	0.671
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.833}	{0.188}	{0.794}	{0.937}	{0.614}	{0.643}	
Westfall–Young p (joint)	0.515	0.303	0.764	0.214	0.976	0.980	0.772
Mean (control)	307	33	1	0	6,329	6,670	
Non-zero % (control)	0.69	0.92	0.35	0.00	7.04	8.31	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. All outcomes are in Moroccan Dirham (MAD). Investment is defined as the value of productive assets acquired in the past 12 months, computed as the acquisition indicator multiplied by the purchase price; within-type median prices are imputed when reported prices are missing. Columns 1–4 cover equipment investment by sector: general (car, pickup, motorcycle), agricultural (14 tool/machinery types), breeding (5 livestock equipment types), and non-agricultural (7 business asset types), each including pre-listed and "other" items. Animal purchases (column 5) are the total value of animals bought in the past 12 months. Total investment (column 6) sums columns 1–5. Variables are not winsorized. See Appendix B.5 for full variable definitions including the unstacking procedure. *Non-zero share* is the percentage of control-group households with a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young (2019); 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), computed across all coefficients in the table, in square brackets. Column 7 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value and (ii) the experiment-wise p-value testing the null that all 12 treatment coefficients across the six equations are jointly zero. *,p<0.10; **,p<0.05; ***,p<0.01.

TABLE 7. Consumption

	Household consumption				WY-p val.
	Durables	Food	Non-food	Total	
	(1)	(2)	(3)	(4)	(5)
Treatment: SEK	-46	-14	-513	-653	
	(105)	(465)	(514)	(897)	0.705
	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.688}	{0.980}	{0.343}	{0.460}	
Treatment: SEK+	114	-31	52	-30	
	(118)	(477)	(518)	(906)	0.665
	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.308}	{0.945}	{0.918}	{0.981}	
Westfall–Young p (joint)	0.492	0.996	0.531	0.663	0.863
Mean (control)	389	24,933	17,468	43,405	
Non-zero % (control)	10.05	100.00	99.65	100.00	
Branch FE + PDS-LASSO	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. All outcomes are annual totals in Moroccan Dirham (MAD). Durables (column 1) is total expenditure on durable household goods (up to 24 item categories) acquired over the past 12 months. Food consumption (column 2) sums food purchases, souk-equivalent value of food donations, out-of-home food, and bread, all measured over the past 7 days and annualized. Non-food consumption (column 3) sums recurring non-food expenditure (past 30 days, annualized), occasional non-food expenditure (past 12 months), and exceptional non-food expenditure (past 12 months). Total consumption (column 4) sums durables, food consumption (including the souk-equivalent value of food donations), and all non-food components. Aggregate variables are winsorized at the upper 5th percentile of strictly positive values. See Appendix B.7 for full variable definitions. *Non-zero share* is the percentage of control-group households with a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young (2019); 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), computed across all coefficients in the table, in square brackets. Column 5 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value and (ii) the experiment-wise p-value testing the null that all 8 treatment coefficients across the four equations are jointly zero. *,p<0.10; **,p<0.05; ***,p<0.01.

TABLE 8. Loan contract characteristics

	Count	Gross amount	Loan duration	Borrowing cost (pp)	WY-p val.
	(1)	(2)	(3)	(4)	(5)
Treatment: SEK	-0.04	207.33	0.62*	1.44***	
	(0.03)	(334.28)	(0.33)	(0.36)	0.002
	[0.191]	[0.384]	[0.137]	[0.001]	
	{0.117}	{0.545}	{0.073}	{0.000}	
Treatment: SEK+	-0.01	425.84	0.89***	0.48	
	(0.03)	(338.17)	(0.34)	(0.36)	0.028
	[0.384]	[0.210]	[0.036]	[0.210]	
	{0.573}	{0.231}	{0.008}	{0.190}	
Westfall–Young p (joint)	0.207	0.370	0.014	0.002	0.002
Mean (control)	1.32	9,507.25	19.48	16.53	
Non-zero % (control)	100.00	100.00	100.00	100.00	
Branch FE + PDS-LASSO	✓	✓	✓	✓	
Observations	2,219	2,219	2,219	2,207	

Notes: Data source: Al Amana administrative records for study participants. Observation unit: individual. Sample restricted to individuals who signed at least one enterprise loan contract with Al Amana during the study period and responded to the endline survey; each individual contributes the first contract signed within the study window. Outcomes are the number of loan contracts signed by the individual during the study period (column 1); the gross amount released, in Moroccan dirhams (column 2); the loan duration, i.e. the scheduled borrowing period in months from disbursement until the final scheduled repayment (column 3); and the borrowing cost share, i.e. the ratio of total scheduled interest payments over the gross amount released, expressed in percentage points (column 4). Each column reports coefficients from a PDS-LASSO regression of the outcome on two treatment-arm dummies (SEK and SEK+), controlling for branch fixed effects and a set of borrower and household baseline characteristics selected by the LASSO procedure; the post-LASSO OLS coefficients are reported. See Belloni et al. (2014) for details on the PDS-LASSO procedure. Heteroskedasticity-robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008) in square brackets, computed across all coefficients in the table. Curly brackets report randomisation-inference p-values based on 1,000 permutation draws (Young, 2019). Westfall–Young stepdown-adjusted p-values, correcting for multiple hypothesis testing across the same 1,000 permutation draws, are reported in Column 5 and in the bottom row: for each treatment row, Column 5 gives the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all four equations; the "Westfall–Young p (joint)" row reports, in Columns 1–4, the joint p-value that both treatment coefficients are zero within a given equation, and, in Column 5, the experiment-wise p-value for the null that all eight treatment coefficients across the four equations are jointly zero.

TABLE 9. Repayment

	30+ days late	Days vs schedule	Unpaid installments	WY-p val.
	(1)	(2)	(3)	(4)
Treatment: SEK	0.01	-14.65*	0.07	
	(0.01)	(8.80)	(0.22)	0.274
	[0.837]	[0.522]	[1.000]	
	{0.450}	{0.111}	{0.770}	
Treatment: SEK+	0.00	-14.20	0.26	
	(0.01)	(8.99)	(0.23)	0.309
	[1.000]	[0.522]	[0.532]	
	{0.976}	{0.123}	{0.254}	
Westfall–Young p (joint)	0.656	0.191	0.417	0.445
Mean (control)	0.08	-78.76	2.92	
% $\neq 0$ (control)	7.59	74.66	48.24	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,219	2,219	2,219	

Notes: Data source: Al Amana administrative records for study participants. Observation unit: individual. Sample restricted to individuals who signed at least one enterprise loan contract with Al Amana during the study period and responded to the endline survey; each individual contributes the first contract signed within the study window. Outcomes are an indicator equal to one if the actual contract end date exceeds the scheduled end date by 30 or more days (column 1); the difference between the actual contract end date and the planned end date, in days, with negative values indicating early completion (column 2); and the number of unpaid installments as recorded in Al Amana's system (column 3). For columns 1 and 2, contracts that had not yet reached their scheduled end date at the time of data extraction (October 2024) are coded as on schedule. "% $\neq 0$ (control)" reports the proportion of control-group individuals with a non-zero outcome—for column 2, a contract that ended off schedule, whether early or late. Each column reports coefficients from a PDS-LASSO regression of the outcome on two treatment-arm dummies (SEK and SEK+), controlling for branch fixed effects and a set of borrower and household baseline characteristics selected by the LASSO procedure; the post-LASSO OLS coefficients are reported. See Belloni et al. (2014) for details on the PDS-LASSO procedure. Heteroskedasticity-robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008) in square brackets, computed across all coefficients in the table. Curly brackets report randomisation-inference p-values based on 1,000 permutation draws (Young, 2019). Westfall–Young stepdown-adjusted p-values, correcting for multiple hypothesis testing across the same 1,000 permutation draws, are reported in Column 4 and in the bottom row: for each treatment row, Column 4 gives the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all three equations; the "Westfall–Young p (joint)" row reports, in Columns 1–3, the joint p-value that both treatment coefficients are zero within a given equation, and, in Column 4, the experiment-wise p-value for the null that all six treatment coefficients across the three equations are jointly zero.

TABLE 10. Mean differences at baseline between compliers and non-compliers

	(1)		(2)		(3)		(4)		Mean differences (t-tests)			
	Complier SEK		Non-Comp. SEK		Complier SEK+		Non-Comp. SEK+		Within arm		Across arm	
									(1)-(2)	(3)-(4)	(1)-(3)	(2)-(4)
Variable	Mean	SE	Mean	SE	Mean	SE	Mean	SE				
Respondent is female	0.299	0.026	0.249	0.019	0.304	0.025	0.224	0.018	0.050	0.081***	-0.005	0.025
Respondent age	41.555	0.725	42.006	0.587	41.581	0.735	42.197	0.610	-0.451	-0.615	-0.027	-0.191
Respondent has any education	0.628	0.028	0.617	0.021	0.627	0.027	0.592	0.022	0.011	0.035	0.001	0.025
Respondent is household head	0.628	0.028	0.729	0.019	0.714	0.025	0.713	0.020	-0.101***	0.001	-0.086**	0.016
# household members	4.814	0.130	4.628	0.082	4.934	0.124	4.790	0.088	0.186	0.144	-0.120	-0.162
Loan from formal institution	0.252	0.025	0.209	0.018	0.220	0.023	0.200	0.018	0.043	0.019	0.033	0.009
Loan from informal source	0.073	0.015	0.052	0.010	0.072	0.014	0.056	0.010	0.021	0.016	0.001	-0.004
Any crop-farming	0.497	0.029	0.330	0.020	0.399	0.027	0.367	0.021	0.166***	0.032	0.097**	-0.037
Any cereal growing	0.372	0.028	0.252	0.019	0.286	0.025	0.268	0.019	0.120***	0.018	0.086**	-0.015
Any arboriculture	0.233	0.024	0.183	0.017	0.211	0.022	0.198	0.018	0.049*	0.012	0.022	-0.015
Any vegetable growing	0.262	0.025	0.092	0.012	0.130	0.018	0.135	0.015	0.171***	-0.005	0.133***	-0.043**
Any livestock breeding	0.537	0.029	0.529	0.022	0.441	0.027	0.538	0.022	0.008	-0.097***	0.096**	-0.008
Any breeding of cattle, sheep, goats	0.465	0.029	0.428	0.021	0.389	0.027	0.478	0.022	0.037	-0.089**	0.077*	-0.050
Any breeding of poultry, rabbits	0.355	0.028	0.262	0.019	0.211	0.022	0.283	0.020	0.094***	-0.072**	0.145***	-0.022
Any production derived from livestock	0.239	0.025	0.125	0.014	0.133	0.019	0.141	0.015	0.114***	-0.008	0.107***	-0.015
Any off-farm activity	0.653	0.028	0.656	0.021	0.688	0.026	0.695	0.021	-0.003	-0.006	-0.036	-0.039
Any commerce	0.346	0.027	0.290	0.020	0.316	0.026	0.368	0.021	0.056*	-0.052	0.029	-0.078***
Any crafts	0.063	0.014	0.140	0.015	0.166	0.020	0.089	0.012	-0.077***	0.077***	-0.103***	0.052***
Any fishing	0.070	0.015	0.050	0.009	0.063	0.013	0.048	0.009	0.019	0.015	0.007	0.002
Any construction	0.110	0.018	0.155	0.016	0.105	0.017	0.123	0.014	-0.046*	-0.018	0.004	0.032
Any transport	0.063	0.014	0.050	0.009	0.039	0.011	0.054	0.010	0.013	-0.015	0.024	-0.003
Any services	0.106	0.018	0.095	0.013	0.096	0.016	0.100	0.013	0.011	-0.004	0.010	-0.005
Any income from salaried labor	0.143	0.020	0.121	0.014	0.102	0.017	0.116	0.014	0.021	-0.013	0.040	0.006
Any income from casual labor	0.243	0.025	0.191	0.017	0.142	0.019	0.272	0.020	0.052*	-0.130***	0.101***	-0.081***
Any income from pension	0.037	0.011	0.056	0.010	0.063	0.013	0.054	0.010	-0.020	0.009	-0.027	0.002
Any income from remittances	0.186	0.022	0.174	0.016	0.166	0.020	0.177	0.017	0.012	-0.012	0.020	-0.003
Any income from donations	0.056	0.013	0.049	0.009	0.033	0.010	0.056	0.010	0.008	-0.023	0.023	-0.007
Any income from asset sales	0.037	0.011	0.024	0.007	0.021	0.008	0.027	0.007	0.012	-0.006	0.015	-0.003
Any other income	0.037	0.011	0.021	0.006	0.024	0.008	0.010	0.004	0.016	0.014	0.012	0.011
Not surveyed at endline	0.140	0.020	0.114	0.014	0.154	0.020	0.127	0.015	0.026	0.026	-0.014	-0.013
Observations	301		535		332		519					

Notes: Sample limited to respondents signing either the assigned or a standard contract within the respective treatment arms. This table presents a broadly prespecified but descriptively implemented selection analysis. Mean differences are estimated by OLS on treatment-group indicators with heteroskedasticity-robust standard errors. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Part B: Village-level intervention

TABLE 11. Village randomization

	Total (1)		Control (2)		SEK info + norms-shifting (3)		SEK pure info (4)		SEK+ info + norms-shifting (5)		SEK+ pure info (6)		Standard pure info (7)	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
Panel A: Village characteristics														
Approx. number HH per village	440	97.23	120	98.17	40	100.38	80	97.56	40	95.00	80	90.46	80	101.78
		(2.36)		(4.42)		(7.89)		(5.47)		(8.06)		(5.50)		(5.81)
Distance to agency (km)	440	9.72	120	9.04	40	9.35	80	9.75	40	11.29	80	9.95	80	9.86
		(0.27)		(0.42)		(0.83)		(0.70)		(0.91)		(0.70)		(0.61)
Village dispersed (=1)	440	0.44	120	0.39	40	0.45	80	0.45	40	0.45	80	0.42	80	0.53
		(0.02)		(0.04)		(0.08)		(0.06)		(0.08)		(0.06)		(0.06)
Panel B: Differences between treatment arms														
	(2)–(3)	(2)–(4)	(2)–(5)	(2)–(6)	(2)–(7)	(3)–(4)	(3)–(5)	(3)–(6)	(3)–(7)	(4)–(5)	(4)–(6)	(4)–(7)	(5)–(6)	(5)–(7)	(6)–(7)
Approx. number HH per village	-2.20	0.61	3.17	7.71	-3.60	2.81	5.38	9.91	-1.40	2.56	7.10	-4.21	4.54	-6.78	-11.31
Distance to agency (km)	-0.32	-0.71	-2.25**	-0.92	-0.82	-0.40	-1.94	-0.60	-0.51	-1.54	-0.20	-0.11	1.34	1.43	0.09
Village dispersed (=1)	-0.06	-0.06	-0.06	-0.03	-0.13*	0.00	0.00	0.03	-0.07	0.00	0.03	-0.07	0.03	-0.07	-0.10

Notes: Data source: Promotional campaign randomization file drawn primarily from the Initiative nationale pour le développement humain (INDH) database (2020–2021) for GPS coordinates and household estimates, supplemented by 2014 data from the Haut Commissariat au Plan (HCP) for dispersal status (i.e., scattered individual dwellings rather than grouped housing), and (when necessary) additional household size estimates. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided t-tests for pairwise mean differences).

TABLE 12. Take-up of village intervention - prospects, signed contracts, and total disbursement

	Prospects	Contracts				Gross amount
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Total	Standard	SEK	SEK+	Total
Treatment: SEK	0.55*	0.14	0.16	-0.02	-	935
	(0.31)	(0.18)	(0.17)	(0.05)		(4,078)
Treatment: SEK+	0.25	-0.02	0.04	-0.06	-	-1,584
	(0.18)	(0.12)	(0.10)	(0.05)		(2,992)
Treatment: Standard	0.32	-0.12	-0.06	-0.06	-	-2,477
	(0.30)	(0.13)	(0.12)	(0.05)		(3,027)
Control group mean	0.76	0.38	0.31	0.07	0	6,191
Non-zero share	0.32	0.18	0.18	0.02	0	0.18
Observations	440	440	440	440	440	440
Branch FE	✓	✓	✓	✓	✓	✓

Notes: Observation unit: village. The sample comprises 440 study villages. Counts are constructed from phone surveys and Al Amana administrative data for prospective borrowers and borrowers who consented to participate in the study. Columns (1)–(6) report separate outcomes: (1) number of prospects identified during promotional campaigns; (2) total loans signed; (3)–(5) loans signed on the conventional *Standard*, *SEK*, or *SEK+* contract, respectively; (6) total gross loan amount disbursed (Moroccan Dirham), summed across all contract types. Amounts are village totals; villages with no disbursement are coded zero, so column (6) uses the same 440-village sample as the count columns. Each column reports OLS estimates from a regression of the outcome on three treatment-arm indicators, controlling for baseline village covariates: dispersed settlement indicator, distance to agency (km), and approximate number of households, as reported in Table 11, and for branch fixed effects, corresponding to the original branch assignment at randomization; Branch AJ, which closed in November 2023 and was absorbed by Branch MK, is treated as a distinct branch throughout. No SEK+ contracts were signed within the study sample, hence the dashes in column (5). Heteroskedasticity-robust standard errors in parentheses. "Control group mean" is the mean of the outcome in pure-control villages (no information campaign). "Non-zero share" is the fraction of control villages with a strictly positive value for the outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 13. Take-up of village intervention - client characteristics

	Demography				Activity (principal)			Loan interest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	New	Female	Above median age	Primary edu. and above	Crops	Breeding	Off-farm	Crops	Breeding	Off-farm
Treatment: SEK	0.05	0.01	0.03	0.14	0.03	0.09	0.04	0.04	0.03	0.13
	(0.10)	(0.04)	(0.08)	(0.12)	(0.06)	(0.06)	(0.04)	(0.06)	(0.06)	(0.10)
Treatment: SEK+	-0.09*	0.02	-0.03	-0.05	0.01	0.04	0.01	-0.03	0.02	-0.00
	(0.05)	(0.04)	(0.07)	(0.07)	(0.05)	(0.05)	(0.03)	(0.05)	(0.06)	(0.05)
Treatment: Standard	-0.00	-0.01	-0.06	-0.11	-0.09**	0.00	-0.01	-0.06	-0.05	0.00
	(0.07)	(0.04)	(0.08)	(0.07)	(0.05)	(0.06)	(0.04)	(0.06)	(0.06)	(0.07)
Control group mean	0.13	0.05	0.20	0.21	0.10	0.10	0.07	0.12	0.11	0.12
Observations	440	440	440	440	440	440	440	440	440	440
Branch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Observation unit: village. The sample comprises 440 study villages. Outcomes are village-level total counts of clients with the indicated characteristic, constructed from phone follow-up surveys of signed borrowers and *Al Amana* administrative data, restricted to clients who consented to participate in the study. Columns (1)–(4) report demographic characteristics: (1) new clients (first-time borrowers at *Al Amana*); (2) female clients; (3) clients above the sample median age; (4) clients with primary education or above. Columns (5)–(7) report the principal economic activity declared by the client: (5) crop farming (cereals, arboriculture, or market gardening); (6) livestock or poultry breeding and livestock products; (7) off-farm activities (commerce, crafts, fishing, construction, transport, services, or other). Columns (8)–(10) report the activity for which the client expressed interest in taking a loan, using the same groupings as columns (5)–(7). Each column reports OLS estimates from a regression of the outcome on three treatment-arm indicators, controlling for baseline village covariates: dispersed settlement indicator, distance to agency (km), and approximate number of households, as reported in Table 11, and for branch fixed effects, corresponding to the original branch assignment at randomization; Branch AJ, which closed in November 2023 and was absorbed by Branch MK, is treated as a distinct branch throughout. Heteroskedasticity-robust standard errors in parentheses. "Control group mean" is the mean of the outcome in pure-control villages (no information campaign). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A. Deviations from the Pre-Analysis Plan

This study was pre-registered as a Stage 1 Registered Report at the *Journal of Development Economics* (AEA RCT Registry number AEARCTR-0002618). We document below all material deviations between the pre-analysis plan (PAP) and the implemented analysis.

A.1. Deviations related to Part A

Multiple-testing adjustment method. The PAP specifies Hochberg-adjusted p-values alongside randomization inference. We retain randomization inference but replace the Hochberg correction with sharpened false-discovery-rate q-values (Anderson, 2008) and Westfall–Young stepdown-adjusted p-values. Both are calculated within each outcome family across the two treatment arms. For the main business-outcomes table, the family comprises the two preregistered primary outcomes, sales and profits; expenses is excluded because it is an input into profits. For all other tables, the family comprises all reported treatment coefficients. These methods respectively control the false discovery rate and account for the joint distribution of the test statistics through resampling.

Consumption normalization. The PAP specifies per-capita household consumption as a primary outcome. We instead report total household consumption to keep it on the same household-level scale as income, profits, and investment, and to avoid adding noise from dividing by measured household size.

LATE estimation, the "constrained ITT," and never-takers. The PAP proposes LATE estimates for compliers and a "constrained ITT" restricted to participants who took any loan. We instead report the standard ITT and describe complier characteristics in Tables 10 and C.4. The ITT corresponds more closely to the lender's decision to offer a product, while the distinct profiles of *SEK* and *SEK+* compliers make a single LATE difficult to interpret. We omit the constrained ITT because take-up of any loan is high and balanced across arms (81–84%), and restricting the sample to borrowers would condition on a post-randomization outcome. We report the baseline characteristics of never-takers in Table C.2, but not their endline outcomes, because never-taker status is determined after randomization and the subgroup differs in composition across arms.

Group 1 and Group 2 participant structure. The PAP envisages one baseline survey for approximately 3,600 participants across 40 branches. COVID-19 interrupted data collection after Group 1 (N = 1,246). Data collection later resumed with a shorter, paper-based Group 2 survey administered by branch officers (N = 1,822), giving 3,068 participants in total. Because several Group 1 modules were omitted from Group 2, pooled PDS-LASSO specifications use only baseline variables available in both groups. The separate estimates for Group 1 and Group 2 in Tables C.17 and C.18 constitute the preregistered exploratory comparison of the earlier and later implementation samples.

Subjective profit expectations. The PAP lists subjective expectations about entrepreneurial profits as a primary outcome under Hypothesis H1:3. Appendix Table C.8 reports the available measures separately for crop farming, livestock breeding, and off-farm activi-

ties. The analysis differs from the PAP because the questions were asked only of respondents operating in the relevant activity at endline, the survey elicited successful- and unsuccessful-scenario profits and the probability of exceeding the last observed profit rather than a single household-level expectation, and the off-farm module was not administered consistently during the early Group 1 rollout. The resulting samples are smaller and sector-specific, so we treat these estimates as supplementary evidence.

Heterogeneity by income seasonality and expected-profit uncertainty. The PAP specifies interactions with above-median quarterly income variance, the coefficient of variation of expected profits, and the range between successful- and unsuccessful-scenario profits. These variables were collected only in the Group 1 baseline, so Appendix Table C.10 reports the analyses for the Group 1 endline sample. The smaller sample reduces precision, and the estimates need not generalize to Group 2. Appendix Table C.9 reports activity-based heterogeneity available in both groups; its respondent-gender interaction is exploratory.

Complier selection analysis. The PAP proposes comparing borrowers who select flexible contracts with those who select the standard contract, but does not prescribe a method. We implement this analysis in Tables 10 and C.4, comparing compliers with standard-contract borrowers within each treatment arm and comparing complier profiles across arms.

A.2. Deviations related to Part B

Village and branch structure. The PAP envisaged 320 villages across all 40 participating branches. The implemented design instead covers 440 villages across 20 rural branches, with 11–55 villages per branch. The smaller number of branches reflected staffing and logistical constraints; increasing the number of villages per branch more than offset this reduction. We also added an exploratory norms-shifting component in 40 *SEK* and 40 *SEK+* villages. The primary analysis pools the norms and information-only sub-arms within each product, while we report the norms results separately in the Appendix.

Phone follow-up, prospect outcome, and village-level lending volume. The PAP specified administrative contract counts, loan amounts, and borrower characteristics as Part B outcomes. We subsequently introduced a phone survey to collect additional borrower characteristics and, importantly, to construct the prospect- and client-to-village crosswalk required to link Al Amana's administrative records to study villages. Contract counts and disbursement amounts therefore cover only consenting respondents who could be linked to a study village and should be interpreted as lower bounds on total village-level credit activity. The number of prospects is a non-preregistered intermediate outcome capturing movement through the borrowing funnel. The PAP referred to average loan size at the village level. Because signed contracts are extremely sparse and most villages have no matched contract, an average conditional on positive take-up would be defined for a small, post-treatment-selected subset of villages. We instead report total gross amount disbursed per village, coding villages with no matched disbursement as zero. This outcome captures aggregate village-level lending volume but combines the extensive and intensive margins.

Repayment and further planned analyses. The PAP also proposed village-level repayment outcomes, a linkage between Parts A and B, a simulation of impacts for marginal borrowers,

qualitative follow-up work, and comparisons of operational costs. We do not report these analyses because take-up in Part B was too limited to support meaningful comparisons: the matched sample contains few signed contracts, no *SEK+* contracts, and very few *SEK* contracts. Moreover, most observed contracts had not reached maturity when the administrative data were extracted in October 2024. The available data therefore cannot reliably identify marginal borrowers, extrapolate Part A impacts to them, or compare repayment performance and implementation costs across products. We consequently restrict the Part B analysis to campaign implementation, stated preferences, prospects, contract counts, and observed borrower characteristics.

B. Variable definitions

B.1. Take-up outcomes

We construct take-up outcomes from Al Amana's administrative contract records for the baseline study sample. We restrict the analysis to the first contract signed by each individual within the study period, defined as the interval from the date of the baseline survey to the endline survey date (or, where the endline date is unavailable, to the end of the overall study window). Contracts with a release date more than 90 days before the individual's baseline survey are excluded.

Each contract in the administrative data is labeled with its product name. We classify contracts into three types as follows:

- **SEK (grace-period loan):** contracts recorded as *Produit Salaf El Karaoui*.
- **SEK+ (tailored loan):** contracts recorded as *Produit Salaf El Karaoui plus*, subject to the reclassification rule described below.
- **Standard:** contracts recorded as *Produit Individuel Entreprise*, *Produit Moumtaz*, or *Produit Idmaj*.[17]

B.1.1. Reclassification of SEK+ contracts

The tailored loan (SEK+) is designed so that the repayment schedule is divided into three equal periods with potentially different installment amounts. However, some borrowers who signed an SEK+ contract have chosen essentially flat repayment schedules, rendering their contract functionally identical to a standard loan. We therefore conservatively reclassify a SEK+ contract as standard if both of the following conditions are met: (i) the repayment schedule data contain at least one third of the contractually agreed installments, providing sufficient information to characterize the repayment profile; and (ii) the ratio of

[17] The latter two are variants of the standard PIE: *Produit Idmaj* applies identical contract terms to borrowers aged 18–35, while *Produit Moumtaz* extends the PIE to larger loan amounts (10,000–50,000 DH) with an additional government guarantee through the *Caisse Centrale de Garantie*. Neither modifies the repayment schedule structure, so both are classified as standard contracts.

the maximum to the minimum recorded installment amount is below 1.05, indicating negligible variation across repayment periods. This conservative reclassification ensures that the SEK+ take-up indicator captures genuine adoption of a tailored repayment structure rather than nominal product assignment alone.

To construct the repayment schedule-based variables, we use Al Amana's installment-level schedule data. After dropping insurance payments (installments of 10 MAD or less) and the initial fee payment, we compute the maximum, minimum, and intermediate installment amounts as well as the total number of recorded installments per contract.

B.1.2. Take-up indicators

We define five binary take-up outcomes at the individual level, indicating whether the individual signed a grace-period, tailored, standard, any, or no contract, respectively. The SEK+ and Standard indicators reflect the reclassification rule described below.

B.2. Business outcomes

Sales (annual, in MAD): Total annual revenues from household own-account activities, computed using AAA (Aggregating, Anchoring, and Adjusting; Anderson et al. (2021)). Sales are the sum of crop-farming revenues over the past 12 months, livestock revenues over the past 12 months, and annualized (×12) non-agricultural own-account revenues over the past 30 days. Non-agricultural revenues are the sum of final adjusted sales across up to three non-agricultural activities, each measured separately for three sale types: contract sales, infrequent sales, and frequent sales. For each sale type, revenues are summed across all activities; the three sale-type totals are then aggregated to obtain total monthly non-agricultural sales, which is annualized by multiplying by 12. Before aggregation, each sales component is right-tail winsorized separately, replacing values above the 95th percentile of its strictly positive observations with that percentile. No lower-tail winsorization is applied.

Expenses (annual, in MAD): Total annual expenditures on inputs, labor, rent and other costs across all household own-account activities, computed using the AAA method. Expenses are the sum of crop-farming expenditures over the past 12 months, livestock expenditures over the past 12 months, and annualized (×12) non-agricultural own-account expenditures over the past 30 days. Non-agricultural expenditures are the sum of final adjusted expenses across up to three non-agricultural activities, each measured separately for two expense elicitation methods: self-estimated total expenses (Type 1) and itemized AAA-computed expenses (Type 2). For each method, expenses are summed across all activities; the AAA-totals are used for the main specification. The resulting monthly non-agricultural expenses are annualized by multiplying by 12. Before aggregation, each expense component is right-tail winsorized separately, replacing values above the 95th percentile of its strictly positive observations with that percentile. No lower-tail winsorization is applied.

Profits (annual, in MAD): Total annual profits from household own-account activities, calculated as total sales minus total expenses. As detailed in Appendix B.2, each underlying sales and expense component is right-tail winsorized at the 95th percentile of its strictly

positive observations before aggregation. Profits are not winsorized directly, and no lower-tail winsorization is applied.

B.3. Time use

Crops (days per week): Total household days worked on crop-farming activities, converted to a weekly measure. The survey asks for the total number of days all household members worked on agriculture over the past 12 months. This is divided by 52 to obtain days per week. Missing values are set to zero.

Livestock (days per week): Total household days worked on livestock activities, converted to a weekly measure. The survey asks for the total number of days all household members worked on livestock over the past 12 months. This is divided by 52 to obtain days per week. Missing values are set to zero.

Off-farm non-agricultural (days per week): Total household days worked on non-agricultural own-account activities, converted to a weekly measure. The survey asks for the total number of days all household members worked on each non-agricultural activity over the past 30 days. However, the response constraint allows values up to 365, suggesting some respondents interpreted the question as referring to the past 12 months. Values exceeding 31 days are therefore treated as 12-month responses and divided by 12 to obtain a monthly figure. All values (corrected and uncorrected) are then divided by 4.5 (i.e., 30 days $\div$ 7) to convert from monthly to weekly. The household aggregate sums across up to three non-agricultural activities. Missing values are set to zero.

Total own-account (days per week): Sum of crops, livestock and off-farm non-agricultural days per week. This measure does not include wage labor.

Salaried work (days per week): Total household days spent in salaried employment over the past 7 days, summed across all household members (up to 15 members;). Already measured in weekly units. Missing values are set to zero.

Casual work (days per week): Total household days of casual or occasional work over the past 12 months, summed across all household members and divided by 52 to obtain a weekly measure. Missing values are set to zero.

The time use outcomes combine variables measured over different recall periods: 12 months for crops, livestock, and casual work; 30 days (with correction) for off-farm activities; and 7 days for salaried work. All are converted to days per week for comparability, but the different recall windows may introduce measurement differences across columns. None of the time use variables are winsorized.

B.4. Income

Salary income (annual, in MAD): Total household income from salaried employment over the past 12 months, summed across all household members (up to 15). Each member reports the total amount earned from salaried work directly. Winsorized at the upper 5th percentile of strictly positive values. Missing values are set to zero.

Occasional wage income (annual, in MAD): Total household income from casual labor

over the past 12 months, summed across all household members. Casual income is imputed for each member as the number of days of casual work over the past 12 months multiplied by the midpoint of the self-reported minimum and maximum daily wage. Winsorized at the upper 5th percentile of strictly positive values. Missing values are set to zero.

Other income (annual, in MAD): Total household income from non-wage, non-casual sources over the past 12 months. For each household member, respondents report the amount received from up to six "other" income sources, which include pensions, remittances, gifts and donations, proceeds from asset sales, and miscellaneous other income. The household aggregate sums across all members and all sources. Winsorized at the upper 5th percentile of strictly positive values. Missing values are set to zero.

Profits (annual, in MAD): Total annual profits from household own-account activities, defined as sales minus expenses, both computed using the AAA method. See Appendix B.2 for details.

Total household income (annual, in MAD): Salary income, occasional wage income, and other income are each right-tail winsorized at the 95th percentile of their strictly positive observations. The profit component follows the distinct construction described in Appendix B.2 and is not directly winsorized. Total household income is not additionally winsorized.

B.5. Investment

Investment is defined as the value of productive assets acquired by the household in the past 12 months. For each asset, investment equals the acquisition indicator multiplied by the reported purchase price. When a household reports acquiring an asset but the purchase price is missing, the within-type sample median price is imputed. Asset types with no recorded purchases have missing median prices; these are treated as zero.

General equipment (in MAD): Investment in general-purpose assets: car, pickup truck, and motorcycle.

Agricultural equipment (in MAD): Investment in agricultural tools and machinery: scooter, cart, truck, tractor, harvester, plough, rake, shovel, axe, wheelbarrow, sickle, oil mill, pump, and pitchfork, plus up to 4 "other" agricultural assets.

Livestock equipment (in MAD): Investment in livestock-related equipment: milk jugs, enclosure, stable, honey box, plus up to 1 "other" breeding asset.

Non-agricultural equipment (in MAD): Investment in non-agricultural business assets: sewing machine, weaving loom, clay turner, soldering iron, plus up to 4 "other" non-agricultural assets.

Animal purchases (in MAD): Total value of animals purchased in the past 12 months, includes up to 6 pre-listed animal types plus 3 "other" types.

Total investment (in MAD): Sum of general, agricultural, breeding, and non-agricultural equipment investment plus animal purchases.

Investment variables in this table are not winsorized. Non-zero shares are very low across all equipment categories (below 1% for most), reflecting the fact that productive asset

acquisition is a rare event in this population. Animal purchases account for the vast majority of total investment in the control group.

B.6. Subjective profit expectations

For each sector in which the household operated at endline, respondents reported annual profits expected under an extremely successful scenario, annual profits expected under an extremely unsuccessful scenario, and the subjective probability, on a 0–100 scale, that profits during the following 12 months would exceed the most recently observed profit level.

Crop-farming and livestock expectations are reported at the sector level. For off-farm activities, successful- and unsuccessful-scenario amounts are summed across up to three activities, while the reported probabilities are averaged across activities with non-missing responses. Households not operating in the relevant sector are left outside the estimation sample rather than assigned zero.

Successful-scenario amounts are winsorized at the upper 5th percentile of the non-missing distribution. Unsuccessful-scenario amounts are winsorized at the 2.5th and 97.5th percentiles because negative values represent economically meaningful expected losses. Probability responses are bounded between zero and 100 and are not winsorized. Coverage of the off-farm questions is substantially lower because the module was not consistently administered during part of the early Group 1 endline rollout.

B.7. Consumption

All consumption variables are annualized to 12-month totals in Moroccan Dirham. Components measured over shorter recall periods are scaled up: 7-day items are multiplied by 52, 30-day items by 12. Aggregate variables are winsorized at the upper 5th percentile of strictly positive values after construction.

Durables (annual, in MAD): Total expenditure on durable household goods acquired over the past 12 months. Covers 23 pre-listed item categories (e.g. furniture, appliances, electronics) plus one "other" category, for a total of up to 24 items. Missing values are set to zero.

Food consumption (annual, in MAD): Total food consumption over the past 12 months, constructed as the sum of four annualized components: (i) food purchases over the past 7 days ($\times 52$); (ii) souk-equivalent value of food received as donations over the past 7 days ($\times 52$); (iii) out-of-home food expenditure over the past 7 days ($\times 52$); and (iv) bread expenditure over the past 7 days ($\times 52$). Missing values are set to zero before annualization.

Non-food consumption (annual, in MAD): Total non-food, non-durable consumption over the past 12 months, constructed as the sum of three components: (i) recurring non-food expenditure over the past 30 days ($\times 12$; covers utilities, transport, personal care, etc.); (ii) occasional non-food expenditure over the past 12 months (covers clothing, education, health, etc.); and (iii) exceptional non-food expenditure over the past 12 months (covers ceremonies, major repairs, etc.).

Total consumption (annual, in MAD): Defined as the sum of durables, food consumption

(including the souk-equivalent value of food donations), and non-food consumption: i.e. total consumption equals durables + food purchases + food donations + out-of-home food + bread + all non-food components.

B.8. Contract characteristics: outcome construction

The contract-characteristics outcomes are constructed entirely from Al Amana's management information system data. The sample is restricted to individuals who signed at least one enterprise loan contract within the study period. Specifically, we retain contracts with a release date no earlier than 90 days before the individual's study start date and no later than their study end date. For individuals with multiple contracts satisfying this criterion, we retain only the first contract.

Number of contracts: For each individual in the administrative analysis sample, we count all enterprise loan contracts signed with Al Amana during the study period. This variable captures both the extensive margin of repeat borrowing and, for returning clients, the intensity of engagement with the lender.

Gross amount released: The gross disbursement amount as recorded in Al Amana's contract database, denominated in Moroccan dirhams (MAD). This is the total principal disbursed at signing, before any deductions for fees or insurance premia.

Loan duration: The scheduled loan duration in months, representing the agreed contractual period from disbursement to the final scheduled repayment.

B.9. Repayment outcomes: outcome construction

30+ days late (indicator): This indicator equals one if the actual contract end date is at least 30 calendar days after the scheduled end date. Both dates come from Al Amana's administrative records. Contracts that were still active but had not yet reached their scheduled end date at the October 2024 extraction are coded as not late.

Days versus schedule: This measure is the number of calendar days between the actual and scheduled contract end dates, calculated as *actual* – *scheduled*. Positive values indicate late completion and negative values early completion. Contracts still active but not yet due at the October 2024 extraction are assigned zero. The control-group mean of –78.76 days indicates substantial early repayment.

Number of unpaid installments: This is Al Amana's cumulative count of installments not paid by their due date. It captures payment irregularities during the loan cycle, including cases in which borrowers later caught up. In the control group, the mean is 2.92 and 48% of borrowers record at least one unpaid installment.

Coding active, not-yet-due contracts as on schedule avoids treating ongoing loans as delinquent. It may nevertheless attenuate estimated effects on late repayment if the treatment arms contain more active contracts because their loan durations are longer, as shown in Table C.3. No such imputation is needed for unpaid installments, which are recorded throughout the loan cycle.

B.10. Heterogeneity variables (Group 1 only)

Above-median income variance: This indicator measures household income seasonality using the Group 1 baseline exercise in which respondents allocated annual income across four quarters. We calculate the variance of the four quarterly amounts and set the indicator equal to one when it exceeds the sample median among respondents with complete data. Higher values indicate a less even income profile, which the pre-analysis plan identifies as a possible predictor of stronger take-up and impacts of the tailored SEK+ loan.

Profit coefficient of variation. Using the Group 1 baseline survey, we construct a measure of uncertainty about future entrepreneurial profits. Respondents reported expected profits in an extremely unsuccessful year, L_i, and an extremely successful year, U_i, as well as the probabilities that profits would fall below or above the midpoint $(L_i + U_i)/2$. We rescale the two probabilities to sum to one and assume that profits are uniformly distributed within each half of the elicited range. From the resulting distribution, we calculate the implied mean $\widehat{\mu}_i$ and standard deviation $\widehat{\sigma}_i$ and define $CV_i = \widehat{\sigma}_i/\widehat{\mu}_i$. The CV is set to missing when the elicited bounds or probabilities are invalid, or when the implied mean is not positive. When $U_i = L_i > 0$, the CV equals zero. In the regressions, the CV is multiplied by 10, so one unit corresponds to a 0.1 increase in the original measure.

Expected-profit range. The expected-profit range is defined as $R_i = U_i - L_i$. Unlike the CV, it does not require an assumption about the distribution of profits within the elicited bounds. It is divided by 10,000 in the regressions, so one unit corresponds to a MAD 10,000 increase in the range.

C. Appendix Tables and Figures

C.1. Individual-level intervention

C.1.1. Additional results

TABLE C.1. Sales and expenditures of own-account household activities

	Crop farming		Livestock		Non-agricultural				WY-p val.
	Sales	Expenses	Sales	Expenses	Contract sales	Low-freq sales	High-freq sales	Total expenses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment: SEK	214	316	468	241	92	164	3,363	1,085	
	(833)	(461)	(432)	(156)	(224)	(1,142)	(2,623)	(1,413)	0.581
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.785}	{0.508}	{0.275}	{0.126}	{0.679}	{0.898}	{0.273}	{0.502}	
Treatment: SEK+	753	404	50	-19	-76	1,046	6,320**	2,687*	
	(886)	(494)	(411)	(151)	(158)	(1,223)	(2,851)	(1,540)	0.226
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.742]	[1.000]	
	{0.396}	{0.418}	{0.908}	{0.916}	{0.708}	{0.408}	{0.037}	{0.096}	
Westfall–Young p (joint)	0.609	0.631	0.440	0.217	0.897	0.611	0.066	0.166	0.376
Mean (control)	3,644.49	2,389.06	2,333.23	1,052.73	235.57	4,141.29	9,593.74	6,828.47	
Non-zero % (control)	11.32	22.75	15.47	21.94	0.46	7.39	9.82	16.05	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Observation unit: household. Outcomes are annual sales and expenses of own-account activities, in Moroccan dirhams, disaggregated by sector and computed by the AAA method (Aggregating, Anchoring and Adjusting; Anderson et al. (2021)): crop-farming sales (column 1) and expenses (column 2); livestock-breeding sales (column 3) and expenses (column 4); and off-farm non-agricultural activities, split into contract sales (column 5), low-frequency sales (column 6), high-frequency sales (column 7), and total expenses (column 8). This disaggregated sector and sales-frequency decomposition was not separately prespecified and is exploratory. Crop-farming and livestock components are measured over the 12 months prior to the survey; non-agricultural components are measured over the past 30 days and annualized. Before aggregation, each underlying sales and expense component is right-tail winsorized separately, replacing values above the 95th percentile of its strictly positive observations with that percentile. No lower-tail winsorization is applied. *Non-zero share* is the proportion of control-group households with a strictly positive outcome. Each column reports coefficients from a PDS-LASSO regression of the outcome on two treatment-arm dummies (SEK and SEK+), controlling for branch fixed effects and baseline borrower and household covariates selected by the LASSO from the set shown in Table 1; the post-LASSO OLS coefficients are reported. See Belloni et al. (2014) for details on the PDS-LASSO procedure. Heteroskedasticity-robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Randomisation-inference p-values (Young (2019); 1,000 permutations) for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values (Anderson, 2008) in square brackets, computed across all coefficients in the table. Column 9 reports Westfall–Young stepdown-adjusted p-values: for each treatment row, the joint p-value that the corresponding treatment coefficient is zero across all eight equations; the "Westfall–Young p (joint)" row reports, in columns 1–8, the joint p-value that both treatment coefficients are zero within each equation, and, in column 9, the experiment-wise p-value for the null that all 16 treatment coefficients across the eight equations are jointly zero.

TABLE C.2. Balance at baseline between nevertakers across treatment arms

	Total	Control	Treatment effect vs control					
	(1)	(2)	(3) SEK		(4) SEK+		(5) Any	
	N	Mean	β	SE	β	SE	β	SE
Panel A: Respondent and household characteristics								
Respondent is female	522	0.247	0.059	(0.047)	0.009	(0.047)	0.036	(0.041)
Respondent age	522	43.462	-2.631*	(1.390)	-4.185***	(1.420)	-3.365***	(1.251)
Respondent has any education	522	0.627	0.027	(0.053)	0.067	(0.051)	0.046	(0.046)
Respondent is household head	518	0.694	-0.033	(0.051)	-0.020	(0.050)	-0.027	(0.044)
# household members	522	5.107	-0.175	(0.311)	-0.340	(0.299)	-0.253	(0.286)
Panel B: Access to credit								
Loan from formal institution	525	0.189	-0.030	(0.038)	0.019	(0.041)	-0.007	(0.034)
Loan from informal source	521	0.075	-0.037	(0.026)	0.013	(0.031)	-0.013	(0.025)
Panel C: Self-employment activities								
# activities	528	2.394	-0.232	(0.171)	-0.148	(0.178)	-0.192	(0.150)
Does crop-farming								
Cereal growing	527	0.269	-0.006	(0.042)	-0.036	(0.040)	-0.020	(0.036)
Arboriculture	527	0.256	-0.072*	(0.041)	-0.049	(0.041)	-0.061*	(0.036)
Vegetable growing	527	0.156	0.035	(0.037)	-0.039	(0.035)	-0.000	(0.031)
Does livestock breeding								
Breeding of cattle, sheep, goats	527	0.356	-0.020	(0.049)	-0.020	(0.050)	-0.020	(0.043)
Breeding of poultry, rabbits	527	0.300	-0.065	(0.045)	-0.023	(0.047)	-0.045	(0.040)
Production derived from livestock	527	0.150	-0.019	(0.037)	-0.005	(0.036)	-0.012	(0.032)
Does off-farm activity								
Commerce	527	0.344	-0.015	(0.052)	0.002	(0.053)	-0.007	(0.046)
Crafts	527	0.119	0.001	(0.034)	0.009	(0.036)	0.004	(0.030)
Fishing	524	0.069	-0.022	(0.022)	-0.007	(0.026)	-0.015	(0.021)
Construction	528	0.181	-0.067*	(0.037)	-0.028	(0.039)	-0.049	(0.034)
Transport	527	0.044	0.009	(0.022)	0.002	(0.022)	0.006	(0.019)
Services	527	0.125	-0.010	(0.035)	0.020	(0.039)	0.004	(0.032)
Other	439	0.030	0.024	(0.024)	0.032	(0.026)	0.028	(0.021)
Panel D: Has income from								
Salaried labor	525	0.119	-0.004	(0.036)	-0.018	(0.037)	-0.010	(0.032)
Casual labor	525	0.213	0.007	(0.039)	-0.002	(0.040)	0.002	(0.034)
Pension	522	0.025	0.031	(0.020)	0.015	(0.020)	0.024	(0.017)
Remittances	522	0.164	0.078*	(0.043)	0.082*	(0.044)	0.080**	(0.037)
Donations	524	0.031	0.018	(0.021)	0.040*	(0.023)	0.029	(0.018)
Asset sales	524	0.038	-0.005	(0.019)	-0.027	(0.017)	-0.016	(0.016)
Other	419	0.008	0.020	(0.017)	0.001	(0.012)	0.012	(0.013)
Panel E: Attrition								
Not surveyed at endline	529	0.200	-0.053	(0.041)	-0.064	(0.042)	-0.058	(0.037)
Omnibus F-test across all variables			1.51 (p=0.051)		1.33 (p=0.125)		2.07 (p=0.002)	
Omnibus F-test (endline sample only)			1.78 (p=0.011)		1.50 (p=0.056)		1.78 (p=0.012)	

Notes: Source: baseline surveys. Sample limited to respondents who did not sign any contract (never-takers) within the respective treatment arms. Column (1) shows the number of non-missing observations for each variable; column (2) reports the control-group mean; columns (3)–(5) report OLS differences in means (β) for SEK, SEK+, and Any treatment (= SEK or SEK+), from regressions of the outcome on treatment dummies with branch fixed effects. Standard errors robust to heteroskedasticity in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. The "Omnibus F-test across all variables" tests joint equality of the Panel A–D covariates across arms by regressing each treatment indicator on that set of balance variables (plus branch fixed effects) and reporting the `testparm` F-statistic and p-value; the second omnibus row ("endline sample only") repeats the same joint test restricting the sample to never-takers observed at endline.

TABLE C.3. Loan contract characteristics by contract type

	Standard	SEK	SEK+	SEK+ by first-period level		
				Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ex-ante contract characteristics						
Gross amount released (MAD)	10,046.37	9,757.80	9,317.92	11,185.64	11,182.70	7,918.16
	[6,873.68]	[7,379.38]	[5,733.08]	[6,559.33]	[6,301.10]	[4,586.81]
Loan duration (months)	19.86	20.99	20.26	22.39	21.29	18.75
	[6.84]	[5.96]	[6.17]	[7.46]	[5.93]	[4.85]
Observations	1,876	321	343	86	26	197
Panel B: Realised contract statistics						
Effective principal (MAD)	7,380.47	6,429.59	6,679.75	8,235.37	9,586.07	6,340.35
	[5,637.89]	[5,810.74]	[4,691.20]	[5,417.18]	[5,429.20]	[3,937.57]
Effective interest (MAD)	1,915.02	2,241.81	1,812.50	2,965.01	2,512.62	1,329.10
	[2,104.80]	[2,351.43]	[2,022.44]	[2,926.24]	[2,133.75]	[1,266.07]
Effective borrowing cost (%)	16.73	21.09	16.69	22.43	19.85	15.17
	[6.95]	[6.83]	[7.65]	[9.22]	[6.66]	[4.98]
Observations	1,866	319	340	86	26	197

Notes: Data source: Al Amana administrative records. Schedule-profile comparisons are exploratory and descriptive; realised schedule choice was not randomized. Columns (1)–(3) correspond to the three contract types: Standard, SEK (grace period), and SEK+ (tailored repayment levels). Columns (4)–(6) decompose SEK+ contracts by the level assigned in the first repayment period (low, medium, or high). Panel A reports ex-ante characteristics determined at disbursement. Panel B reports realised repayment statistics computed from scheduled amortisation tables. The number of observations is lower in Panel B because schedules are unavailable for contracts that were prematurely terminated or converted to a different contract type before completion.

TABLE C.4. Mean differences at baseline between compliers and non-compliers: administrative eligibility data

		(1)		(2)		(3)		(4)		Mean differences (t-tests)			
		Complier SEK		Non-Comp. SEK		Complier SEK+		Non-Comp. SEK+		Within arm		Across arm	
										(1)-(2)	(3)-(4)	(1)-(3)	(2)-(4)
Variable	N	Mean	SE	Mean	SE	Mean	SE	Mean	SE				
Client characteristics													
Renewal client (vs. first-time)	1687	0.58	0.03	0.64	0.02	0.66	0.03	0.62	0.02	-0.06*	0.04	-0.08**	0.02
Financial capacity (MAD)													
Repayment capacity – PIE score (MAD/month)	1687	1512.24	36.07	1527.64	32.41	1467.92	38.46	1603.97	30.57	-15.40	-136.05***	44.32	-76.33*
Business assets (MAD)	1687	37498.24	2693.61	35313.00	2057.50	31161.31	1987.41	37139.69	1818.79	2185.24	-5978.39**	6336.94*	-1826.69
Company equity (MAD)	1687	37352.40	2679.09	35275.68	2057.74	30854.20	1920.61	37100.35	1813.85	2076.72	-6246.15**	6498.20**	-1824.67
Household assets (MAD)	1657	87182.42	8955.53	63885.54	7167.17	54938.23	5126.00	72268.21	4083.44	23296.88**	-1.7e+04***	32244.20***	-8382.67
Monthly business expenses (MAD)	1678	909.61	72.26	926.26	61.63	929.21	77.87	911.99	57.91	-16.65	17.22	-19.60	14.27
Monthly sales (MAD)	1687	10574.01	686.99	8832.40	381.72	9653.06	474.81	9648.95	510.88	1741.60**	4.10	920.95	-816.55
Cash holdings (MAD)	1683	2716.90	269.89	2728.63	142.45	2512.88	135.03	2858.32	187.94	-11.73	-345.44	204.02	-129.69
Fixed assets (MAD)	1624	17926.66	1559.72	20021.13	1673.99	12959.97	1083.49	21000.20	1344.16	-2094.47	-8040.23***	4966.69***	-979.07
Other monthly income (MAD) †	1179	980.09	40.67	994.08	42.31	932.54	43.02	1106.41	38.12	-13.99	-173.87***	47.55	-112.33**
Purchases (MAD) †	1179	10187.08	816.95	6828.77	441.87	7863.35	489.01	8211.80	612.08	3358.31***	-348.45	2323.73**	-1383.03*
Finished products stock (MAD) †	1083	15946.76	1596.06	12885.70	1127.78	14084.52	1136.02	12771.84	726.31	3061.06	1312.67	1862.24	113.86
Sector of activity													
Sector: Agriculture	1687	0.38	0.03	0.29	0.02	0.28	0.02	0.33	0.02	0.09***	-0.05	0.10***	-0.03
Sector: Commerce	1687	0.39	0.03	0.39	0.02	0.40	0.03	0.45	0.02	-0.00	-0.04	-0.01	-0.05*
Sector: Artisanat	1687	0.11	0.02	0.14	0.02	0.16	0.02	0.09	0.01	-0.04	0.06***	-0.05*	0.05**
Sector: Services	1687	0.12	0.02	0.17	0.02	0.17	0.02	0.13	0.02	-0.05**	0.03	-0.05*	0.04
Loan use													
Loan use: Purchase merchandise	1687	0.79	0.02	0.69	0.02	0.63	0.03	0.80	0.02	0.10***	-0.17***	0.16***	-0.11***
Loan use: Purchase raw materials	1687	0.06	0.01	0.12	0.01	0.10	0.02	0.05	0.01	-0.06***	0.06***	-0.04**	0.08***
Loan use: Purchase fixed assets	1687	0.14	0.02	0.15	0.02	0.22	0.02	0.13	0.02	-0.01	0.09***	-0.08***	0.02
Loan use: Micro-enterprise setup	1687	0.01	0.01	0.04	0.01	0.04	0.01	0.02	0.01	-0.03***	0.02*	-0.03***	0.02
Observations		301		535		332		519					

Notes: This table reports a broadly prespecified but descriptively implemented selection analysis of means and standard errors of financial eligibility-assessment variables collected by Al Amana loan officers at application. *Compliers* are borrowers in the SEK or SEK+ arm who signed the contract offered to them; *non-compliers* are borrowers in the same arm who instead signed a standard contract. Individuals who did not sign a contract are excluded. Compliers are borrowers in the SEK (SEK+) arm who took up the grace-period (tailored) contract offered to them; non-compliers are borrowers in the same arms who opted for the standard contract instead. Individuals who did not sign any contract are excluded. Column N reports the number of non-missing observations pooled across all four groups. Mean differences are estimated via OLS with heteroskedasticity-robust standard errors; within-arm comparisons test for differential selection into the made-to-measure contract within each treatment arm, while across-arm comparisons assess the comparability of compliers and non-compliers across arms. Variables marked † have a fill rate of approximately 64–70% and should be interpreted with caution. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.5. Household asset ownership, extensive margin

	Business assets					Animal types	
	General	Agri.	Breeding	Non-agri.	Total		
	(1)	(2)	(3)	(4)	(5)	(6)	
Any ownership (= 1 if owns ≥ 1)							
Treatment: SEK	-0.008	0.012	0.016	0.009	0.013	0.009	
	(0.013)	(0.021)	(0.017)	(0.012)	(0.021)	(0.019)	0.879
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.510}	{0.583}	{0.344}	{0.475}	{0.541}	{0.654}	
Treatment: SEK+	-0.016	-0.005	0.020	0.031**	0.012	-0.003	
	(0.013)	(0.021)	(0.017)	(0.013)	(0.021)	(0.019)	0.106
	[1.000]	[1.000]	[1.000]	[0.305]	[1.000]	[1.000]	
	{0.216}	{0.786}	{0.248}	{0.023}	{0.550}	{0.880}	
Westfall–Young p (joint)	0.360	0.798	0.404	0.041	0.762	0.852	0.192
Mean (control)	0.084	0.408	0.173	0.073	0.470	0.259	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. This asset-ownership analysis was added post hoc because the preregistered investment-flow measure covers only purchases during the 12 months preceding endline. Each column reports ITT estimates from a separate regression of a binary ownership indicator (= 1 if the household owns at least one asset in the category) on the two treatment-arm dummies. Columns (1)–(5) cover business assets for professional use: general, i.e. car, pickup, or motorcycle (column 1); agricultural, 14 tool and vehicle types (column 2); breeding, 5 equipment types (column 3); non-agricultural, 7 equipment types (column 4); and any business asset (column 5). Column (6) indicates ownership of any animal across seven categories (local cattle, improved cattle, sheep, goats, draft animals, poultry, and rabbits). Missing ownership indicators are imputed as zero. Covariates are selected by PDS-LASSO from the baseline control set shown in Table 1 (respondent demographics, baseline activity indicators, and baseline income sources, with missing-value indicators); branch fixed effects are included in every specification. Heteroskedasticity-robust standard errors in parentheses. Randomisation-inference p-values (Young (2019); 1,000 permutations stratified by branch) in curly brackets. Sharpened false-discovery-rate (FDR) q-values (Anderson, 2008) in square brackets, computed across all coefficients in the table. The final column reports Westfall–Young stepdown-adjusted p-values: for each treatment row, the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all six outcomes; the "Westfall–Young p (joint)" row reports, in columns (1)–(6), the joint p-value that both treatment coefficients are zero within each outcome, and, in the final column, the experiment-wise p-value that all twelve treatment coefficients across the six outcomes are jointly zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.6. Household activities by main sector (extensive margin)

	Crop farming	Livestock	Off-farm	WY-p val.
	(1)	(2)	(3)	
Treatment: SEK	0.028	0.017	-0.001	
	(0.020)	(0.019)	(0.021)	0.390
	[1.000]	[1.000]	[1.000]	
	{0.159}	{0.386}	{0.980}	
Treatment: SEK+	-0.010	-0.002	0.017	
	(0.020)	(0.019)	(0.021)	0.836
	[1.000]	[1.000]	[1.000]	
	{0.638}	{0.897}	{0.456}	
Westfall–Young p (joint)	0.270	0.582	0.680	0.601
Mean (control, %)	35.8	26.1	45.7	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. This extensive-margin decomposition was not separately prespecified and is exploratory. Each column reports ITT estimates from a separate regression of an endline sector-participation indicator on the two treatment-arm dummies. The dependent variable equals one if any household member participated in at least one self-employment activity within the corresponding sector during the 12 months preceding the endline survey: crop farming (column 1; activities in columns 1–3 of Table C.7), livestock breeding (column 2; columns 4–6), or off-farm activities (column 3; columns 7–12). Covariates are selected by PDS-LASSO from the baseline control set (respondent demographics, baseline activity indicators, and baseline income sources, with missing-value indicators); branch fixed effects are included in every specification. Heteroskedasticity-robust standard errors in parentheses. Randomisation-inference p-values (Young, 2019), based on 1,000 permutations stratified by branch, in curly braces. Sharpened false-discovery-rate (FDR) q-values (Anderson, 2008) in square brackets, computed across all coefficients in the table. The last column reports Westfall–Young stepdown-adjusted p-values: for each treatment row, the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all three sector outcomes; the "Westfall–Young p (joint)" row reports, in columns 1–3, the joint p-value that both treatment coefficients are zero within each sector, and, in the last column, the experiment-wise p-value that all six treatment coefficients across the three sectors are jointly zero. "Mean (control, %)" reports the percentage of control-group households participating in each sector. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.7. Household activities

	Agricultural activities						Non-agricultural activities						WY-p val.
	Cereals	Arboricult.	Vegetables	Livestock	Poultry	Livest. prod.	Commerce	Transport	Crafts	Services	Fishing	Construction	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
Treatment: SEK	0.014	0.032**	-0.002	0.014	0.001	-0.006	0.000	-0.004	-0.004	-0.009	-0.004	0.005	
	(0.017)	(0.016)	(0.014)	(0.017)	(0.016)	(0.008)	(0.014)	(0.011)	(0.008)	(0.018)	(0.005)	(0.010)	0.438
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.398}	{0.050}	{0.892}	{0.425}	{0.953}	{0.503}	{0.982}	{0.683}	{0.586}	{0.629}	{0.348}	{0.637}	
Treatment: SEK+	-0.009	0.014	0.002	-0.008	-0.015	-0.001	0.016	-0.009	-0.002	-0.015	-0.005	0.017	
	(0.017)	(0.016)	(0.015)	(0.017)	(0.016)	(0.009)	(0.015)	(0.011)	(0.008)	(0.018)	(0.005)	(0.011)	0.802
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.595}	{0.356}	{0.881}	{0.663}	{0.338}	{0.941}	{0.266}	{0.422}	{0.777}	{0.424}	{0.259}	{0.126}	
Westfall–Young p (joint)	0.598	0.091	0.984	0.634	0.525	0.725	0.443	0.643	0.799	0.636	0.427	0.221	0.653
Mean (control, %)	18.8	13.0	13.4	20.7	14.3	3.5	11.3	6.1	2.9	23.7	1.4	5.9	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664	

Notes: Data source: endline household survey. Unit of observation: household. This extensive-margin decomposition was not separately prespecified and is exploratory. Each column reports ITT estimates from a separate regression of an endline activity indicator on the two treatment-arm dummies. The dependent variable equals one if any household member participated in the corresponding self-employment activity during the 12 months preceding the endline survey; columns 1–6 report agricultural activities and columns 7–12 non-agricultural activities. Covariates are selected by PDS-LASSO from the baseline control set (respondent demographics, baseline activity indicators, and baseline income sources, with missing-value indicators); branch fixed effects are included in every specification. Heteroskedasticity-robust standard errors in parentheses. Randomisation-inference p-values (Young, 2019), based on 1,000 permutations stratified by branch, in curly braces. Sharpened false-discovery-rate (FDR) q-values (Anderson, 2008) in square brackets, computed across all coefficients in the table. The last column reports Westfall–Young stepdown-adjusted p-values: for each treatment row, the joint p-value that the corresponding treatment coefficient (SEK or SEK+) is zero across all twelve activity outcomes; the "Westfall–Young p (joint)" row reports, in columns 1–12, the joint p-value that both treatment coefficients are zero within each activity, and, in the last column, the experiment-wise p-value that all twenty-four treatment coefficients across the twelve activities are jointly zero. "Mean (control, %)" reports the percentage of control-group households participating in each activity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.8. Subjective expectations of profits from own-account activities

	Crop-farming			Livestock breeding			Off-farm			WY-p val.
	Amount		Percent	Amount		Percent	Amount		Percent	
	Success	Fail	Success	Success	Fail	Success	Success	Fail	Success	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment: SEK	-1,146	296	-1	-1,240	-455	0	45	1,451	2	
	(2,221)	(683)	(2)	(1,429)	(548)	(2)	(2,565)	(1,619)	(2)	0.959
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.618}	{0.680}	{0.751}	{0.366}	{0.420}	{0.986}	{0.980}	{0.370}	{0.342}	
Treatment: SEK+	-1,112	-19	2	-555	-195	-0	1,038	1,160	1	
	(2,280)	(738)	(2)	(1,408)	(560)	(2)	(2,440)	(1,481)	(2)	0.970
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
	{0.660}	{0.986}	{0.375}	{0.696}	{0.723}	{0.845}	{0.682}	{0.469}	{0.579}	
Westfall–Young p (joint)	0.846	0.898	0.571	0.556	0.642	0.976	0.892	0.565	0.529	0.996
Mean (control)	19,381	3,413	49	13,642	3,148	49	23,486	9,845	51	
Non-zero % (control)	87.13	43.07	94.53	80.63	50.00	93.21	100.00	95.05	100.00	
Branch FE + PDS-LASSO	✓	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	652	652	649	700	699	696	329	329	329	

Notes: Data source: endline household survey. Unit of observation: household. For each activity sector (crop-farming, livestock, off-farm), respondents state (i) expected profit under an extremely successful scenario (columns 1, 4, 7), (ii) expected profit under an extremely unsuccessful scenario — profits may be negative (columns 2, 5, 8), and (iii) the perceived probability (0–100%) that profit over the next 12 months will exceed the household's last observed profit (columns 3, 6, 9). All amounts are annual totals in Moroccan Dirham. Off-farm expectations aggregate across up to three non-agricultural activities (amounts summed, probabilities averaged). The sample is restricted to households with an activity in the relevant sector; no zeros are imputed for non-respondents. Response rates for off-farm expectations (columns 7–9) were very low among Group 1 households due to a survey rollout effect; see Appendix A. Success-scenario amounts are winsorized at the upper 5th percentile of non-missing values (right tail only). Failure-scenario amounts are winsorized at both tails — at the 2.5th and 97.5th percentiles of non-missing values — since negative values represent economically meaningful expected losses. Probability variables are bounded on [0, 100] and are not winsorized. *Non-zero share* is the percentage of control-group households with a strictly positive outcome. Coefficients from a PDS-LASSO regression of each outcome on two treatment-arm dummies, controlling for branch fixed effects and baseline covariates selected by the LASSO from the set shown in Table 1; see Belloni et al. (2014). Heteroskedasticity-robust standard errors in parentheses. Randomization-inference p-values (Young (2019); 1,000 permutations) in curly brackets. Sharpened FDR q-values (Anderson, 2008), calculated jointly across both treatment arms in square brackets. Column 10 reports Westfall–Young stepdown-adjusted p-values: per-equation joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value and (ii) the experiment-wise p-value testing the null that all 18 treatment coefficients across the nine equations are jointly zero. *,p<0.10; **,p<0.05; ***,p<0.01.

C.1.2. Heterogeneity analyses

TABLE C.9. Sales and profits of household business activities, heterogeneity analysis

	Female		Crop farming		Breeding		Off-farm	
	Sales	Profits	Sales	Profits	Sales	Profits	Sales	Profits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SEK	5,915	3,449	3,858	2,516	2,208	2,082	-1,552	-784
	(3,858)	(2,388)	(4,017)	(2,665)	(4,159)	(2,943)	(4,333)	(2,762)
SEK+	10,189**	6,909***	5,741	3,664	4,322	1,712	605	273
	(4,132)	(2,508)	(4,109)	(2,605)	(4,492)	(2,879)	(4,916)	(2,966)
SEK × Female	-4,376	-2,200						
	(5,554)	(3,810)						
SEK+ × Female	-8,951	-7,887**						
	(5,756)	(3,821)						
Female	-11,980***	-3,754	-15,968***	-6,897***	-16,260***	-7,145***	-17,067***	-7,471***
	(3,996)	(2,799)	(2,531)	(1,714)	(2,473)	(1,663)	(2,570)	(1,733)
SEK × Crop farming			2,007	820				
			(6,036)	(3,764)				
SEK+ × Crop farming			5,833	3,347				
			(6,727)	(4,068)				
Crop farming			1,579	495				
			(3,882)	(2,345)				
SEK × Breeding					4,863	1,421		
					(5,991)	(3,831)		
SEK+ × Breeding					7,014	6,249		
					(6,830)	(4,248)		
Breeding					-1,916	-2,358		
					(4,235)	(2,706)		
SEK × Off-farm							10,619*	6,107
							(5,957)	(3,787)
SEK+ × Off-farm							12,104*	7,570*
							(6,543)	(3,937)
Off-farm							5,957	2,342
							(3,998)	(2,632)
Mean outcome (control)	19,948.31	9,678.04	19,948.31	9,678.04	19,948.31	9,678.04	19,948.31	9,678.04
Mean interaction var. (control)	0.259	0.259	0.345	0.345	0.509	0.509	0.680	0.680
Branch FE + PDS-LASSO controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2,664	2,664	2,664	2,664	2,664	2,664	2,664	2,664

Notes: This table reports heterogeneous treatment effects on sales and profits of household own-account activities (annual totals in Moroccan Dirham). Sales and expense components are winsorized at the 95th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized. Each pair of columns interacts the treatment indicators *SEK* and *SEK+* with a baseline characteristic: respondent gender, baseline household activity in crop farming (cereal growing, arboriculture, or vegetable growing), livestock breeding (livestock farming, poultry farming, or livestock products), and off-farm activities (commerce, crafts, fishing, construction, transport, or services). The respondent-gender interaction was not prespecified and is exploratory. Baseline activity controls that are components of the interaction variable are excluded from the candidate set in each specification. The three aggregate activity variables have 23–166 missing values at baseline; missing values are imputed to zero and a missing indicator is included as a control. All regressions include branch fixed effects and baseline controls selected by PDS-LASSO, with the treatment variables, their interactions, the heterogeneity variable level, and branch fixed effects always included. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.10. Sales and profits of household business activities, heterogeneity analysis (Group 1)

	Income variance		Expected profits			
	(experienced)		CV (per 0.1)		Range (per 10k DH)	
	Sales	Profits	Sales	Profits	Sales	Profits
	(1)	(2)	(3)	(4)	(5)	(6)
SEK	-416	-1,880	9,919	8,028	10,536**	6,542*
	(6,915)	(4,809)	(10,061)	(7,220)	(5,068)	(3,624)
SEK+	425	-4,142	35,170***	23,474***	7,317	2,275
	(7,112)	(4,679)	(12,081)	(9,044)	(5,726)	(4,588)
SEK × High income variance	13,073	11,807				
	(10,534)	(7,395)				
SEK+ × High income variance	27,489**	23,579***				
	(12,170)	(8,523)				
High income variance	1,162	-816				
	(6,933)	(5,172)				
SEK × Profit CV			183	-382		
			(3,366)	(2,380)		
SEK+ × Profit CV			-8,863**	-6,139*		
			(4,376)	(3,225)		
Profit CV			-3,169	-1,807		
			(2,317)	(1,494)		
SEK × Profit range					-15	355
					(822)	(721)
SEK+ × Profit range					3,462**	3,161**
					(1,512)	(1,459)
Profit range					690	85
					(518)	(572)
Mean outcome (control)	20,501.11	11,453.73	18,195.56	9,342.58	18,162.78	9,320.77
Mean interaction var. (control)	0.485	0.485	2.082	2.082	2.456	2.456
Branch FE + PDS baseline controls	✓	✓	✓	✓	✓	✓
Observations	897	897	828	828	839	839

Notes: This table reports heterogeneous treatment effects on sales and profits of household own-account activities (annual totals in Moroccan Dirham). Sales and expense components are winsorized at the 95th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized. All interaction variables are measured at baseline and available for the Group 1 sample only; observations with a missing interaction variable are dropped, so sample sizes differ across column pairs. Columns (1)–(2) interact the treatment indicators *SEK* and *SEK+* with an indicator for above-median *experienced* income variability: the variance of quarterly household income over the 12 months before baseline. Approximately one third of the sample reports zero income variance, so the median split partly captures the extensive margin of income-generating activity. Columns (3)–(6) use two summaries of the respondent's *expected* profit distribution, elicited from profits in an extremely successful year (U) and an extremely unsuccessful year (L). Columns (3)–(4) interact with a coefficient of variation (CV): a piecewise-uniform distribution is fitted by taking the midpoint $M = (L + U)/2$ and using the reported probabilities that profits fall above and below M renormalized to sum to one as the masses of the lower interval $[L, M]$ and upper interval $[M, U]$, each assumed uniform; the CV is the implied standard deviation divided by the implied mean, defined only when that mean is strictly positive, and rescaled by a factor of 10 so that the coefficient corresponds to a 0.1 increase in the CV. Columns (5)–(6) interact with the range of expected profits ($U - L$), rescaled per 10,000 Dirham. All regressions include branch fixed effects and baseline controls selected by PDS-LASSO, with the treatment variables, their interactions, the interaction-variable level, and branch fixed effects always included. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.1.3. Robustness

TABLE C.11. Sales, expenditures and profits of own-account household activities, winsorized 2 percent

	HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	4,502	1,918	2,548	
	(4,460)	(2,155)	(2,904)	0.571
	[0.235]		[0.235]	
	{0.367}	{0.412}	{0.407}	
Treatment: SEK+	9,151*	4,125*	5,020*	
	(4,797)	(2,302)	(2,972)	0.100
	[0.224]		[0.224]	
	{0.054}	{0.075}	{0.106}	
Westfall–Young p (joint)	0.098	0.133	0.180	0.181
Mean (control)	23,912.69	11,918.75	11,993.95	
Non-zero % (control)	35.33	41.80	30.72	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Sales and expense components are winsorized at the 98th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.12. Sales, expenditures and profits of own-account household activities, winsorized 1 percent

	HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	2,301	1,483	884	
	(6,125)	(2,524)	(4,458)	0.844
	[1.000]		[1.000]	
	{0.740}	{0.632}	{0.851}	
Treatment: SEK+	8,200	5,038*	3,793	
	(6,582)	(2,851)	(4,392)	0.140
	[1.000]		[1.000]	
	{0.184}	{0.071}	{0.392}	
Westfall–Young p (joint)	0.323	0.128	0.619	0.245
Mean (control)	27,670.02	12,892.87	14,777.14	
Non-zero % (control)	35.33	41.80	30.72	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Sales and expense components are winsorized at the 99th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.13. Sales, expenditures and profits of own-account household activities, unwinsorized

	HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	-10,793	-12,874	2,136	
	(13,143)	(11,874)	(5,658)	0.538
	[1.000]		[1.000]	
	{0.425}	{0.292}	{0.691}	
Treatment: SEK+	769	-3,973	4,560	
	(14,082)	(12,573)	(5,825)	0.664
	[1.000]		[1.000]	
	{0.962}	{0.767}	{0.388}	
Westfall–Young p (joint)	0.648	0.505	0.598	0.784
Mean (control)	42,947.15	29,469.78	13,477.37	
% > 0 (control)	35.33	41.80	30.60	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Profits are reported in column 3 as sales minus expenses. Variables are unwinsorised. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.14. Sales, expenditures and profits of own-account household activities, self-reported measures via direct survey items

	HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	2,206	1,127	1,013	
	(2,176)	(955)	(907)	0.417
	[0.508]		[0.508]	
	{0.355}	{0.245}	{0.274}	
Treatment: SEK+	3,712	798	869	
	(2,347)	(961)	(904)	0.195
	[0.508]		[0.508]	
	{0.108}	{0.392}	{0.349}	
Westfall–Young p (joint)	0.190	0.396	0.443	0.331
Mean (control)	11,644.28	5,787.03	5,057.78	
Non-zero % (control)	23.09	30.83	21.13	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	2,664	2,664	2,664	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household. Sales, expenses and profits are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities elicited via direct survey items. Variables are winsorized at the 95th percentile (upper 5%) of strictly positive values for sales and expenses and at the 2.5th and 97.5th percentiles for profits; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.15. Sales, expenditures and profits of own-account household activities, OLS without PDS-LASSO selected control variables

	HH own-account activities		
	Sales	Expenses	Profits
	(1)	(2)	(3)
Treatment: SEK	4,495	1,649	2,846
	(3,102)	(1,586)	(1,962)
Treatment: SEK+	9,138***	3,638**	5,500***
	(3,309)	(1,676)	(2,043)
Mean (control)	19,948.31	10,270.26	9,678.04
Non-zero % (control)	35.33	41.80	30.95
Branch FE	✓	✓	✓
Observations	2,664	2,664	2,664

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Profits are reported in column 3 as sales minus expenses. Sales and expense components are winsorized at the 95th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on two treatment-arm dummies, controlling for branch fixed effects, without PDS-LASSO-selected controls. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$.

C.1.4. Results by survey group

TABLE C.16. Take-up of loan contracts

	(1)	(2)	(3)	(4)	(5)
	SEK	SEK+	Standard	Any	None
Panel A: Group 1					
Treatment: SEK	0.31***	0.00	0.49***	0.80***	0.20***
	(0.02)	(0.00)	(0.02)	(0.02)	(0.02)
Treatment: SEK+	0.01*	0.41***	0.40***	0.82***	0.18***
	(0.00)	(0.02)	(0.02)	(0.02)	(0.02)
Control: Standard	0.00	0.00	0.81***	0.81***	0.19***
	(0.00)	(.)	(0.02)	(0.02)	(0.02)
Observations	1,246	1,246	1,246	1,246	1,246
Panel B: Group 2					
Treatment: SEK	0.28***	0.01**	0.54***	0.83***	0.17***
	(0.02)	(0.00)	(0.02)	(0.02)	(0.02)
Treatment: SEK+	0.01***	0.25***	0.57***	0.84***	0.16***
	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)
Control: Standard	0.01***	0.00*	0.84***	0.86***	0.14***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Observations	1,822	1,822	1,822	1,822	1,822

Notes: Data source: Al Amana administrative records. Observation unit: individual study participant. Each column reports a binary indicator equal to one if the respondent's first loan contract of the indicated type at Al Amana fell within the individual study period, defined conservatively as the interval from 90 days before the study start date through the end of the study window (contracts of individuals who attrited before the window closed are retained). Panels A and B report these estimates separately for Group 1 and Group 2 study participants. "SEK" denotes the grace-period loan (*Produit Salaf El Karaoui*); "SEK+" denotes the tailored loan (*Produit Salaf El Karaoui plus*); "Standard" includes all other individual enterprise loans (*Produit Individuel Entreprise*, *Produit Moumtaz*, and *Produit Idmaj*). Contracts nominally classified as SEK+ were reclassified as Standard when the recorded repayment schedule exhibited negligible variation across installment periods (maximum-to-minimum installment ratio below 1.05) and sufficient schedule data were available (more than one third of scheduled installments recorded); see Appendix B.1 for details. Each coefficient is the within-group mean of the outcome, estimated by regressing the take-up indicator on three treatment-arm dummies without a constant. Heteroskedasticity-robust standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.17. Sales, expenditures and profits of own-account household activities, Group 1

	Group 1: HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	4,524	1,555	3,486	
	(4,890)	(1,732)	(3,649)	0.632
	[0.216]		[0.216]	
	{0.449}	{0.429}	{0.441}	
Treatment: SEK+	12,124**	3,382*	10,049**	
	(5,117)	(1,820)	(3,926)	0.052
	[0.037]		[0.037]	
	{0.038}	{0.063}	{0.029}	
Westfall–Young p (joint)	0.072	0.111	0.052	0.101
Mean (control)	20,108.28	8,317.74	11,790.53	
Non-zero % (control)	37.08	45.59	31.00	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	1,040	1,040	1,040	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household; sample restricted to Group 1 study participants. This table forms part of the preregistered exploratory comparison of the two implementation groups. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Sales and expense components are winsorized at the 95th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.18. Sales, expenditures and profits of own-account household activities, Group 2

	Group 2: HH own-account activities			WY-p val.
	Sales	Expenses	Profits	
	(1)	(2)	(3)	(4)
Treatment: SEK	2,016	1,150	294	
	(5,116)	(2,876)	(3,165)	0.903
	[1.000]		[1.000]	
	{0.729}	{0.699}	{0.921}	
Treatment: SEK+	5,875	2,827	2,995	
	(5,578)	(3,037)	(3,154)	0.458
	[1.000]		[1.000]	
	{0.292}	{0.363}	{0.348}	
Westfall–Young p (joint)	0.477	0.581	0.544	0.704
Mean (control)	22,844.71	12,883.78	9,960.93	
Non-zero % (control)	34.26	39.48	30.54	
Branch FE + PDS-LASSO	✓	✓	✓	
Observations	1,624	1,624	1,624	

Notes: Data source: Outcomes measured via Endline household survey. Observation unit: household; sample restricted to Group 2 study participants. This table forms part of the preregistered exploratory comparison of the two implementation groups. Sales and expenses are annual totals in Moroccan Dirham across crop-farming, livestock-breeding and off-farm own-account activities computed by the AAA method (Aggregating, Anchoring and Adjusting following Anderson et al. (2021)). Sales and expense components are winsorized at the 95th percentile of their strictly positive distributions before aggregation. Profits are then calculated as total sales minus total expenses and are not independently winsorized; "% > 0" is the proportion of control-group households with a strictly positive outcome. Coefficients and standard errors (in parentheses) from a PDS-LASSO regression of the variable on two treatment-arm dummies, controlling for branch fixed effects and covariates selected by the LASSO and used in the OLS post-LASSO estimation and in the PDS structural estimation, from the set of borrower and household characteristics shown in Table 1 and measured at baseline. See Belloni et al. (2014) for details about the PDS-LASSO procedure. Standard errors robust to heteroskedasticity in parentheses. Significance levels are indicated by * $p < .10$, ** $p < .05$, *** $p < .01$. Randomization-inference (RI) p-values following Young (2019), with permutation testing (number of resampling replications = 1,000), for individual coefficients in curly brackets. Sharpened false-discovery-rate (FDR) q-values following Anderson (2008), computed across the sales and profits coefficients (both treatment arms), shown in square brackets. Column 4 reports Westfall–Young stepdown-adjusted p-values: per-coefficient joint p-values for each treatment row, and at the bottom (i) the equation-wise joint p-value ("Westfall–Young p (joint)") and (ii) the experiment-wise p-value testing the null that all 6 treatment coefficients across the three equations are jointly zero ("Westfall–Young p (all outcomes)").

TABLE C.19. Balance at baseline (Group 1)

	Total	Control	Treatment effect vs control					
	(1)	(2)	(3) SEK		(4) SEK+		(5) Any	
	N	Mean	β	SE	β	SE	β	SE
Panel A: Respondent and household characteristics								
Respondent is female	1,198	0.235	0.014	(0.029)	-0.001	(0.029)	0.006	(0.026)
Respondent age	1,197	42.666	0.229	(0.897)	-1.276	(0.893)	-0.523	(0.788)
Respondent has any education	1,195	0.595	0.014	(0.034)	0.042	(0.035)	0.028	(0.030)
Respondent is household head	1,200	0.688	0.002	(0.032)	-0.014	(0.033)	-0.006	(0.028)
# household members	1,246	5.165	-0.306**	(0.147)	-0.001	(0.146)	-0.153	(0.130)
Panel B: Access to credit								
Loan from formal institution	1,220	0.255	0.048*	(0.029)	0.030	(0.028)	0.039	(0.025)
Loan from informal source	1,218	0.055	0.005	(0.016)	0.034*	(0.017)	0.019	(0.014)
Panel C: Self-employment activities								
# activities	1,236	1.932	-0.011	(0.092)	0.062	(0.093)	0.026	(0.081)
Does crop-farming								
Cereal growing	1,236	0.261	0.047*	(0.026)	0.035	(0.027)	0.041*	(0.023)
Arboriculture	1,236	0.232	-0.019	(0.025)	-0.007	(0.025)	-0.013	(0.022)
Vegetable growing	1,236	0.110	0.007	(0.021)	0.010	(0.021)	0.008	(0.018)
Does livestock breeding								
Breeding of cattle, sheep, goats	1,236	0.381	-0.026	(0.031)	-0.012	(0.031)	-0.019	(0.027)
Breeding of poultry, rabbits	1,236	0.251	-0.028	(0.027)	-0.042	(0.026)	-0.035	(0.023)
Production derived from livestock	1,236	0.123	-0.020	(0.018)	-0.020	(0.019)	-0.020	(0.016)
Does off-farm activity								
Commerce	1,236	0.235	0.043	(0.030)	0.080**	(0.031)	0.061**	(0.027)
Crafts	1,236	0.070	-0.020	(0.017)	-0.002	(0.018)	-0.011	(0.015)
Fishing	1,236	0.078	-0.007	(0.018)	0.004	(0.018)	-0.002	(0.016)
Construction	1,236	0.076	-0.007	(0.018)	0.003	(0.018)	-0.002	(0.016)
Transport	1,236	0.008	-0.004	(0.006)	-0.001	(0.006)	-0.003	(0.005)
Services	1,236	0.065	0.018	(0.018)	0.006	(0.017)	0.012	(0.015)
Other	1,236	0.042	0.004	(0.014)	0.009	(0.014)	0.006	(0.012)
Panel D: Has income from								
Salaried labor	1,225	0.058	0.009	(0.017)	-0.003	(0.016)	0.003	(0.014)
Casual labor	1,224	0.073	-0.018	(0.017)	-0.008	(0.018)	-0.013	(0.016)
Pension	1,222	0.021	-0.011	(0.009)	-0.004	(0.010)	-0.007	(0.008)
Remittances	1,223	0.113	0.020	(0.023)	0.009	(0.022)	0.014	(0.019)
Donations	1,223	0.016	-0.004	(0.008)	0.001	(0.009)	-0.002	(0.008)
Asset sales	1,221	0.026	0.002	(0.012)	-0.011	(0.010)	-0.004	(0.010)
Other	1,222	0.013	0.012	(0.009)	-0.002	(0.008)	0.005	(0.007)
Panel E: Attrition								
Not surveyed at endline	1,246	0.152	0.004	(0.025)	0.044*	(0.026)	0.024	(0.022)
Omnibus F-test across all variables			0.85 (p=0.689)		0.80 (p=0.767)		1.08 (p=0.359)	
Omnibus F-test (endline sample only)			0.64 (p=0.925)		0.92 (p=0.584)		1.24 (p=0.183)	

Notes: Source: Baseline surveys (Group 1 only). Unit of observation: respondent level where indicated and otherwise household level. Table reports baseline balance across three treatment arms with SEK N = 428, SEK+ N = 430, and Control/Standard N = 388 out of N = 1,246 Group 1 participants. Column (1) shows the total number of non-missing observations for each variable. Column (2) reports the control-group mean. Columns (3)–(5) report OLS differences in means ($\hat{\beta}$) for SEK, SEK+, and Any treatment (= SEK or SEK+), estimated from regressions of the outcome on treatment dummies with branch fixed effects. Standard errors robust to heteroskedasticity are shown in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. The "Omnibus F-test across all variables" tests joint equality of the Panel A–D covariates across arms by regressing each treatment indicator on that set of balance variables (plus branch fixed effects) and reporting the `testparm` F-statistic and p-value; the second omnibus row ("endline sample only") repeats the same joint test restricting the sample to Group 1 units observed at endline.

TABLE C.20. Balance at baseline, additional covariates (Group 1)

	Total	Control	Treatment effect vs control					
	(1)	(2)	(3) SEK		(4) SEK+		(5) Any	
	N	Mean	β	SE	β	SE	β	SE
Panel F: Business outcomes (winsorized 5%)								
Total sales, 12 months	1,246	32722	1,724	(3,084)	2,389	(3,114)	2,057	(2,687)
Total sales, 30 days	1,246	3412	81	(453)	61	(440)	71	(386)
Total profits, 12 months	1,246	12595	377	(1,379)	1,138	(1,391)	758	(1,193)
Total profits, 30 days	1,246	1288	249	(182)	-21	(159)	114	(147)
Panel G: Subjective expectations & risk								
Profit coefficient of variation	974	0.140	0.001	(0.008)	0.004	(0.007)	0.003	(0.006)
Above-median income variance	1,071	0.485	-0.007	(0.035)	0.013	(0.035)	0.003	(0.030)
Panel H: Assets & credit demand								
Has savings	1,216	0.272	-0.001	(0.029)	-0.000	(0.029)	-0.001	(0.025)
Total savings (DH)	1,246	2224	942	(754)	332	(596)	637	(541)
Desired loan amount (DH)	1,217	10328	36	(874)	740	(856)	390	(659)
Panel I: Enterprise characteristics								
Had permanent employees	1,229	0.047	0.018	(0.017)	0.008	(0.015)	0.013	(0.014)
Had casual employees	1,229	0.249	0.003	(0.029)	0.020	(0.029)	0.011	(0.025)
Wants to expand scale	1,217	0.818	-0.018	(0.025)	0.022	(0.025)	0.002	(0.022)
Wants to diversify	1,217	0.689	0.041	(0.027)	-0.017	(0.028)	0.012	(0.024)
Top hurdle: lack of funds	837	0.900	0.008	(0.025)	-0.008	(0.026)	-0.001	(0.022)
Top hurdle: lack of credit	837	0.076	-0.006	(0.022)	-0.005	(0.023)	-0.005	(0.020)
Keeps written records	1,229	0.139	0.014	(0.022)	-0.014	(0.021)	-0.000	(0.018)
Has bank account	1,218	0.324	0.028	(0.032)	0.001	(0.031)	0.014	(0.028)
Omnibus F-test across all variables			1.12 (p=0.335)		1.11 (p=0.340)		0.66 (p=0.839)	
Omnibus F-test (endline sample only)			1.19 (p=0.274)		1.22 (p=0.248)		0.73 (p=0.766)	

Notes: Source: Baseline surveys (Group 1 only). Unit of observation: household level. Table reports baseline balance on Group 1-specific variables across three treatment arms with SEK N = 428, SEK+ N = 430, and Control/Standard N = 388 out of N = 1,246 Group 1 participants. These variables were not collected in the shorter Group 2 instrument and are used in Group-1-specific heterogeneity analyses only; they do not enter the pooled PDS-LASSO covariate set. Column (1) shows the total number of non-missing observations for each variable. Column (2) reports the control-group mean. Columns (3)–(5) report OLS differences in means ($\hat{\beta}$) for SEK, SEK+, and Any treatment (= SEK or SEK+), estimated from regressions of the outcome on treatment dummies with branch fixed effects. Standard errors robust to heteroskedasticity are shown in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. The omnibus F-tests follow the same procedure as in Table C.19. In Panel F, sales and profits (12-month and 30-day) have missing values set to zero and are winsorized at the upper 5th percentile of strictly positive values (right tail only), with thresholds computed on the pooled sample of Group 1 respondents.

C.2. Village-level intervention

TABLE C.21. Village promotions

	Campaign reach			Loan demand		Preferred loan type				
	HH visits	Total women	Total men	Share prior borrower	Share want loan 3m	Share want Standard	Share want SEK	Share want SEK+	Share want other	Share undecided
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SEK pure info	0.237	-1.038	1.313	0.034*	0.021	-0.613***	0.648***	0.001	-0.003	-0.016***
	(0.550)	(0.955)	(0.814)	(0.019)	(0.026)	(0.031)	(0.028)	(0.017)	(0.003)	(0.006)
SEK+ pure info	-0.038	-1.025	1.138	0.078***	-0.016	-0.605***	-0.010	0.625***	-0.001	-0.010
	(0.506)	(0.888)	(0.814)	(0.019)	(0.024)	(0.031)	(0.016)	(0.030)	(0.003)	(0.007)
SEK info + norms-shifting	-0.500	-0.475	0.500	0.038**	0.011	-0.607***	0.608***	0.026	-0.003	-0.001
	(0.565)	(1.091)	(0.860)	(0.019)	(0.034)	(0.033)	(0.042)	(0.027)	(0.004)	(0.008)
SEK+ info + norms-shifting	-0.175	0.200	0.800	0.009	0.033	-0.608***	-0.010	0.654***	0.001	-0.009
	(0.614)	(1.070)	(1.036)	(0.016)	(0.033)	(0.032)	(0.019)	(0.038)	(0.004)	(0.007)
Mean (control)	18.175	14.025	15.025	0.149	0.552	0.623	0.017	0.007	0.008	0.033
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Villages	320	320	320	320	320	320	320	320	320	320

Notes: Data source: household-level promotional campaign survey, collapsed to the village level. The sample covers 320 villages across 20 branch catchment areas that were assigned to one of five information treatment arms as part of the village-level randomization (Part B). This table reports implementation measures and exploratory manipulation-check outcomes; it does not measure preferences in pure-control villages or in the village population as a whole. The omitted category is the Standard pure information treatment. Pure control villages (120 villages receiving no promotional visit) are excluded. Columns (1)–(3) report village-level sums; columns (4)–(10) report village-level shares. Preferred loan type shares in columns (6)–(10) are unconditional on expressing a loan preference (households not stating a preference are coded as zero). Promotional visits consisted of door-to-door presentations showing product-specific video capsules about the assigned loan type; norms-shifting arms additionally included testimonial videos. All regressions include branch fixed effects, corresponding to the original branch assignment at randomization; Branch AJ, which closed in November 2023 and was absorbed by Branch MK, is treated as a distinct branch throughout; robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.22. Take-up of village intervention — prospect and client counts, by norms sub-arm

	Prospects	Contracts				Amount (MAD)
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Total	Standard	SEK	SEK+	Total
SEK (info only)	0.80*	0.27	0.30	-0.02	-	2,811
	(0.44)	(0.25)	(0.24)	(0.05)		(5,355)
SEK (info + norms)	0.05	-0.13	-0.11	-0.01	-	-2,830
	(0.24)	(0.14)	(0.13)	(0.06)		(3,195)
SEK+ (info only)	0.38*	0.08	0.15	-0.06	-	-358
	(0.22)	(0.13)	(0.12)	(0.05)		(3,127)
SEK+ (info + norms)	-0.03	-0.24**	-0.17*	-0.06	-	-4,042
	(0.21)	(0.12)	(0.10)	(0.05)		(3,038)
Standard	0.32	-0.12	-0.06	-0.06	-	-2,487
	(0.30)	(0.13)	(0.12)	(0.05)		(3,034)
Control group mean	0.76	0.38	0.31	0.07	0	6,191
Observations	440	440	440	440	440	440
Branch FE	✓	✓	✓	✓	✓	✓

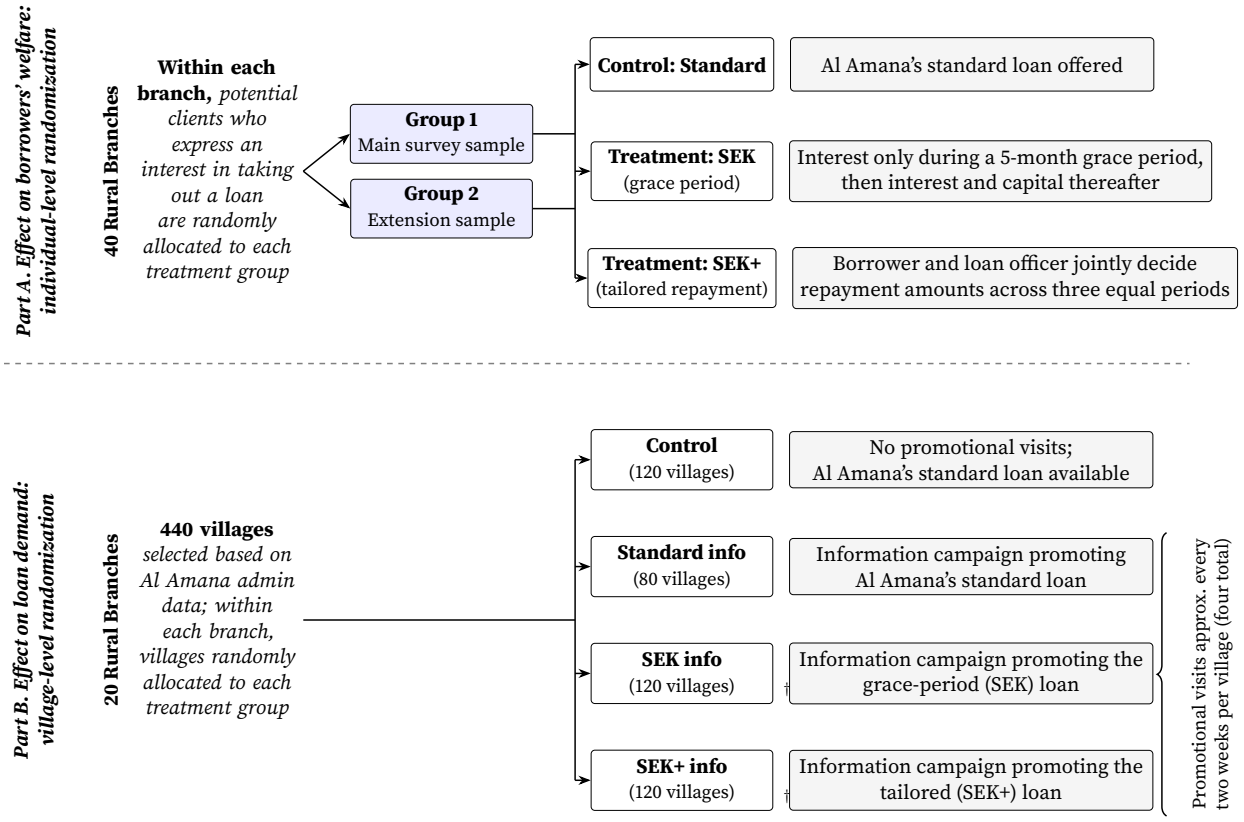
Notes: This table replicates Table 12 but disaggregates the SEK and SEK+ arms into their norms-shifting and non-norms-shifting sub-arms. The unit of observation is the village; the sample comprises 440 study villages. Counts are constructed from phone surveys and *Al Amana* administrative data on prospective borrowers and signed loan contracts that consented to participate in the study. Column (1) is the number of prospects identified during promotional campaigns. Columns (2)–(5) are loans signed on any contract, the Standard contract, the *SEK* contract, and the *SEK+* contract, respectively. Column (5) is zero throughout, shown as "–". Column (6) is the total gross loan amount disbursed (Moroccan Dirham), summed across all contract types; villages with no disbursement are coded zero, so it uses the same 440-village sample. Each column reports OLS estimates from a regression of the outcome on five treatment-arm indicators (SEK info-only, SEK info+norms, SEK+ info-only, SEK+ info+norms, and Standard, with pure control as the omitted category), controlling for baseline village covariates: dispersed settlement indicator, distance to agency (km), and approximate number of households, as reported in Table 11, and for branch fixed effects, corresponding to the original branch assignment at randomization; Branch AJ, which closed in November 2023 and was absorbed by Branch MK, is treated as a distinct branch throughout. Heteroskedasticity-robust standard errors in parentheses. "Control group mean" is the mean of the outcome in pure-control villages (no information campaign). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.23. Take-up of village intervention — client characteristics, by norms sub-arm

	Demography				Activity (principal)			Loan interest		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	New	Female	Above median age	Primary edu. and above	Crops	Breeding	Off-farm	Crops	Breeding	Off-farm
SEK (info only)	0.09	0.04	0.11	0.23	0.07	0.12	0.07	0.08	0.04	0.23
	(0.14)	(0.06)	(0.11)	(0.17)	(0.08)	(0.08)	(0.06)	(0.07)	(0.08)	(0.15)
SEK (info + norms)	-0.05	-0.06*	-0.13*	-0.04	-0.06	0.04	-0.02	-0.05	0.01	-0.05
	(0.07)	(0.04)	(0.07)	(0.09)	(0.06)	(0.07)	(0.04)	(0.06)	(0.07)	(0.07)
SEK+ (info only)	-0.09	0.06	0.01	-0.02	0.03	0.05	0.03	0.02	0.05	0.03
	(0.06)	(0.05)	(0.08)	(0.08)	(0.06)	(0.06)	(0.04)	(0.06)	(0.07)	(0.06)
SEK+ (info + norms)	-0.09	-0.06*	-0.10	-0.13	-0.03	0.02	-0.04	-0.12**	-0.04	-0.07
	(0.06)	(0.04)	(0.07)	(0.08)	(0.06)	(0.06)	(0.04)	(0.05)	(0.06)	(0.05)
Standard	-0.00	-0.01	-0.06	-0.11	-0.09**	0.00	-0.01	-0.06	-0.05	0.00
	(0.07)	(0.04)	(0.08)	(0.07)	(0.05)	(0.06)	(0.04)	(0.06)	(0.06)	(0.07)
Control group mean	0.13	0.05	0.20	0.21	0.10	0.10	0.07	0.12	0.11	0.12
Observations	440	440	440	440	440	440	440	440	440	440
Branch FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table replicates Table 13 but disaggregates the SEK and SEK+ arms into their norms-shifting and non-norms-shifting sub-arms. The unit of observation is the village; the sample comprises 440 study villages. Outcomes are village-level total counts of clients with the indicated characteristic, constructed from phone follow-up surveys. Columns (1)–(4) report demographic characteristics: (1) new clients (first-time borrowers at *Al Amana*); (2) female clients; (3) clients above the sample median age (42 years); (4) clients with primary education or above. Columns (5)–(7) report the principal economic activity declared by the client: (5) crop farming (cereals, arboriculture, or market gardening); (6) livestock or poultry breeding and livestock products; (7) off-farm activities (commerce, crafts, fishing, construction, transport, services, or other). Columns (8)–(10) report the activity for which the client expressed interest in taking a loan, using the same groupings as columns (5)–(7). Each column reports OLS estimates from a regression of the outcome on five treatment-arm indicators (SEK info-only, SEK info+norms, SEK+ info-only, SEK+ info+norms, and Standard, with pure control as the omitted category), controlling for baseline village covariates: dispersed settlement indicator, distance to agency (km), and approximate number of households, as reported in Table 11, and for branch fixed effects, corresponding to the original branch assignment at randomization; Branch AJ, which closed in November 2023 and was absorbed by Branch MK, is treated as a distinct branch throughout. Heteroskedasticity-robust standard errors in parentheses. "Control group mean" is the mean of the outcome in pure-control villages (no information campaign). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

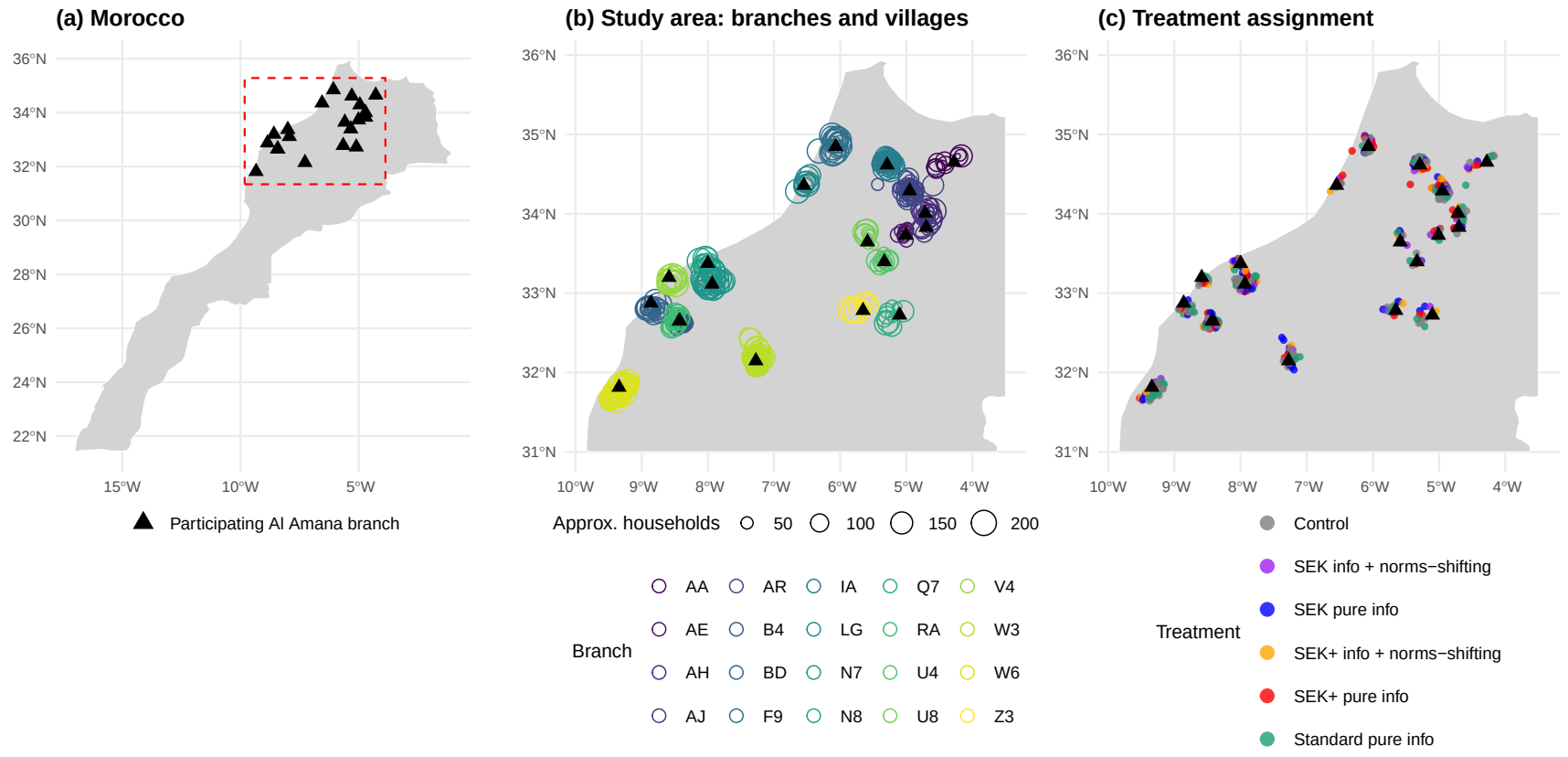
FIGURE C.1. Overview of research design (as implemented)



† In a subset of SEK and SEK+ villages (40 each), the information campaign additionally included a norms-shifting component.

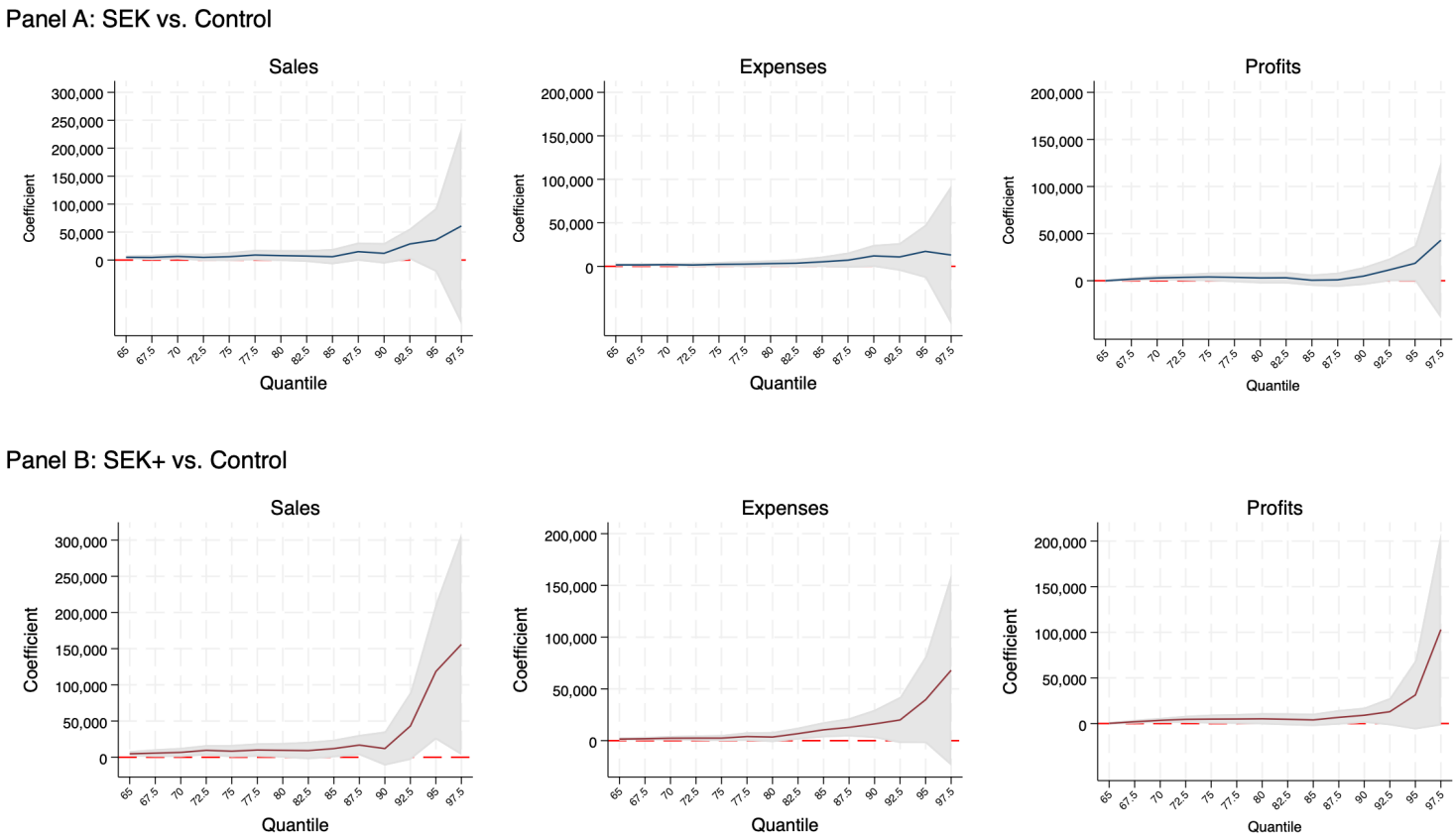
Notes: Group 1 (G1) is the main survey sample with a comprehensive CAPI baseline; Group 2 (G2) is an extension sample with a shorter paper-based baseline administered by branch officers. Both groups were randomized into the same three treatment arms. Part B operated in 20 branches, seven of which overlapped with Part A.

FIGURE C.2. Village locations and treatment assignment



Notes: Promotional campaign randomization file drawn primarily from the Initiative nationale pour le développement humain (INDH) database (2020–2021) for GPS coordinates and household estimates, supplemented by 2014 data from the Haut Commissariat au Plan (HCP) for rurality, dispersal status, and (when necessary) additional household size estimates. Panel (a) shows the locations of participating Al Amana branches across Morocco. Panel (b) displays the study area in northern Morocco with villages grouped by branch and sized by approximate number of households. Panel (c) shows the village-level treatment assignment.

FIGURE C.3. Quantile regressions (ITT), household business activities



Note: Each panel reports intention-to-treat (ITT) estimates from inverse-probability-weighted conditional quantile regressions (CQR) of annual household business outcomes measured at Endline. The three outcomes are total Sales, total Expenses, and Profits (defined as Sales – Expenses), all denominated in Moroccan Dirham (MAD) and constructed using the AAA elicitation method (anchoring via sub-category enumeration followed by a respondent-adjusted aggregate; see Appendix B.2 for details). Variables are unwinsorized. Panel A plots coefficients from regressions of each outcome on a SEK treatment indicator, restricting the sample to SEK-assigned and control-assigned individuals. Panel B does the same for SEK+ versus control. Branches 9 (BQ) and 24 (PO) are excluded throughout due to extreme treatment-arm imbalance within those branches. Inverse-probability-of-treatment weights (IPTW) account for branch-level variation in treatment-assignment probabilities without introducing incidental-parameters bias into the quantile regressions. Propensity scores are estimated separately for each panel via logistic regression of the relevant treatment indicator on branch fixed effects, $T_g \sim$ branch FEs. Predicted probabilities are clipped to [0.05, 0.95] to limit extreme weights, and individual weights are formed as $w_i = T_i/\hat{P}_i + (1 - T_i)/(1 - \hat{P}_i)$. CQRs are then estimated at quantiles $\tau \in \{0.650, 0.675, 0.700, \ldots, 0.975\}$ (14 grid points in 2.5-percentage-point increments) using Stata's `qreg` command with probability weights. Solid lines plot point estimates of the treatment coefficient $\hat{\beta}(\tau)$; shaded bands show pointwise 95 % confidence intervals ($\hat{\beta} \pm 1.96 \times \widehat{\mathrm{SE}}$). The dashed horizontal line marks zero. Confidence intervals are pointwise at each quantile and do not carry simultaneous coverage over the quantile process.