Selection and Incentives in Microequity: Evidence from a Field Experiment*

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Abstract

Firms often cite financing constraints as key barriers, but lenders rarely innovate beyond debt. Equity could serve firms that debt deters, but it raises adverse selection and moral hazard concerns. Using a two-stage experiment with Egyptian livestock farmers, we design and test a microequity contract against flexible debt and separate selection from contract effects. Almost half of potential clients would accept only equity, and this group is *advantageously* selected, with fewer animal deaths than borrowers open to both contracts. Conditional on selection, net revenue is lower under equity than debt, consistent with moral hazard, though the gap disappears during a currency devaluation. A simple structural model suggests that adding equity is only profitable at below-typical delivery costs, but subsidizing it can increase welfare more than equal spending on cash transfers. Tailoring financing to borrowers' needs can direct capital to productive firms, but lenders may not individually pursue this efficiency gain.

Keywords: microfinance, microequity, adverse selection, moral hazard, field experiment
JEL Codes: D22, D24, G21, G32, L25, L26, O16

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1 Introduction

Capital allocation is an essential function of financial markets and contributes to aggregate productivity and growth (Hsieh and Klenow, 2009). For small and medium enterprises (SMEs), that capital comes primarily in the form of debt (Beck et al., 2008; Robb and Robinson, 2014). Debt markets have proven robust in practice, and the economics literature provides the theoretical underpinnings for why debt is often preferable to equity contracts (e.g., Townsend, 1979). However, equity contracts have brought success in many industries (e.g., venture capital in the tech sector); can they bring value for the most common type of business in the world—SMEs (IFC, 2024)?

Theoretically, equity also offers distinct advantages. By sharing downside risk, it can attract productive but debt- or risk-averse borrowers who are otherwise excluded by the rigidity of debt. Furthermore, this implicit insurance may encourage innovation and higher average returns. For these reasons, equity contracts may be inefficiently underprovided. Counterbalancing these benefits, however, are classic agency frictions. Adverse selection may lead lower-quality borrowers to self-select into equity contracts, and conditional on selection, moral hazard may induce borrowers to reduce effort because their downside risk is mitigated (and their upside is taxed). Compounding these issues is the challenge of costly state verification: the difficulty of ensuring businesses accurately report their revenue such that contracts can be conditioned on it.

But how important are these concerns in practice? Evaluating these competing forces requires isolating the selection and contract effects of equity relative to debt.[1] To answer this question we implement a multistage randomized experiment that enables us to estimate each effect separately. Working with livestock farmers in rural Egypt—a sector that supports the livelihoods of approximately 1.3 billion people globally (Food and Agriculture Organization, 2018)—we design an equity-like revenue sharing contract. This contract provides borrowers with young livestock (sheep) which they take care of for a fattening cycle and then sell on the market. They then share a portion of the net proceeds (positive or negative) with the lender after returning the principal.[2] We provide borrowers with the opportunity to apply for

[1] Recent empirical work has begun measuring these mechanisms: Burchardi et al. (2019) document moral hazard in Kenyan sharecropping but cannot separately identify selection; Herbst and Hendren (2024) show adverse selection in income-contingent student loans but do not measure moral hazard, while de Silva (2025) shows moral hazard in such loans in a context with limited scope for selection.

[2] We refer to the contract as equity-like as it shares some distinctions from more standard equity contracts (e.g., in venture capital). These microenterprises cannot easily be valued at entry, returns are modest rather than explosive, and there is no exit event for the firm as a whole. In response, our equity product focuses on net

this type of financing as well as a debt product with a similar payment schedule. We stratify the sample based on whether they apply for one or both of the products and then randomize them into an equity offer, a debt offer, or control. This design allows us to separately identify selection effects from contract effects for equity versus debt.

We document three main sets of results. First, there is strong demand for microequity that appears to increase financial inclusion. Almost half of potential clients state that they would be willing to borrow only under the equity contract and would decline a debt contract. This equity-only group is not composed of the poorest or most constrained households. Instead we find that, relative to those willing to take either contract, equity-only borrowers are modestly more educated, perform better on a brief cognitive test, are more risk averse, and overwhelmingly look similar on a range of other observables. These patterns indicate that offering equity can substantially enlarge the pool of bankable clients, and that the borrowers who sort into equity look at least as promising as those willing to borrow under either contract in terms of their observable characteristics.

Second, we examine how selection—whether driven by observables or unobservables—affects performance. Holding contract type fixed, we find evidence of *advantageous* selection into equity. Among borrowers who receive the equity contract, those who are only willing to accept equity ("equity-only") have substantially fewer bad project outcomes (e.g., animal deaths) than those who are willing to take either contract type, and they exhibit equal or better performance on realized net revenues and input use. Thus, in our setting, the borrowers who are willing to take equity but not debt look like relatively higher types.

Third, holding selection fixed, we find that equity underperforms debt along several key margins, consistent with moral hazard. Among borrowers willing to accept either contract, those randomly assigned to equity (vs. debt) experience more adverse livestock outcomes, report lower spending on feed (though imprecisely measured), and generate lower net revenues. The lender also earns substantially less per financed project, even though the two types of contracts finance the same asset. We also cross-randomized a monitoring intervention; however, it did not mitigate these effects (though the test is underpowered and the intervention was likely too light-touch). These results suggest that the insurance embedded in equity significantly weakens incentives relative to debt, at least among those willing to accept either contract type. Turning to borrower outcomes, we find that equity

revenue rather than profit, which limits the accounting-fidelity problems that undermined earlier profit-sharing attempts (de Mel et al., 2019), and it is tied to a specific observable asset that self-liquidates at the sale. The cost is that these simplifications cap some of the upside channels of equity, such as productive risk-taking in the choice of project.

financing meaningfully raises short-run income relative to the pure control group, with a somewhat smaller (though not statistically different) gain for debt borrowers as well.

Our experiment experienced a period of sharp currency devaluation and high inflation. This changed the relative payoffs of the two contracts. Because of the fixed interest rate on debt, the devaluation lowers the real cost of financing for borrowers with existing loans. On the other hand, the devaluation increases asset prices, part of which would be shared with the lender as part of the equity contract. Equity borrowers still gain in absolute terms, but less than they would have under debt. We show that this period alters both the behavior of borrowers and the relative performance of equity and debt. In particular, among borrowers exposed to this highly inflationary period, project performance improves under equity, and much of the resulting gain accrues to the lender. These results show that the macroeconomic environment affects the extent of moral hazard in equity contracts in general and relative to debt.

Taken together, our findings identify both a benefit of microequity and a cost. The benefit is inclusion: equity reaches households that refuse debt, financing about 37 percentage points more of the eligible population than debt alone, and these households are, on average, better stewards of their projects. The cost is cannibalization. A lender offering equity alongside debt would also draw in households that would have borrowed anyway, and when they switch from debt to equity they take less care of their animals and the lender earns less on each of them. So while the equity product we helped design and test reaches equity-only borrowers, a lender offering it on its own could see its bottom line suffer. A natural response would be to adjust prices, for example by raising the lender's equity share, to target the equity-only households, who are both better risks and appear less sensitive to the price of equity. But changing prices also changes who selects into each contract and how much effort they put in.

To account for these forces, we build a simple structural model of contract pricing and welfare. It reproduces the experiment's take-up and lender margins, uses the devaluation to identify how effort responds to contract terms, and draws demand elasticities from our related experiments in Egypt and Jordan.[3] The model shows that adding equity to the contract menu can enhance lender profits, but only if loans are cheap to deliver. At the administrative costs Egyptian microlenders actually report, 7 to 9 percent of principal per loan, a lender acting on its own would offer only debt. Our welfare calculations, however,

[3]The experiment did not vary prices. We use an elasticity of -1 for debt and for indifferent households' demand for equity (Gazeaud et al., 2026), and -0.5 for equity-only households, guided by Karlan et al. (2021).

show substantial surplus for borrowers, most of it going to equity-only households who would otherwise go unfinanced. A sponsor looking to support financial inclusion could subsidize a lender to offer equity and receive about 1.4 EGP of borrower welfare per EGP spent at a 7 percent delivery cost. By comparison, an unconditional cash transfer is worth at most one EGP to its recipients for each EGP transferred, and less once delivery costs are counted (Finkelstein and Hendren, 2020; Hendren and Sprung-Keyser, 2020).[4]

Our findings contribute to several literatures. A first contribution is to research on microfinance and contract design. A large literature documents modest average effects of standard microcredit on firm growth and household welfare (e.g., Banerjee et al., 2015), though recent work shows that impacts are concentrated among upper-tail borrowers and households with prior business experience (e.g., Banerjee et al., 2019; Crépon et al., 2024), and that altering repayment timing and flexibility can meaningfully affect investment and default (e.g., Barboni and Agarwal, 2023; Battaglia et al., 2024). We complement this literature by studying a more radical departure from fixed-repayment debt: replacing fixed obligations with a fully state-contingent equity contract that shares realized project returns. Our experimental design compares this equity product to a flexible, asset-based debt contract that already incorporates many recent innovations, and allows us to separately measure selection and contract effects using rich data on project-level outcomes and borrower welfare. In this sense, our results also complement recent work on equity-like and performance-contingent contracts for small firms (e.g., de Mel et al., 2019; Cordaro et al., 2025; Meki, 2025; Meki and Quinn, 2024; Clarke et al., 2025).[5] We distinguish our contribution to this growing agenda in three ways. First, we study a setting where the lender is at arm's length from the borrower's business, just observing a single sale after six months of only lightly supervised performance. This setting is closer to the one most informal firms face, and information problems are arguably more severe than in settings where a lender has access to borrowers' high frequency administrative records (e.g., as in Cordaro et al., 2025). Second, building on the proof-of-concept evidence from these earlier and contemporaneous studies, our two-stage design allows us to separate selection from moral hazard to better

[4]Delivery costs typically absorb roughly 5 to 20 percent of cash-transfer spending (McIntosh and Zeitlin, 2022, 2024). The closest credit comparison, Kaboski and Townsend (2011), finds that households valued Thailand's Million Baht Village Fund at about 0.7 baht of cash per baht of seed funding. Cash transfers can also create value through local spillovers and fiscal savings (Egger et al., 2022; Gerard et al., 2026), which neither this benchmark nor our welfare measure includes.

[5]On the venture capital side of the market, Colonnelli et al. (2026) use a survey experiment with start-up founders in Africa and find that they prefer equity to debt over any plausible range of contract terms, valuing the switch as much as an 11 percentage-point reduction in the interest rate.

understand mechanisms underlying performance differences. Third, we provide evidence on the viability of microequity in the livestock sector, which represents a massive share of the asset portfolios of low-income households globally (Food and Agriculture Organization, 2018).

A second contribution is to the broader literature on state-contingent contracts, agricultural sharecropping (e.g., Stiglitz, 1974; Eswaran and Kotwal, 1985; Ghatak and Pandey, 2000), and income-contingent repayment (Friedman, 1955). Our study provides the first comprehensive field test of whether the theoretical barriers to equity financing—moral hazard and adverse selection—actually impede performance for low-income borrowers when both mechanisms are measured simultaneously within a single experimental design. This contributes to recent empirical work that has begun measuring these mechanisms separately across related contract types (Burchardi et al., 2019; Herbst and Hendren, 2024).[6]

Finally, our results speak to work on debt aversion. Experimental studies document that many individuals are reluctant to take on contracts labeled as "debt" (Caetano et al., 2019), even when borrowing would be profitable (Martínez-Marquina and Shi, 2024). Our results on the financial inclusion effects of equity provide evidence of this debt aversion among entrepreneurs in a high-stakes field setting.

2 Background and Context

Livestock rearing is a central economic activity for the rural poor, serving simultaneously as a source of income, a store of value, and a form of insurance. In rural Egypt, where this study takes place, small-scale farmers often have considerable excess capacity—the space, labor, and knowledge required to raise animals—yet lack the capital to expand their herds to an efficient scale. While Egypt has a robust microfinance sector, the standard credit contracts available to these households are often ill-suited for agriculture. Microfinance institutions (MFIs) typically offer small cash loans with immediate monthly repayment obligations, a structure mismatched with the cash flow of livestock fattening, which ties up money for several months before any revenue arrives.

[6]Our two-stage design is inspired by Karlan and Zinman (2009) who randomize interest rate offers with a South African lender and then, once a borrower accepts, the rate on the loan itself for a subset (allowing them to vary selection while holding contract terms constant). They find stronger evidence of moral hazard than selection. We vary the form of the contract rather than its price, and our first stage elicits which contract a borrower would take rather than randomizing the two. Two other studies separate mechanisms using non-random variation in the contracts a single firm offered. Adams et al. (2009) find both in subprime auto lending, and Einav et al. (2013) find that employees choose health coverage partly on their own anticipated spending response, which they term selection on moral hazard.

We conduct our experiment in the governorates of Assiut and Minya to test whether equity-like contracts can resolve these and other frictions. Located approximately 400 kilometers south of Cairo, this region provides an ideal setting for three reasons. First, it is a region of high agricultural potential but deep poverty. The two governorates hold a combined population of over 9 million, agriculture dominates their economy, and both rank historically among the poorest in the country. Second, small farmers are underserved by the formal financial system, and many households already have experience raising livestock but cannot access the capital needed to expand.

Third, the cultural and religious context suggests latent demand for non-debt financing. As a conservative Muslim-majority region, social norms against paying interest on debt ("*Riba*" in Arabic) remain strong. While state religious authorities permit banking interest, in our census (described more below), 53% of Muslim respondents reported that interest is religiously impermissible (a further 14% were unsure), compared with 16% (10% unsure) of Christian respondents (N = 2,685 and 1,597).[7] Informal equity arrangements—where a wealthier relative finances an animal in exchange for a share of the profit—are already a familiar cultural practice. This familiarity offers a unique opportunity to formalize equity contracts ("*Musharaka*" in Arabic) in a setting where they may be intuitively understood.

To implement these contracts at scale, we partnered with the Assiut Businesswomen's Association (ABWA). Established in 2000, ABWA is a nonprofit lender. It had 13 branch offices and over 5,000 active clients at the time of the study. ABWA does not serve women exclusively. The experiment works on top of their existing infrastructure, using their own loan officers to screen clients and to handle asset delivery and sales.[8]

2.1 Experimental Products

We worked with ABWA to design two new asset-based financial products: a *Microequity* contract and a flexible *Debt* contract. Both provided financing as an in-kind transfer of young livestock (sheep). The financing amounts were sufficient for purchasing roughly three young sheep. Both contracts matched repayment to the production cycle, requiring no payments until the animals were sold six months later.[9] Both products also included mandatory

[7] Specifically, our survey question asks about interest charged by a "*gameya*" to anchor respondents on the interest charged by a typical microfinance institution like our partner organization.

[8] Implementation costs were partially covered by the Sawiris Foundation for Social Development while research costs were covered by USAID's Development Innovation Ventures.

[9] The financing amount fluctuated across cohorts to maintain a consistent number of animals as local prices varied with exchange rate movements during the study period.

insurance for non-negligent animal death, assessed by a veterinarian.[10] The contracts thus share several key features but differ in their liability structure:

The Equity Contract (Revenue Sharing): In this contract the borrower receives the livestock and provides all labor, feed, and care. Upon sale of the mature animals, the principal (the original purchase price) is returned to the lender. The remaining net revenue is split: two-thirds to the farmer and one-third to the lender. Unlike debt, this contract shares downside risk. If the animals are sold at a loss, the lender absorbs one-third of that financial loss. The structure provides implicit insurance, which may encourage risk-averse farmers to invest. To mitigate the risk of side-selling (verifiability), the sale process was typically supervised by a bank representative or conducted through a trusted buyer.[11,12]

The Debt Contract (Asset-Based Loan): The debt contract serves as a flexible benchmark. The borrower receives the same livestock but owes a fixed repayment equal to the principal plus a flat fee of 13.5% (roughly 27% APR), due as a single payment at sale. Under this contract, the borrower retains 100% of the upside but also bears 100% of the downside risk. Absent an insured death, equity pays the lender more than debt (and the borrower less) whenever net revenue exceeds 40.5 percent of principal (three times the fee), and less otherwise.

2.2 Experimental Design

Our experimental design allows us to separately identify *selection effects*—effects driven by who chooses each contract—from *contract effects*—how each contract affects outcomes conditional on selection. We achieve this through a two-stage procedure: first eliciting preferences over contracts, then randomizing contract offers within preference groups, inspired by the classic experimental design in Karlan and Zinman (2009).

[10] For the insurance, a veterinarian assessed each loss, and an ABWA committee ruled on negligence. 96 percent of losses were ruled no-fault. During the study the Sawiris Foundation funded both the financing capital and the insurance claims, so the experiment operated the product in its subsidized form (compensation was the dead animal's purchase price net of any distressed-sale proceeds); the model in Section 6 prices the same product for a lender that bears both costs.

[11] In practice, the bank did not always follow through on this requirement, being present for about 70% of the sales.

[12] If a borrower wished to exit the contract early, they had to pay the greater of (i) what borrowers in the debt arm would owe at that point, or (ii) the principal plus one-third of the current market value above principal. 58 of the 572 financed equity contracts ended this way, and in all but two the binding term was the debt amount. Appendix B reports the details. Appendices A to C form the Supplemental Appendix, whose pages are numbered S1 onward.

2.2.1 *Preference Elicitation*

After explaining both products via an animated video that illustrated payoffs under multiple scenarios, we asked potential clients to indicate their willingness to accept each contract.[13] Clients could express interest in the equity product only ("equity-only"), the debt product only ("debt-only"), both products ("indifferent"), or neither. This generates four preference groups based on revealed willingness to participate.

To encourage truthful revelation—and to avoid limiting the indifferent group to knife-edge cases of true indifference—we informed clients that those willing to accept both products would have approximately double the probability of receiving financing compared to those interested in only one product. Specifically, clients were told they had roughly a one-in-three chance of being assigned to each of three states (equity, debt, control), and so a client willing to accept both products faced approximately a two-in-three chance of receiving some financing, while a client interested in only one product faced approximately a one-in-three chance.

2.2.2 *Monitoring Treatment*

A key concern with equity contracts is moral hazard: because borrowers share downside risk with the lender, they may exert less effort in caring for the animals. To assess the scope of moral hazard and the potential for monitoring to mitigate it, we cross-randomized a monitoring treatment among clients who received financing.

Under the protocol, borrowers assigned to monitoring were told that a veterinarian would visit them during the cycle to assess how well they were caring for the animals, and that clear negligence could cost them the animals and an administrative fee; half of financed clients were assigned to this treatment.[14] We implemented these visits in both the equity and debt arms to control for any pure reminder or information effects of veterinary contact.[15]

[13] The video can be found at https://www.youtube.com/watch?v=ONM9E04d3JY. Appendix Figure A1 reproduces the payoff table the video closes on.

[14] Every borrower, monitored or not, was told to report a sick animal to the loan officer, after which a veterinarian assessed it and a forced sale or slaughter followed, and that failing to report counted as negligence and voided the insurance compensation. Loan officers followed up with all clients under the lender's standard procedure.

[15] Veterinarians were instructed to collect data on animal health and feed quality but not to share this information with the borrower, so that the monitoring treatment operated through the salience of oversight rather than through the information itself.

2.2.3 *Randomization*

Randomization was conducted at the individual level in batches, stratified by stated preference over the contract type. Clients who indicated willingness to accept both products were randomized into one of three groups: equity, debt, or control. Clients who indicated interest in equity only were randomized into either equity or control. Those interested in debt only or neither product were not included in the primary experimental sample.[16]

This design produces eight distinct experimental cells (Figure 1), which support several comparisons of interest. First, we can estimate the causal effect of each contract type on outcomes by comparing treated clients to their respective control groups within preference strata. Second, we can isolate selection effects by comparing outcomes for clients who receive equity from the equity-only group to those who receive equity from the indifferent group—these clients receive identical contracts but differ in their revealed preferences. Third, we can test for contract effects by comparing equity to debt recipients within the indifferent group, who by construction were willing to accept either contract. Finally, within each preference/contract cell we can compare those that were randomized into the monitoring arm with those that were not to estimate the extent of moral hazard that could be mitigated through additional monitoring.

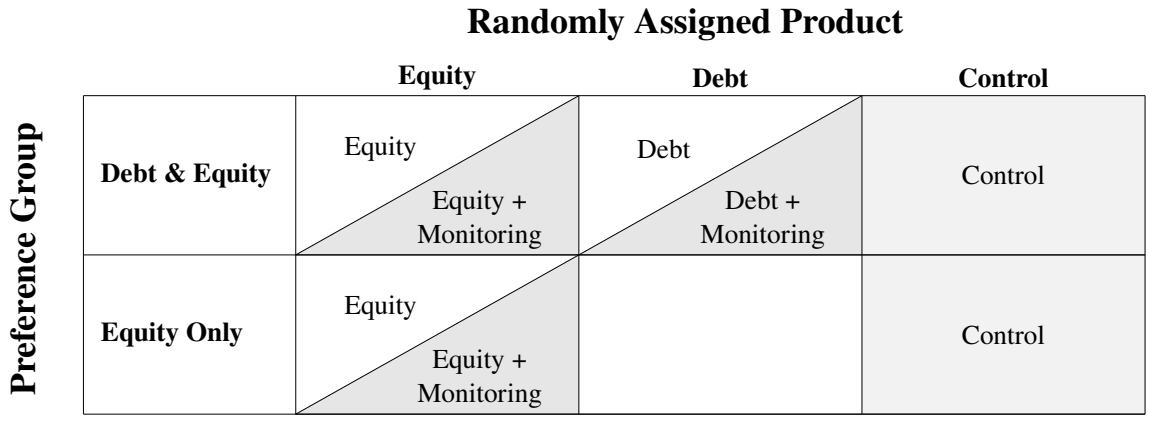
2.3 *Experimental Sample Recruitment & Data Collection*

Loan officers from our implementing partner would enter a location and market the product widely. Loan officers would then pass along information about which individuals were interested in participating in the study to our team. Then trained enumerators would go to applicants' homes and administer an in-person baseline survey prior to randomization. These surveys collected information on demographics, household composition, prior experience with livestock and finance, and behavioral preferences. In addition to questions about risk aversion we included measures of debt aversion ("If I need money, I try to avoid taking a loan, even if I know I can pay it back") and debt stigma ("It is embarrassing if someone else knows I have loans from a bank"). Cognitive ability was assessed via digit span recall, and we measured psychological well-being using the GAD-2 anxiety scale and self-reported stress and financial worry.

We conducted follow-up surveys at approximately 7 and 13 months after disbursement.

[16] We randomized a subset of debt-only applicants into receiving the debt product to ensure credibility of the stated probabilities, but did not include them in the survey/analysis samples since the impacts of debt on this selected population are not the focus of our study.

Figure 1: Experimental Design



Notes: Participants were grouped by their stated willingness to accept equity and/or debt. Those who were only interested in debt or in neither product were not included in the primary experimental sample.

The 7-month survey, conducted shortly after the end of the initial contract period, captures immediate impacts on income, livestock holdings, effort and input investments (hours spent grazing, spending on feed, feed quality), and subjective well-being. The 13-month survey captures medium-term persistence and was meant to capture any effects on subsequent borrowing behavior. Due to delays from Egypt's Financial Regulatory Authority, the lender was unable to provide a second round of equity financing to the sample, so we focus on 7-month results.[17] A companion report presents the available 13-month outcomes and reconciles the full analysis with the pre-analysis plan, including the prespecified tests not reported here (Haggag and Osman, 2026).

We also obtained detailed administrative data from ABWA covering all aspects of the contract cycle. This includes the purchase prices and weights of animals at disbursement, sale prices and weights at contract completion, exact repayment dates and amounts, and any instances of animal death or insurance claims.[18] For clients in the monitoring arm who were

[17]Borrowers were not promised further financing. Neither the consent script nor the product description mentioned a second round of financing, and we asked ABWA not to lend to borrowers (with standard debt products) until after the 13-month survey. Our design thus narrowed the scope for the type of dynamic incentives (e.g., providing a second round of financing to those who performed well) that might typically be at a bank's disposal and which could have mitigated the moral hazard we document in Section 4.

[18]We winsorize the survey-based outcomes at the top 1 percent, following the pre-analysis plan's outlier rule. We leave the administrative outcomes unwinsorized since we are less worried about reporting errors; in practice, however, it changes little.

visited by the veterinarians we have recorded assessments of animal health, feed quality, and any evidence of borrower neglect during each visit.

Finally, we collected data on client beliefs and hypothetical choices. At baseline, we elicited beliefs about market prices for livestock and asked clients to report their ideal project scope under each contract type. At midline, we asked borrowers financed on equity what they would take under debt and borrowers financed on debt were asked what they would take under equity. These measures allow us to examine whether contract structure affects intended investment decisions at the extensive and intensive margins, which we report in our companion report.[19]

2.4 Census Demand Elicitation

To cleanly assess how demand for equity and debt differ we implemented a census in 7 villages. In each village enumerators attempted to visit every household. Households were first screened for (i) prior experience raising livestock, (ii) interest in expanding current livestock holdings, and (iii) whether the respondent was a financial decision-maker. Eligible respondents were then told about the products, including being shown a standardized video explaining both financial products. If they were interested in applying for one or both they completed the full baseline survey. If they were not interested in hearing more, we asked why. We were careful to describe the opportunity as a way to expand their flock as a precaution against households assuming we were only offering an interest-bearing loan and refusing on religious grounds.[20]

This procedure allows us to accurately assess selection into these products in a way that does not cause us to miss individuals who may be interested in financing but are not typically reached by conventional lenders, due to concerns such as debt aversion and religion. By approaching households directly, we capture a more complete picture of latent demand and can more fully assess selection into equity versus debt.

Enumerators visited 11,416 households. Of these, 4,042 had prior experience raising livestock, and 484 of those expressed interest in expanding their flock; 213 completed the baseline survey and 196 were eligible to apply. Among the eligible respondents, 47 percent of Muslims would accept financing only as equity, against 25 percent of Christians.

[19] Beliefs about livestock prices are nearly identical across the three groups: equity-only, equity-indifferent, and debt-indifferent clients expect the amount named in the question (10,000 to 25,000 EGP, depending on the cohort) to buy 3.17, 3.15, and 3.18 sheep.

[20] Enumerators noted any household that raised religion on its own and, in that case, explained that both products carry approval from Egypt's central religious authority.

The experimental sample was recruited almost entirely in Muslim-majority areas, so this comparison is available only in the census (Appendix Table A12 reports the full intake, by religion).

2.5 Devaluations

The experiment took place during a time with elevated macroeconomic uncertainty. Egypt imported roughly 80% of its wheat from Russia and Ukraine, and the February 2022 invasion caused food and animal feed costs to spike. This external shock exposed deeper structural vulnerabilities. The Egyptian pound underwent two large devaluations between March 2022 and January 2023, losing more than 50% of its value against the US dollar, and a third large devaluation came in March 2024, when the government moved the pound to a float.

In this environment, the effective relative price of debt versus equity can shift sharply even if contract terms are unchanged: a devaluation lowers the real cost of debt repayments, while an equity/revenue-share contract tends to extract more from the borrower as revenues rise with the price level. Mechanically, this makes debt cheaper relative to equity after a devaluation.

Appendix Figure A2 collects the exchange rate and the purchase and sale prices our borrowers faced. Throughout the paper we refer to the stretch from the January 2023 devaluation through the March 2024 float, comprising the two step devaluations and the sustained high inflation between them, as the *devaluation period*. Operationally, we classify a contract as exposed if it could have spanned (or fallen between) either the 2023 or the 2024 episode, which is the case for cohorts 27 and earlier. Because these macro swings can confound clean comparisons, our main analysis in Section 4 focuses on the later cohorts, financed once exchange rates and inflation dynamics had stabilized.[21] In Section 5 we report how borrower behavior differs during the devaluation period relative to the main analysis.

[21] Where the boundary falls is a judgment call about when the devaluation regime ends. Appendix Figure A2 plots purchase and sale prices per kilogram by cohort. Purchase prices climb through the mid-twenties cohorts, peak at 27, and ease back over 28 to 31; sale prices drift down across the whole window without breaking at any cohort. Cohorts 26 through 29 all sit somewhere in that transition, and cohort 28 is the closest call: three quarters of its borrowers were financed just before the March 2024 float and sold roughly five months after it. Appendix Table A7 re-estimates the main results at cutoffs of 26 through 29. The estimates are largely consistent across the four. The contract effects hold at every cutoff, and among the selection effects animal deaths holds throughout while forced sales weakens as the sample narrows.

3 Demand and Sample Characteristics

3.1 Differences in Demand for Equity and Debt

In our experimental sample we find that 51% of randomized clients expressed interest in equity only, while 49% indicated willingness to accept either product. When those in the indifferent group were forced to choose between the two contracts, about 80% stated a preference for equity, which is consistent with substantial demand for risk-sharing in this population.

Table 1 presents summary statistics for the analysis sample. We report the average characteristics for individuals who applied for both the equity product and the debt product in Column 1. We find that on average those who applied for both products have 6.5 years of schooling, 65% are women, and 19% already own sheep or goats.

Average income in the previous month was around 3,600 EGP, approximately $117 at the official exchange rate of December 2023, the month of the median baseline interview.[22] We also collected several different behavioral measures. This includes measures of risk tolerance, debt aversion and debt stigma, as well as measures of anxiety and stress. Those who are "indifferent" between equity and debt report high levels of positive reciprocity and a debt aversion score of 6 out of 10, and about a quarter qualify as having significant anxiety.

Column 2 shows how those individuals who are equity-only differ on observable characteristics relative to those who are "indifferent". We see that they are slightly more educated, perform better on the digit span recall test, and report lower risk tolerance on the incentivized quantitative elicitation. Positive reciprocity, debt aversion, and debt stigma are higher, but not significantly so.[23] Several baseline characteristics we expected to see differences in are surprisingly balanced across groups. For example, while the equity-only group has higher levels of debt aversion and debt stigma these differences are not statistically significant.

[22] Baseline interviews ran from June 2022 to December 2024, straddling the March 2024 float. The rate is the weekly interbank series plotted in Appendix Figure A2 (Yahoo Finance, series `EGP=X`), which tracks the official rate: roughly 30.9 EGP per dollar from January 2023 through February 2024, about 48 in the months after the float, and about 50 by December 2024. The non-devaluation cohorts that form our main analysis sample were interviewed between February and December 2024, with a median in September 2024; their average income was around 4,300 EGP, approximately $88 at that month's rate of 48.4. The devaluation-period cohorts, interviewed between June 2022 and January 2024 with a median in July 2023, averaged around 3,100 EGP, approximately $100 at the peg.

[23] The qualitative and quantitative elicitations show only a weak positive correlation in our data, and surveyors anecdotally reported more confusion with the qualitative elicitation. Both patterns are consistent with Chapman et al. (2025), who find that qualitative self-assessments are weakly correlated with incentivized quantitative measures. It is thus somewhat reassuring that the quantitative risk tolerance measure correlates in the expected direction with demand for equity-only.

Table 1: Selection into Equity-Only Preference Group

	Experiment Sample			Census Sample
	Indifferent Mean (1)	Equity-Only Mean Diff. (2)	Multivariate Coefficient (3)	Multivariate Coefficient (4)
Education (Years)	6.47	1.01***	0.003	-0.002
	[6.38]	(0.39)	(0.003)	(0.010)
Cognition (Digit Span Recall)	2.43	0.20**	0.015	0.036
	[1.48]	(0.08)	(0.010)	(0.041)
Risk Tolerance (Quantitative)	6.66	-0.95*	-0.003**	-0.006
	[9.48]	(0.50)	(0.001)	(0.008)
Risk Tolerance (Qualitative)	5.76	0.04	0.001	0.029*
	[3.67]	(0.19)	(0.003)	(0.016)
Positive Reciprocity (1-6)	5.09	0.08	0.009	0.026
	[1.39]	(0.07)	(0.009)	(0.048)
Debt Aversion (0-10)	6.10	0.19	0.001	0.011
	[4.10]	(0.22)	(0.003)	(0.010)
Debt Stigma (0-10)	3.83	0.28	0.002	0.005
	[4.32]	(0.23)	(0.003)	(0.011)
GAD-2 (Anxiety) ≥ 3	0.26	0.01	0.005	0.059
	[0.44]	(0.02)	(0.028)	(0.128)
Stress (0-100)	33.08	3.32	0.000	-0.001
	[37.60]	(2.02)	(0.000)	(0.001)
Female	0.65	-0.01	-0.008	0.131
	[0.48]	(0.03)	(0.027)	(0.149)
Muslim	0.98	0.00	-0.004	0.033
	[0.14]	(0.01)	(0.079)	(0.159)
Income Last Month (1,000 EGP)	3.60	-0.14	-0.003	-0.012
	[4.08]	(0.22)	(0.003)	(0.010)
Owns Sheep or Goats	0.19	0.01	0.038	-0.098
	[0.39]	(0.02)	(0.033)	(0.131)
Prior Equity Experience	0.17	-0.00	0.000	0.176
	[0.38]	(0.03)	(0.038)	(0.154)
Previously Applied for Loans	0.33	0.03	0.023	-0.148
	[0.47]	(0.03)	(0.030)	(0.127)
p-value (Joint Test)			0.269	0.561
N			1,731	145

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Column (1) reports means for the indifferent group, with standard deviations in brackets. Column (2) reports the equity-only minus indifferent difference, from a regression of each characteristic on an equity-only indicator. Columns (3) and (4) report coefficients from a single linear probability model of the equity-only indicator on all characteristics at once, in the experiment and census samples. In these two columns, missing values are set to the sample mean and flagged with indicators, and the joint test is an F-test that all coefficients are zero. Income is winsorized at the top 1 percent. Experiment-sample regressions include cohort and baseline-enumerator fixed effects, and the census regression includes baseline-enumerator fixed effects. Robust standard errors in parentheses.

There are also no differences in prior equity experience or prior formal loan experience. This suggests that preferences for equity relative to debt are not simple to identify.

Column 3 runs a multivariate regression where a binary measure of demand for equity-only is on the left-hand side and all of Table 1's baseline variables are included on the right-hand side. Overall we find that "equity-only" borrowers are observationally similar to those who are "indifferent" between equity and debt. This suggests limited selection on

observables, meaning that lenders would be hard pressed to target the equity product to the different groups using observables alone.

Column 4 runs a similar multivariate regression but with the census sample. This allows us to compare how selection in our analysis sample may differ from selection in a sample that is explicitly representative. Once enumerator effects are absorbed, no characteristic predicts equity-only demand at the 5 percent level in the census sample, and the fifteen characteristics are jointly uninformative ($p = 0.56$), as in the experimental sample ($p = 0.27$). The census therefore gives no sign that the equity-only preference is confined to a subgroup that standard recruitment would miss.

3.2 Randomization Balance

Appendix Table A1 provides additional information about the baseline characteristics of our sample. In it we split the sample into the different experimental groups. Since we have two distinct strata, (1) those who only applied to equity and (2) those who applied to both, we have a control group for each, reported in Columns 1 & 3. Column 2 reports the difference in the equity-only treatment group relative to the equity-only control group. Similarly, Columns 4 & 5 report differences among groups in the "indifferent" strata. We report a joint test of balance for each treatment group relative to its respective control group and find that the groups are statistically balanced. Attrition is negligible, with 99.1 percent of the 1,901 randomized clients appearing in the 7-month survey data and no differential attrition by arm.[24] After a loan-officer exclusion described in footnote 25, the analysis sample consists of 1,731 clients (727 in the primary non-devaluation sample).

4 Contract and Selection Effects

Our design lets us compare experimental groups in ways that separate differential selection into the products from the effect of the contract structure itself on borrower behavior and outcomes. To do this we run a series of related regressions. The more general regression is:

$$Y_i = \beta_0 + \beta_1 Equity_i + \beta_2 Debt_i + \tau_s + Y_{0i} + \epsilon_i \tag{1}$$

where we regress the outcome variable on treatment indicators while controlling for strata fixed effects, and the baseline value of the outcome variable when available.

[24] We are grateful for exceptional field implementation for achieving such high response rates.

To estimate the "selection effect" into equity, we compare performance of those individuals who expressed "equity-only" preferences to those who expressed a willingness to take both equity and debt, conditional on being allocated to the equity treatment (i.e., the "equity-indifferent" group). We do this using the following regression:

$$Y_i = \beta_0 + \beta_1 EquityOnly_i + \tau_c + Y_{0i} + \epsilon_i \tag{2}$$

In this regression we restrict the sample to only those randomized into the equity treatment arms. The only differences between these groups are how they selected into the project. The regressor is an indicator equal to one if the individual selected in as an "equity-only" type. We control for cohort fixed effects in this regression, ensuring that we compare individuals who applied for financing at the same time and location.

We are also able to estimate "contract effects" by comparing individuals randomized into equity to those randomized into debt, conditional on being in the "indifferent" group. We run the following regression, in which the regressor is an indicator equal to one if the individual was randomized into the equity treatment. We restrict this regression to those that applied for both contracts, once again including strata fixed effects and controlling for baseline values of the outcome variable when available.

$$Y_i = \beta_0 + \beta_1 EquityIndifferent_i + \tau_s + Y_{0i} + \epsilon_i \tag{3}$$

4.1 Animal Deaths & Repayment

Table 2 provides a stark view of potential "selection" and "contract" effects on animal production and repayment. We begin by looking at deaths and "forced sales" (i.e., cases where the animal was sick and needed to be sold earlier than intended). In all of these comparisons we restrict the sample to those that received the animals so that we can focus on intensive margin effects.[25] Reassuringly, take-up is high across all groups, with 90% take-up in the equity-only and equity-indifferent groups, and 96% in the debt group (see

[25] We also exclude the clients of one loan officer from cohort 30 onward. Midline surveys and administrative records show cases in which he took borrowers' animals, sold them, and did not pass the proceeds to the association, which then pursued the borrowers for money they had never received; cohort 30 is the first in which such a case appears. His clients appear in both arms, and randomization was stratified by preference group and cohort, so the exclusion does not disturb balance. Appendix Table A11 shows that the estimates are stable under retaining all of his clients, dropping them from cohort 24 (the first, milder complaint), or dropping all of them.

Appendix Tables A2 & A3).[26]

Table 2: (Bad) Product Performance: Deaths, Forced Sales, Delinquency

	(1) Total Deaths	(2) Total Forced Sales	(3) Delinquent 30+ Days	(4) Default
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)				
Equity-Only	-0.11**	-0.12**	0.03	0.01
	(0.05)	(0.05)	(0.02)	(0.01)
Equity-Indiff Mean	0.17	0.17	0.01	0.00
N	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)				
Equity-Indifferent	0.04	0.13**	-0.07***	-0.06***
	(0.05)	(0.05)	(0.03)	(0.02)
Debt-Indiff Mean	0.13	0.05	0.10	0.06
N	210	210	210	210

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to non-devaluation cohorts (cohort ≥ 28). Delinquent 30+ Days = reached 95% of the death-adjusted obligation 30 or more days late. Borrowers who never reached 95% (the defaulters) are counted as delinquent. Default = left at least 5% of death-adjusted obligation unpaid. See Appendix Tables A8 and A9 for sensitivity to different delinquency and default thresholds. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Panel A shows that equity-only borrowers were much less likely to have had an animal die, with 0.11 fewer animal deaths relative to equity-indifferent borrowers. This is a 65% decrease relative to equity-indifferent borrowers who had 0.17 animals die (since all borrowers received 3 animals, this implies that a little less than 6% of their animals died). Similarly, we see fewer forced sales, a distinct outcome from a death but also indicative of a poor animal outcome.[27] Equity-only borrowers have 0.12 fewer forced sales, a 69% decrease relative to equity-indifferent borrowers. Both of these effects are statistically significant. These two

[26] If our analysis included everyone in treatment, including those who did not take up, we would conflate changes in animal outcomes with differences in take-up rates across groups. By conditioning on take-up we risk a small amount of bias due to potential differential take-up in the debt condition, but we think that is worth the clarity provided by focusing on intensive margin responses in these tables. When we consider the impact of the products on overall borrower income we return to intent-to-treat specifications.

[27] Forced sales were often borrower initiated, typically reflecting a case where an animal was in poor health and the veterinarian determined that the animal could only be salvaged by prematurely slaughtering and selling at a deflated price. Appendix B reports how often deaths, forced sales, and early exits occurred, how rarely a contract experienced more than one, and how each is settled under the contract.

results provide strong evidence of *advantageous* selection—equity-only borrowers perform *better* relative to those who are "indifferent". This differs from conventional worries that risk-sharing contracts encourage lower-effort borrowers to apply for financing (Townsend, 1979).

Turning to Panel B, we compare how equity-indifferent borrowers behaved relative to debt borrowers, conditional on selection. We find that equity-indifferent borrowers have 0.04 more animal deaths and 0.13 more forced sales, the latter being more than three times as much as the debt group. This provides evidence of moral hazard—the equity contract is leading borrowers to exert lower effort, or to take on more risk, because of the risk-sharing structure of the contract.

Columns 3 & 4 show how this impacts high-level measures of repayment—delinquency and default. Thirty-day delinquency and default are both limited overall, with small and statistically insignificant differences in Panel A.[28] Panel B on the other hand shows much lower levels of delinquency and default in equity relative to debt. This seems initially surprising, since the equity-indifferent group has more deaths and forced sales, but lower default suggests lower moral hazard on the *repayment* margin. One way to rationalize this is that profit-sharing keeps borrowers "above water": under equity, obligations adjust with outcomes. Indeed, a large share of equity borrowers paid less than what they would have owed under debt, suggesting some may have avoided default because their obligations were lower.[29,30]

Overall, these results provide evidence of *advantageous* selection in equity and moral

[28] Appendix Tables A8 and A9 examines sensitivity to the delinquency and default definitions. At shorter horizons the pattern differs: equity-only borrowers are roughly twice as likely as the equity-indifferent to be at least one day late (33 versus 17 percent), but these delays do not persist, and the two groups are statistically indistinguishable at 30 and 60 days and on default. Our companion report presents the prespecified variance-equality tests. For days delinquent, equality is rejected between the two equity groups (standard deviations 20.2 versus 6.9, p = 0.014): four of 126 equity-only takers were more than 90 days late (versus none of 99 equity-indifferent), and these four account for the coefficients at the 90-day threshold and on days delinquent in Appendix Table A8. Under the contract comparison the dispersion runs the other way: days delinquent vary far more under debt than under equity (33.2 versus 6.9, p = 0.004).

[29] An alternative explanation is differential loan officer monitoring. While we did not collect an administrative measure of monitoring at sale, a proxy is whether the client reported that the loan officer was present for weighing the animals (which typically, but not always, happened during sale). Equity borrowers were about 10 percentage points more likely than debt borrowers to report the loan officer present for weighing (p = 0.075), although the loan officer's presence does not predict default. While establishing a causal link between (endogenous) loan officer monitoring of sales and delinquency/default is beyond the scope of this exercise, it remains a possible explanation for this particular result.

[30] Exploratory heterogeneity cuts by gender, risk tolerance, cognitive ability, and baseline hypothetical contract preference (among the "indifferent") are in the companion report (Haggag and Osman, 2026); only contract preference shows a significant interaction with contract performance.

hazard in performance. The next tables explore the mechanisms behind these outcomes in more detail.

4.2 Animal Production Outcomes

Careful data collection lets us trace how selection and contract effects changed borrower behavior. Table 3 looks at animal outcomes in more detail. Column 1 reports the value of the animals at sale. Panel A shows that the value at sale for equity-only borrowers is about 700 EGP larger than that of equity-indifferent borrowers. A positive effect here again shows *advantageous selection*, i.e., equity-only borrowers perform *better* relative to those who are indifferent. While this represents only a 3% increase in sales value, it is a 43% increase in *net* revenues, as shown in Column 4, but is not statistically significant.

Table 3: (Good) Product Performance: Sales Revenue, Weights, and Net Revenue

	(1) Total Value at Sale	(2) Weight at Sale	(3) Value/KG at Sale	(4) Net Revenue
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)				
Equity-Only	699	3	2	713
	(708)	(4)	(2)	(705)
Equity-Indiff Mean	22,372	126	178	1,651
N	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)				
Equity-Indifferent	-1,385**	-5	-4*	-1,423**
	(665)	(4)	(2)	(663)
Debt-Indiff Mean	23,678	131	181	2,956
N	210	210	210	210

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to borrowers who took up financing in non-devaluation cohorts (cohort ≥ 28). Outcomes come from the lender's sale records. Total value at sale (column 1) is the price received for all financed animals sold, in normal and forced sales, in EGP. Weight at sale (column 2) is the total weight of those animals in kilograms, and column 3 divides column 1 by column 2. Net revenue (column 4) is total sale value less acquisition principal, before division between borrower and lender. Table A4 reports the division and the lender's compensation for deaths and forced sales, which columns 1 and 4 exclude. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Panel B compares performance under equity to performance under debt, holding selection fixed. Here we see that those randomly allocated to the equity contract sell their animals for 1,385 EGP *less* than those who had the debt contract. This is a 6% decrease in total value

at sale, but is a 48% decrease in net revenue and is statistically significant. This provides strong evidence of moral hazard in equity contracts relative to debt. On the lender's side, the contractual claim net of the insurance it funds is 2,342 EGP lower per equity-indifferent contract than per debt contract (Appendix Table A4).[31]

Columns 2 & 3 break animal revenue into weight by animal and value per kilogram. This allows us to understand what is leading to the differences in sales prices. We see that the equity-only group's animals are slightly heavier overall and are higher quality, commanding more money per kilogram. But these differences are not statistically significant. On the other hand, we find that those in the equity-indifferent group have lighter animals with lower quality, receiving lower value per kilogram.

Table 4 explores the potential mechanisms that can explain the differences in outcomes reported above. We consider total livestock costs in column 1, the borrower's own estimate of average monthly spending on the animals, which covers what the borrower paid for, such as feed and medication, but not unpaid inputs like crops from the borrower's own land (column 3). We also include information about the number of hours spent grazing the financed animals. Overall borrowers report small differences in how they are treating the animals. Equity-only individuals spend a little more and graze more relative to the equity-indifferent group (about 4.6 more hours a week, or 16 percent), which is consistent with having slightly larger and higher quality animals. Similarly equity-indifferent borrowers spend slightly less on their animals and graze less relative to debt, leading to smaller animals and lower quality per kilogram.

While these reported differences are small, they move in the same direction as the results in Tables 2 & 3.[32] Given the size of the impacts on animal deaths, effort is likely to differ substantially across the groups even though the survey measures move only a little. Effort is notoriously difficult to measure in surveys, but our evidence is consistent with lower

[31]The gap reflects the contracts' division of proceeds rather than repayment behavior; the effect on the margin actually repaid is −2,188 (SE 468). The advantageous selection in Table 2 also shows up in the insurance cost, since an equity-indifferent borrower cost roughly 1,000 EGP more to insure than an equity-only one (1,571 versus 556 EGP), leaving the lender's net-of-insurance margin 1,267 EGP higher on equity-only takers. Section 6 prices the selection effect in this form.

[32]This result stands in contrast to the closest other tests of similar contracts. Specifically, Cordaro et al. (2025) find higher profits under a performance-contingent hybrid than under debt among Kenyan micro-distributors and attribute the gain to effort. This may reflect a notable contract design difference, as repayment under their hybrid stops once it reaches the debt amount, so extra effort shortens the obligation, whereas our uncapped share takes a third of marginal revenue for the full cycle. Our result also contrasts with the laboratory-style designs of Meki (2025) and Fischer (2013) where the payoff of each option is fixed, so the contract there changes which project a borrower selects rather than how much care she puts into it.

Table 4: Product Performance: Inputs

	(1) Total Livestock Costs	(2) Feed Cost	(3) Supplies Value	(4) Medication Cost	(5) Grazing Hours
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)					
Equity-Only	10	-9	41	-28	5**
	(111)	(49)	(98)	(30)	(2)
Equity-Indiff Mean	1,392	690	464	172	29
N	219	217	217	220	224
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-20	-89	86	10	-1
	(108)	(57)	(78)	(29)	(2)
Debt-Indiff Mean	1,404	764	395	162	30
N	206	206	207	206	209

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to borrowers who took up financing in non-devaluation cohorts (cohort $\geq$ 28). All outcomes are from the 7-month survey, in EGP except column 5, and winsorized at the top 1 percent. Column 1 is the borrower's estimate of monthly spending on the financed animals. Column 2 is the value of feed given in the last week before sale, whether bought or home-grown. Column 3 is the monthly value of inputs the borrower used but did not pay for, such as crops from the borrower's own land, so it is not part of column 1. Column 4 is monthly spending on the animals' treatment, and column 5 is the hours per week spent grazing the financed animals. A borrower who was not asked a question, or did not answer it, is left out of that column. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

effort due to the contract structure, and higher effort among those who positively select into equity-only applications.

4.3 Impacts on Borrower Income

Table 5 reports impacts on borrower income. Unlike previous tables that compared the treatment groups to each other, this table compares the treatment group to their respective control groups, using an intent-to-treat specification.[33] Panel A compares those who expressed equity-only preferences and were allocated to treatment with those that were allocated to control. Column 1 looks at last-month income, the respondent's own or the

[33] We control for baseline income, imputed with a missing indicator where missing. Using the double post-lasso control selection specified in the pre-analysis plan instead leaves every headline coefficient essentially unchanged.

household's main earner's (see the table note), with the borrower's computed share of the animal sale added unless the borrower said, in a follow-up question, that the figure already included it (Appendix Table A13). This measure increased by 1,072 EGP. While large, it is the culmination of 6 months of effort, and so spread over the 6 months of the loan it would be considered a 4.5% monthly increase in income. Column 2 reports the survey figure on its own (without adding in the animal sale proceeds), and shows a small positive and insignificant effect.[34] We find little evidence that effort on the animals crowded out other income-generating activities; the one exception is monthly crop-farming profit, which falls by about 150 EGP for equity-only borrowers, roughly half the control mean (column 4). This aligns with our priors: many of these borrowers had excess capacity to take care of these animals that was underutilized due to financing constraints.

Panel B considers the impacts on individuals who applied for both equity and debt. Similarly we find large positive impacts for the equity-indifferent group relative to the control-indifferent group. Their income is 1,735 EGP larger, a 46% increase. Again, this would average out to 7.6% monthly over the course of the financing. Those in the debt group had a smaller and marginally significant increase in income of 918 EGP, a 24% increase relative to control, which would average out to 4% monthly. This is because, as shown in Table 3 while the debt group sold their animals for the greatest average value, the bank share of the profits was much larger for the debt borrowers relative to the equity borrowers (Appendix Table A4, column 4).

We also examine effects on subjective well-being. Among equity-only borrowers, financing raises self-reported stress by 7.2 points against a control mean of 46 and the GAD-2 anxiety score by 0.32 against a mean of 1.9, each significant at the ten percent level, and both effects are larger at 13 months (Haggag and Osman, 2026); effects for the indifferent group are near zero under either contract (Appendix Table A6). The group that responds is the one that would not borrow under debt at all, so we read this suggestive result as a cost of taking on a formal obligation rather than of the contract form.[35]

[34] Note that the control mean is the same between columns 1 & 2 because the control group has no income from ABWA funded animals.

[35] Microcredit itself can lower subjective well-being: Karlan and Zinman (2011) find a small decline among randomly approved borrowers in the Philippines, and Field et al. (2012) show that the repayment schedule alone moves reported financial stress among Indian microfinance clients.

Table 5: Impacts on Borrower Income

	(1) Last Month Total	(2) Last Month Excl. ABWA	(3) Monthly: Livestock Profit	(4) Monthly: Crop Farm Profit	(5) Monthly: Wages And Other
Panel A: Equity Only					
Equity	1,072**	107	20	-153**	-64
	(424)	(353)	(55)	(72)	(235)
Control Mean	4,000	4,000	100	293	3,204
N	387	387	387	387	387
Panel B: Indifferent (Equity vs Debt)					
Equity	1,735***	274	55	92	64
	(485)	(393)	(38)	(133)	(276)
Debt	918*	186	93*	166	-129
	(496)	(381)	(56)	(124)	(301)
Control Mean	3,799	3,799	12	148	3,323
N	327	327	327	327	327
Selection Effect (Equity-Only - Equity-Indif)	0.38	0.88	0.67	0.09*	0.85
Contract Effect (Equity-Indif - Debt-Indif)	0.14	0.82	0.54	0.59	0.54

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to non-devaluation cohorts (cohort ≥ 28). Columns 1 and 2 report income in the *last* month, and columns 3–5 report the household's monthly income by source (the sum of the participant's and the household head's answers). Column 1 adds the borrower's contractual share of the ABWA animals' proceeds under the full contract rules (sale proceeds plus insurance compensation, the early-exit floor, and contractual overrides, net of the lender's claim; for debt, compensation-inclusive proceeds net of principal and the 13.5 percent fee), except for the 35 of 326 takers asked whose reported figure already included them. In columns 1 and 2, income is the respondent's own if the respondent reports any income source and otherwise that of the named main earner, so it measures one earner rather than the household. Column 5 combines irregular and regular wages, self-employment, and other income (pensions and government transfers). The last two rows report p-values for the selection effect (equity-only vs. equity-indifferent) and the contract effect (equity vs. debt among indifferent borrowers). Regressions control for baseline values where available (with missing-value indicators) and include strata fixed effects, except the pooled regression behind the selection-effect p-value, which uses cohort fixed effects instead. Robust standard errors in parentheses.

4.4 Product Satisfaction

Table 6 reports on borrowers' views of their experience with the product. Panel A again considers selection effects, while Panel B considers contract effects. Column 1 estimates impacts on a 1-5 satisfaction scale, with 5 being "very satisfied". Column 2 is a binary for whether the individual would like to immediately repeat borrowing the same kind of product they had last time. Overall we see moderate satisfaction, with satisfaction levels being statistically equivalent across all groups. When looking at repeat borrowing, we see a similar pattern.

These results mitigate concerns that the demand we found for equity was primarily driven by people wanting to try a novel product. If that were true, equity borrowers should be less satisfied and less likely to borrow again after completing a full cycle. Instead, after

Table 6: Product Satisfaction

			Product Preference (Next Time)		
	(1) Satisfaction	(2) Repeat?	(3) Equity	(4) Debt	(5) Neither
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)					
Equity-Only	-0.04	0.02	-0.05	0.02	0.03
	(0.25)	(0.06)	(0.07)	(0.03)	(0.07)
Equity-Indiff Mean	2.86	0.22	0.57	0.04	0.39
N	224	224	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	0.33	0.08	0.27***	-0.16***	-0.10
	(0.24)	(0.05)	(0.06)	(0.04)	(0.07)
Debt-Indiff Mean	2.52	0.15	0.30	0.22	0.49
N	209	209	210	210	210

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to non-devaluation cohorts (cohort ≥ 28). Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses. Satisfaction is the borrower's rating of the financing received, from 1 (not at all satisfied) to 5 (very satisfied). Repeat? indicates answering yes to whether the borrower would be interested in getting another product like this one. Columns (3)–(5) indicate the answer to "Between in-kind debt financing and equity, which do you prefer?", asked after roughly six months of contract experience.

firsthand experience with the contract, equity borrowers are no less satisfied than debt borrowers. Columns 3-5 break down responses to a question on which product the borrower would take if they were to repeat a round of borrowing from ABWA. We see that equity-only borrowers continue to avoid debt, with the majority preferring equity again or preferring not to borrow at all. On the other hand, those who were initially indifferent between equity and debt built a strong preference for equity and are significantly more likely to prefer it the next time around.

4.5 Monitoring

As described in Section 2.2.2 we cross-cut a monitoring intervention in each of the main experimental cells. This allows us to assess jointly whether moral hazard exists and whether active monitoring can mitigate it. Appendix Table A5 estimates the impacts of the monitoring treatment on primary outcomes of interest. We find no evidence that the monitoring arm led to economically meaningful differences across the equity or debt arms,

and the pre-specified test of whether the monitoring effect differs between contracts never rejects (p-values from 0.17 to 0.84).

We found evidence of moral hazard in Tables 2 & 3 above. This suggests that an intervention like monitoring could decrease moral hazard in this context. It is possible that our particular monitoring treatment was too light-touch to make a difference. Delivery also fell short of the design: borrowers assigned to monitoring received 2.4 visits on average, 45 percent received the intended three or more, and 6 percent received none.[36]

5 Devaluation

As mentioned above, the project began during a period of macroeconomic uncertainty: the devaluation period described in Section 2.5 covered roughly the first half of the study window. In Table 7 we consider how product performance differs for the earlier cohorts whose contracts spanned this highly inflationary period. The devaluation period changed the relative incentives of the two products: since interest rates were fixed and the equity revenue share proportion was also fixed, rising output prices made the debt product cheaper for the borrower and the equity product more expensive.[37,38]

Table 7 reports impacts across 8 primary outcomes. Our earlier results showed evidence of advantageous selection, i.e., that the equity-only group behaved in ways that led to higher productivity relative to the equity-indifferent group. We also showed evidence of moral hazard, i.e., that the equity-indifferent group behaved in ways that led to lower productivity relative to the debt-indifferent group. Table 7 shows that during the devaluation

[36] Every borrower (both those in the monitoring treatment and those not) was told to report a sick animal and that unreported neglect would void the insurance. Loan officers followed up with all clients, so the monitoring treatment added scheduled visits to obligations all borrowers already faced.

[37] A stylized example makes the shift concrete. Take a borrower financed with 25,000 EGP of animals who spends 2,000 EGP on inputs and sells for 34,000 EGP. Under equity she returns the principal, the lender takes a third of the 9,000 EGP that remains, and she keeps about 4,000 EGP once inputs are paid; under debt she owes the fixed fee of 3,375 EGP and keeps about 3,600. Now let inflation raise the sale price to 40,000 EGP and her input costs to 3,000 EGP, which she bears alone. The debt borrower keeps about 8,600 EGP, because her obligation has not moved, while the equity borrower keeps about 7,000, because the lender's share rises with the price level to 5,000. The same inflation that hands the debt borrower a windfall raises what the equity borrower pays for her financing, even as it raises the return to her marginal effort. These figures are illustrative rather than sample means.

[38] Appendix Table A2 shows that take-up remained high in the devaluation period and there is no evidence of differential take-up by treatment due to this period, which provides some evidence that the timing of these devaluations came as a surprise. Anticipation is least likely for cohorts already under contract when an episode hit, while those recruited mid-period chose contracts under visibly elevated inflation. The identifying assumption for the structural model is accordingly comparability across cohorts rather than surprise alone (Section 6.2).

the differences between the equity-indifferent group and both comparison groups disappear, consistent with the pricing pressure from the devaluation raising this group's effort.

Table 7: Devaluation Effects on Selected Outcomes

	Bad Outcomes		Good Outcomes		Inputs		Satisfaction	
	(1) Deaths/ Forced Sales	(2) Default	(3) Sale Value	(4) Value per KG	(5) Livestock Costs	(6) Grazing Hours	(7) Satisfaction	(8) Repeat?
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)								
Equity-Only	-0.23***	0.01	699.36	1.82	10.28	4.64**	-0.04	0.02
	(0.07)	(0.01)	(699.73)	(2.38)	(109.78)	(2.08)	(0.24)	(0.06)
Equity-Only X Deval	0.26***	-0.01	-941.62	-3.74	80.89	-4.82*	0.10	-0.03
	(0.09)	(0.02)	(872.37)	(3.28)	(161.52)	(2.70)	(0.30)	(0.08)
Equity-Indiff Mean (Non-Deval)	0.34	0.00	22,372.37	178.02	1,391.67	29.04	2.86	0.22
Equity-Indiff Mean (Deval)	0.20	0.01	20,511.51	165.70	1,323.55	31.99	3.42	0.40
N	572	572	572	572	493	571	571	571
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)								
Equity-Indifferent	0.17**	-0.06***	-1384.94**	-3.81*	-19.74	-1.16	0.33	0.08
	(0.07)	(0.02)	(658.28)	(2.21)	(107.76)	(2.11)	(0.24)	(0.05)
Equity-Indiff X Deval	-0.24***	0.05**	1411.19*	4.35	-109.09	2.04	-0.36	-0.15*
	(0.09)	(0.02)	(834.82)	(3.08)	(151.80)	(2.67)	(0.30)	(0.08)
Debt-Indiff Mean (Non-Deval)	0.17	0.06	23,678.39	181.06	1,403.71	30.33	2.52	0.15
Debt-Indiff Mean (Deval)	0.27	0.02	20,173.50	163.77	1,439.44	31.27	3.36	0.46
N	501	501	501	501	436	500	500	500

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Deval equals 1 for contracts that could have spanned (or fallen between) the 2023 or 2024 devaluation (cohorts 27 and earlier). A borrower who was not asked a question, or did not answer it, is left out of that column. Livestock costs (column 5) were not asked of most of cohorts 1 to 6. Grazing hours (column 6) are hours per week spent grazing the financed animals. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Column 1 shows that in the inflationary period, equity-indifferent borrowers erase any differences between them and the debt group in deaths & forced sales of the animals. Similarly, their performance steps up to match that of the equity-only group. Overall the pattern suggests that the equity-indifferent group was shirking in the non-devaluation state when the risk-sharing aspects of the contract made it relatively more attractive to lower effort. During the highly inflationary period, incentives were such that this group acted in a way consistent with increased effort aimed at maximizing livestock productivity.

The remaining columns show a similar story. Whereas during the non-devaluation period the overall sales price was lower for the equity-indifferent group, this difference disappears and they sell their animals for about the same amount overall and the same quality per kilogram of the animal. Effects on inputs are still relatively noisy, although the grazing gap between equity-only and equity-indifferent borrowers (about 4.6 hours a week in normal times) closes during the devaluation (column 6). Appendix Table A10 reports each input separately: relative to debt, equity-indifferent borrowers shift toward feed and away from unpaid inputs during the devaluation, though none of these shifts is estimated

precisely. Interestingly, while the devaluation has limited impacts on product satisfaction for the equity-only group, interest in repeating the contract rose in both groups but about 15 percentage points less among equity-indifferent borrowers than among debt borrowers (column 8), consistent with the shift in incentives making equity relatively less attractive to them. This latter result is consistent with evidence from Meki (2025), who finds that enthusiasm for profit sharing faded among borrowers who had experienced unusually high realized payments to the lender, which is precisely the state the devaluation created for our equity borrowers. Appendix Table A4, Panel B prices this shift for the lender: the 2,342 EGP per-contract gap between equity and debt closes entirely during the devaluation period, as the equity share rose with nominal sale prices while the fixed debt fee did not, and the insurance-claims gap between the two arms disappeared.

These results suggest that adverse selection and moral hazard are not inherent to all equity contracts. The extent of these forces will depend on the macroeconomic environment and changes in relative prices.

6 A Simple Structural Model of Pricing and Welfare

Our experiment allows us to assess the extent of selection and contract effects at one price combination, but it is reasonable to expect that both of these forces would shift as prices change. Ideal evidence would come from an experiment that maintained our two-part design but layered additional price randomizations on top of it, but the cost and logistical challenges of such a study are non-trivial. In our context, the devaluation period provides an unintentional substitute: it made the debt product cheaper in real terms, giving us an additional pair of *relative* prices to analyze. In this section we use that variation to discipline a simple structural model of how borrower effort and project outcomes respond to price changes. We combine this with demand elasticities from related experiments to ask which combinations of the interest rate and equity share make offering equity profitable for a lender (if any), and what offering it would do to borrower and lender welfare.

6.1 Model Description

Consider a lender facing a population of prospective borrowers, each with a single risky project. The lender can offer a *debt* contract, under which the borrower owes a fixed repayment whatever the project earns, or an *equity* contract, under which the lender takes a share of the project's realized surplus and bears part of its downside. It may offer either contract on its own or both together as a *menu*, and it chooses the terms of each: the interest

rate on debt and the lender's share under equity. Borrower behavior will be subject to two latent traits that we explain below: *debt aversion* and *stewardship*. On selection, borrowers differ in how willing they are to take on a fixed obligation, so debt and equity attract different types. Prices then determine who takes which contract, and how good a risk each resulting group is. On effort, equity dilutes the borrower's claim on the marginal return to her own work, so a larger equity share can weaken the incentive to look after the project and lower its realized surplus. Both forces move with prices: changing the interest rate or the equity share changes who borrows under each contract and how hard they work.

The Borrower's Problem

We build the model up from the borrower's problem. Each household i decides whether to finance her project and, if so, under which contract, choosing whichever of debt, equity, or no finance carries the highest payoff. The payoff indices are

$$U_i^D(r) \;=\; \alpha_D \;-\; d_i \;-\; \beta_D \,\ln\frac{r}{r_0}, \qquad U_i^E(s) \;=\; \alpha_E \;-\; \beta_E \,\ln\frac{s}{s_0}, \qquad U_i^{\varnothing} \;=\; 0, \tag{4}$$

and household i chooses

$$c_i^* \;=\; \underset{c\,\in\,\{D,\,E,\,\varnothing\}}{\arg\max}\; U_i^c. \tag{5}$$

Here α_D and α_E are baseline attractiveness levels for each contract, β_D and β_E govern how take-up responds to the price of each contract (the interest rate for debt, the share of the venture the borrower gives up for equity), and the value of no finance is normalized to zero. The parameter d_i is household i's *debt aversion*: an extra, latent non-financial cost of owing a fixed sum, over and above the repayment itself. Debt aversion varies across households and it is what separates the types. Households with high d_i refuse debt at any interest rate we consider and will finance only through equity, while households with d_i near zero weigh the two contracts on price alone. The parameter is a stand-in rather than a single behavioral primitive: it bundles risk and loss aversion, religious and social costs of debt, and any psychological burden of owing a fixed sum. The model requires only that some latent trait makes fixed obligations differentially costly. We denote by ω_θ the population share of type θ, and by $q_\theta^E(r,s)$ and $q_\theta^D(r,s)$ the fractions of that type for whom equity and debt are the best option at terms (r,s); the rest take no finance. These payoffs describe the mechanism schematically; the take-up schedules we simulate are calibrated to match observed take-up and outside price elasticities (Appendix C).

Take-up falls as a contract becomes more expensive. But we only have one set of nominal prices in our experiment and we treat the devaluation-led change in relative real prices as a surprise, so we cannot use that variation to estimate the demand response to prices.[39] Instead we use demand elasticities from related work from the authors where they explicitly randomized interest rates to measure how demand responds. In Gazeaud et al. (2026) they randomize interest rates for SMEs over conventional loans in Southern Egypt and find an elasticity of approximately -1. In Karlan et al. (2021), the authors randomize the relative price of conventional and Sharia-compliant loans in Jordan and find that more religious individuals are about half as price sensitive in their demand for the Sharia-compliant loan. As such we model demand using a -1 price elasticity for our "indifferent" borrowers and a -0.5 price elasticity for equity for our "equity-only" borrowers.

Once financed, the borrower chooses how much effort to put into the project. Under debt she is the residual claimant: she keeps every additional dollar her effort produces, so her effort does not depend on the interest rate. A higher rate raises what she owes but leaves the marginal return to her own effort untouched, changing what she is willing to pay rather than how hard she works. Under equity the lender takes a share s of that marginal return, so the borrower internalizes only the remaining $1 - s$; a higher share dulls her incentive and she supplies less effort. This asymmetry, that output-sharing distorts effort while a fixed claim does not, is the classic moral-hazard feature of share contracts (Stiglitz, 1974; Eswaran and Kotwal, 1985), and it is the central supply-side concern in recent work on equity-style finance for small firms (Meki and Quinn, 2024; Cordaro et al., 2025). We therefore let effort respond to the equity share but not to the debt rate.[40]

We write the moral-hazard pressure a contract puts on the borrower as an index $m(s)$, which rises as the lender's share climbs. The venture surplus available to divide falls with

[39] Consistent with the devaluations being treated as though they were a "surprise," the ways equity-only households differ from equity-indifferent households are the same in the devaluation and non-devaluation periods. We regress equity-only status on all 15 baseline characteristics and their interactions with a devaluation-cohort indicator and find no statistical difference ($p = 0.56$). The difference in take-up rates between equity-only and equity-indifferent households is also statistically equivalent in the devaluation and non-devaluation cohorts (Appendix Table A2).

[40] Interest rates can also induce moral hazard, but through a different channel: under limited liability a higher fixed obligation expands the region where default is attractive and weakens the incentive to generate the cash to repay (Karlan and Zinman, 2009). This channel is weaker in our setting, where loans are collateralized and default is rare and shallow. Only 6 percent of the debt arm "defaults"; every "defaulter" paid back the full sale proceeds, and none left more than a quarter of the death-adjusted obligation unpaid (Table 2; Appendix Table A9).

that pressure,

$$v(s) \;=\; v_0 \;-\; \lambda_v \left(m(s) - m(s_0)\right), \tag{6}$$

where v_0 is the venture surplus at the observed contract and $\lambda_v > 0$ is the surplus lost per unit of pressure. Pressure lowers surplus by raising adverse outcomes: a slope λ governs how adverse outcomes respond to pressure, and λ_v is its value in lost sale proceeds. Because m is an index, its scale is a normalization. We estimate λ below using the variation from the devaluation period, which shifted this pressure for equity borrowers while leaving the incentives of debt borrowers, who are residual claimants throughout, unchanged.

Some households are more susceptible to moral hazard than others. A second latent trait, *stewardship*, σ_i, captures how well a borrower looks after the project: more careful borrowers suffer fewer animal deaths and forced sales at any contract terms. Moral-hazard pressure scales a household's baseline risk, so high-stewardship borrowers are less affected when moral hazard pressure rises. The same slope λ governs this response, so stewardship adds no separate moral-hazard parameter.

The two traits could in principle be unrelated, but in our setting they move together, and this is what makes equity worth offering.[41] The equity-only households, identified by their refusal of debt, are also the more careful stewards: they suffer fewer animal deaths and forced sales and match or beat other borrowers on realized revenue (Sections 3 and 4; Tables 1, 2, and 3). This *advantageous selection*, which the experiment documents directly, is reflected in the parameters the model will estimate.

The Lender's Decision

With borrower behavior pinned down, we turn to the lender. Each loan it writes earns a per-loan margin, $\mu_D(r)$ on debt and $\mu_E(s)$ on equity. Raising the interest rate lifts the debt margin directly; raising the equity share lifts the equity margin mechanically, but only partly, because the same increase in s decreases effort and shrinks the surplus $v(s)$ from which the share is taken. The lender's decision is whether to add equity to its existing debt business: it offers both contracts as a *menu* at terms (r, s) only if doing so does not lose money relative to lending debt alone at the observed rate. We summarize the value of adding equity as the

[41] This parallels Finkelstein and McGarry (2006), who find advantageous selection in long-term care insurance; de Meza and Webb (2001) show how it arises when risk aversion and precaution go together (for a review, see Einav and Finkelstein, 2011).

difference in profits between offering the menu (equity & debt) and offering debt only:

$$\Delta\pi(r,s) \;=\; \sum_{\theta} \omega_\theta \left[q^E_\theta(r,s)\,\mu_E(s) + q^D_\theta(r,s)\,\mu_D(r) \right] \;-\; \Pi_{\text{debt}}(r_0) \;-\; a\,\Delta M(r,s), \quad (7)$$

where ω_θ is the population share of type θ and q^E_θ and q^D_θ are that type's take-up of each contract, $\Pi_{\text{debt}}(r_0)$ is profit from offering debt alone at the observed rate, and a is the administrative cost of each loan. The menu brings in borrowers who would not take debt, and $\Delta M(r,s)$ counts these extra loans, so the last term is the added administrative cost of serving them. We compute $\Delta\pi$ over a wide range of (r,s).

Welfare

Finally, we measure what the equity option is worth to borrowers. We follow the standard approach to valuing a new product (Hausman, 1996; Petrin, 2002): its value is the compensating variation from taking it away, which we approximate by the area under its demand curve between the observed price and a full lender share. The price of equity is the lender's share rather than a percent of principal, so we convert it into a monetary amount: $p(s)$ is how much more, in EGP, the borrower expects to pay the lender for each increase in the share. At the observed terms, raising the lender's share by one percentage point costs the borrower about 27 EGP. The value of the equity option to borrowers is then

$$\text{CV} \;=\; \int_{s_0}^{1} \Big(\textstyle\sum_\theta \omega_\theta\, q^E_\theta(r_0,s) \Big)\, p(s)\,\mathrm{d}s. \quad (8)$$

This is the measure without an insurance premium; Appendix C describes the version with one.

6.2 *Model Estimation, Identifying Assumptions, and Fit*

We fit the model in two steps. First, we calibrate it to the levels we observed in the experiment: the population shares ω_θ of the different types come from the census, and we use take-up rates, lender margins, and insurance costs directly. How demand responds as prices change comes from demand elasticities estimated in related experiments (Section 6.1; Karlan et al., 2021; Gazeaud et al., 2026). Second, we estimate the behavioral parameters using GMM, with each moment weighted by the inverse of its sampling variance.

Each behavioral parameter of Section 6.1 is informed by the part of the experiment that speaks most directly to it. For debt aversion d_i, we use how often households choose

equity when forced to choose between the two contracts. The equity-only households, all of whom chose equity, show how much more debt aversion it takes to refuse debt altogether relative to the equity-indifferent borrowers. For stewardship σ_i, we compare how equity-only borrowers and equity-indifferent borrowers differ in the number of animal deaths and forced sales (Table 2). For moral hazard, we estimate the slope λ, how strongly adverse outcomes respond to moral-hazard pressure $m(s)$, from how the effect of equity relative to debt differs in normal times relative to the devaluation times for indifferent borrowers. In total, we estimate three behavioral parameters, the extra debt aversion of equity-only households, the stewardship effect on adverse outcomes, and the moral-hazard slope λ. The six moments we target have four independent restrictions, so our model is over-identified by one (Appendix C).

Our identification and counterfactuals rely on three assumptions. First, borrowers who chose each contract are comparable across the cohorts financed before and during the devaluation, so the change in equity's effect reflects incentives rather than who borrowed. Randomization within cohorts, stable relative take-up (take-up declined, with no significant difference across groups), and balance at disbursement support this (footnote 39; Tables A2 and A3). Second, the devaluation moved the same moral-hazard pressure $m(s)$ that a higher equity share would, which lets us use its effect to predict behavior at other shares; only the product of λ and the change in pressure is identified, so we normalize the devaluation to one unit of pressure. Third, we treat the partner as the only formal lender in its area, so debt-only lending at the observed rate is the status quo against which we measure every counterfactual.

The model reproduces the six targeted moments closely; its one over-identifying moment, the normal-time gap in sale values, is matched within a tenth of a standard error. The estimates give a simple picture of the two forces at work. Because of their better stewardship, equity-only borrowers face roughly 40 percent of the baseline risk of a bad outcome that indifferent borrowers do. And raising the lender's share from one-third to one-half would shrink project surplus by about 12 percent, as borrowers keep less of the return to their effort, so the lender would take a larger share of a smaller surplus.

To assess whether the model captures behavior beyond these targets, we examine outcomes it was not estimated to fit. Its main prediction is that selection and contract effects should be concentrated among projects with low sale values, where animal deaths and forced sales matter most. Quantile regressions are consistent with this. Both effects are large in the bottom third of the sale-value distribution and close to zero in the top third.

The devaluation's effect on feed spending, which we never used in estimation, responds how the model predicts, and borrowers' stated interest in borrowing again is in line with the borrower surplus the model estimated for each group. On the other hand, the model is less successful during the devaluation, as it predicts forced-sale and death rates of about 5 percent among equity-indifferent borrowers relative to observed rates of about 10 percent. Appendix C.7 reports the full set of checks.

6.3 Which Terms Make Offering Equity Profitable?

With the model estimated, we ask two questions the experiment alone cannot answer: (1) how do different prices of debt and equity affect lender profitability? and (2) what are the welfare effects of offering equity products? For each pair of terms (r, s), we hold the estimated parameters fixed and re-solve the model to determine who takes each contract, how their projects fare, and what the lender earns. Throughout, we compare these outcomes with debt-only lending at the observed interest rate of 13.5 percent.

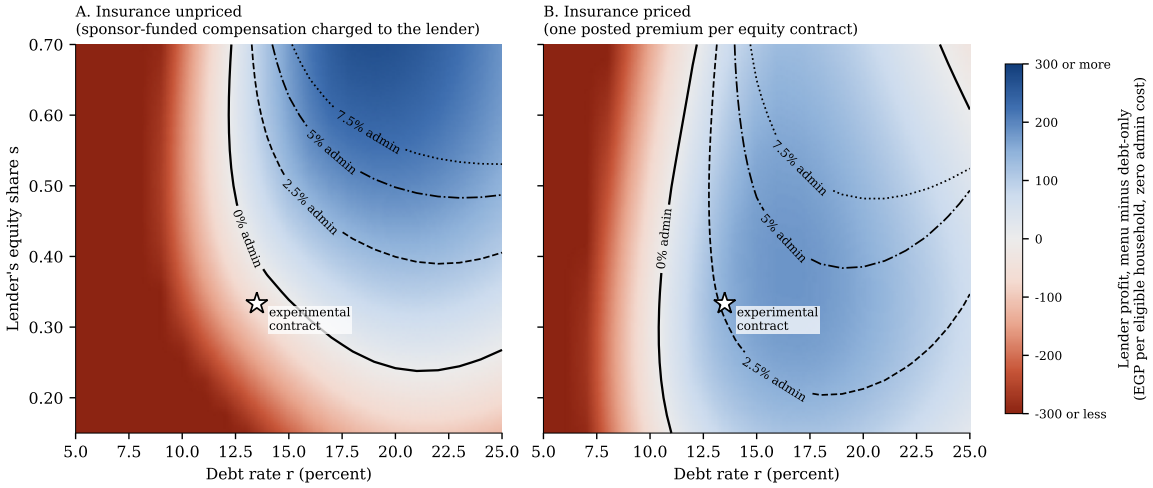
When the lender adds equity to its menu, two different mechanisms come into play. The first is inclusion. Equity-only households, who would otherwise remain unfinanced, take up the new contract; each is new business for the lender, and relatively safe business, since in our context we find that they are the better stewards. The second is cannibalization: the same offer lets indifferent households, who would have taken debt anyway, switch into equity. Cannibalization is costly in two ways. The lender earns more from a debt contract and now will make less from the debt borrower who switched to equity. And the switchers are precisely the households whose effort decreases most when incentives weaken, so the lender's overall equity portfolio becomes riskier. The contract terms affect both inclusion and cannibalization at once: a lower lender share draws in more of the equity-only population, and also shifts profitable debt borrowers into equity.

When the lender in our experiment offered both equity and debt (the *menu*), the project's sponsor covered the costs of the insurance. Setting insurance aside, the lender made money on every borrower group before administrative costs (Appendix Table A4, column 4). The rest of this section asks whether the lender can still make money once it has to pay for insurance itself.

Figure 2 maps the lender's profit from the menu, relative to debt-only lending at 13.5 percent, across contract terms (r, s). Panel A considers lender profits when they need to cover all the costs of insurance themselves. At the prices we used in the experiment the menu earns about 57 EGP *less* per census-eligible household relative to debt-only lending.

Holding debt at 13.5 percent, equity shares above about 0.4 beat debt alone, by up to 89 EGP near $s = 0.65$, but by then equity take-up has halved and the borrower gain has fallen from about 470 to 125 EGP. At the 0.33 share used in the experiment, the menu becomes profitable only if the lender also raises the interest rate, to about 15 percent, essentially subsidizing equity borrowers through higher costs for debt borrowers.

Figure 2: Lender Profitability of the Menu Across Contract Terms



Notes: Lender profit from offering the menu minus profit from debt-only lending at the experimental rate of 13.5%, per census-eligible household and before administrative costs; blue means the menu earns more. Black curves are break-even frontiers at administrative costs of 0, 2.5, 5, and 7.5% of principal (a missing frontier lies outside the plotted region). Panel A charges the sponsor-funded animal-death compensation to the lender. In Panel B the lender charges one premium on every contract, set to cover its expected payouts, which fall when better care means fewer deaths (Appendix C.3), borrowers bear 25% of a dead animal's value, and care responds through the moral-hazard channel; the debt-only comparator prices its own book the same way. Demand elasticities are -0.5 for equity among equity-only households and -1 otherwise. The star marks the experimental contract $(r_0, s_0) = (13.5\%, 1/3)$: -57 EGP in Panel A and $+147$ EGP in Panel B, where the lender offers the menu unaided at administrative costs up to 2.7%.

Insurance costs are the main reason why equity is not profitable. Expected payouts are 556 EGP per contract for equity-only takers and 1,571 EGP for indifferent takers. During the study the insurance claims committee ruled 96 percent of losses as "no-fault", so the sponsor absorbed nearly every loss. But in reality many of those losses reflect weaker care: the experiment shows debt borrowers have about half as many adverse events relative to equity-indifferent borrowers (Table 2, Panel B). Without a sponsor covering insurance, a lender would typically charge a premium and have borrowers bear a share of each loss, as is common in livestock insurance (World Bank, 2018).

Panel B considers how lender profit would change if the lender adopted a practical

livestock insurance product, charging an actuarially fair premium and covering 75% of the loss.[42] With something at stake when an animal dies, borrowers may take better care. We assume they respond through the moral-hazard channel we estimate from the devaluation (Appendix C.3); deaths then fall to 0.091 per equity contract, against 0.112 under the same premium with care held fixed. The premium and care response makes offering equity profitable for the lender relative to debt only at many price combinations. For example, at the prices used in the experiment (13.5% interest and a one third lender share) the menu earns 147 EGP more per census-eligible household than debt-only lending. It stays profitable across a wide range of equity shares. On the other hand, once administrative costs are counted, offering equity is only profitable up to admin costs of about 2.7 percent of principal. Typical administrative costs per loan are in the 7-9% range (Al Tadamun Microfinance Foundation, 2024).

6.4 The Welfare Case, with and without a Subsidy

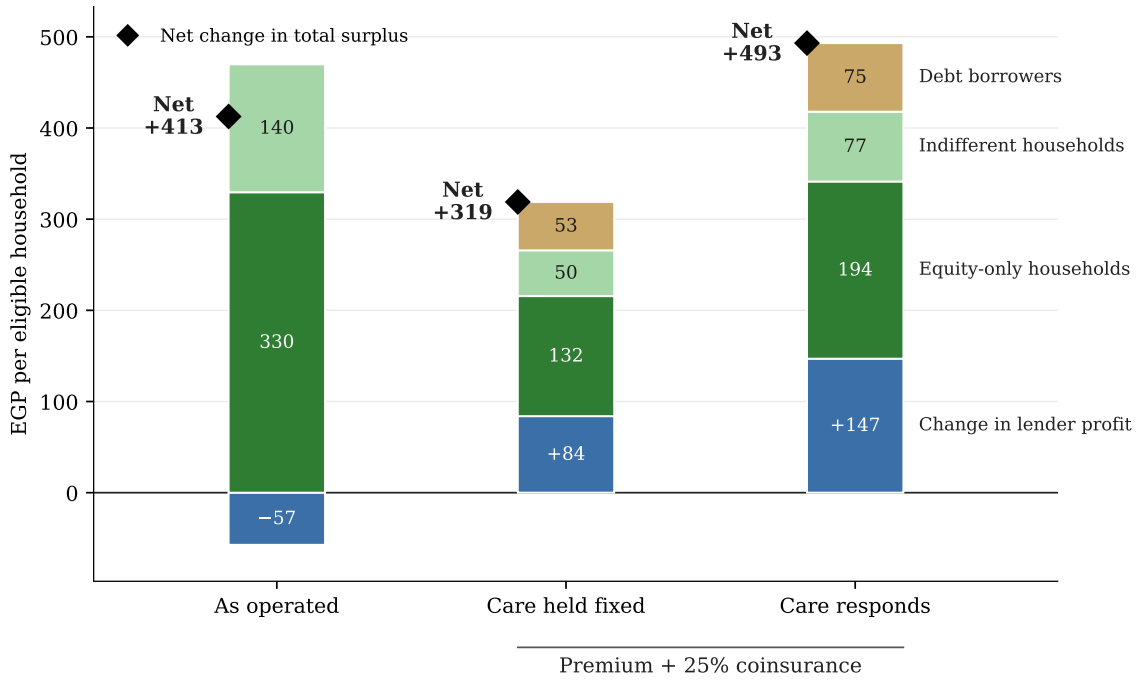
Even if offering equity is not profitable for lenders, it is possible that it can raise total surplus. In this subsection we ask how large the welfare gains from offering equity are, who receives them, and whether subsidizing a lender to offer it could be a good investment for a philanthropist/social planner.

Figure 3 shows where the gains come from under three versions of the program. In the first, the lender pays all insurance claims itself, as the sponsor did in the experiment. In the second, the lender charges an insurance premium and borrowers bear 25 percent of each loss, but borrowers' care does not change. In the third, our preferred scenario, borrowers respond to bearing part of each loss by taking better care, so fewer animals die. For each scenario, the bars split the change in total surplus into four parts: (i) the change in the lender's profit; (ii) the gain to equity-only households, who get access to finance; (iii) the gain to indifferent households, who get a choice of contract; and (iv) the gain to debt takers, who pay a lower premium because the menu's borrowers are safer on average. All values are before delivery costs, which Figure 4 will expand on below.

When the lender covers all insurance costs, borrowers benefit. Their gain is about 470 EGP per eligible household most of it going to equity-only households, whose alternative is no finance at all. But the lender earns about 57 EGP less than it would lending debt alone, so this arrangement is not sustainable without a sponsor. In the middle bar, the lender

[42] Co-insurance rates of 20-30% are common in livestock products and the Egyptian Government's Livestock Insurance Fund historically compensated 75% of an animal's value (Arab Republic of Egypt, 1959).

Figure 3: Who Gains under Alternative Insurance Arrangements?



Notes: Change in total surplus from offering the menu at the experimental terms, relative to debt-only lending at 13.5%, per census-eligible household and before delivery costs, split into the change in lender profit and the gains to equity-only households, indifferent households, and debt borrowers (who pay a lower premium on the menu). Diamonds mark the net change. As operated, the lender pays all insurance claims. In the other two bars the lender charges one premium on every contract and borrowers bear 25% of a dead animal's value, with care held fixed (middle) or responding (right). Deaths per equity contract are 0.115 as operated, 0.112 with care held fixed (the premium discourages the riskier indifferent households more), and 0.091 when care responds.

charges for insurance. Borrowers gain less, because they now pay a premium and bear part of each loss, but the menu becomes profitable for the lender before administrative costs, which makes this the more realistic benchmark. In the insurance scenarios, debt takers also benefit because equity-only borrowers are lower risk, which brings down insurance premiums for debt borrowers as well. In the final bar, our preferred specification, borrowers take better care of their animals because they are on the hook for part of each loss. Fewer animals die, and compared with the middle bar the gains rise for the lender and for every group of borrowers, taking the net gain to about 493 EGP per eligible household.

Even in our preferred specification, the lender only profits from offering equity if administrative costs are below about 2.7 percent of loan principal. Typical costs in this setting are closer to 7 to 9 percent (Al Tadamun Microfinance Foundation, 2024), so a

lender would not offer the product.

This raises a natural question: would it be worth it for a sponsor, such as a government or a foundation, to pay the lender to offer equity anyway?[43] Figure 4 shows when that's the case. The red line shows how much the sponsor would need to pay the lender so that offering equity is as profitable as offering debt alone. The subsidy rises with administrative costs because offering equity means making more loans, and each extra loan costs the lender more to deliver. The green line shows what borrowers gain from having equity available. Wherever the green line sits above the red one, borrowers gain more than the sponsor spends, and the subsidy is worth paying.

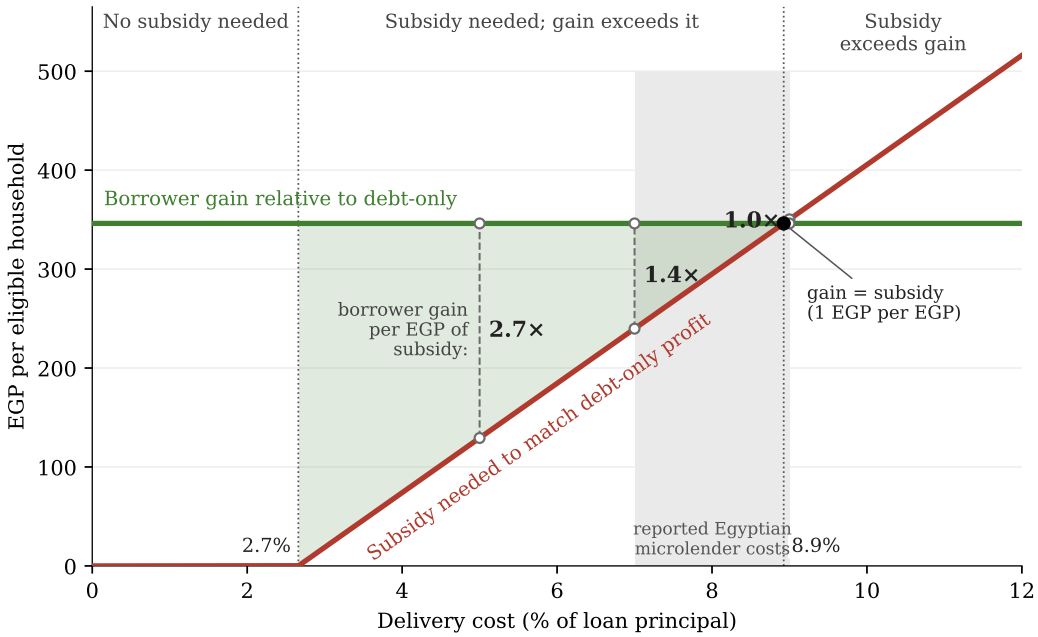
We find that across most realistic costs subsidizing equity is welfare enhancing. At an administrative cost of 7 percent of principal, each EGP a sponsor spends generates about 1.4 EGP of value for borrowers. For comparison, simply handing out cash returns at most 1 EGP per EGP spent (Hendren and Sprung-Keyser, 2020). The return shrinks as costs rise and is roughly one for one at 9 percent; beyond about 9 percent the subsidy costs more than borrowers gain (6.1 percent if care does not respond to coinsurance). Most of that value goes to equity-only households, who would otherwise receive no financing at all. Subsidizing equity is therefore a cost-effective way to reach households that debt leaves out, as long as delivery costs can be kept near the range we observe.[44]

Several considerations suggest these returns understate the case for equity. First, administrative cost estimates are averages, and the marginal cost of serving an additional client may be lower. Second, our welfare measure prices a single financing cycle: if a first cycle reveals ability and moves some households into larger and more productive financing relationships, as in Bryan et al. (2024), the return to including them exceeds what one cycle's surplus can show. Third, the value of insurance enters only through its effect on take-up, so the measure understates equity's worth if borrowers value downside protection beyond what their demand reveals (Appendix C). Fourth, households that never borrow can gain too: if financing puts more or better animals into local markets, buyers benefit through prices and quality. Finally, lower delivery costs would widen the case further, which may be easier to

[43] Several governments have subsidized equity through the tax code. Belgium's 2006 notional interest deduction let firms reduce taxable income in proportion to their equity, narrowing the corporate tax advantage of debt, and Meki (2023) finds that it raised affected firms' equity ratios. The aggregate stakes can be large. Peter (2026) estimates, in a model fit to Eurozone data, that equity-market frictions reduce output by much more than debt-market frictions do.

[44] The cutoff depends on how demand is extrapolated beyond the experimental terms. Replacing our demand curve with alternatives that match the same take-up and elasticities but fall to zero at a full lender share gives cutoffs of 8.8 percent (power demand) and 7.2 percent (bounded logit).

Figure 4: When Do Borrower Gains Justify Subsidizing Equity?



Notes: This figure reports returns to subsidies using our preferred specification (one premium on every contract, 25% coinsurance, care responding) per census-eligible household. The green line is the borrower gain from offering the menu relative to debt-only lending, including debt borrowers' gain from the lower premium. The red line is the subsidy the lender needs for the menu to match its debt-only profit. The subsidy rises with the delivery cost because the menu finances more households. The gain does not rise because prices are held at the experimental terms. Labels give the borrower gain per EGP of subsidy at delivery costs of 5, 7, and 9%. The gray band marks delivery costs reported by a large Egyptian microlender, 7.2 and 9.1% of principal in 2023 and 2024 (Al Tadamun Microfinance Foundation, 2024).

achieve in settings such as digital services, where financing can be sent electronically and monitoring is much simpler.

6.5 Limitations

The experiment shows how selection and incentives shape outcomes at the terms we offered. The counterfactuals extend that evidence to other prices and insurance arrangements, and their magnitudes should be read with a few assumptions in mind.

The magnitudes rest on two responses the experiment does not observe directly. Demand elasticities come from related experiments rather than our own, although the subsidy result is robust to how demand is extrapolated (footnote 44). The care response to coinsurance is inferred from the devaluation, which changed incentives through prices rather than insurance terms. Without it, the lender still gains from the menu but total surplus falls from 493 to

319 EGP, so the preferred results show what coinsurance could achieve if borrowers respond as they did to the devaluation.

Implementation could make equity less attractive than we find. We charge both contracts the same administrative cost, although verifying revenue likely makes equity more costly to run. We also treat the partner as the only formal lender; competitors offering cheaper debt would limit its ability to reprice and change the benchmark against which the subsidy is set.

The model is deliberately coarse about preferences. A single distaste for fixed obligations cannot separate the reasons households prefer equity, such as risk or loss aversion (Meki, 2025), so it cannot value contract designs we did not offer. Capped revenue sharing, which limits what the lender can collect (Cordaro et al., 2025), is a natural candidate for testing in settings like ours.

Finally, our estimates describe normal times. Equity's insurance is most valuable in a broad collapse in revenues, which our data do not contain and which would also hit a local lender's whole portfolio at once. Who can bear that risk, and whether the insurance is worth its cost, remain open questions.

7 Conclusion

We implemented a field experiment that separates selection from contract effects in microequity lending. Nearly half of eligible households would accept equity but refuse debt, and these equity-only households are *advantageously* selected: they suffer fewer bad animal outcomes than typical borrowers. At the same time, we find evidence of moral hazard. Among households willing to take either contract, those assigned equity see worse outcomes than those assigned debt. This gap closed when the devaluation strengthened equity borrowers' incentives.

These results also highlight the role of debt aversion. The households that equity brings into the market are held back by a dislike of fixed repayment obligations, not by poor prospects: they are, if anything, the more careful borrowers. The downside to equity for lenders comes from households that would have borrowed anyway. When they switch from debt to equity, they keep less of the return to their effort and take less care of their animals, which lowers lender profits. The challenge for lenders is therefore how to target equity, i.e., how to reach households held back by debt aversion without cannibalizing their debt business. The gains of offering equity also extend beyond equity takers. In our model, because equity-only borrowers are safer, adding them to the lender's portfolio lowers the cost of insuring its debt borrowers as well.

Our results suggest that equity financing can work for small firms in low- and middle-income countries. It reaches households that debt leaves out, and those households perform well once financed. Future work should identify the contexts and contract details that make microequity profitable for lenders and borrowers alike. One direction is finding better contexts. Equity is easier to deliver where monitoring is already cheap, such as where sales flow through digital platforms, cooperatives, or established buyers that record output and route payments. Another is incentive-compatible contract design. Equity contracts could pair revenue sharing with rewards for verifiable actions that protect a project's value: if borrowers earn back part of the lender's share by documenting such actions, they have a reason to report their own behavior truthfully, turning costly monitoring into self-monitoring and restoring some of the effort that revenue sharing weakens. Equity has long been central to how firms in high-income countries finance their growth. With the right design, it can do the same for the poor.

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Supplemental Appendix

A Appendix Figures and Tables

Figure A1: The Product-Explanation Video Shown Before Preference Elicitation

(a) The same loss, under equity (left) and under debt (right)



(b) The summary table that closes the video

أمثلة لأسعار البيع	إجمالي المكسب	التمويل العيني		المشاركة	
		المبلغ المدفوع للجمعية	مكسب العميل	المبلغ المدفوع للجمعية	مكسب العميل
٢٤,٠٠٠	٩,٠٠٠	١٧,٠٢٥	٦,٩٧٥	١٨,٠٠٠	٦,٠٠٠
٢١,٠٠٠	٦,٠٠٠	١٧,٠٢٥	٣,٩٧٥	١٧,٠٠٠	٤,٠٠٠
١٤,٠٠٠	١,٠٠٠–	١٧,٠٢٥	٣,٠٢٥–	١٤,٦٦٧	٦٦٧–

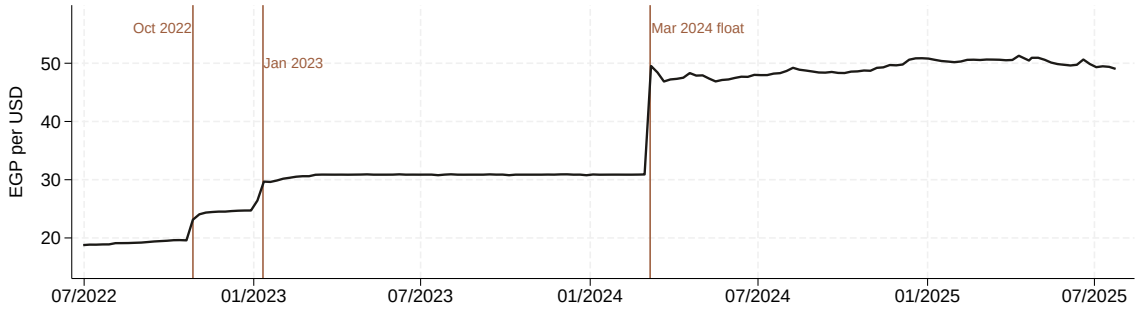
(c) The same table, translated

		Equity (*Musharaka*)		Debt (in-kind financing)	
Example sale price	Total profit	Paid to the association	Client profit	Paid to the association	Client profit
24,000	9,000	18,000	6,000	17,025	6,975
21,000	6,000	17,000	4,000	17,025	3,975
14,000	−1,000	14,667	−667	17,025	−3,025

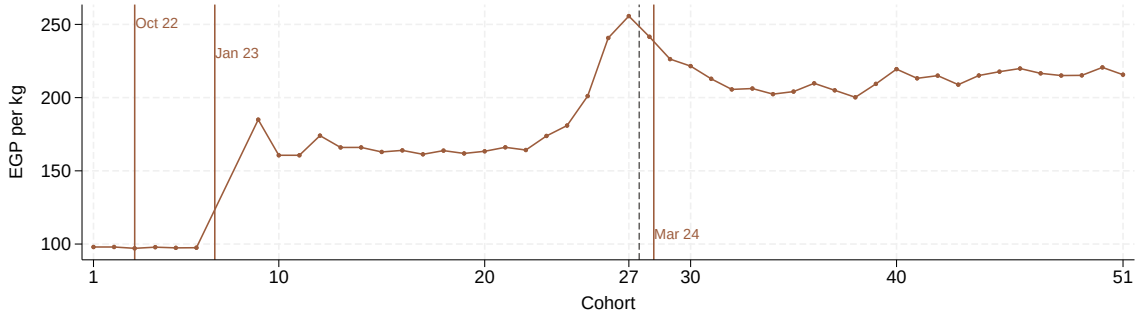
Notes: Applicants watched a short animated video explaining both products before stating which they were willing to accept (Section 2.2). Panel A takes the same bad outcome—animals bought with 15,000 EGP of financing and sold for 14,000—through each contract. The equity panel reads "You sold the animals at a loss. You share the loss. You return 14,667 EGP," against a sale of 14,000. The debt panel reads "It sold for 14,000 EGP. You are responsible for returning 17,025 EGP. You will bear 2,025 EGP of fee plus 1,000 EGP of loss." Panels B and C give the summary table that closes the video, in the original Arabic and in translation. Every number follows the contract terms: the borrower takes two-thirds of revenue net of principal and the association receives the principal plus or minus its third, while under debt the borrower owes $15{,}000 \times 1.135 = 17{,}025$ EGP whatever the sale price. Financing amounts moved across cohorts with animal prices (Section 2.1); later versions of the video work the same examples through a 25,000 EGP base.

Figure A2: The Devaluations and the Prices Our Borrowers Faced

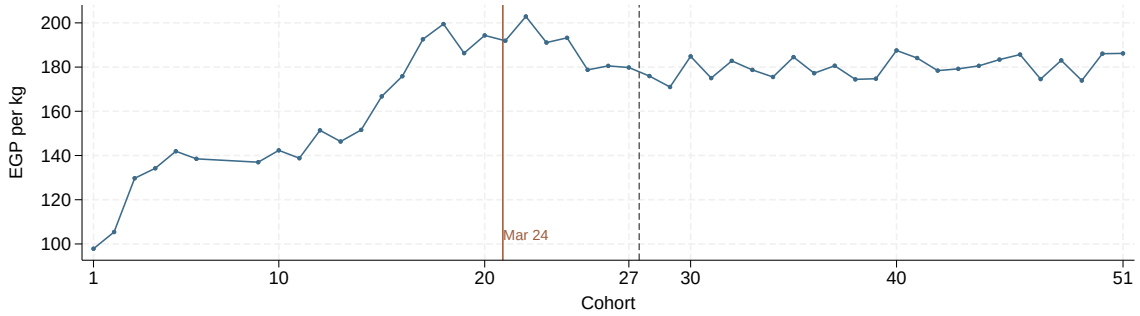
(a) Exchange rate, EGP per USD, weekly



(b) Purchase price per kg, by cohort (priced at disbursement)

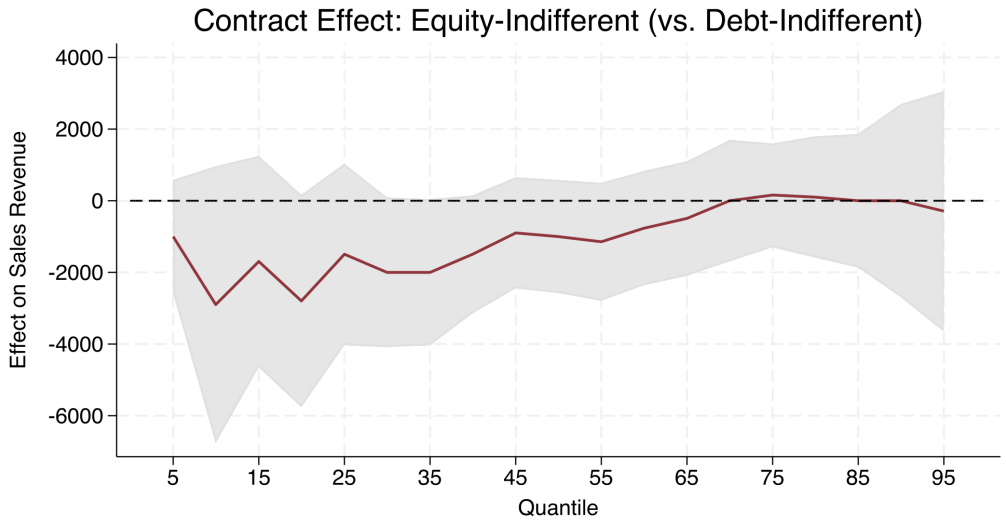


(c) Sale price per kg, by cohort (priced about six months later)

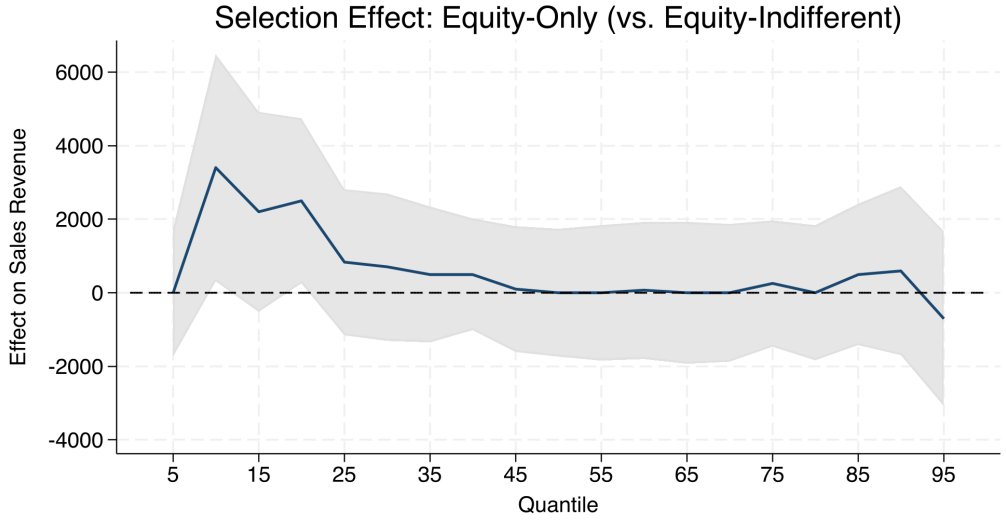


Notes: Solid vertical rules mark the three step devaluations; the dashed rule is the cohort 27/28 split. Panels (b) and (c) are indexed by cohort rather than by date, since the sample split is stated in cohort numbers. A cohort buys at disbursement and sells a mean of 175 days later, so a given devaluation lands at a different cohort in each panel. The March 2024 float reaches purchase prices at cohort 28 but sale prices back at cohort 21. Only the float lies inside the plotted range in (c), since no contract reached its sale until 26 December 2022 and by the January 2023 step only cohorts 1 and 2 had begun selling. Because the lender's share is taken at sale, that is the point at which a devaluation reaches an equity contract through realized revenue. Exchange rate is the weekly EGP/USD interbank quote (Yahoo Finance, series `EGP=X`), which tracks the official rate and sits flat at roughly 30.9 through the 2023 peg. Prices and contract dates are administrative; the sample is the 838 borrowers who took up across 49 cohorts.

Figure A3: Quantile Treatment Effects



Note: Shaded area represents 95% confidence interval.
Quantile regression estimates from 5th to 95th percentile in 5-point increments.
Sample restricted to contracts not exposed to devaluation.



Note: Shaded area represents 95% confidence interval.
Quantile regression estimates from 5th to 95th percentile in 5-point increments.
Sample restricted to contracts not exposed to devaluation.

Table A1: Balance of Baseline Characteristics by Treatment Arm

	Equity-Only		Indifferent		
	Control Mean (1)	Treated − Control (2)	Control Mean (3)	Treated (Equity) − Control (4)	Treated (Debt) − Control (5)
Panel A: Demographics					
Age	38.69	0.06 (0.66)	38.53	0.53 (0.91)	-0.30 (0.88)
Household Size	6.09	0.05 (0.16)	5.98	0.25 (0.22)	-0.27 (0.18)
Female	0.68	-0.04 (0.03)	0.67	-0.04 (0.04)	-0.02 (0.04)
Married	0.86	0.04** (0.02)	0.87	0.02 (0.03)	-0.01 (0.03)
Client is HH Head	0.38	0.01 (0.03)	0.37	0.04 (0.04)	0.04 (0.04)
Client Raises Livestock	0.42	-0.02 (0.03)	0.49	-0.02 (0.04)	-0.02 (0.04)
Education (Years)	7.24	0.18 (0.49)	6.29	0.09 (0.62)	0.27 (0.62)
Business Training	0.10	-0.01 (0.02)	0.10	-0.01 (0.02)	0.00 (0.02)
Muslim	0.98	-0.01 (0.01)	0.97	0.00 (0.01)	0.02** (0.01)
Panel B: Beliefs & Preferences					
Prefers Musharaka (Forced Choice)	1.00		0.80	0.00 (0.03)	-0.01 (0.03)
Beliefs: Goats per X EGP	4.54	0.11 (0.11)	4.60	0.01 (0.15)	0.19 (0.14)
Beliefs: Sheep per X EGP	3.18	-0.02 (0.05)	3.12	0.03 (0.07)	0.11 (0.07)
Ideal Amount: Musharaka	29,464.88	-631.96 (936.73)	26,743.49	1,533.73 (1,094.57)	1,660.27 (1,124.88)
Ideal Amount: Qard	25,888.43	-546.78 (791.58)	25,685.19	2,188.90** (1,098.25)	2,089.59** (1,024.99)
Hypothetical Sheep-to-Goat Ratio	0.92	0.00 (0.01)	0.91	0.01 (0.02)	-0.01 (0.02)
Panel C: Business & Financial Background					
Monthly Livestock Profit (HH)	427.90	74.64 (91.34)	462.18	16.31 (112.69)	68.43 (107.74)
Share of Income: Livestock	0.12	0.01 (0.02)	0.15	-0.01 (0.03)	0.02 (0.03)
Owns Goats or Sheep	0.19	-0.00 (0.03)	0.21	-0.04 (0.03)	-0.02 (0.03)
Owns Cows	0.17	0.03 (0.03)	0.22	-0.03 (0.03)	-0.01 (0.03)
Owns Other Livestock	0.12	0.02 (0.02)	0.17	-0.03 (0.03)	-0.01 (0.03)
Experience: Goats/Sheep (Yrs)	3.43	0.33 (0.60)	3.40	-0.22 (0.71)	-0.02 (0.68)
Previously Applied for Loans	0.35	0.02 (0.04)	0.34	-0.02 (0.05)	0.04 (0.05)
Currently Paying Loans	0.18	0.01 (0.03)	0.18	0.01 (0.03)	0.03 (0.03)
Total Income Last Month	3,679.35	94.56 (251.53)	3,629.40	484.59 (382.83)	-295.32 (327.86)
Prior Equity Experience	0.18	-0.02 (0.03)	0.15	0.01 (0.04)	0.07* (0.04)
Panel D: Behavioral & Psychological Measures					
Debt Stigma (0-10)	3.94	0.09 (0.29)	3.57	0.19 (0.37)	0.77** (0.37)
Debt Aversion (0-10)	6.40	0.21 (0.28)	5.96	0.36 (0.35)	0.21 (0.35)
Risk Tolerance (Qualitative)	6.29	-0.18 (0.24)	5.58	0.49 (0.31)	0.15 (0.30)
Risk Tolerance (Quantitative)	6.20	-0.54 (0.60)	6.64	-0.01 (0.80)	0.23 (0.81)
Patience (Qualitative)	7.14	0.03 (0.22)	6.75	0.42 (0.29)	0.19 (0.28)
Positive Reciprocity (Gift)	5.23	0.04 (0.08)	5.09	0.05 (0.12)	-0.04 (0.12)
Positive Reciprocity (Combined)	9.53	0.02 (0.10)	9.29	-0.01 (0.15)	0.15 (0.14)
Trust (0-10)	5.80	-0.38* (0.22)	5.18	0.12 (0.28)	0.32 (0.27)
Cognition (Digit Span Recall)	2.68	-0.05 (0.10)	2.39	0.01 (0.13)	0.14 (0.12)
GAD-2 (Anxiety) ≥ 3	0.26	-0.03 (0.03)	0.27	-0.03 (0.04)	-0.01 (0.04)
Stress (0-100)	33.43	0.72 (2.61)	33.67	-0.36 (3.29)	-0.45 (3.25)
Financial Worry (0-3)	1.03	0.03 (0.07)	1.05	0.01 (0.10)	-0.05 (0.10)
N		890		551	561
Joint F-test (Std. p-value)		0.832		0.171	0.100
Joint F-test (RI p-value)		0.833		0.955	0.327

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Columns (1) and (3) report control group means for the equity-only and indifferent samples, respectively. Columns (2), (4), and (5) report differences between the treated and control groups, estimated via OLS with strata fixed effects and robust standard errors in parentheses. N reports the number of treated and control individuals in each comparison. The standard joint F-test p-value is from a regression of the treatment indicator on all baseline variables (with missing values imputed at the mean and missing indicators included). The RI p-value is from a randomization inference procedure that permutes treatment assignment within strata (5,000 replications). Beliefs about animal quantities are elicited as "how many goats/sheep would X EGP buy at current market prices," where X was 10,000 EGP for cohorts 1–6, 15,000 EGP for cohorts 9–24, 18,000 EGP for cohorts 25–26, and 25,000 EGP for cohorts 27–51.

Table A2: Take-Up by Preference Group and Product

	(1) Equity-Indiff vs. Equity-Only	(2) Equity-Indiff vs. Debt-Indiff
	Selection Effect	Contract Effect
Equity-Indifferent	0.004	-0.038
	(0.038)	(0.032)
× Devaluation	-0.010	-0.061
	(0.056)	(0.050)
Equity-Only Mean (Non-Deval)	0.900	
Debt-Indiff Mean (Non-Deval)		0.957
N	680	570

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Dependent variable is take-up of assigned product. Column 1 compares equity-indifferent to equity-only (selection effect) with cohort FE. Column 2 compares equity-indifferent to debt-indifferent (contract effect) with strata FE. Devaluation indicates contracts exposed to an EGP devaluation episode. Robust standard errors in parentheses.

Table A3: Balance at Disbursement

	(1) Total Value at Disbursement	(2) Weight at Disbursement	(3) Value/KG at Disbursement
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)			
Equity-Only	-16.02	-0.23	-0.21
	(47.76)	(0.35)	(0.65)
Equity-Indiff Mean	17,262.20	95.37	181.71
N	572	572	572
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)			
Equity-Indifferent	-58.62	0.03	-0.54
	(48.18)	(0.31)	(0.58)
Debt-Indiff Mean	17,391.95	95.19	183.37
N	501	501	501

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Table A4: Division of Proceeds and Insurance Costs

	(1)	(2)	(3)	(4)	(5)
				Lender Margin	
	Net Revenue Sales Only	Divided Pool = (1) + Ins.	Borrower Share of (2)	Sponsor Pays Ins.	Lender Pays Ins.
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)					
Equity-Only	713	-501	-554	53	1,267***
	(696)	(553)	(422)	(180)	(453)
Equity-Only X Deval	-938	436	511	-76	-1,450***
	(874)	(699)	(516)	(226)	(544)
Non-Deval Mean	1,651	3,222	1,926	1,296	-275
Deval Mean	5,767	6,555	4,313	2,242	1,454
N	572	572	572	572	572
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-1,423**	-568	919**	-1,487***	-2,342***
	(657)	(505)	(441)	(131)	(411)
Equity-Indiff X Deval	1,576*	353	-1,293**	1,645***	2,869***
	(839)	(650)	(563)	(188)	(498)
Non-Deval Mean	2,956	3,705	908	2,797	2,049
Deval Mean	5,166	6,386	4,361	2,026	806
N	501	501	501	501	501

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors in parentheses. Both panels estimate on all takers and interact each comparison with the devaluation-period indicator. Panel A includes cohort fixed effects; Panel B includes strata fixed effects. The dependent variable means are for the comparison group in each panel (equity-indifferent takers in Panel A, debt-indifferent takers in Panel B), outside and inside the devaluation period. Column (1) is Table 3's net revenue: total sale value minus acquisition principal. When an animal died through no fault of the borrower, the insurance paid the borrower the animal's value, and most forced sales were compensated too; column (2) adds those payments to column (1) and is the pool the contract divides. Column (3) is the borrower's contractual share of that pool (also the share reflected in Table 5, column (1)). Column (4) would be the lender's share of the pool when the insurance payments are fully subsidized by the sponsor (these payments ultimately averaged 556 EGP per equity-only contract, 1,571 per equity-indifferent, and 749 per debt, among borrowers in the primary, non-devaluation period). Column (5) is the lender's share of the pool after subtracting the insurance payments (i.e., the lender's share when insurance is fully unsubsidized by the sponsor) – this is the lender margin that is used in the structural model in Section 6. Because Panel A includes cohort fixed effects, its Equity-Only coefficients are within-cohort contrasts rather than differences in means. The structural model uses each group's mean outside the devaluation period, 727 EGP for equity-only and −275 EGP for equity-indifferent takers in column (5). All entries are contractual entitlements evaluated at realized sales; they do not net out repayment shortfalls, which enter the structural model separately as default losses. The Panel B effect on the margin actually repaid is −2,188 (SE 468), close to column (5)'s −2,342, so repayment behavior is not driving the lender gap.

Table A5: Moral Hazard Mitigation (Monitoring)

	Bad Outcomes		Good Outcomes	
	(1) Any Delinq. 30+ Days	(2) Total Deaths or Forced Sales	(3) Total Value at Sale	(4) Value/KG at Sale
Panel A: Monitoring for Equity				
Monitoring	-0.01	-0.04	157.04	1.76
	(0.02)	(0.07)	(659.36)	(2.11)
Monitoring X Deval	0.05*	-0.00	-340.17	-0.72
	(0.03)	(0.09)	(844.82)	(3.02)
No Monitor Mean	0.03	0.26	21,654.90	174.48
N	558	558	558	558
Panel B: Placebo (Monitoring for Debt)				
Monitoring	-0.03	-0.01	-687.70	-3.57
	(0.05)	(0.08)	(910.87)	(2.83)
Monitoring X Deval	0.07	0.09	26.85	0.31
	(0.06)	(0.12)	(1209.42)	(4.56)
No Monitor Mean	0.07	0.22	22,201.06	174.91
N	257	257	257	257

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Devaluation indicator equals 1 if the contract could have spanned (or fallen between) either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). Column 1 counts the defaulters as delinquent, as in Table 2. All regressions include strata fixed effects. Robust standard errors in parentheses.

Table A6: Psychological Outcomes

	(1) Stress Level (0 to 100)	(2) GAD-2 Score	(3) GAD-2 Above Threshold	(4) Financial Worry (0-3)
Panel A: Equity Only				
Equity	7.21*	0.32*	0.05	0.23*
	(4.20)	(0.17)	(0.05)	(0.13)
Equity X Deval	-0.70	-0.10	0.03	-0.16
	(5.44)	(0.21)	(0.06)	(0.16)
Control Mean	45.67	1.855	0.316	1.447
N	890	890	890	890
Panel B: Indifferent				
Equity	0.85	0.06	0.05	0.05
	(5.45)	(0.23)	(0.07)	(0.16)
Debt	3.14	0.19	0.09	0.16
	(5.38)	(0.23)	(0.07)	(0.16)
Equity X Deval	1.23	0.03	-0.03	0.22
	(6.95)	(0.29)	(0.08)	(0.20)
Debt X Deval	-1.76	-0.26	-0.10	0.12
	(6.84)	(0.29)	(0.08)	(0.20)
Control Mean	48.37	2.055	0.347	1.395
N	841	841	841	841
Selection Effect (Equity-Only - Equity-Indif)	0.39	0.41	0.95	0.60
Contract Effect (Equity-Indif - Debt-Indif)	0.66	0.55	0.52	0.52

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Devaluation indicator equals 1 if the contract could have spanned (or fallen between) either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). Main treatment coefficients show effects when devaluation=0; interaction coefficients show additional effects during devaluation periods. All regressions include strata fixed effects and control for the baseline value of the outcome (missing values imputed, with a missing indicator). Selection effect regression includes cohort fixed effects. Robust standard errors in parentheses.

Table A7: Sensitivity to the Devaluation-Period Cutoff

	Non-devaluation sample defined as			
	Cohort ≥ 26	Cohort ≥ 27	Cohort ≥ 28	Cohort ≥ 29
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)				
Animal Deaths	-0.107**	-0.108**	-0.111**	-0.121**
	(0.051)	(0.052)	(0.052)	(0.054)
Forced Sales	-0.101*	-0.121**	-0.118**	-0.056
	(0.052)	(0.053)	(0.053)	(0.044)
30+ Day Delinquency	-0.005	0.007	0.026	0.018
	(0.026)	(0.022)	(0.020)	(0.020)
Default	0.007	0.020	0.014	0.011
	(0.019)	(0.012)	(0.011)	(0.011)
Net Revenue	857	880	713	362
	(659)	(680)	(705)	(708)
Bank Revenue Share	107	81	53	63
	(172)	(177)	(183)	(191)
Observations	250	235	225	210
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)				
Animal Deaths	0.003	0.020	0.038	0.040
	(0.055)	(0.054)	(0.052)	(0.054)
Forced Sales	0.136***	0.141***	0.134**	0.088*
	(0.051)	(0.053)	(0.054)	(0.051)
30+ Day Delinquency	-0.076***	-0.069***	-0.071***	-0.059**
	(0.028)	(0.026)	(0.027)	(0.026)
Default	-0.056**	-0.063***	-0.058***	-0.060***
	(0.025)	(0.022)	(0.022)	(0.022)
Net Revenue	-1,268**	-1,413**	-1,423**	-1,175*
	(634)	(645)	(663)	(672)
Bank Revenue Share	-1,640***	-1,608***	-1,487***	-1,435***
	(135)	(138)	(133)	(136)
Observations	226	218	210	202

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Each cell is a separate regression, identical to the corresponding column of Tables 2 and 3 except for the sample cut; the Bank Revenue Share row applies the same specification to the lender's share in column 4 of Table A4. Column 3 (cohort ≥ 28) is the definition used throughout the paper. Cohort 29 is the first group disbursed entirely after the March 2024 float; cohorts 27 and 28 straddle it. Panel A compares equity-only with equity-indifferent borrowers, both financed on equity, with cohort fixed effects. Panel B compares equity-indifferent with debt-indifferent borrowers with strata fixed effects. Samples are restricted to borrowers who took up financing. Robust standard errors in parentheses.

Table A8: Delinquency Robustness: Different Thresholds

	(1) Delinq. 1+ Days	(2) Delinq. 14+ Days	(3) Delinq. 30+ Days	(4) Delinq. 60+ Days	(5) Delinq. 90+ Days	(6) Days Delinquent
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)						
Equity-Only	0.15***	0.10***	0.03	0.02	0.03*	4.49**
	(0.06)	(0.04)	(0.02)	(0.02)	(0.01)	(1.85)
Equity-Indiff Mean	0.17	0.03	0.01	0.01	0.00	1.63
N	225	225	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)						
Equity-Indifferent	-0.06	-0.06**	-0.07***	-0.07***	-0.07***	-8.50***
	(0.05)	(0.03)	(0.03)	(0.03)	(0.02)	(2.94)
Debt-Indiff Mean	0.25	0.11	0.10	0.09	0.08	11.41
N	210	210	210	210	210	210

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to non-devaluation cohorts (cohort ≥ 28). Columns 1-5 are binary indicators for being delinquent at different thresholds. Borrowers who never reached 95% of the death-adjusted obligation (the defaulters) are counted as delinquent at every threshold. Column 6 is the number of days delinquent (0 for on-time or early payments). Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Table A9: Default Robustness: Different Thresholds

	(1) Default ≥ 1%	(2) Default ≥ 5%	(3) Default ≥ 10%	(4) Default ≥ 20%	(5) Default Percent
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)					
Equity-Only	0.05*	0.01	0.00	0.00	0.21
	(0.02)	(0.01)	(0.00)	—	(0.14)
Equity-Indiff Mean	0.01	0.00	0.00	0.00	0.05
N	225	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-0.05**	-0.06***	-0.04**	-0.01	-0.74**
	(0.02)	(0.02)	(0.02)	(0.01)	(0.32)
Debt-Indiff Mean	0.06	0.06	0.05	0.02	0.90
N	210	210	210	210	210

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample restricted to non-devaluation cohorts (cohort ≥ 28). Default ≥ 1% = paid less than 99% of death-adjusted obligation. Columns 2-4 use progressively higher thresholds for the share of obligation left unpaid. Column 5 is the continuous percent of obligation unpaid (0 for non-defaulters). No borrower in either arm left 25 percent or more of the death-adjusted obligation unpaid. The two borrowers who left 20 percent or more are both in the debt arm, so the column 4 estimate in Panel A is mechanically zero rather than estimated and no standard error is defined. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Table A10: Input Cost Decomposition During the Devaluation

	(1) Total Costs	(2) Feed	(3) Supplies	(4) Medication	(5) Grazing Hours
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)					
Equity-Only	10.28	-9.33	41.14	-28.04	4.64**
	(109.78)	(48.16)	(97.14)	(30.05)	(2.08)
Equity-Only X Deval	80.89	-1.93	-69.29	-15.52	-4.82*
	(161.52)	(79.99)	(140.73)	(72.95)	(2.70)
Equity-Indiff Mean (Non-Deval)	1,391.67	690.10	463.54	172.41	29.04
Equity-Indiff Mean (Deval)	1,323.55	809.85	607.03	246.67	31.99
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-19.74	-88.96	85.70	10.13	-1.16
	(107.76)	(56.44)	(77.66)	(28.73)	(2.11)
Equity-Indiff X Deval	-109.09	132.88	-147.94	-10.46	2.04
	(151.80)	(83.63)	(131.72)	(87.32)	(2.67)
Debt-Indiff Mean (Non-Deval)	1,403.71	763.82	395.50	162.33	30.33
Debt-Indiff Mean (Deval)	1,439.44	767.70	632.34	219.29	31.27
N	436	486	442	226	500

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Devaluation indicator equals 1 if the contract could have spanned (or fallen between) either the 2023 or 2024 devaluation episodes (cohorts 27 and earlier). Columns are defined as in Table 4, and column 1 is the cost measure reported in the main devaluation table. Monetary measures are winsorized at the top 1 percent. A borrower who was not asked a question, or did not answer it, is left out of that column. Medication (column 4) was added to the survey in August 2024, so it is missing for nearly all borrowers before cohort 26, and columns 1 and 3 were not asked of most of cohorts 1 to 6, whose feed (column 2) comes from a per-animal question instead. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Table A11: Loan Officer Robustness: Problematic Loan Officer

	Drop None		Drop ≥30 (Baseline)		Drop ≥24		Drop All	
	(1) Deaths/ Forced	(2) Default	(3) Deaths/ Forced	(4) Default	(5) Deaths/ Forced	(6) Default	(7) Deaths/ Forced	(8) Default
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)								
Equity-Only	-0.20***	0.01	-0.23***	0.01	-0.22***	0.01	-0.22***	0.01
	(0.07)	(0.02)	(0.07)	(0.01)	(0.07)	(0.01)	(0.07)	(0.01)
Equity-Indiff Mean	0.34	0.02	0.34	0.00	0.34	0.00	0.34	0.00
N	270	270	225	225	223	223	223	223
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)								
Equity-Indifferent	0.15**	-0.09***	0.17**	-0.06***	0.16**	-0.06***	0.16**	-0.06***
	(0.07)	(0.03)	(0.07)	(0.02)	(0.07)	(0.02)	(0.07)	(0.02)
Debt-Indiff Mean	0.19	0.12	0.17	0.06	0.17	0.06	0.17	0.06
N	249	249	210	210	207	207	207	207
Panel C: Selection Effect, Devaluation Interaction (Full Sample)								
Equity-Only	-0.20***	0.01	-0.23***	0.01	-0.22***	0.01	-0.22***	0.01
	(0.07)	(0.02)	(0.07)	(0.01)	(0.07)	(0.01)	(0.07)	(0.01)
× Devaluation	0.23***	-0.01	0.26***	-0.01	0.27***	-0.01	0.27***	-0.01
	(0.09)	(0.02)	(0.09)	(0.02)	(0.09)	(0.01)	(0.09)	(0.01)
N	617	617	572	572	544	544	518	518
Panel D: Contract Effect, Devaluation Interaction (Full Sample)								
Equity-Indifferent	0.15**	-0.09***	0.17**	-0.06***	0.16**	-0.06***	0.16**	-0.06***
	(0.07)	(0.03)	(0.07)	(0.02)	(0.07)	(0.02)	(0.07)	(0.02)
× Devaluation	-0.22**	0.08***	-0.24***	0.05**	-0.22**	0.06***	-0.22**	0.06***
	(0.09)	(0.03)	(0.09)	(0.02)	(0.09)	(0.02)	(0.09)	(0.02)
N	540	540	501	501	477	477	471	471

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Deaths/Forced Sales combines the outcomes from Table 2 Columns 1 and 2. This table examines robustness to different approaches for handling a problematic loan officer. Drop None includes all observations. Drop ≥30 excludes this officer's clients from cohort 30 onward (first serious fraud case, baseline). Drop ≥24 excludes his clients from cohort 24 onward (first complaint). Drop All excludes all observations from this officer. Panels A and B restrict to non-devaluation cohorts, where the three dropping rules differ from one another by at most three observations. Panels C and D use the full sample and add the devaluation interaction, which is where the retained clients of this officer are concentrated: he accounts for 52 percent of cohorts 24–27. Panels A and C include cohort FE. Panels B and D include strata FE. Robust standard errors in parentheses.

Table A12: Census Intake Funnel

	Number or Percent Surviving							
	Full		Muslim		Christian		Not Recorded	
Condition	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Census Sample	11,416	100.0%	3,788	100.0%	2,269	100.0%	5,359	100.0%
Livestock Experience	4,042	35.4%	1,068	28.2%	480	21.2%	2,494	46.5%
Interested in Opportunity	484	12.0%	237	22.2%	26	5.4%	221	8.9%
Decision Maker	310	64.0%	233	98.3%	25	96.2%	52	23.5%
Enrolled	213	68.7%	176	75.5%	22	88.0%	15	28.8%
Eligible (Any Product)	196	92.0%	163	92.6%	20	90.9%	13	86.7%
Eligible (Equity or Indifferent)	145	74.0%	124	76.1%	13	65.0%	8	61.5%

Notes: Percentages represent conditional survival rates from the previous row. *Interested in Opportunity* based on the question: *Would you be interested in hearing about an opportunity that would allow you to expand your flock?*. Religion was recorded by the enumerator without asking the respondent. In the 2024 round of the census the enumerator made an entry for every household. In the 2022 round the question followed the consent question and was first used on 30 October 2022; for 33 households interviewed earlier, religion was inferred from the name. *Not Recorded* covers the other 2022 households and those whose religion could not be classified.

Table A13: Coverage of the Income Probe on the Sale Proceeds, by Sample

	(1) Non-devaluation cohorts	(2) Devaluation cohorts	(3) All takers
Takers	336	502	838
Asked whether the figure included the sale proceeds	326 (97.0)	42 (8.4)	368 (43.9)
Said it did not	291 (89.3)	41 (97.6)	332 (90.2)
Said it did	35 (10.7)	1 (2.4)	36 (9.8)
Not asked	10 (3.0)	460 (91.6)	470 (56.1)
Surveyed before the question was added (9 August 2024)	0 (0.0)	459 (91.4)	459 (54.8)
No income figure to ask about	9 (2.7)	1 (0.2)	10 (1.2)
Other	1 (0.3)	0 (0.0)	1 (0.1)

Notes: Sample is the financed borrowers (takers) in the analysis sample: the non-devaluation cohorts (cohort 28 onward, the paper's primary sample; column 1), the devaluation-period cohorts (column 2), and both together (column 3). From 9 August 2024 the 7-month survey asked, immediately after the last-month income question, whether the figure the respondent had just reported included the proceeds from selling the animals received from ABWA. The question was asked about the respondent's own income and, where the respondent reported no income source, about the main earner's income; a taker counts as asked if either was answered. Percentages in parentheses are of the column's takers, except in the two answer rows, where they are of the takers asked. Table 5, column 1, adds the borrower's computed share of the sale to the survey figure for takers who said it did not include the proceeds and for takers who were not asked, and leaves the figure as reported for takers who said it did.

B Adverse Events and Early Exits

Three kinds of events interrupt the standard production cycle: the death of a financed animal, a forced early sale of a sick animal, and early exit from the contract. Among the 572 financed equity contracts, 66 (roughly 12 percent) experienced at least one death, 54 (9 percent) a forced sale, and 58 (10 percent) an early exit; 403 contracts (70 percent) experienced none of the three. The events rarely compound. Nine contracts record two event types, and none records all three.[45] Early exit is not one of the adverse outcomes in Table 2 since ending the contract early is the borrower's decision, and so what drives an exit is more ambiguous than what drives a death or a forced sale (moreover, the debt arm has no counterpart against which to compare it).

The three events also differ in timing. Contracts that ran to an ordinary sale cluster tightly around the intended six-month cycle (median 178 days from disbursement to sale among the 514 non-exiting contracts). Early exits are resolved at a median of 116.5 days, and 8 of the 58 occurred before day 90, so an exit typically cuts the fattening period short by about two months.

Each event is settled under a different contractual provision. Deaths trigger the embedded insurance, under which the lender compensates the purchase cost of the dead animal; the incidence of this insurance across borrowers, the lender, and the sponsor is reported in Table A4. Forced sales are sales at unfavorable timing whose proceeds enter the divided pool, and most were also compensated (42 of the 51 equity contracts with a forced sale and no death received a payment). Early exits are settled under a floor provision: a borrower who ends the contract early owes the greater of one third of net proceeds and a flat charge equal to the debt contract's 13.5 percent fee. The floor prices the option to leave. Without it a borrower could end the contract while the animals were still light and pay a third of a small pool, so the exit option would open a second selection margin on top of the contract choice the experiment studies. Borrowers who carry the contract to term face the equity terms in full, including the animal-death insurance.

[45] This appendix describes all financed equity contracts. The paper's main tables restrict to the non-devaluation cohorts, and similar patterns hold there. Among those 225 contracts, 24 (11 percent) had a death, 19 (8 percent) a forced sale, and 30 (13 percent) an early exit; 156 (69 percent) had none of the three, four had two, and none had all three. Median duration is 178 days for contracts running to an ordinary sale and 110 days for exits, 6 of which came before day 90. The floor bound in 29 of the 30 exits: in 17 the proceeds fell short of principal plus the charge, by about 1,900 EGP on average, and in 12 they covered it and left the borrower about 1,900 EGP. Twenty-nine of the 30 settled in full, and the floor collected 57,680 EGP more than a pure one-third rule would have. Exit rates are similar across preference groups, at 17 of 126 equity-only takers and 13 of 99 indifferent takers.

Because an early sale cuts the fattening period short, pools are small, and in 56 of the 58 exits one third of net proceeds fell short of the flat charge, so the lender collected the charge. The claim was the same in all 56, but the sales left borrowers in different positions. In 37 of them the proceeds covered principal and the charge and left the borrower about 2,200 EGP on average. In the other 19 the animals sold for less than principal plus the charge, so those borrowers covered a shortfall averaging roughly 1,800 EGP from other resources, and 18 of the 19 settled in full. Only 2 exits produced a pool whose third exceeded the charge, and there the ordinary division applied. Relative to a pure one-third rule, the floor collected an additional 77,822 EGP across the exiting contracts. Exit is not concentrated in either preference group (35 of the 337 equity-only takers and 23 of the 235 indifferent takers). The delinquency measures in Table A8 treat these cases under the same amount-based rules as ordinary completions.

C Structural Model Details

This appendix documents the model behind Section 6: its primitives, borrower behavior, the insurance arrangements behind Figures 2–4, and welfare, then the parameters set before estimation, estimation and identification, fit and out-of-sample validation, the counterfactual results, and robustness. The model takes take-up, lender margins, insurance costs, and default rates directly from the experiment and estimates four behavioral parameters from it. It provides us with counterfactuals regarding alternative prices and insurance arrangements the experiment did not run.

C.1 Primitives

Types and contracts. The eligible population consists of three types, $\theta \in \{\text{EO}, \text{IND}, \text{DO}\}$ (equity-only, indifferent, and debt-only households), with shares $\omega_\theta = (0.44, 0.30, 0.26)$ from the census demand elicitation. Every financed household receives principal $P =$ 20,796 EGP. A debt contract charges interest r per cycle, while an equity contract gives the lender a share s of the divided pool, which is sale revenue net of principal plus any insurance payment. The experimental terms are $(r_0, s_0) = (13.5\%, 1/3)$. All amounts are in EGP per census-eligible household per cycle, and all headline figures are for the normal macroeconomic state.

Lender margins. The lender's per-loan margins are

$$\mu_D(r) = \bar{\mu}_D + P\,(r - r_0) - \ell_D, \qquad \mu_{E,g}(s) = \bar{\mu}_{E,g} + c_g(s) - c_g(s_0) - \kappa(s) - \ell_E, \quad \text{(C1)}$$

where $\bar{\mu}_D$ is the observed debt margin, $\bar{\mu}_{E,g}$ is group g's observed equity margin at the experimental terms, $c_g(s)$ is the group's expected contractual claim at share s, which is not proportional to s because early exits settle at the contract's floor, and $\kappa(s)$ is the moral-hazard cost defined in Section C.2. The loss terms ℓ_D and ℓ_E are expected default losses, and ℓ_E also includes the change in compensation when care changes the number of deaths and forced sales. Compensation per adverse outcome is held at each group's observed level, which covers forced sales as well as deaths, and, as with the compensation observed in the experiment, the payment enters the divided pool, so the lender's net cost is $(1 - \phi)(1 - s)$ of it (Section C.3); the sale proceeds those animals would have earned are lost through $\kappa(s)$. The observed margins are net of realized animal losses but gross of default losses, which enter separately, so no loss is counted twice. Premium and coinsurance terms enter as described in Section C.3. The lender's profit sums these group margins over the model's take-up, which follows the census shares.

C.2 Behavior

Contract choice and demand. Households choose among debt, equity, and no finance using the payoffs of Section 6.1, plus a taste shock on each payoff difference that follows a logistic distribution, so choice probabilities are logit. Heterogeneity within a type runs along two dimensions, debt aversion d and stewardship σ, which are independently normally distributed around the type's mean, and we integrate over each with three-point Gauss–Hermite quadrature. For indifferent households, equity take-up is the probability of preferring equity to debt times the probability of borrowing at all. The price of equity is each group's expected contractual payment, including any premium and the borrower's expected share of losses under coinsurance, relative to its value at the experimental terms. The price of debt is the interest rate plus the same two charges, expressed as a share of principal. The choice intercepts are chosen so that the model reproduces observed take-up at the experimental terms exactly, and the price coefficients so that own-price elasticities there equal -1 for debt and for equity among indifferent households (Gazeaud et al., 2026), and -0.5 for equity among equity-only households (Karlan et al., 2021).

Stewardship and moral hazard. Stewardship σ lowers a household's baseline rate of adverse outcomes (animal deaths and forced sales), with protective effect δ. Moral-hazard pressure scales that baseline rate proportionally, so careful households also respond less when incentives weaken. In normal times the pressure index is linear in the lender's share,

$$m(s) = 1 + \frac{s - s_0}{1 - s_0}, \tag{C2}$$

so $m(s_0) = 1$ and $m(1) = 2$. The devaluation lowers the index by one unit, to $m = 0$ at the experimental share.

Adverse outcomes rise with pressure at a slope λ, estimated in Section C.6, scaled by each household's baseline risk relative to the indifferent average. Sale values fall with pressure by the same amount for every borrower, set so that for indifferent borrowers the ratio of the sale-value and adverse-outcome responses matches the two devaluation interactions in the data (Table 7, Panel B). In money, λ implies a loss of λ_v of venture surplus per unit of pressure, so that

$$v(s) = v_0 - \lambda_v\big(m(s) - m(s_0)\big). \tag{C3}$$

The lender bears its share of the lost surplus, $\kappa(s) = s\,\lambda_v\big(m(s) - m(s_0)\big)$, while the borrower bears the rest through her own share of a smaller pool.

C.3 Insurance Arrangements

As operated. In the experiment the sponsor paid compensation for animals that died of non-negligent causes. In the as-operated counterfactual the lender pays it instead. On an equity contract the compensation enters the divided pool, so the lender recovers its share s of it and its net cost is $(1 - s)$ of the payment. On a debt contract the lender forgives the dead animal's share of principal. Expected compensation per equity contract is 556 EGP for equity-only takers and 1,571 EGP for indifferent takers, and expected forgiven principal per debt contract is 874 EGP, the observed death rate among indifferent debt borrowers (12.6 percent) times one animal's share of principal.

Premium and coinsurance. In the coinsurance design we suggest, borrowers bear a share $\phi = 0.25$ of each loss. On an equity contract, cutting a dead animal's compensation by ϕ shrinks the divided pool, so the borrower bears $\phi(1-s)$ of the compensation and the lender's net cost falls from $(1-s)$ to $(1-\phi)(1-s)$. On a debt contract, the borrower repays ϕ of the dead animal's principal. The lender charges one premium ρ on every contract in the menu,

set so that premiums cover the expected compensation it still pays across its portfolio,

$$\rho = \frac{M_E\, T_E + M_D\,(1-\phi)\, C_D}{M_E + M_D}, \qquad T_E = \eta\,(1-\phi)(1-s)\,\bar{C}(e), \tag{C4}$$

where M_E and M_D are the masses of equity and debt takers, $\bar{C}(e)$ is expected compensation per equity contract when equity-only households make up a share e of the equity pool, and C_D is the expected dead-animal principal per debt contract. The factor η is the ratio of deaths per equity contract with care responding to deaths with care held fixed, so the premium reflects the fall in deaths from the care response. Compensation per equity contract is otherwise held at each group's observed level, and the lender's margin applies the same rule to the deaths and forced sales that care adds or removes, so at the fixed point the premium covers the compensation the lender pays. Because the premium changes who borrows, and hence e and deaths, we solve for ρ as a fixed point. The debt-only comparator faces the same coinsurance and charges its own fair premium, $(1-\phi)\, C_D = 656$ EGP.

Care response. Bearing part of each loss restores some of the incentive that revenue sharing weakens. We map coinsurance onto the moral-hazard index: a borrower who bears a share ϕ of each loss faces ϕ fewer units of pressure. Full coinsurance would therefore restore incentives by as much as the devaluation did, and $\phi = 0.25$ lowers the index by a quarter of that change. The mapping is an assumption rather than an estimate, since the devaluation changed incentives through prices, not insurance terms. Through λ, it lowers adverse outcomes and raises sale proceeds.

C.4 Welfare

The equity option. We measure the borrower's gain from the equity option by the compensating variation of withdrawing it: the payment that would leave households as well off without equity as with it. We denote by $Q^E(s, \rho_E) = \sum_\theta \omega_\theta\, q^E_\theta(r_0, s; \rho_E)$ equity take-up at lender share s when equity contracts carry premium ρ_E, holding the interest rate at r_0 and the premium on debt contracts at ρ. In the menu both contracts carry the same premium, $\rho_E = \rho$. We compute the gain as the area under the equity demand curve as the lender's share rises from s_0 to one, without a premium, less the cost of the premium at the experimental share,

$$\mathrm{CV}_E = \int_{s_0}^{1} Q^E(s, 0)\, p(s)\, \mathrm{d}s \;-\; \int_0^{\rho} Q^E(s_0, \rho_E)\, \mathrm{d}\rho_E, \tag{C5}$$

where the second term is zero as operated, since there is no premium. Some demand remains at a full lender share, so the robustness checks below also use demand curves that fall to zero there. The lender's gain and the case for a subsidy hold under both, but under one of them the borrower gain is nearly 40 percent smaller. Because the demand curves are calibrated to outside elasticities rather than derived from a single utility function, we read CV_E as a demand-based approximation to compensating variation, not an exact money-metric measure.

The price of a marginal increase in the share is the borrower's marginal expected payment, $p(s) = \partial E[\text{payment}(s)]/\partial s$, computed group by group. Since payments on early exits settled at the contract's floor do not rise with the share, $p(s_0)$ is about 2,738 EGP, 9 percent below the mean divided pool (sale revenue net of principal plus any insurance payment). Weaker care also lowers sale values, so $p(s)$ declines with the share.

Cheaper debt. Under the priced insurance arrangements, debt takers also gain when the menu's common premium lies below the premium a debt-only lender would charge. Their gain, CV_D, is the area under the debt demand curve between the two premiums. We value this gain first and then value the equity option against debt at the common premium. If lenders charged a different actuarially fair premium for equity and for debt, it would make equity cheaper and even more attractive to borrowers.

Lender gain and delivery-cost thresholds. The lender's incremental profit and the delivery-cost thresholds follow from the margins:

$$\Delta\pi(r, s) = \Pi_{\text{menu}}(r, s) - \Pi_{\text{debt}}(r_0), \qquad a^{\text{crit}} = \frac{\mathrm{CV}_E + \mathrm{CV}_D + \Delta\pi(r_0, s_0)\big|_{a=0}}{\Delta M}, \tag{C6}$$

where ΔM is the additional mass of households the menu finances and a^{crit} is the administrative cost per loan at which total surplus reaches zero.

C.5 Parameters Set before Estimation

Table C1 lists every parameter set before estimation, with its value and how it is set: (i) taken from the data, (ii) chosen so that the model reproduces an observed quantity exactly, (iii) fixed by normalization, or (iv) set by assumption. Section C.6 estimates the remaining four.

Table C1: Parameters Set before Estimation

Parameter	Value	How set
Type shares ω_θ (EO, IND, DO)	0.44, 0.30, 0.26	Census
Principal P	20,796 EGP	Data
Observed margins $\bar{\mu}_D$; $\bar{\mu}_{E,g}$ (EO, IND)	2,049; 727, −275 EGP	Data (Appendix Table A4)
Venture surplus v_0	3,004 EGP	Data (mean divided pool, 225 equity borrowers)
Expected compensation per equity contract (EO, IND)	556, 1,571 EGP	Data
Expected forgiven principal per debt contract	874 EGP	Data
Default rates (debt; equity, EO and IND)	0.06; 0.01, 0.00	Data (Appendix Table A9)
Ratio of sale-value to adverse-outcome response	5,910 EGP	Data (Table 7, Panel B)
Choice intercepts	–	Matched to take-up exactly; not reported
Price coefficients (debt; equity, EO and IND)	4.34; 3.17, 3.62	Matched to elasticities of −1 and −0.5
Devaluation shift in equity-only adverse outcomes	0.80	Set from Table 7, Panel A; devaluation predictions only
Within-type standard deviation of each latent trait	1	Normalization
Loss given default	1	Assumption
Coinsurance share ϕ	0.25	Assumption
Pressure removed by coinsurance	ϕ units	Assumption

Notes: Latent traits are in units of their within-type standard deviation. Margins are means outside the devaluation period, and equity margins are the lender's share of the divided pool net of insurance payments at the experimental terms. The ratio is the sale value lost per additional adverse outcome among indifferent borrowers, the ratio of the two devaluation interactions. Default rates are shares leaving at least 5 percent of the death-adjusted obligation unpaid.

C.6 Estimation and Identification

Estimator. We estimate three behavioral objects by minimum distance: the extra debt aversion of equity-only households, the stewardship effect on adverse outcomes, and the moral-hazard slope λ. The stewardship effect is the product of equity-only households' stewardship advantage and its protective effect δ. The data pin down mainly the product, and the counterfactuals change little along the space where the product is constant, so we estimate both factors and let a ridge penalty pin the split. Each of the six moments is weighted by the inverse of its sampling variance, and the penalty pulls the four parameters toward their starting values θ^0,

$$\hat{\theta} = \arg\min_{\theta} \sum_{k=1}^{6} \frac{\left(\hat{m}_k - m_k(\theta)\right)^2}{\hat{v}_k} + 0.05 \sum_{j=1}^{4} \left(\frac{\theta_j - \theta_j^0}{w_j}\right)^2, \tag{C7}$$

where $\hat{m}_k$ and $\hat{v}_k$ are the data moment and its sampling variance, and w_j is the width of parameter j's search range. The penalty stabilizes the objective along the stewardship split, the one direction in which it is nearly flat. Every other parameter is set before estimation (Table C1).

Moments and identification. The six moments are (1) the share of equity-only households choosing equity in the forced choice, targeted at 0.98 rather than the observed 890 of 890 because a logit cannot produce a share of exactly one; (2) and (3) the gaps in forced sales and in deaths between equity-only and indifferent borrowers under equity; (4) the equity–debt gap in sale values among indifferent borrowers in normal times; and (5) and (6) the change during the devaluation in the equity–debt gaps in adverse outcomes and in sale values. They carry four independent restrictions for the three estimated objects, so the model is over-identified by one. The forced-sale and death gaps count as one, because the split of adverse outcomes between them comes from the same cells, and the devaluation interaction in sale values follows from the interaction in adverse outcomes through the fixed ratio above.

Each estimated object is identified by one of these restrictions. The forced-choice share identifies the extra debt aversion of equity-only households: the more often they choose equity when forced to choose, the larger it is. The gap in adverse outcomes between equity-only and indifferent borrowers on the same contract identifies the stewardship effect: a larger gap implies a larger effect. The devaluation identifies λ. It lowered pressure on equity borrowers by one unit while leaving debt borrowers' incentives unchanged, so it substitutes for variation in the equity share, which the experiment did not randomize, and a larger fall in the equity–debt gap in adverse outcomes implies a larger λ. The normal-time sale-value gap is the one over-identifying restriction.

Estimates. Table C2 reports the estimates. The estimated slope $\lambda = 0.27$ implies $\lambda_v \approx$ 1,403 EGP of venture surplus per unit of pressure, 47 percent of v_0. The debt-aversion estimate enters no counterfactual, because the choice intercepts match take-up at the experimental terms. Standard errors come from a linearized parametric bootstrap. We estimate the covariance of the six moments from 1,200 household resamples drawn within randomization strata, draw 10,000 moment vectors from that covariance, and pass each through a linearization of the estimator at the estimates, keeping each draw within the parameter bounds.

Table C2: Estimated Parameters

Parameter	Estimate
Extra debt aversion of equity-only households	2.67 (1.04)
Stewardship effect on adverse outcomes	0.26 (0.08)
Moral-hazard slope λ	0.27 (0.15)
Surplus lost per unit of pressure λ_v (implied by λ)	1,403 EGP

Notes: Standard errors in parentheses, from a linearized parametric bootstrap with 10,000 draws. The stewardship effect is the product of the equity-only households' stewardship advantage (1.31) and its protective effect δ (0.20). Latent traits are in units of their within-type standard deviation. The slope λ is in expected adverse outcomes per contract per unit of moral-hazard pressure.

C.7 Fit and Out-of-Sample Validation

Table C3: Out-of-Sample Validation Summary

Check	Data	Model	Gap (SEs)
A. Does bad-outcome risk drive sale values?			
Sale level, equity-only (kEGP)	23.21 (0.44)	23.58	+0.8
Sale level, equity-indifferent (kEGP)	22.37 (0.57)	21.96	−0.7
Sale level, debt-indifferent (kEGP)	23.68 (0.45)	23.24	−1.0
Selection gap in sale value (kEGP)	0.70 (0.71)	1.62	+1.3
Selection effect by sale quantile (EGP)	low +1,447 / high +93	lower tail	–
Contract effect by sale quantile (EGP)	low −1,986 / high −79	lower tail	–
B. Does the pressure mechanism transfer to the devaluation?			
Feed interaction, deval (EGP/week)	+132.9 (83.6)	+89.0	−0.5
Forced/death rates, equity-only	0.123 / 0.142	0.092 / 0.153	−1.2 / +0.4
Forced/death rates, equity-indiff.	0.088 / 0.110	0.054 / 0.054	−1.4 / −1.8
Forced/death rates, debt-indiff. (invariance)	0.103 / 0.168	0.045 / 0.126	−2.4 / −1.2
Deval selection shift (equity-only minus indiff.)	+0.259 (0.090)	+0.354	+1.1

Notes: None of these outcomes is used in estimation, but the model column combines the estimated parameters with observed inputs: each group's sale level prices its predicted chance of an adverse outcome at the observed sale values of contracts with and without one, the feed row compares the devaluation interaction with the negative of the normal-period contrast, and the debt-indifferent row holds the normal-period rates fixed. The gap column is the model−data difference in units of the data cell's sampling standard error, a common scale rather than a formal test. Panel A's quantile rows average the quantile coefficients at or below the 35th and at or above the 65th percentile. Panel B uses the paper's devaluation definition (cohorts 27 and earlier). Its equity-only rows include the calibrated devaluation shift in equity-only adverse outcomes (Appendix Table C1), which is set from Table 7, Panel A, so those rows are not out of sample.

The model matches its one over-identifying restriction, the normal-time equity–debt gap in sale values, within a tenth of its sampling standard error. We also evaluate the model on outcomes excluded from estimation, organized around three claims. Table C3 reports checks

on the first two in units of each data cell's sampling standard error. First, bad-outcome risk links selection and contract incentives to sale values. The model matches four untargeted sale-value magnitudes, and, as it predicts, selection and contract effects concentrate in the lower tail of the sale-value distribution (Figure A3). Second, the devaluation reverses normal-state pressure one for one. The change in weekly feed spending, the one outcome never used in estimation, matches that restriction to about half a standard error. Third, borrower surplus ranks demand across groups. Stated demand moves with modeled surplus in all four available contrasts (Tables 6 and 7). These are sign tests. Re-estimating the model without the normal-time sale-value gap, or without the two devaluation interactions, predicts the dropped moments to within 0.05 standard errors.

The validation also reveals failures. During the devaluation the model predicts forced-sale and death rates of 0.05 each among equity-indifferent borrowers, against observed rates of 0.09 and 0.11, and it predicts a forced-sale rate of 0.05 for debt borrowers, against 0.10 in the data. Preference-by-contract interactions also suggest that debt aversion and stewardship are not fully independent within the indifferent group (default interaction −0.09, SE 0.04).

C.8 Counterfactual Results

Premiums and the care response. With care responding, the common premium that solves the fixed point is 486 EGP, and it is 535 EGP with care held fixed. Both lie below the 656 EGP a debt-only lender would charge, so debt takers gain $\mathrm{CV}_D = 75$ EGP per eligible household with care responding. The care response lowers deaths (Figure 3). Without it, total surplus falls from 493 to 319 EGP (Section 6.5).

Headline results and uncertainty. Table C4 reports the headline results, with intervals from a household bootstrap at the preferred specification. Each of 500 samples is redrawn within randomization strata, every input except the census shares and the 0.98 forced-choice target is recomputed, and the parameters are re-estimated, so the intervals reflect uncertainty in take-up, insurance costs, the debt margin, and default rates as well as in the behavioral parameters. Across the household replicates, the menu with priced insurance beats debt-only lending before administrative costs in 90 percent of cases, the borrower gain is always positive, and the delivery cost up to which a subsidy is worth paying exceeds 7 percent in 78 percent of cases and 9 percent in 51 percent.

Table C4: Uncertainty in the Headline Results

	Point estimate	Household bootstrap 95% interval
As operated		
Lender gain over debt-only (EGP)	−57	[−400, 265]
One premium, 25% coinsurance, care responds		
Lender gain over debt-only (EGP)	147	[−86, 370]
Borrower gain (EGP)	346	[204, 498]
Total surplus (EGP)	493	[194, 796]
Common premium (EGP)	486	[320, 679]
Lender profitable unaided up to (% of principal)	2.7	[−1.8, 6.4]
Subsidy worth paying up to (% of principal)	8.9	[4.1, 13.1]

Notes: Per census-eligible household, relative to debt-only lending at the experimental terms, before delivery costs unless stated. Intervals are 2.5th to 97.5th percentiles over 500 household bootstrap replicates.

C.9 Robustness

Demand beyond the experimental terms. Under the logistic demand, some households still take equity at a full lender share, because the insurance and the return of principal do not scale with the share. We therefore recompute the preferred specification with two demand curves that match the same take-up and local elasticities but fall to zero there, a power curve and a bounded logit. Across the logistic, power, and bounded-logit curves, the lender's gain before delivery costs is 147, 91, and 128 EGP, the borrower gain is 346, 344, and 214 EGP, and the delivery cost up to which a subsidy is worth paying is 8.9, 8.8, and 7.2 percent. The lender's profitability and the case for a subsidy at delivery costs up to about 7 percent hold under all three, while the size of the borrower gain depends more on the shape of demand.

Loss given default. We assume the lender recovers nothing on a defaulted loan, debt or equity, so default losses enter the lender's margin as the default rate times surviving principal. Better recovery helps the larger debt-only book more, so it weakens the case for equity. With priced insurance and care responding, recovering 20 percent, half, and all of the defaulted principal lowers the lender's gain before delivery costs from 147 EGP to 123, 87, and 28 EGP, and lowers the delivery cost up to which a subsidy is worth paying from 8.9 to 8.5, 7.8, and 6.8 percent. At a 7 percent delivery cost, each EGP of subsidy returns 1.4, 1.3, 1.2, and 1.0 EGP across the four cases.

Proportional or additive moral hazard. We assume that moral-hazard pressure scales each household's baseline risk, so careful households respond less when incentives weaken. If instead pressure raised every household's adverse outcomes by the same amount, each EGP of subsidy would return about 1.5 EGP at a 7 percent delivery cost and about 1.03 EGP at 9 percent, and a subsidy would be worth paying up to a delivery cost of about 9.2 percent.

The lender's outside option. Above a delivery cost of about 8.3 percent, debt-only lending itself loses money, so the lender's real alternative to the menu would be to stop lending. Against that alternative the required subsidy is larger (413 rather than 350 EGP at a 9 percent delivery cost), but it also preserves debt borrowers' access to credit, worth about 1,029 EGP per eligible household, so a subsidy remains worth paying up to a delivery cost of about 16 percent (8.5 percent if debt access is given no value). Because that value rests on the high-rate tail of a demand curve calibrated only near the observed rate, Figure 4 keeps debt-only lending as the benchmark.