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

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Preliminary - Do Not Cite

September 11, 2026

Abstract

Firms often cite financing constraints as key barriers, yet lenders rarely innovate beyond debt products. One alternative is equity: by sharing risk, it could serve firms that debt deters, but theory predicts such contracts may invite adverse selection and moral hazard, mechanisms that are difficult to isolate empirically. We make progress on this challenge by designing and testing a microequity product with Egyptian livestock farmers that provides animals and splits the net sales proceeds in fixed proportions. We market this product alongside a flexible debt contract. Using a two-stage randomized controlled trial, we disentangle selection from contract effects. We find strong demand for equity, with many participants willing to accept lower financing probability to avoid debt. These "equity-only" participants exhibit *advantageous* selection on some key contract performance metrics relative to those willing to accept either contract. However, holding selection fixed, equity causes a drop in net revenue relative to debt, consistent with moral hazard. Among borrowers whose contracts ran through a period of high inflation and currency devaluations, this gap between equity and debt performance disappears. The severity of moral hazard therefore depends on the return the borrower keeps at the margin, which moves with prices and with the contract's terms, rather than being a fixed feature of equity. Our findings suggest microequity can enhance financial inclusion by attracting new clients, but managing moral hazard remains a primary, context-dependent challenge.

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 (Petersen and Rajan, 1994; 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.[1] 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.[2] 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.[3] 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—we design an

[1] Absent market frictions, the Modigliani–Miller theorem implies that capital structure is irrelevant for firm value (Modigliani and Miller, 1958), and costly state verification models help explain why debt is the dominant contract form (e.g. Townsend, 1979). However, the resulting scarcity of equity is costly, and not all of the frictions behind it are fundamental. Peter (2025) documents substantial variation in the extent of equity financing across the Eurozone and estimates large output losses from equity frictions across these markets, while Meki (2023) documents how policy-induced biases toward debt significantly distort firm financing.

[2] While micro-equity contracts are rare in practice, state-contingent financial contracts have a long pedigree in economic theory, from Friedman (1955)'s proposal for income-contingent student finance to extensive work on agricultural sharecropping (e.g. Stiglitz, 1974; Eswaran and Kotwal, 1985; Ghatak and Pandey, 2000).

[3] 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.

equity-like revenue sharing contract.[4] 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.[5] We provide borrowers with the opportunity to apply for 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 micro-equity 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

[4]The estimate is from Food and Agriculture Organization (2018); livestock value chains support an estimated 60% of rural households in developing countries (Pica-Ciamarra et al., 2011).

[5]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 revenue rather than profit, which limits the accounting-fidelity problems that undermined earlier profit-sharing attempts (de Mel et al., 2019*a*), 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.

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 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 micro-equity and a cost. On the one hand, equity contracts appear to relax important borrower-side barriers to participation, substantially expanding access to finance and drawing in borrowers who, on average, manage projects more successfully. On the other hand, conditional on participation, equity weakens incentives relative to debt and substantially reduces lender revenues in normal times. From the perspective of a microfinance institution, the trade-off between greater inclusion and lower per-client profitability will depend on the value placed on outreach, the costs of raising new capital, and the scope for complementary tools—such as monitoring technologies or performance-based pricing—to contain moral hazard. From the perspective of borrowers, equity offers meaningful insurance and access to productive assets, but they give up a share of the upside, and the equity-only borrowers it draws in report worse mental health after taking on financing.

To quantify the trade-off between inclusion and profitability, we develop a simple

structural model of contract pricing and welfare, disciplined to reproduce the experiment's take-up rates and lender margins and using the devaluation period to identify how borrower effort responds to contract terms.[6] Offering equity alongside debt finances roughly 37 percentage points more of the eligible population than debt alone, because debt-only lending reaches none of the equity-only households, and borrowers value that access at roughly 340 to 570 EGP per eligible household. It also costs the lender roughly 95 EGP per eligible household. That cost comes from the animal-death insurance bundled into the equity contract rather than from the equity share itself, and charging borrowers a premium that covers it makes the menu profitable while leaving care incentives unchanged. Delivery cost is the binding constraint. Total surplus stays positive only up to administrative costs of roughly 3 to 6 percent of principal, just under what Egyptian microlenders report, and at an administrative cost of 2.5 percent each 1 EGP of subsidy returns roughly 1 to 2 EGP of borrower surplus.

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*b*; Cordaro et al., 2025; Meki, 2025; Meki and Quinn, 2024).[7] We distinguish our contribution to this growing agenda in three ways. First, we study a setting of "arms-length" lending with relatively weak dynamic incentives and where revenue verification is low-tech. This pro-

[6]The experiment did not vary prices, so we solve the model under a range of price elasticities from our experiments in Egypt (Gazeaud et al., 2026) and Jordan (Karlan et al., 2021, which estimates a price elasticity for sharia-compliant offerings).

[7]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.

vides a test of equity in a high-friction environment relevant to the broad population of informal firms, where information asymmetries are arguably more severe than in settings with tighter vertical integration. Second, building on the vital proof-of-concept evidence from these earlier and contemporaneous studies, our large-scale two-stage design provides the statistical power and design choices necessary to separately identify selection from moral hazard. Third, we provide evidence on the viability of micro-equity 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, sharecropping, and income-contingent repayment. 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).[8]

Finally, our results speak to work on debt aversion, the psychological costs of debt, and the behavioral consequences of financial strain. Experimental studies document that many individuals are reluctant to take on contracts labeled as "debt" (Caetano et al., 2019), and that debt obligations can distort decision-making (Amar et al., 2011; Martínez-Marquina and Shi, 2024; Kaur et al., 2024). Our experiment brings these insights into a field setting with real entrepreneurs and contractually enforced obligations. Equity contracts may appeal disproportionately to debt-averse borrowers with high productive capacity, thereby expanding financial inclusion on the extensive margin, but the same insurance that relaxes downside risk can weaken effort incentives on the intensive margin. By measuring who selects into equity, how they perform under equity versus debt, and how monitoring interacts with contract structure, we provide a unified test of whether these behavioral and informational frictions ultimately help or hinder the case for equity financing in microenterprise contexts.

[8]Outside risk-sharing contracts, the closest designs come from consumer credit and health insurance. Karlan and Zinman (2009) randomize a South African lender's offer rate, its contract rate, and a dynamic repayment incentive, and find stronger evidence of moral hazard than of selection; Adams et al. (2009) separate the two in subprime auto lending using company-set variation in minimum down payments and car prices, and find both; and Einav et al. (2013) use differences in the health-plan menus offered to different groups of employees of a single firm at different times to show that individuals choose coverage partly on their own anticipated spending response, which they term selection on moral hazard. Each of these varies the price or size of one contract form. Our design varies the form itself and elicits each borrower's preference before randomizing, so that the indifferent group holds selection fixed across debt and equity.

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.

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, so 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), 66% of Muslim respondents reported that bank interest is religiously impermissible, compared with 26% of Christian respondents (N = 2,771 and 1,597). 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. They have 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.[9]

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

2.1 *Experimental Products*

We worked with ABWA to design two new asset-based financial products: a *Micro-Equity* 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.[10] Both products also included mandatory insurance for non-negligent animal death, assessed by a veterinarian.[11] 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.[12,13]

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.

[10]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.

[11]For the insurance, a veterinarian assessed each loss, and an ABWA committee ruled on negligence. 96 percent of losses were rules 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; the model in Section 6 prices the same product for a lender that bears both costs. Appendix Table A6 provides more details about how insurance costs affects borrower and lender-side accounting.

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

[13]If a borrower wished to exit the contract early, they must 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.

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).

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.[14] 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.

All clients were informed that the lender had the right to send a veterinarian who could conduct unannounced visits to assess livestock health and the quality of care. Clients were told that evidence of clear negligence could result in forfeiture of the animals to the lender and an administrative fee. Half of the clients were randomized into a treatment that actually sent the veterinarian to check on them periodically. We implemented these visits

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

in both the equity and debt arms to control for any pure reminder or information effects of veterinary contact.[15]

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.

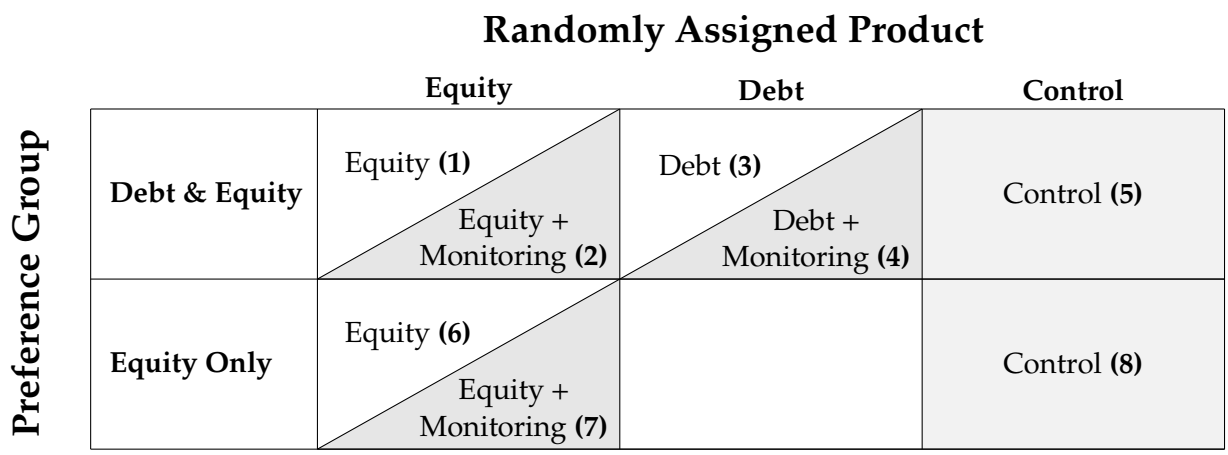
2.3 *Experimental Sample Recruitment & Data Collection*

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 embar-

[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.

[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. Numbers in parentheses refer to treatment arm IDs. Those who were only interested in debt or in neither product were not included in the experimental sample.

rassing 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. 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; Appendix D reports the available 13-month outcomes and reconciles the full analysis with the pre-analysis plan.

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.[17] For clients in the monitoring arm who were visited by the veterinarians we have recorded assessments of animal health,

[17]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, however, in practice it changes little

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 Appendix Table A4.

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.[18]

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. Appendix Table A19 reports the full intake. Among the eligible respondents, 46 percent of Muslims would accept financing only as equity, against 25 percent of Christians. The experimental sample was recruited almost entirely in Muslim-majority areas, so this comparison is available only in the census.

[18]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.

2.5 *Devaluations*

The experiment took place during a time with elevated macroeconomic uncertainty in Egypt. Egypt imported roughly 80% of its wheat from Russia and Ukraine, and the February 2022 invasion caused food & 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 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.[19] In Section 5 we report how borrower behavior differs during the devaluation period relative to the main analysis.

3 Demand and Sample Characteristics

3.1 *Differences in Demand for Equity and Debt*

In our experimental sample we find that 45% of eligible respondents expressed interest in equity only, while 55% indicated willingness to accept either product. When those in the

[19] 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 A9 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.

indifferent group were forced to choose between the two contracts, 80% stated a preference for equity—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 6.4 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.[20] 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.[21] Several baseline characteristics we expected to be 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. 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

[20] 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.

[21] 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.

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. Reassuringly, we find that the coefficients typically run in the same direction, meaning that the same characteristics that predict take-up in the analysis sample also predict take-up in the census sample and largely in the same way. Two exceptions are gender and prior loan experience. We find that women are more likely to prefer equity-only in the census sample, and those who have prior loan experience are less likely to prefer equity-only in the census sample. This suggests that standard recruitment channels may miss interest in the equity product from women and those without prior loan experience.

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.

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.

To estimate the "selection effect" into equity, we compare performance of those indi-

viduals 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. 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 Appendix Tables A2 & A3).[22]

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 64% decrease relative to equity-indifferent borrowers who had 0.17 animals die (since all

[22]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.

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.[23] Equity-only borrowers have 0.12 fewer forced sales, a 70% decrease relative to equity indifferent borrowers. Both of these effects are statistically significant. These two 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.[24] 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.[25]

[23]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.

[24]Appendix Table A10 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. Appendix Table A12 reports 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 repaid more than 90 days late, against none of 99 equity-indifferent, and these four account for the marginal coefficients at the 90-day threshold and on days delinquent in Appendix Table A10. 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, $p = 0.004$).

[25]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

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

Panel B compares how performance in equity compares to 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.

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, which is a summary measure that includes money spent on feed, animal supplies and medication. We also include information about the number of hours spent grazing the 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, which is consistent with having slightly larger and higher quality animals. Similarly equity-indifferent

for weighing the animals (which typically, but not always, happened during sale). Appendix Table A20 shows that both equity groups were 10-20pp more likely to have the loan officer present at this moment of sale, and open-ended responses to a question on the loan officer's involvement in the sale also suggests such a wedge. 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.

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. 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 effort due to the contract structure, and higher effort among those who positively select in to 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.[26] 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 household's main earner's (see the table note), which increased by 1,072 EGP, a 27% increase relative to control. 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 how all other income changed, not including proceeds from the animal sale, and shows a small positive effect. This rules out that effort on the animals crowded out other income generating activities.[27] 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

[26] We control for baseline income, chosen ex ante and imputed with a missing indicator where missing. The pre-analysis plan specifies double post-lasso control selection; Appendix D reports that version, which leaves every headline coefficient essentially unchanged (Table D7).

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

borrowers.

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 "fully 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 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 A7 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.

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. We also told all participants that the lender had the right to seize their assets if they found they were negligent in their handling of the animals and that we might send a veterinarian to check on them and the animals. That threat of seizure in the "monitoring control" groups may have been enough to make borrowers take the task seriously, which would leave the

actual veterinarian visits of the monitoring treatment arms redundant.

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.[28,29]

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 the pricing pressure from the devaluation led the equity-indifferent group to engage in higher effort, erasing the differences between them and both comparison groups.

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.

[28] 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.

[29] 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 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. Appendix Table A13 breaks total costs into their components: spending shifts toward feed and away from supplies during the devaluation, by amounts that roughly cancel and none of which is estimated precisely. Interestingly, while the devaluation has limited impacts on product satisfaction for the equity-only group, it does lead the equity-indifferent individuals to express less willingness to take equity again, consistent with dissatisfaction over the shift in incentives that prompted them to exert greater effort. 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 A5, 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, and what offering it would do to borrower and lender welfare.

6.1 A compact framework

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. This will be subject to two latent traits that we explain below, *debt aversion*, and *stewardship*.

As above we consider selection and effort. 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.

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

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

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 b_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 around type-specific means, and it is what separates the types: households with high b_i refuse debt at any interest rate we consider and will finance only through equity, while households with b_i near zero weigh the two contracts on price alone. The parameter is a stand-in rather than a single psychological primitive: it bundles risk and loss aversion over a fixed obligation's downside, 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, and our baseline measures of debt aversion, debt stigma, and risk tolerance (Table 1) point in this direction, as does the census attitude gradient on the religious component (Section 2). We denote by π_θ the population share of type θ, and by $q^E_\theta(r,s)$ and $q^D_\theta(r,s)$ the fractions of that type for whom equity and debt are the best option at terms (r,s); the rest take no finance. Take-up falls as a contract becomes more expensive. Because each borrower's contract choice was made once, before the relative-price consequences of the devaluation period were realized, the period provides no price variation in selection. Instead we model the change in demand using price elasticities from related experiments, which we describe in more detail below.

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.[30]

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

$$v(s) \;=\; v_0 \;-\; \lambda\,\big(g(s) - g(s_0)\big), \tag{6}$$

where v_0 is the venture surplus at the observed contract and $\lambda > 0$ is the moral-hazard slope converting pressure into lost surplus. Because g is an index, its scale is a normalization; λ is meant to represent the surplus cost of a given change in incentives. We estimate λ below using the variation from the devaluation period, which shifted this pressure for eq-

[30] 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", and 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 A11).

uity borrowers while leaving the incentives of debt borrowers, who are residual claimants throughout, unchanged (Appendix C states the sense in which the mapping from that variation to a slope in s is imposed rather than identified).

Households are not equally exposed to this channel. 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 then scales this baseline risk proportionally,

$$B_i(s) \;=\; \big(B_0 - \delta\,\sigma_i\big)\big[1 + \lambda_B\,\big(g(s) - g(s_0)\big)\big], \tag{7}$$

where B_0 is the adverse-outcome rate of a borrower with no stewardship advantage and $\delta > 0$ is the protective effect of stewardship, so that $B_0 - \delta\,\sigma_i$ is household i's baseline rate of adverse outcomes, declining in stewardship. The parameter λ_B is the proportional response of adverse outcomes to pressure, common to all households. It is not estimated separately: a single estimated loading governs both the loss of venture surplus above and the rise in adverse outcomes here, and λ_B is that loading divided by the baseline adverse-outcome rate of indifferent borrowers under equity (0.39/0.34), roughly 1.15 (Appendix C). A careful borrower therefore carries little baseline risk for weakened incentives to amplify; a careless one responds in full. These differences flow into the lender's losses on each contract, so who selects into equity shapes what the equity book costs.

The two traits could in principle be unrelated, but in our setting they move together, and this is what makes equity worth offering.[31] 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 the fact the model is disciplined to reproduce, and it is central to the value of adding equity.

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 erodes effort and shrinks the surplus $v(s)$ from

[31] This parallels Fang et al. (2008), who find in a study of the Medigap market, the private supplemental coverage bought by Medicare beneficiaries, that conditional on the characteristics insurers can price on, those with coverage spend about $4,000 less on medical care than those without. Finkelstein and McGarry (2006) find a similar pattern in long-term care insurance. de Meza and Webb (2001) show how such advantageous selection arises when risk aversion and precaution go together (for a review, see Einav and Finkelstein, 2011).

which the share is taken. The lender's question is whether to offer both contracts as a menu or debt alone, and we summarize the value of adding equity by the difference in its profit between the two:

$$\Delta\pi(r,s) \;=\; \sum_{\theta} \pi_\theta \left[q^E_\theta(r,s)\,\mu_E(s) + q^D_\theta(r,s)\,\mu_D(r) \right] \;-\; \Pi_{\text{debt}}(r) \;-\; a\,\Delta M(r,s), \tag{8}$$

where $\Pi_{\text{debt}}(r)$ is profit under debt-only lending, a is a common administrative cost charged on each financed loan, and $\Delta M(r,s)$ is the additional mass of households the menu finances relative to debt alone.[32] Because the menu finances more households, the cost term $a\,\Delta M$ weighs against it; but each additional household also brings the lender its margin, so the extra business is profitable at low administrative cost and turns unprofitable only once a exceeds the margin the marginal household contributes. Where that crossover falls is the question the counterfactuals answer.

Finally, we estimate the welfare borrowers can get from having equity as an option. We calculate the worth of making equity available as the compensating variation of taking it away. The price is the marginal contractual payment $p(x)$: the derivative, with respect to the share, of the borrower's expected payment to the lender under the contract. We call that expected payment as a function of the share the *payment schedule*; it is built from each financed borrower's realized sale under the contract's rules, so

$$\text{CV} \;=\; \int_{s_0}^{1} \Big(\textstyle\sum_\theta \pi_\theta\, q^E_\theta(r_0,x) \Big)\, p(x)\,\mathrm{d}x. \tag{9}$$

Because early-exit payments for which the contract's floor binds (the provision described in Section 2) do not move one for one with the share, this price sits below the average divided pool, by about nine percent at the observed terms (Appendix C). Demand does not vanish at $s = 1$, since the insurance and the return of principal do not scale with the share; under $\varepsilon_E = -0.5$ about a third of the observed equity takers would still take the contract there, under $\varepsilon_E = -1$ about five percent. The integral is therefore the value of raising the lender's share from one-third to one, and a lower bound on the value of

[32] At the experimental terms this mass has a closed form: debt-only lending finances none of the equity-only households, and the indifferent and debt-only households are financed at the same overall rate under either regime (some indifferent households switch contract, but their total participation is unchanged), so ΔM reduces to the census equity-only share multiplied by the equity-only arm's take-up rate, $0.44 \times 0.84 \approx 0.37$, where 0.84 is take-up over the full experiment and the roughly 90 percent reported in Section 4 is the non-devaluation analysis sample. The mass does not vary with the elasticity scenario because prices at the observed terms are normalized.

withdrawing the option.

6.2 Model Estimation, Identifying Assumptions, and Fit

The identification strategy follows the structure of the experiment, and we fit the model in two steps. First, we take the population shares of the three preference groups from the census and calibrate the model to reproduce take-up, lender margins, and insurance costs at the experimental terms. This makes sure the model in informed by the participation and profitability data we found. We set demand elasticities using evidence from related experiments to govern how participation changes as the lender adjusts its terms.

Second, we estimate four behavioral parameters: (i) the additional debt aversion of equity-only households, d; (ii) their stewardship advantage, $\Delta\sigma$; (iii) the protective effect of stewardship on animal outcomes, δ; and (iv) the response of animal outcomes to moral-hazard pressure, λ. We choose these parameters to match six experimental moments: the share of equity-only households choosing equity in the forced-choice question; their differences from equity-indifferent borrowers in animal deaths and forced sales; the normal-time sale-value gap between equity-indifferent and debt-indifferent borrowers; and how the equity-indifferent versus debt-indifferent gaps in adverse outcomes and sale values change during the devaluation period. These comparisons distinguish selection, by holding the contract fixed, from contract effects, by comparing randomly assigned equity and debt among borrowers willing to accept either. And because the devaluation hit after borrowers had chosen, it changed incentives without changing who held each contract, which is what lets it identify λ.

The model depends on three identifying assumptions. First, borrowers selecting each contract are comparable across the devaluation and non-devaluation cohorts, and changes in their relative outcomes reflect incentives rather than composition or other differential effects of the period. Randomization within cohorts, stable take-up, and balance at disbursement support comparability. Second, the devaluation changes the same moral-hazard pressure that varies with the equity share, allowing us to use the observed response to predict behavior at alternative shares. We normalize the devaluation to a one-unit reduction in pressure; only the slope times the change in pressure is identified, so this mapping is imposed rather than tested. Third, demand responds to prices according to elasticities drawn from related experiments, since ours offered only one set of terms. We consider two sets of elasticities in our counterfactual exercises.

We treat the partner as the sole formal provider in its catchment, so the status quo

against which we measure everything is debt-only lending at the observed rate. To generate counterfactuals we re-solve the household problem and the resulting take-up, outcomes, and margins at each candidate (r, s) on a grid, holding the calibrated parameters fixed.

Appendix C gives the full model and its estimation: how each input and parameter is pinned down, the functional forms, the estimator, and parameter estimates with standard errors.

To summarize, we estimate the model by GMM, choosing the parameters to bring the model's six moments as close as possible to the data and giving more weight to moments measured more precisely. The model reproduces the six targets closely. The estimates give a simple picture of the two forces at work. Equity-only borrowers' stewardship lowers their baseline risk of a bad outcome to roughly 40% of indifferent borrowers. The estimates imply that raising the lender's share from one-third to one-half would reduce total project surplus by about 12 percent, as borrowers keep less of the return to their effort. The lender would therefore receive a larger share of a smaller surplus.

To assess whether the model captures behavior beyond these targets, we examine outcomes it was not asked to fit. Its central 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 show this pattern: both effects are large in the bottom third of the sale-value distribution and close to zero in the top third. Borrowers' stated interest in borrowing again also follows the ordering of borrower surplus the model assigns to each group. The model is less successful at reproducing adverse outcomes during the devaluation period, particularly for equity-indifferent borrowers, whose risks it understates. These checks support its main mechanism while showing where the simplified model falls short. Appendix C reports the full results.

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 household problem 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 their 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 (i.e. offer the *menu*), they benefited from the insurance costs being covered by the project's sponsor. Outside of the insurance costs, the lender made money across all every borrower group, before taking into account administrative costs (Appendix Table A5, column 4). What we answer below is whether the lender can be profitable without a sponsor covering any costs.

Figure 2 plots the lender's total profit from the menu relative to debt-only lending across the space of contract terms (r, s), with darker blue indicating a larger gain and the star marking the experimental contract. Two facts stand out. First, offering equity and debt at the rates we used in the experiment is not profitable for the lender. The menu earns roughly 95 EGP less per census-eligible household than debt-only lending before any administrative cost.[33] Even increasing the equity share to the profit-maximizing level of s, between 0.38 and 0.42 relative to the 0.33 in the experiment, leaves the menu 62 EGP lower than debt only. Second, the region where the menu privately beats debt-only does exist, but it sits at higher debt rates. The lender's unconstrained optimum re-prices debt to roughly 20 percent and earns about 60 EGP more than debt-only at zero administrative cost, financing the equity option out of its debt takers; that region shrinks as the administrative cost rises, because the menu's advantage comes from financing more households and each additional financed household carries administrative cost.

A big reason why the offering equity is not profitable for the lender are the insurance costs. Expected insurance payouts run to 556 EGP per contract on equity-only takers but are 1,571 EGP for equity-indifferent takers. During the study the committee that considered insurance claims rules 96 percent of losses no-fault. We expect that a lender funding its own claims would police those claims more closely. We consider how lender profitability

[33]The quoted gap includes the 64 EGP adjustment to the lender's margin described in Section 6.2; without it the gap is 57 EGP (Appendix C).

would change as insurance claims are more tightly adjusicated.

The denial rate at which the menu becomes profitable for the lender depends on whether borrowers' care responds. Holding care fixed, the lender must deny between 18 and 48 percent of marginal claims. If care responds to stricter insurance similarly to how they responded to the devaluation, denial rates of 7 to 9 percent suffice, and total surplus rises with insurer strictness, because better care leads to fewer animal deaths. The strength of that response is capped by the data. Among indifferent borrowers in the non-devaluation cohorts, equity raises the share of contracts with a compensated animal death by 0.090 on a debt-arm base of 0.117 (the specification of Table 2, Panel B), so at most about 40% of claims can be attributed to negligence.

The lender can also reprice the insurance rather than restrict it. Suppose borrowers pay an actuarially fair premium for the animal-death insurance, with claims paid exactly as before. Because the animals remain insured, care incentives do not move, and the premium operates through pricing and take-up alone. Priced this way, the menu is profitable at the experimental terms, earning the lender roughly 100 EGP more per contract than debt-only lending in both demand scenarios.[34]

6.4 The welfare case, with and without a subsidy

Even if offering equity is not profitable for the lender it could still raise *total* surplus. The second counterfactual asks how large are the welfare gains from offering equity, who receives them, and whether a subsidy to the lender could be welfare enhancing.

We begin by considering borrower welfare. Evaluated at the prices in the experiment, the compensating variation of the equity option comes to about 573 EGP per census-eligible household under a price elasticity of $\varepsilon_E = -0.5$ (about 337 EGP under $\varepsilon_E = -1$), roughly 3 to 5 percent of the loan principal, most of which goes to equity-only households, whose revealed alternative is no finance at all. [35]

Who receives these gains, and who pays for them, depends on the terms the lender chooses. Figure 3 decomposes the total-surplus change from the lender's preferred menu, relative to debt-only lending at different prices & administrative costs, into its components:

[34]The premium prices the lender's net cost of a death after the contract's loss sharing, rather than the gross payout.

[35]Three omissions suggest that this value is a lower bound. Insurance value enters only as far as its impact on take-up, so the measure understates equity's worth if borrowers value downside protection beyond what their demand shows (Appendix C). Dynamic returns to a first financing cycle, such as revealing one's ability and graduating to larger loans (Bryan et al., 2024), are outside the demand curve entirely. So are gains to households that never borrow: if financing puts more or better animals into local markets, buyers gain through prices and quality.

the change in lender profit, the inclusion gain to equity-only households, the contract-choice gain to indifferent households, and the change in surplus to debt takers when the interest rate changes. Each pair of bars contrasts two versions of the lender's problem at the same administrative cost. In the first, the lender holds the debt rate at the experimental 13.5% and can only choose the equity share. In the second, the lender may also re-price debt.

Holding the debt rate fixed yields the larger total surplus at every administrative cost, the benefits of access to equity do not outweigh the losses that cannibalization introduce. On the other hand, when the lender is free to re-price debt, it increases the interest rate and extracts surplus from debt takers, making equity feasible and offering both products socially optimal. This that debt takers cannot defect to a debt-only competitor who offers lower prices, hence it is fragile to entry, and the subsidy comparison below does not rely on it.

If a sponsor was willing to provide a lender subsidy in an effort to maximize societal surplus the picture changes. Figure 4 holds the contract at the experimental terms (r_0, s_0). It then compares two quantities as the administrative cost rises: (i) the subsidy a funder would pay to keep a profit-maximizing lender offering the menu rather than reverting to debt-only, and (ii) the social gain that subsidy secures for borrowers. When admin costs are 2.5% of principle the subsidy is roughly 287 EGP per census-eligible household.

The green region is where a sponsor's transfer buys more surplus than it costs. We include the ratio of how much borrower surplus is gained for each EGP spent by the sponsor. For example at each EGP spent by the sponsor generates 2 EGP in borrower surplus if admin costs are 2.5% of the loan under our preferred price elasticity of -0.5. At 5% admin costs each EGP spent by the sponsor generates 1.2EGP in borrower surplus. Benefits are smaller in the -1 elasticity case. Admin costs for Egyptian and regional microlenders, on the order of 5–9% of principal per cycle for standard individual lending in Egypt(Al Tadamun Microfinance Foundation, 2023, 2024; Food and Agriculture Organization of the United Nations, 2024), which places realistic costs above the thresholds. This suggests that subsidies at the existing experimental prices are likely not socially efficient.

Two considerations cut the other way. First, administrative cost estimates are average values, whereas the marginal cost of an additional client may sit below average costs. 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. Together this suggests that the benefits of equity contracts can manifest when implementation costs are able to be kept low. This may be easier to do in certain industries, such as digital services where financing can be sent electronically and monitoring is much simpler.

6.5 Limitations

First, our counterfactuals rest on demand elasticities that the experiment does not identify, since all borrowers faced one set of prices. We anchor the debt elasticity to our ongoing randomized interest-rate experiment in Egypt (Gazeaud et al., 2026) and the equity elasticity to our randomized pricing experiment in Jordan (Karlan et al., 2021); randomized estimates elsewhere span the same range (Karlan and Zinman, 2008, 2019), and we report results for both $\varepsilon_E = -0.5$ and $\varepsilon_E = -1$.

Second, we charge equity and debt a common administrative cost, which is the case most favorable to the menu: equity is a costly-state contract and so it plausibly carries a monitoring premium that could shift values away from equity.

Third, the extraction and subsidy mechanics assume the partner is the sole formal provider and holds the debt-only benchmark at the observed rate; competitive entry could undo the re-pricing regime and is why we frame the socially preferable menu as one a committed funder sustains.

Fourth, our model is deliberately coarse about preferences. Demand enters through a single latent distaste for fixed obligations and external price elasticities, so the model is silent on the primitives—risk aversion, loss aversion, etc—that determine how borrowers value the two contracts' payoff distributions. Meki (2025) estimates these primitives for small-firm owners from incentivized laboratory-style choice tasks and finds them important to contract choice, which would be a natural extension of our work.

Finally, our welfare estimates are for normal times, and one might worry that they miss equity's main advantage: insurance against bad aggregate states. The devaluation cannot speak to this, because it was a shock that helped debt borrowers, raising the nominal value of livestock sales while debt payments stayed fixed. The scenario in which equity's insurance is most valuable is the opposite one, a collapse in revenues, and our data contain no such episode.

7 Conclusion

We implemented a field experiment that allows us to separate selection effects and contract effects in microequity lending. Nearly half of eligible households would accept equity but reject debt, and these equity-only households experience fewer bad animal outcomes than borrowers willing to accept either contract. These findings suggest that expanding the range of financing contracts can reach households that debt leaves out, including borrowers who perform well once financed.

While selection is advantageous, moral hazard is present and important. Among households willing to accept either contract, assignment to equity reduces effort and project performance relative to debt. However, this gap largely disappears during the devaluation period, when incentives under equity strengthened. Together, these results show why evaluating equity requires attention to both whom it attracts and how borrowers respond to the incentives created by the contract and the economic environment.

Our model brings these forces together and estimates substantial borrower welfare gains from adding equity to the lending menu, concentrated among households unwilling to take debt. Those gains do not necessarily make equity attractive to the lender. At our point estimates, a lender bearing the full costs of the implemented product would prefer debt-only lending. Adjusting the equity share offers limited improvement because it also changes participation and incentives. Realizing equity's inclusion benefits therefore requires attention to the costs of providing it.

An important next step is to identify settings where equity can be administered at lower cost. Dairy farmers supplying cooperatives or processors, small manufacturers fulfilling orders for established buyers, and businesses selling through digital platforms offer promising opportunities. In each case, existing commercial arrangements may already record output, verify sales, and route payments, reducing the additional work required to administer an equity contract. Testing equity in these settings would help establish whether lower operating costs can make it sustainable while preserving access for households that reject debt.

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Table 1: Selection into Equity-Only Preference Group

	Experiment Sample			Census Sample
	Indifferent Mean	Equity-Only Mean Diff.	Multivariate Coefficient	Multivariate Coefficient
	(1)	(2)	(3)	(4)
Education (Years)	6.44	1.01***	0.003	-0.002
	[6.35]	(0.39)	(0.003)	(0.010)
Cognition (Digital Span Recall)	2.43	0.20**	0.016	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	0.000	0.029*
	[3.67]	(0.19)	(0.003)	(0.016)
Positive Reciprocity (0-6)	5.09	0.08	0.008	0.026
	[1.39]	(0.07)	(0.009)	(0.048)
Debt Aversion (1-10)	6.10	0.19	0.002	0.011
	[4.10]	(0.22)	(0.003)	(0.010)
Debt Stigma (1-10)	3.83	0.28	0.003	0.005
	[4.32]	(0.23)	(0.003)	(0.011)
GAD-2 (Anxiety) ≥ 3	0.26	0.01	0.007	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	-0.004	0.130
	[0.48]	(0.03)	(0.027)	(0.149)
Muslim	0.98	0.00	0.013	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	0.036	-0.098
	[0.39]	(0.02)	(0.034)	(0.131)
Prior Equity Experience	0.17	-0.00	-0.002	0.176
	[0.38]	(0.03)	(0.039)	(0.154)
Prior Formal Loan Experience	0.33	0.03	0.026	-0.148
	[0.47]	(0.03)	(0.030)	(0.127)
p-value (Joint Test)			0.295	0.561
N			1,714	145

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

Notes: Column (1) shows means for the indifferent group, with standard deviations in brackets. Column (2) reports the equity-only minus indifferent difference in each row variable, in the variable's own units, from a regression of the variable on the equity-only indicator. Columns (3) and (4) report coefficients from a single linear probability model of the equity-only indicator on all row variables jointly: each is the change in the probability of being equity-only per one unit of the row variable, holding the others fixed, and is not comparable to column (2). The joint test is the F-test that all column coefficients are zero. Income is in thousands of EGP, winsorized at the top 1 percent. For joint test regressions, missing values are imputed with means. Experiment sample regressions include cohort and baseline-enumerator fixed effects, as in the pre-analysis plan; robust standard errors in parentheses.

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. Default = left at least 5% of death-adjusted obligation unpaid. See Appendix Tables A10 and A11 for sensitivity to different delinquency and default thresholds. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

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 non-devaluation cohorts (cohort $\geq$ 28). Net revenue (column 4) is total sale value less acquisition principal, without distinguishing how the resulting proceeds are divided between the borrower and lender. The allocations under the different accounting rules are reported in Table A5. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

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	32	-6	24	-24	3
	(97)	(49)	(85)	(25)	(2)
Equity-Indiff Mean	1,357	683	454	156	30
N	220	218	218	221	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-34	-87	75	8	-1
	(97)	(54)	(75)	(23)	(2)
Debt-Indiff Mean	1,381	755	395	147	31
N	207	207	208	207	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.

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. Column 1 adds the borrower's contractual revenue share from the ABWA animals, computed under the full contract rules: sale proceeds plus any insurance compensation enter the divided pool, the early-exit floor and per-client contractual overrides apply, and the lender's claim is netted out; for debt borrowers the share is compensation-inclusive proceeds net of principal and the 13.5 percent fee. The share is added only where the survey figure does not already include the sale proceeds: the survey asked, and for the 35 takers (of 326 asked in these cohorts) who said their figure included them, column 1 is the figure as reported (Appendix Table D11). This is the borrower-side counterpart of the appendix compensation ledger, and it is not the borrower's share of Table 3's sales-only net revenue, which excludes the insurance. The survey questions for the components (Columns 3-5) simply ask about *each* month. Income in columns 1 and 2 is the respondent's own last-month income when the respondent reports any income source, and otherwise the last-month income of the household member the respondent names as the main earner; the survey never asks both, so the measure is one earner's income rather than a household total. The components in columns 3–5 follow the same rule. Column 5 aggregates irregular wage, regular wage, self-employment, and other income (pension/gov transfers). We control for baseline values where available for all outcomes (and impute and control for a missing indicator where missing). All regressions include strata fixed effects. Selection effect regression includes cohort fixed effects.

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. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Satisfaction is the response to "On a scale from 1 to 5, how satisfied are you with the financing you received (in-kind debt financing or equity)?", where 1 = not at all satisfied and 5 = very satisfied. Repeat? is an indicator for answering yes to "Would you be interested in getting another product like this one again?" Columns (3)–(5) are indicators for the response to "Between in-kind debt financing and equity, which do you prefer?", asked after roughly six months of contract experience.

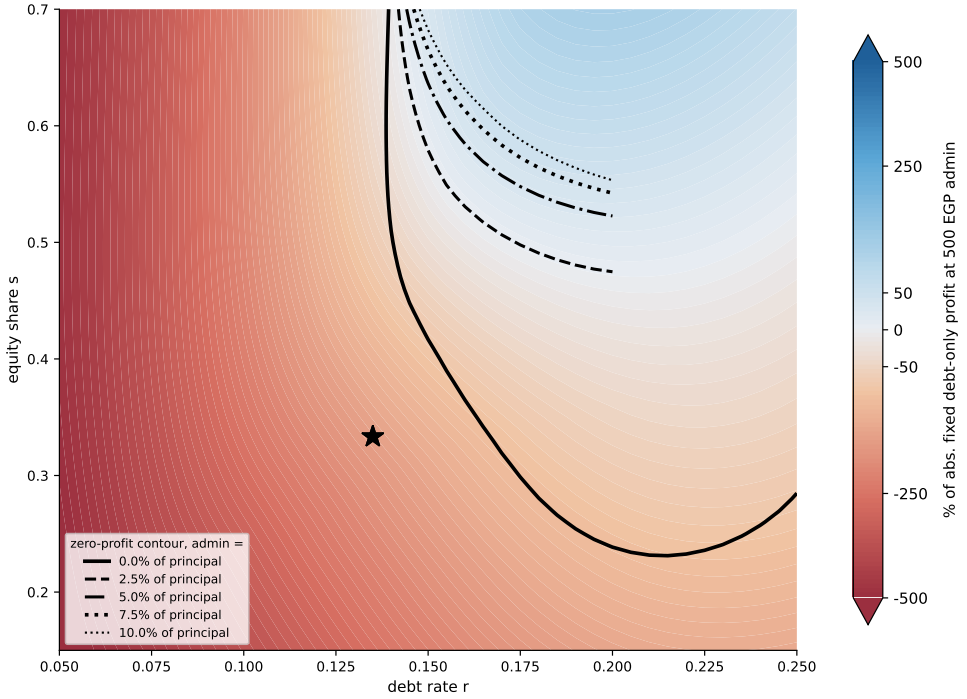
Table 7: Devaluation Effects on Selected Outcomes

	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	31.65	3.32	-0.04	0.02
	(0.07)	(0.01)	(699.73)	(2.38)	(95.63)	(2.14)	(0.24)	(0.06)
Equity-Only X Deval	0.26***	-0.01	-941.62	-3.74	39.03	-2.94	0.10	-0.03
	(0.09)	(0.02)	(872.37)	(3.28)	(134.62)	(2.74)	(0.30)	(0.08)
Equity-Indiff Mean (Non-Deval)	0.34	0.00	22,372.37	178.02	1,356.70	29.91	2.86	0.22
Equity-Indiff Mean (Deval)	0.20	0.01	20,511.51	165.70	1,071.51	30.73	3.42	0.40
N	572	572	572	572	542	572	571	571
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)								
Equity-Indifferent	0.17**	-0.06***	-1384.94**	-3.81*	-33.58	-0.96	0.33	0.08
	(0.07)	(0.02)	(658.28)	(2.21)	(95.85)	(2.04)	(0.24)	(0.05)
Equity-Indiff X Deval	-0.24***	0.05**	1411.19*	4.35	-72.85	-0.65	-0.36	-0.15*
	(0.09)	(0.02)	(834.82)	(3.08)	(127.63)	(2.67)	(0.30)	(0.08)
Debt-Indiff Mean (Non-Deval)	0.17	0.06	23,678.39	181.06	1,380.98	30.84	2.52	0.15
Debt-Indiff Mean (Deval)	0.27	0.02	20,173.50	163.77	1,175.43	32.55	3.36	0.46
N	501	501	501	501	488	501	500	500

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

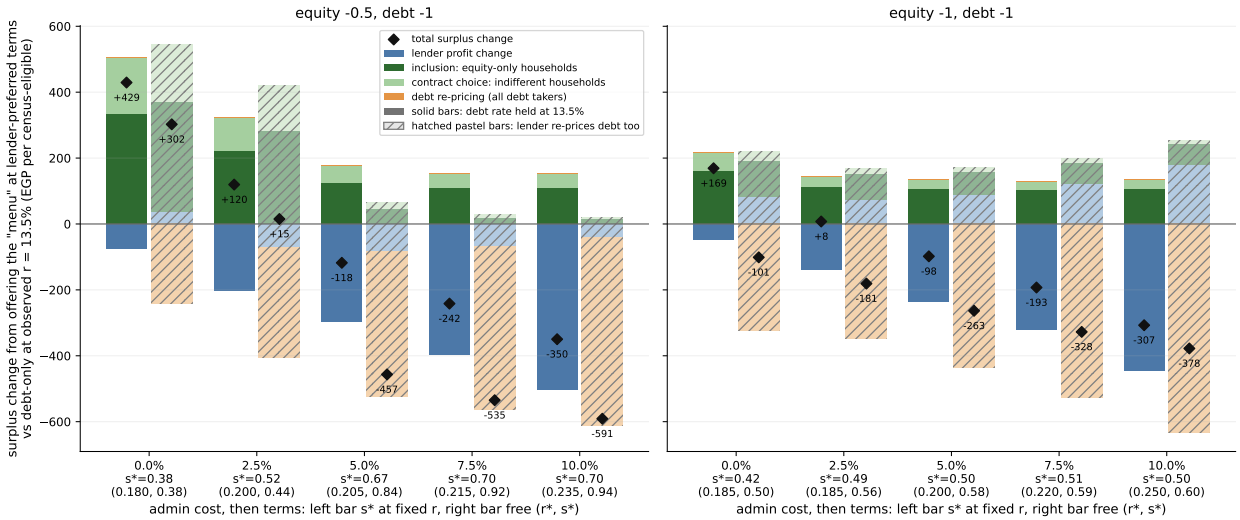
Notes: Devaluation indicator equals 1 if the contract could have spanned either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Figure 2: Lender Profitability of the Menu Across Contract Terms



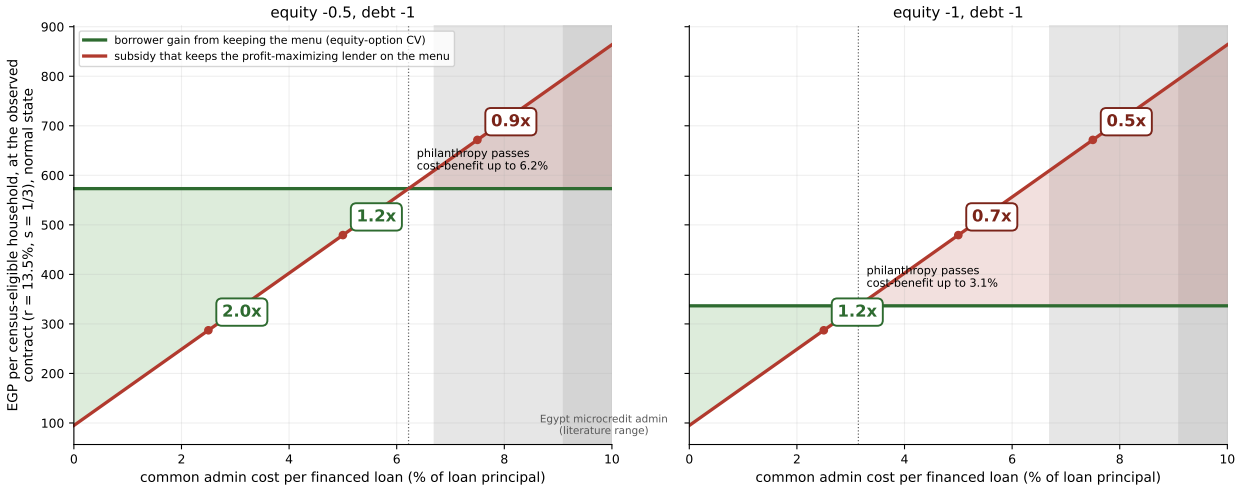
Notes: The color surface is the profit difference between the menu and debt-only lending at $r_0 = 13.5\%$ at a fixed administrative cost of 500 EGP per loan, expressed as a percent of the absolute debt-only profit at that cost (which is small, so the percentages are large); blue means the menu is more profitable. The color scale is background for the star; the substantive object is the set of black curves, which are the break-even (zero-profit-difference) frontiers at administrative costs of 0, 2.5, 5, 7.5, and 10% of loan principal; because profit is linear in the administrative cost, each frontier follows exactly from the plotted surface, and a frontier absent from the panel lies entirely outside the plotted region. As the cost rises the break-even frontier retreats toward higher debt rates, so the region in which the menu is profitable shrinks. The star marks the experimental contract $(r_0, s_0) = (13.5\%, 1/3)$. Under the labeled equity-demand elasticities.

Figure 3: Who Gains from the Lender-Preferred Menu, and How Debt Pricing Matters



Notes: At each administrative cost the lender chooses the menu terms that maximize its profit, subject to equity take-up staying at or above half its observed level and an equity share of at least 1/3, with ties among terms within 5% of maximum profit broken toward the observed contract. The bar decomposes the resulting total-surplus change relative to debt-only lending at $r_0 = 13.5\%$ into lender profit, inclusion (equity-only households), contract choice (indifferent households), and debt re-pricing (all debt takers). Solid bars hold the debt rate at 13.5%; hatched pastel bars let the lender re-price debt. Diamonds mark net totals. The lender's preferred equity share stays near 1/3 at low cost but drifts upward at the highest costs shown, to roughly 0.5–0.6 under $\varepsilon_E = -1$ and as far as 0.7–0.9 under $\varepsilon_E = -0.5$, visible in the term labels. Panels report $\varepsilon_E = -0.5$ (left) and $\varepsilon_E = -1$ (right). The debt-only benchmark is the observed contract, not re-optimized. Letting the lender re-price raises its profit but lowers total surplus: the higher debt rate is mostly a transfer from debt takers, which nets out of the total, plus a deadweight loss from households priced out of financing; the extra instrument serves the lender's objective, not the planner's. Term labels report s^* at the fixed rate, then (r^*, s^*) when both are free; the debt re-pricing component is zero in the solid bars by construction.

Figure 4: The Cost-Benefit Case for Subsidizing Equity at the Observed Terms



Notes: With the contract fixed at $(r_0, s_0) = (13.5\%, 1/3)$, the red line is the subsidy required to keep a profit-maximizing lender offering the menu instead of debt-only, and the green line is the borrower gain (equity-option compensating variation) that subsidy secures, both per census-eligible household. The subsidy rises with the administrative cost because the menu finances roughly 37 percentage points more of the eligible population than debt-only lending; the borrower gain does not vary with that cost because the model does not pass it through to prices. Green shading marks the region where the transfer buys more surplus than it costs; boxed multiples give the benefit-cost ratio (EGP of borrower surplus per EGP of subsidy) at 2.5%, 5%, and 7.5% administrative cost, in green above one and red below. The gray vertical bands mark literature operating-cost ranges for Egyptian microlenders (Al Tadamun Microfinance Foundation, 2023, 2024; Food and Agriculture Organization of the United Nations, 2024). Panels report $\varepsilon_E = -0.5$ (left) and $\varepsilon_E = -1$ (right).

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–

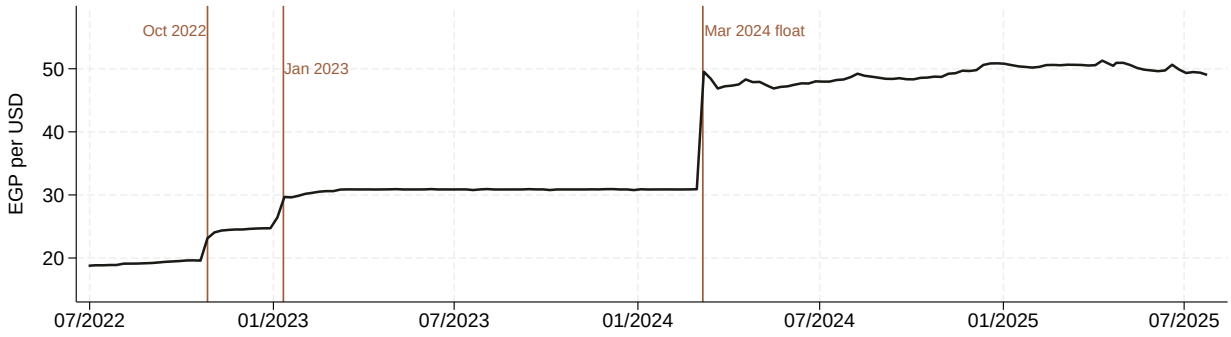
(c) The same table, translated

		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

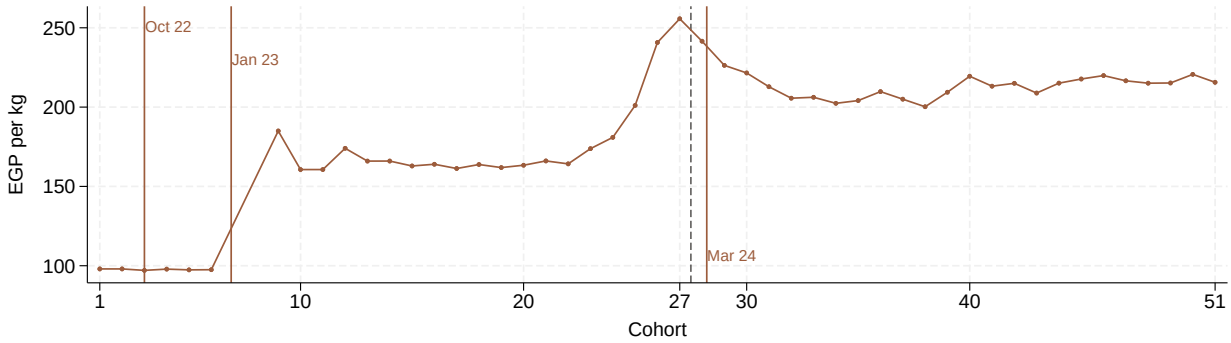
Notes: Applicants watched a short animated video explaining both products before stating which they were willing to accept (Section 2.2). Panel A takes the same bad outcome—animals bought with 15,000 EGP of financing and sold for 14,000—through each contract. The equity panel reads "You sold the animals 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

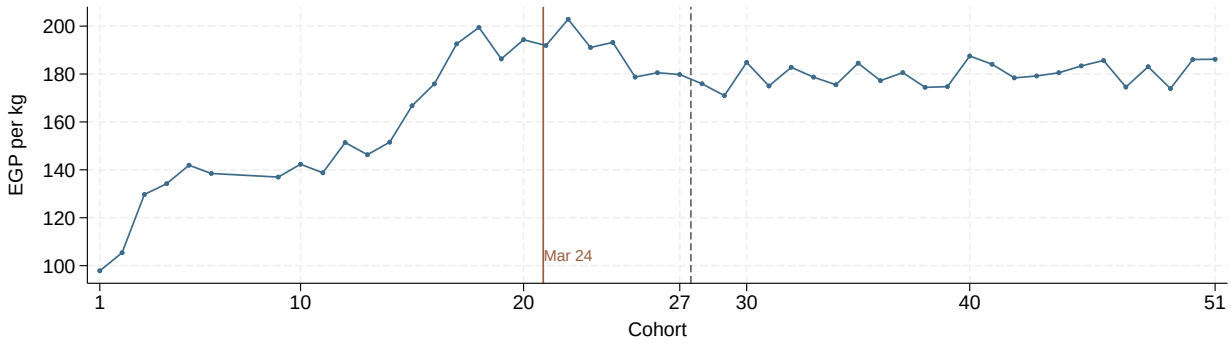
(a) Exchange rate, EGP per USD, weekly



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

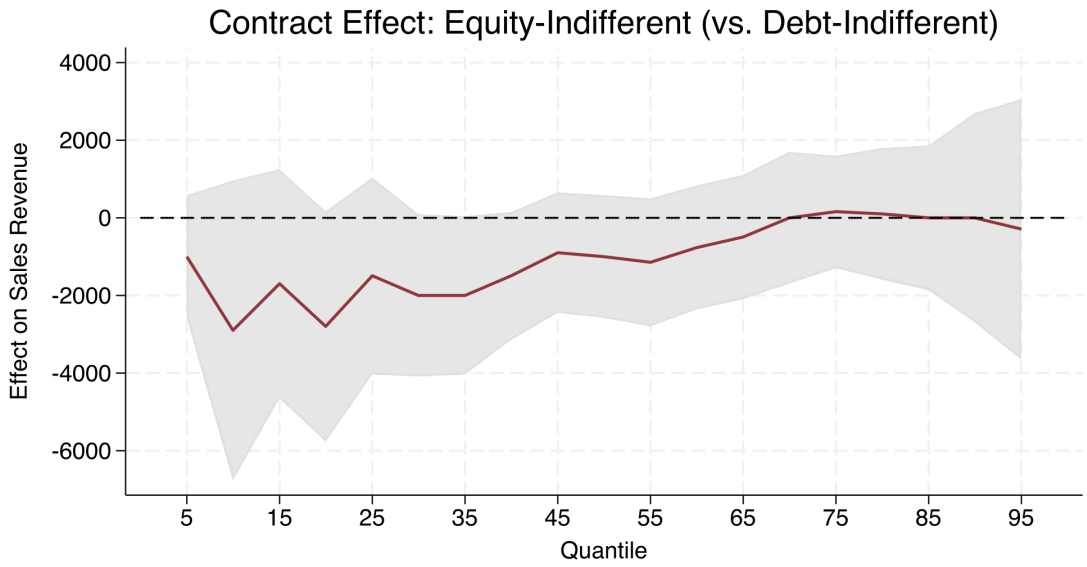


(c) Sale price per kg, by cohort (priced about six months later)

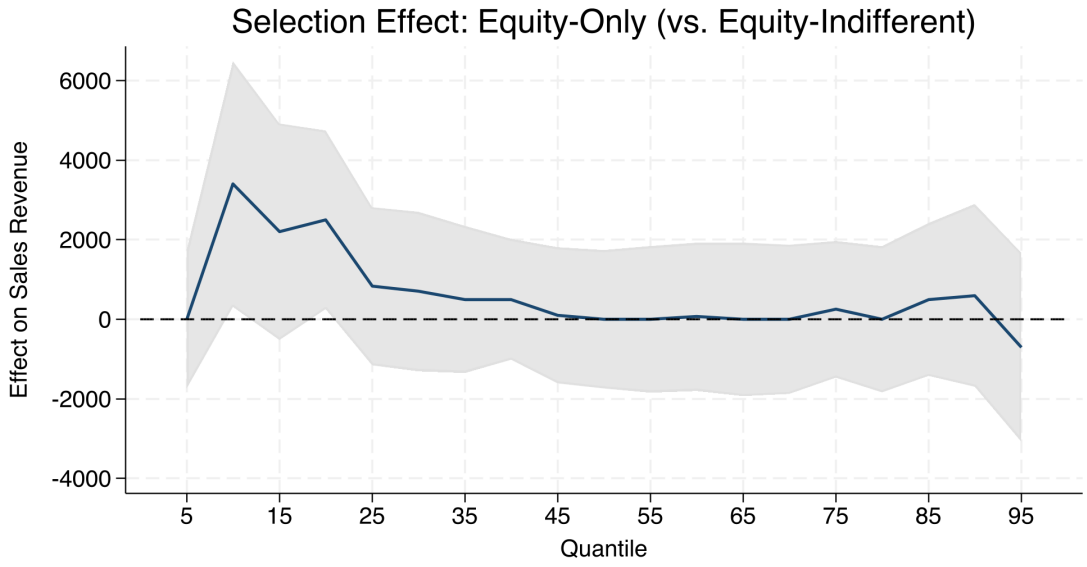


Notes: Solid vertical rules mark the three step devaluations; the dashed rule is the cohort 27/28 split. Panels (b) and (c) are indexed by cohort rather than by date, since the sample split is stated in cohort numbers. A cohort buys at disbursement and sells a mean of 175 days later, so a given devaluation 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	40.38	0.06 (0.66)	40.41	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)	7.21	0.17 (0.49)	6.28	0.07 (0.62)	0.25 (0.61)
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 (1-10)	3.94	0.09 (0.29)	3.57	0.19 (0.37)	0.77** (0.37)
Debt Aversion (1-10)	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 (1-10)	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)		0.927		0.307	0.092
Joint F-test (RI p-value)		0.442		0.971	0.316

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

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.

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.

Table A4: Project Scope and Beliefs About Prices

	Equity-Only Mean/SD	Equity-Indiff Mean/SD	Debt-Indiff Mean/SD	p-value [1 vs 2]	p-value [2 vs 3]	p-value [1 vs 3]
Panel A: Baseline Elicitation (Full Sample)						
Ideal Amount (Debt)	24,595	27,560	26,752	0.000***	0.306	0.000***
	(13,301)	(16,580)	(14,176)			
Ideal Amount (Equity)	28,138	27,791	27,545	0.365	0.507	0.858
	(15,912)	(14,621)	(15,063)			
Equity less Debt (within person)	3,058***	178	782*	0.000***	0.466	0.000***
	(10,834)	(9,363)	(10,301)			
Sheep Price Beliefs (Num Per 10k)	3.17	3.15	3.18	0.771	0.665	0.902
	(0.77)	(0.78)	(0.85)			
Observations (answered both)	833	276	555			
Panel B: Midline, Ideal Amount Under the Contract Not Received						
Debt, Asked of Equity Borrowers	3,040	3,394	—	0.783	n/a	n/a
	(9,480)	(9,623)				
Conditional on Wanting Any	29,231	28,000	—	0.745	n/a	n/a
	(9,968)	(8,571)				
Share Wanting Any	0.104	0.121	—	0.686	n/a	n/a
	(0.306)	(0.328)				
Equity, Asked of Debt Borrowers	—	—	7,583	n/a	n/a	n/a
			(15,874)			
Conditional on Wanting Any	—	—	30,611	n/a	n/a	n/a
			(17,740)			
Share Wanting Any	—	—	0.248	n/a	n/a	n/a
			(0.434)			

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

Notes: Panel A reports baseline elicitations for the full analysis sample. Clients reported the financing amount they would ideally take under each contract, and their beliefs about how many sheep 10,000 EGP would buy. Equity less Debt is the within-person difference between the two ideal amounts; stars on those means come from a paired test that the difference is zero within group, and the p-value columns compare that difference across groups. Columns 4 and 6 come from regressions with cohort fixed effects, matching Table 1. Column 5 compares equity-indifferent with debt-indifferent borrowers, who were randomly assigned within the indifferent group, and uses strata fixed effects; for a baseline characteristic it is a balance check rather than a selection comparison. Panel B reports the midline elicitation, which asked each borrower about the contract they did not receive, and is therefore restricted to borrowers who took up financing in non-devaluation cohorts (cohort ≥ 28). Amounts are in EGP and winsorized at the top 1 percent. Robust standard errors throughout.

Table A5: 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; column (2) adds those payments to column (1) and is the pool the contract divides. Column (3) reflects the borrower's share of that pool (what they actually received, and also what's 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. 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 A6: Example Lender Margins With and Without an Insured Death

	A: No Death	B: One Animal Dies
Animals sold	3	2
Total sales (EGP)	14,000	14,000
Insurance payment to the borrower	0	6,667
Pool the contract divides	14,000	20,667
Profit above principal	−6,000	+667
Borrower repays	18,000	13,556
Lender's cash position	**−2,000**	**−6,444**
Contractual share, before insurance	−2,000	+222
Contractual share, net of insurance	−2,000	−6,444

Notes: Two hypothetical borrowers, each financed 20,000 EGP of animals, each ending with 14,000 EGP of sales. A price shortfall is shared, one third to the lender and two thirds to the borrower. A no-fault death is not shared: the insurance makes the borrower whole and the lender absorbs the animal. The same sales line therefore leaves the lender down 2,000 or down 6,444 depending on which happened.

Table A7: 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 either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). All regressions include strata fixed effects.

Table A8: Psychological Outcomes

	(1) Stress Level (0 to 100)	(2) GAD-2 Score	(3) GAD-2 Above Threshold	(4) Financial Worry (1-10)
Panel A: Equity Only				
Equity	7.46*	0.32*	0.05	0.23*
	(4.23)	(0.17)	(0.05)	(0.13)
Equity X Deval	-0.71	-0.05	0.05	-0.16
	(5.53)	(0.22)	(0.06)	(0.16)
Control Mean	45.67	1.855	0.316	1.447
N	855	861	861	861
Panel B: Indifferent				
Equity	1.38	0.06	0.05	0.05
	(5.47)	(0.23)	(0.07)	(0.16)
Debt	4.20	0.19	0.09	0.16
	(5.37)	(0.23)	(0.07)	(0.16)
Equity X Deval	1.41	0.08	-0.02	0.23
	(7.10)	(0.29)	(0.09)	(0.21)
Debt X Deval	-1.99	-0.26	-0.10	0.11
	(6.96)	(0.29)	(0.08)	(0.21)
Control Mean	48.37	2.055	0.347	1.395
N	795	800	800	800
Selection Effect (Equity-Only - Equity-Indif)	0.39	0.41	0.95	0.60
Contract Effect (Equity-Indif - Debt-Indif)	0.59	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 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 baseline values. Selection effect regression includes cohort fixed effects.

Table A9: 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. 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 A10: 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. 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.

Table A11: 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.

Table A12: Variance-Equality Tests Across Arms

Panel A: Selection Effect (equity takers: Equity-Only vs. Equity-Indifferent)

	SD Equity-Only	SD Equity-Indiff.	Brown-Forsythe p-value	N
Last-month total income (incl. ABWA)	5,398	4,739	0.546	221
Total livestock sale revenue	4,964	5,697	0.159	225
Sale price per kg	15.2	16.6	0.263	225
Sale weight (kg)	25.2	30.3	0.067	225
Days delinquent	20.2	6.9	0.014	225

Panel B: Contract Effect (indifferent takers: Equity vs. Debt)

	SD Equity-Indiff.	SD Debt-Indiff.	Brown-Forsythe p-value	N
Last-month total income (incl. ABWA)	4,739	4,754	0.725	204
Total livestock sale revenue	5,697	4,762	0.066	210
Sale price per kg	16.6	14.5	0.216	210
Sale weight (kg)	30.3	25.8	0.129	210
Days delinquent	6.9	33.2	0.004	210

Panel C: Treatment vs. Control (ITT, last-month total income incl. ABWA)

	SD Control	SD Equity	SD Debt	Brown-Forsythe p-value	N
Equity-Only stratum	4,216	5,221		0.009	387
Indifferent stratum	3,269	4,717	4,674	0.001	327

Notes: Tests of equality of variances across arms, prespecified alongside the quantile analysis (quantile treatment effects appear in Appendix Figure A3). Reported p-values are Brown-Forsythe (median-centered) statistics; Levene (mean-centered) versions are similar. Panels A and B use takers in non-devaluation cohorts, the frames of the product-outcome tables; Panel C uses the full intent-to-treat sample of non-devaluation cohorts, the frame of the borrower-income table. Sale and repayment outcomes are administrative and exist only for borrowers, so the control-group comparison in Panel C is available only for survey income. Among the indifferent, equity and debt income variances are indistinguishable once the control group is excluded ($p = 0.92$).

Table A13: 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	31.65	-5.90	23.98	-24.36	3.32
	(95.63)	(48.01)	(83.43)	(24.20)	(2.14)
Equity-Only X Deval	39.03	22.43	-37.52	23.87	-2.94
	(134.62)	(69.56)	(111.04)	(24.60)	(2.74)
Equity-Indiff Mean (Non-Deval)	1,356.70	682.99	453.61	155.58	29.91
Equity-Indiff Mean (Deval)	1,071.51	600.31	469.48	10.88	30.73
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)					
Equity-Indifferent	-33.58	-86.93	74.75	8.02	-0.96
	(95.85)	(53.67)	(74.54)	(22.83)	(2.04)
Equity-Indiff X Deval	-72.85	110.63	-107.70	-11.53	-0.65
	(127.63)	(69.18)	(104.42)	(23.35)	(2.67)
Debt-Indiff Mean (Non-Deval)	1,380.98	754.73	395.50	147.33	30.84
Debt-Indiff Mean (Deval)	1,175.43	578.48	486.16	19.81	32.55
N	488	487	494	498	501

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

Notes: Devaluation indicator equals 1 if the contract could have spanned either the 2023 or 2024 devaluation episodes (cohorts 27 and earlier). Column 1 is the total of columns 2 to 4 plus other minor categories, and is the cost measure reported in the main devaluation table. Columns 1 to 4 are in EGP and column 5 is in hours. Monetary measures are winsorized at the top 1 percent. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Table A14: 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 observations from cohort 30 onward (first serious fraud case, baseline). Drop ≥24 excludes 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.

Table A15: Heterogeneity by Gender

	(1) Bad Outcomes	(2) Default	(3) Total Value at Sale
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)			
Equity-Only	-0.219*	-0.005	482.557
	(0.115)	(0.009)	(1112.311)
Female	-0.006	0.003	38.839
	(0.120)	(0.008)	(1060.431)
Equity-Only × Female	-0.015	0.030	331.433
	(0.143)	(0.022)	(1455.020)
Equity-Indiff Mean	0.34	0.00	22,372.37
N	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)			
Equity-Indifferent	0.132	-0.018	-1621.726
	(0.124)	(0.031)	(1208.035)
Female	-0.045	0.064	-500.239
	(0.089)	(0.043)	(1077.177)
Equity-Indiff × Female	0.057	-0.054	291.040
	(0.159)	(0.045)	(1461.315)
Debt-Indiff Mean	0.17	0.06	23,678.39
N	210	210	210

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

Notes: Sample restricted to non-devaluation cohorts (cohort≥28). Female is from baseline survey. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Table A16: Heterogeneity by Risk Tolerance

	(1) Bad Outcomes	(2) Default	(3) Total Value at Sale
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)			
Equity-Only	-0.236**	0.025	484.081
	(0.119)	(0.019)	(993.300)
High Risk Tolerance	-0.090	0.006	-636.578
	(0.138)	(0.007)	(1098.791)
Equity-Only × High Risk Tolerance	-0.021	-0.029	467.280
	(0.155)	(0.021)	(1456.427)
Equity-Indiff Mean	0.34	0.00	22,372.37
N	214	214	214
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)			
Equity-Indifferent	0.200	-0.075**	-1006.218
	(0.128)	(0.033)	(1044.191)
High Risk Tolerance	-0.064	-0.049	420.159
	(0.093)	(0.044)	(956.576)
Equity-Indiff × High Risk Tolerance	-0.057	0.052	-826.906
	(0.164)	(0.044)	(1412.880)
Debt-Indiff Mean	0.17	0.06	23,678.39
N	199	199	199

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

Notes: Sample restricted to non-devaluation cohorts (cohort≥28). High Risk Tolerance is above-median on quantitative risk measure from baseline. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Table A17: Heterogeneity by Cognitive Ability

	(1) Bad Outcomes	(2) Default	(3) Total Value at Sale
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)			
Equity-Only	-0.200**	0.033	-107.580
	(0.092)	(0.024)	(837.353)
High Cognitive Ability	0.081	-0.002	394.830
	(0.129)	(0.005)	(1095.509)
Equity-Only × High Cognitive Ability	-0.068	-0.039	1589.541
	(0.148)	(0.028)	(1395.984)
Equity-Indiff Mean	0.34	0.00	22,372.37
N	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)			
Equity-Indifferent	0.075	-0.069**	-1073.428
	(0.100)	(0.032)	(867.152)
High Cognitive Ability	-0.130	-0.005	1216.441
	(0.084)	(0.040)	(974.828)
Equity-Indiff × High Cognitive Ability	0.220	0.024	-728.331
	(0.159)	(0.041)	(1408.736)
Debt-Indiff Mean	0.17	0.06	23,678.39
N	210	210	210

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

Notes: Sample restricted to non-devaluation cohorts (cohort≥28). High Cognitive Ability is above-median on digit span recall test from baseline. Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Table A18: Heterogeneity by Baseline Equity Preference

	(1) Bad Outcomes	(2) Default	(3) Total Value at Sale
Contract Effect (Equity-Indifferent vs. Debt-Indifferent)			
Equity-Indifferent	0.071	0.013	748.321
	(0.087)	(0.026)	(1202.720)
Prefers Equity	0.071	0.073**	-955.142
	(0.086)	(0.031)	(1062.729)
Equity-Indiff × Prefers Equity	0.118	-0.086**	-2522.711*
	(0.124)	(0.042)	(1425.152)
Debt-Indiff Mean	0.17	0.06	23,678.39
N	210	210	210

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

Notes: Sample restricted to indifferent group, non-devaluation cohorts (cohort≥28). Prefers Equity is based on forced-choice question at baseline. Selection effect panel omitted because equity-only group has no variation in baseline preference (all prefer equity by definition). Strata fixed effects included.

Table A19: Census Intake Funnel

	Number or Percent Surviving					
	Full		Muslim		Christian	
Condition	Number	Percent	Number	Percent	Number	Percent
Census Sample	11,416	100.0%	9,147	100.0%	2,269	100.0%
Livestock Experience	4,042	35.4%	3,562	38.9%	480	21.2%
Interested in Opportunity	484	12.0%	458	12.9%	26	5.4%
Decision Maker	310	64.0%	285	62.2%	25	96.2%
Enrolled	213	68.7%	191	67.0%	22	88.0%
Eligible (Any Product)	196	92.0%	176	92.1%	20	90.9%
Eligible (Equity or Indifferent)	145	74.0%	132	75.0%	13	65.0%

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 either noted from ID card or inferred based on name.

Table A20: LO Presence at Sale by Treatment Group

	Equity-Only (N=337)	Equity-Indiff (N=235)	Debt-Indiff (N=266)	p(Eq = D)
Panel A: LO Role at Sale (Free Response, LLM-Classified)				
Physically Present				
Final sale	32.0	36.6	24.1	0.004
Weighing/monitoring only	38.0	23.8	29.7	0.474
Initial attempt only	2.4	6.4	4.5	0.742
Any physical presence	*72.4*	*66.8*	*58.3*	<0.001
Not Physically Present				
Phone only	13.1	15.3	15.4	0.585
Not present	3.0	3.4	11.7	<0.001
Ambiguous	2.4	2.1	3.0	0.527
Missing response	9.2	12.3	11.7	0.614
Panel B: LO Present for Weighing (Binary Survey Question)				
Yes	73.0	68.1	56.8	<0.001
No	15.7	17.4	28.2	<0.001
Missing response	11.3	14.5	15.0	0.333

Notes: Column percentages among all borrowers who took up financing. p(Eq = D) reports p-values from two-sample t-tests comparing all equity borrowers (Equity-Only + Equity-Indifferent) to Debt-Indifferent. *Panel A*: LO presence classified from open-ended Arabic midline survey responses using an LLM (Claude Opus 4.6, temp=0; 99% test-retest agreement). *Panel B*: Binary survey question asking whether the LO accompanied the borrower to weigh the animals at the time of sale.

Table A21: Outside Financing: Intent-to-Treat Effects on Outside Borrowing

	(1) Asked Module	(2) Any Outside Loan	(3) Value: MFIs	(4) Value: Banks	(5) Value: Friends & Family	(6) Value: Total
Panel A: Equity Only						
Equity	-0.05	0.15	2,910.42*	1,663.52	315.52	2,967.51
	(0.04)	(0.12)	(1,725.36)	(3,632.46)	(550.99)	(4,542.58)
Control Mean	0.25	0.23	1,048.39	2,903.23	320.97	6,837.10
N	392	90	90	90	90	90
Panel B: Indifferent						
Equity	0.06	0.22*	-2,690.30	144.33	774.53	-1,771.43
	(0.05)	(0.12)	(2,581.17)	(415.58)	(541.66)	(2,540.05)
Debt	0.07	0.08	-2,389.09	916.99	101.18	-1,370.92
	(0.05)	(0.11)	(2,742.60)	(953.76)	(214.97)	(2,736.42)
Control Mean	0.20	0.14	2,272.73	0.00	0.00	2,272.73
N	335	76	76	76	76	76

Notes: Outcomes are from the 7-month survey's outside-borrowing module: loans from other microfinance institutions, banks, informal lenders, and friends or family. From July 2023 the instrument asked the module only of respondents reporting livestock, crop, or self-employment income (respondent or household head) and asked loan values only of those reporting having ever applied for a loan (for friends and family, ever borrowing from them for the business). Screening answers that imply no borrowing are coded as zeros, and respondents never asked the module are missing. Column 1 reports the intent-to-treat effect on being asked the module, which does not differ across arms. Columns 3–6 report the total value borrowed in the last 7 months from each source, winsorized at the top 1 percent. Loans from informal lenders are omitted as a column because only two respondents in the midline report any (they are included in the total). Sample restricted to non-devaluation cohorts. All regressions include strata fixed effects and control for a baseline ever-applied indicator (imputed, with a missing indicator), following the specification of Table 5. Sharpened q-values within this prespecified family leave no estimate significant at $q < 0.10$. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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, so the adverse-outcome indicator in Table 2 is close to a count of affected contracts rather than of overlapping episodes.[36] 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 A5. Forced sales are ordinary sales at unfavorable timing, and their proceeds enter the divided pool. 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.

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 A10 treat these cases under the same amount-based rules as ordinary completions.

[36] This appendix describes all financed equity contracts. The paper's main tables restrict to the non-devaluation cohorts, and the same facts 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.

C Structural Model Details

This appendix records the primitives, calibration targets, and assumptions behind the pricing and welfare results in Section 6. The model is intentionally minimal: it matches the experimental take-up and margins exactly, estimates a single behavioral slope from the devaluation, and treats demand away from the observed contract as a labeled, elasticity-based extrapolation rather than an identified object.

Two accounting conventions from the main text are recorded here. The first is the constant that aligns the model's equity margin with the observed one. We shift the payment schedule, the borrower's expected contractual payment as a function of the share, by a constant of 63.7 EGP in the lender's margin, so that the model's own equity pool reproduces the observed lender margin at the experimental terms, weighted by the experiment's composition of equity takers. The constant corrects a difference in composition, not a sampling error. In the model 62 percent of equity takers are equity-only; among the experiment's 225 equity takers in the non-devaluation cohorts the share is 56 percent, and reweighting those takers by their design probability of an equity offer moves the share down rather than up. Two things produce the difference. Applicants in the non-devaluation cohorts were less equity-only-heavy than the census: equity-only households are 54 percent of the equity-only and indifferent groups combined in those cohorts, against 59 percent in the census. And the model counts as indifferent equity takers only the 80 percent who prefer equity in the forced choice, whereas in the experiment every indifferent household assigned to equity could take it. The constant applies to the lender's margin only; the borrower's payment, demand, and the compensating variation use the unshifted schedule. Without it the lender gap at the observed terms is 57 EGP per census-eligible household rather than 95.

The second convention is the claim-denial share ϕ of Section 6.3. A denied equity claim reduces the divided pool, so the borrower loses $(1 - s)$ of it and the lender saves the same $(1 - s)$; a denied debt claim reaches the borrower only through a collection rate that we bound at 0 and 1; and the same payment schedule prices demand and the compensating variation throughout. When care is allowed to respond, full denial restores care incentives with the strength of the observed devaluation. We treat denial rates above roughly 0.4 as extrapolation because, among indifferent borrowers in the non-devaluation cohorts, equity raises the share of contracts with a compensated animal death by 0.090 on a debt-arm base of 0.117 (the specification of Table 2, Panel B), which supports attributing at most about two-fifths of claims to negligence.

Types and contracts. The eligible population comprises three types $\theta \in \{\text{EO}, \text{IND}, \text{DO}\}$ with population shares $\pi_\theta = (0.44, 0.30, 0.26)$ taken from the census demand elicitation. A debt contract charges interest r per cycle; an equity contract takes share s of realized venture surplus and bears the corresponding downside. The lender's per-loan margins on the two contracts are

$$\mu_D(r) = \bar{\mu}_D + P\,(r - r_0) - \ell_D, \qquad \mu_E(s) = \bar{\mu}_E\,\frac{s}{s_0} - \kappa(s) - \ell_E, \tag{C1}$$

where P is loan principal, $\bar{\mu}_D$ and $\bar{\mu}_E$ are the observed margins at the experimental terms, ℓ_D and ℓ_E are expected default losses, and $\kappa(s)$ is the moral-hazard cost defined below. The observed margins are net of realized animal losses but gross of default losses, which enter separately through ℓ_D and ℓ_E: deaths and forced sales therefore reduce margins only incrementally relative to their observed baseline rates, and no loss is counted twice. The venture surplus v_0 is the mean divided pool over

the 225 financed equity borrowers of the non-devaluation cohorts, about 3,004 EGP; it is measured directly rather than derived from the observed margin, which already disciplines the lender side. Take-up and margins at $(r_0, s_0) = (13.5\%, 1/3)$ are matched exactly, with the 63.7 EGP constant recorded above aligning the model's equity margin with the observed one.

Selection and moral hazard. Households carry two latent traits. Debt aversion b enters the payoff to debt as an additive distaste for a fixed obligation and shifts the contract-choice intercepts α_θ^E below; equity-only households carry the largest b, which is why they refuse debt. Stewardship σ enters adverse outcomes, not tastes: it lowers the bad-outcome rate, and because moral-hazard pressure scales that rate proportionally (Section 6.1), a lower baseline also mutes the response to weakened incentives; the venture-surplus response is common across households. The gaps in b and σ between equity-only and other households are calibrated to the observed selection into equity and the outcome differences across contracts (Tables 1–3). In implementation the discrete choice over the payoff indices of Section 6.1 is smoothed logistically, with heterogeneity in b integrated over a discrete distribution around each type's mean. Contract choice then takes a logistic form: the probability a type-θ household takes equity is

$$q_\theta^E(r, s) = \Lambda\Big(\alpha_\theta^E - k_E \ln \tfrac{s}{s_0} + \mathbf{1}\{\theta = \text{IND}\}\, k_D \ln \tfrac{r}{r_0}\Big), \tag{C2}$$

with Λ the logistic function, intercepts α_θ^E set to match observed take-up at (r_0, s_0), and the price coefficients k_E, k_D chosen so that the own-price demand elasticities at the experimental terms equal the target values $\varepsilon_E, \varepsilon_D$; debt take-up is defined analogously. The moral-hazard channel makes the venture surplus decline in the equity share with a common slope,

$$v(s) = v_0 - \lambda\big(g(s) - g(s_0)\big), \tag{C3}$$

where v_0 is the mean divided pool defined above and $g(s)$ is the moral-hazard pressure index rising in s; adverse outcomes respond to the same pressure in proportion to each household's baseline risk, as in Section 6.1, and both responses are governed by one estimated slope. In normal times the index is linear, $g(s) = 1 + (s - s_0)/(1 - s_0)$, so $g(1/3) = 1$ and $g(1) = 2$, and the devaluation state lowers it by one unit, to $g = 0$. That is a normalization of the period's incentive change rather than a literal residual-claimant benchmark: under the linear index a residual claimant, $s = 0$, sits at $g = 0.5$. The moral-hazard cost in the lender's margin is the lender's share of the destroyed surplus, $\kappa(s) = s\,\lambda\,\big(g(s) - g(s_0)\big)$; the borrower-side surplus in the welfare calculation carries no such factor, because the borrower bears her full share of the loss. We calibrate that slope from the devaluation period by matching the differential change in project outcomes under equity versus debt across it (Table 7), under the comparability assumption stated in Section 6.2. Because the period varied the macroeconomic state and realized payments rather than the contractual share s itself, mapping its outcome response onto a slope in s is an extrapolation we impose; we therefore treat λ as calibrated and report the compensating variation with the moral-hazard adjustment switched off ($\lambda = 0$) as a sensitivity below.

Demand and welfare. Demand away from the observed terms is governed by the own-price elasticities $\varepsilon_E \in \{-0.5, -1\}$ (reported as a range) and $\varepsilon_D = -1$, neither estimated from this experiment, which varies no prices: the debt value comes from our ongoing randomized interest-rate

experiment in Egypt (Gazeaud et al., 2026) and the preferred equity value halves it, guided by the lower price sensitivity of sharia-compliant borrowing in our Jordan experiment (Karlan et al., 2021). One feature of the forced-choice data bears on the choice between the two values. At the realized terms, an indifferent borrower's expected payment under equity, net of the compensation she receives, lies below the debt fee at every share up to one. The indifferent households who chose debt in the forced choice therefore either valued a fixed obligation for its own sake or expected the share to cost more than it did; the logistic choice treats that preference as an intercept and does not distinguish the two. Under $\varepsilon_E = -1$ the same intercept implies that removing equity's price advantage would move most indifferent households to debt, a steeper response than under $\varepsilon_E = -0.5$, which is one reason we treat -0.5 as primary. Borrower welfare from the equity option is the compensating variation of withdrawing it,

$$\mathrm{CV} = \int_{s_0}^{1} \Big(\sum_{\theta} \pi_\theta \, q_\theta^E(r_0, x) \Big) \, p(x) \, \mathrm{d}x, \tag{C4}$$

a money-metric surplus index under quasi-linear preferences in which the price of one more unit of s is the borrower's marginal contractual payment $p(x) = \partial E[\text{payment}(x)]/\partial x$, computed group by group from the payment schedule. Payments subject to a binding early-exit floor do not change one for one with the share, so the pooled marginal payment at the observed terms is 2,738 EGP, about 9% below the mean divided pool. In the primary variant this payment declines with s through the estimated sale-value response; by the envelope theorem it is the borrower's marginal welfare cost of s at chosen behavior.

At $s = 1$ the borrower keeps none of the divided surplus, but demand remains positive, because the insurance and the return of principal do not scale with s: equity participation at that share is 35 percent of its observed level under $\varepsilon_E = -0.5$ and 5 percent under $\varepsilon_E = -1$. The integral therefore values raising the share from s_0 to one, and it is a lower bound on the value of withdrawing equity. The payment schedule also gives an upper bound that needs no elasticity. Because demand weakly decreases in the share, the compensating variation cannot exceed each type's observed equity mass multiplied by the rise in its expected payment from s_0 to one: 684 EGP for equity-only households and 450 EGP for indifferent households, 1,134 EGP per census-eligible household in total. The two scenarios equal 51 and 30 percent of that bound. Quasi-linearity prices insurance only as it is revealed in take-up: to the extent borrowers value equity's protection against idiosyncratic risk beyond what demand reveals, the compensating variation is a lower bound, and the preference-based welfare magnitudes in Meki (2025) suggest this omission is not second-order. The lender's incremental profit from the menu and the welfare thresholds follow from the margins above:

$$\Delta\pi(r, s) = \Pi_{\text{menu}}(r, s) - \Pi_{\text{debt}}(r), \qquad a^{\text{crit}} = \frac{\mathrm{CV} + \Delta\pi(r_0, s_0)\big|_{a=0}}{\Delta M}, \tag{C5}$$

where a^{crit} is the common administrative cost per loan at which the menu's total surplus crosses zero and ΔM is the extra mass financed. Two alternative demand-curve variants bracket functional form. Holding the venture surplus constant, which switches the moral-hazard adjustment off ($\lambda = 0$ in the payment), gives compensating variations of 718 and 390 EGP under $\varepsilon_E = -0.5$ and -1; a constant-elasticity demand curve gives 644 and 494 EGP; the model's own logistic participation demand, which gives 573 and 337, is the primary and the most conservative.

Estimation and fit. The estimator is penalized minimum distance. Parameters are chosen to minimize the sum of squared deviations between the data moments and their model counterparts, each weighted by the inverse of its sampling variance, plus a small ridge penalty toward the starting values, which stabilizes an objective that is otherwise nearly flat along the ridge described below. The choice intercepts are solved so take-up of each contract among indifferent households and equity take-up among equity-only households match the data exactly; the price coefficients are solved so the model's own-price elasticities at the observed terms equal the target values; the venture surplus is set to the mean divided pool; and the change in the selection effect on adverse outcomes during the devaluation (Table 7, Panel A) is matched by a calibrated shifter. None of these solved or calibrated quantities adds over-identifying content. The four remaining parameters are estimated jointly from six moments: the share of equity-only households preferring equity in a forced choice (targeted at 0.98 as a finite-logit proxy for the raw value: all 890 of 890 equity-only households chose equity); the forced-sale and death-rate gaps between equity-only and indifferent borrowers under equity; the equity–debt gap in sale values in normal times; and the differential movement of adverse outcomes and sale values across the devaluation. Not all six carry independent information; three are matched by construction once the others are set. First, the forced-sale gap and the death-rate gap are not two restrictions: the split of adverse outcomes into forced sales and deaths is calibrated, group by group, from the same cells, so the two gaps together impose one restriction on the stewardship product. Second, the devaluation interaction for sale values follows from the interaction for adverse outcomes, because the sale-value loading equals the adverse-outcome loading times a constant fixed by the ratio of the two devaluation interactions in the data. That leaves three moments that each pin one parameter: the forced-choice share pins the debt-aversion gap, the adverse-outcome gap pins the stewardship product, and the devaluation interaction in adverse outcomes pins λ. The sixth moment, the normal-time sale-value gap, is the one over-identifying restriction, and the model fits it within a tenth of its sampling standard error. A full-covariance overidentification statistic is essentially zero (0.007); because it is evaluated at the penalized diagonal-weight estimate, we read it as a diagnostic rather than a formal Hansen test. Estimated values, with latent traits measured in standard deviations of their within-type distributions:

Parameter	Symbol	Estimate	SE
Extra debt aversion of equity-only households		2.55	(1.02)
Stewardship selection effect on adverse outcomes		0.22	(0.08)
Moral-hazard loading on adverse outcomes		0.39	(0.18)

The moral-hazard row reports the estimated loading in its own units: expected adverse outcomes per contract per unit of pressure. The slope λ that enters $v(s)$ is the implied monetary object, about 1,411 EGP per unit of pressure (roughly $0.47\,v_0$). With the devaluation set to one unit of pressure, that slope equals the devaluation interaction on sale values in Table 7; the two loadings are linked through the ratio of the two devaluation-interaction moments. The stewardship row reports the product of two objects from the adverse-outcome equation of Section 6.1: the gap in mean stewardship σ between equity-only and indifferent households (1.61, in within-type standard deviations) and the protective effect δ that converts stewardship into a lower baseline adverse-outcome rate (0.14). They share one row because only their product is identified: the objective is nearly flat along a ridge on which the product is constant, while the product, and every counterfactual in the

paper, is stable along that ridge. The debt-aversion estimate also depends on the strength of the forced-choice target for equity-only households, so we interpret its level qualitatively; it enters no counterfactual, since take-up at the experimental terms is matched by the choice intercepts and selection on terms runs through each type's participation. Standard errors come from a linearized parametric bootstrap. We first estimate the covariance of the empirical moment vector from 1,200 stratified household resamples, then draw 10,000 moment vectors from that covariance and propagate each through a linearization of the estimator, with the seed fixed throughout; we read the resulting intervals as a first-pass uncertainty check rather than a full resampling exercise. The average equity-only household's baseline risk, and hence its response to moral-hazard pressure, is roughly four-tenths that of the indifferent.

Sensitivity to loss-given-default. The headline figures use the baseline loss-given-default of 1.0. Because default losses enter the lender's margin linearly, the results move smoothly with this assumption, and better collateral recovery works against the menu: it is a windfall to plain debt lending, which the equity-heavy menu captures less of. The critical administrative cost at which total surplus turns negative falls from 6.2% (primary) and 3.1% (conservative) of principal at a loss-given-default of 1.0 to about 5.7% and 2.6% at 0.8, and to 3.7% and 0.7% with full recovery. The zero-profit-implied cost, the administrative cost at which the debt arm exactly breaks even at the observed rate, moves the other way, from about 4.1% of principal at a loss-given-default of 1.0 to 9.9% under full recovery. At that implied cost the menu fails the total-surplus test under the conservative elasticity at every recovery assumption, while under the primary elasticity it passes at the baseline, narrowly survives at 0.8, and fails only under full recovery. Assumed strong recovery therefore weakens the normal-state case for the menu. In stress states the sign reverses: to the extent recoveries deteriorate when many borrowers are in difficulty at once, as during the devaluation period, the same assumption that hurts the menu in normal times favors it in the states where insurance matters.

Uncertainty. We report two procedures. The first is the linearized parametric bootstrap described above. It draws the six behavioral moments from their bootstrapped covariance and holds every other input (the payment schedules, insurance costs, the debt margin, default rates, and take-up) at its sample value. Propagating 10,000 draws through the headline calculations gives, at the baseline loss-given-default: a lender gap of −95 EGP with a 95% interval of −96 to −91; a compensating variation of 573 EGP (444 to 703) under $\varepsilon_E = -0.5$ and 337 (289 to 384) under $\varepsilon_E = -1$; critical administrative costs of 6.2% of principal (4.6 to 8.0) and 3.1% (2.6 to 3.8); and benefit-cost ratios at a 2.5% cost of 2.0 (1.6 to 2.5) and 1.2 (1.0 to 1.4), falling at a 5% cost to 1.2 (0.9 to 1.5) and 0.7 (0.6 to 0.8). The lender-gap interval is tight because at the observed terms the gap depends only weakly on the behavioral parameters; it is mostly arithmetic on the sample inputs, which this procedure holds fixed.

The second procedure resamples households. We draw 500 bootstrap samples of the analysis data within randomization strata. In each sample we recompute the six fitted moments, the take-up and forced-choice inputs, each group's payment schedule, insurance cost, and divided pool, and the forced-sale schedule; we recompute the default rates and the debt margin as changes around their baseline values; we then re-estimate the four parameters and recalculate the headline results. Census shares stay fixed throughout. The lender gap at the observed terms then has a bootstrap standard error of 172 EGP and a 95% interval of −438 to +227 EGP, with the menu ahead of debt-

only in 29 percent of replicates. Five inputs, each measured on about a hundred borrowers, account for most of that spread: the debt-arm default rate (a one-standard-error move of 0.02 shifts the gap by about 99 EGP), the insurance costs of the two equity groups (about 65 EGP per standard error each), and their contractual payments at the observed share (about 60 and 25 EGP). The behavioral parameters contribute little, and their own standard errors are close to the first procedure's (0.17 for λ against 0.18). The compensating variation is 573 EGP (374 to 788) and 337 (248 to 431), positive in every replicate; the critical administrative costs are 6.2% (0.3 to 12.2) and 3.1% (-1.9 to 7.9); and the menu passes the total-surplus test at a 2.5% cost in 88 and 60 percent of replicates. We therefore read the sign of the lender gap as the model's central estimate rather than as a precisely measured fact. On the borrower side the elasticity bracket remains the larger source of uncertainty, since the two scenarios' compensating-variation intervals overlap only at their edges, which is why we report every result under both values.

Scope. Every quantity evaluated away from (r_0, s_0) is conditional on the labeled elasticities, for which the experiment provides no direct price variation, and all headline figures are for the normal macroeconomic state. The devaluation enters only as identifying variation for the moral-hazard slope; the inflationary episode raised nominal revenues and eroded fixed debt, so it is not a downside state in which equity's insurance value would be large, and we do not use it to argue one.

Out-of-sample validation. We evaluate the estimated model on outcomes and distributional features excluded from the estimation objective. Table C1 organizes the checks around the three claims the model rests on: that bad-outcome risk is the channel linking selection and contract incentives to sale values, that the devaluation reverses the normal-state pressure one for one, and that borrower surplus ranks demand across groups. Each discrepancy is reported in units of the data cell's sampling standard error, so successes and failures are on the same scale.

On the first claim, the model approximately matches four untargeted sale-value magnitudes, the sale level for each of the three borrower groups and the equity-only versus indifferent sale gap, all within about 1.3 sampling standard errors under the mapping from bad-outcome risk to sale values described in the notes to Table C1. Most clearly, the model predicts that selection and contract effects should concentrate in the lower tail of the sale-value distribution, and across thirty-eight never-fitted quantile coefficients (nineteen quantiles for each of the two contrasts, the set reported in Section 6.2) they do: the mean selection effect is about 1,447 EGP at or below the 35th percentile against 93 at or above the 65th, and the mean contract effect is about $-1{,}986$ against -79 (Figure A3). On the second claim, the devaluation change in weekly feed spending, the one outcome never used in estimation, reverses the normal-state feed gap with a ratio of 1.3 against the model's restriction of one, within half a sampling standard error. The same imposed reversal reproduces the devaluation interactions for sale value and bad outcomes to within one standard error, though those two moments are fitted, so we read that pair as a consistency check rather than a holdout. The validation also reveals failures, which we report alongside the successes. During the devaluation the model predicts forced-sale and death rates close to zero for equity-indifferent borrowers, against observed rates of 0.09 and 0.11, misses of roughly three and a half standard errors. Its restriction that debt borrowers' risk does not vary with the state misses their devaluation forced-sale rate (0.10 against a predicted 0.05). And the imposed devaluation response predicts no change in the selection effect on adverse outcomes, against an observed 0.26; a separately calibrated

adjustment matches that change. On the third claim, stated demand moves with modeled surplus in all four available contrasts. Among indifferent borrowers, equity recipients report higher repeat-borrowing intent than debt recipients (0.08, SE 0.05); assignment to equity raises the preference for equity in a next contract by 0.27 (SE 0.06); equity-only borrowers report greater repeat intent than equity-indifferent borrowers (0.02, SE 0.06); and repeat intent is 0.21 (SE 0.03) higher in devaluation cohorts, where the modeled option value is also larger (Tables 6 and 7). These are sign tests under the assumption that stated demand rises with realized surplus; they do not validate the level of compensating variation. Finally, we re-estimate the model four times, each time dropping a block of fitted moments, and the re-estimated model predicts the dropped moments to within 0.21 standard errors.

Table C1: Out-of-Sample Validation Summary

Check	Data	Model	Gap (SEs)
A. Does bad-outcome risk drive sale values?			
Sale objects never fitted; each group's sale value is priced from its predicted bad-outcome risk.			
Sale level, equity-only (kEGP)	23.21 (0.44)	23.58	+0.8
Sale level, equity-indifferent (kEGP)	22.37 (0.57)	21.97	−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)	1.61	+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?			
The devaluation must reverse normal-state pressure one for one; cells use the paper's devaluation definition.			
Feed interaction, deval (EGP/week)	+110.6 (69.2)	+86.9	−0.3
Forced/death rates, equity-only	0.123 / 0.142	0.087 / 0.145	−1.4 / +0.1
Forced/death rates, equity-indiff.	0.088 / 0.110	0.003 / 0.003	−3.5 / −3.5
Forced/death rates, debt-indiff. (invariance)	0.103 / 0.168	0.045 / 0.126	−2.4 / −1.2
Deval selection shift, no calibrated shifter	+0.259 (0.090)	0.000	−2.9
C. Does realized surplus rank demand as predicted?			
Stated repeat demand should rise with each group's modeled surplus from the equity option.			
Repeat-demand orderings (four contrasts)	4 of 4 signs agree	all positive	–
D. Do the fitted blocks predict one another?			
Drop a block of fitted moments, re-estimate, and predict the dropped block from what remains.			
Leave-block-out rotations, six excluded moments	moment targets	re-predictions	max 0.2

Notes: Model values are computed from the estimated parameters without reference to the data column; where the model implies only a sign or a shape, that implication is stated in place of a number. The gap column is the model−data difference in units of the data cell's sampling standard error; it reports discrepancies on a common scale but is not a formal test, because parameter uncertainty is not propagated. Panel A prices each group's sale value by its predicted bad-outcome risk using the pooled good- and bad-outcome sale anchors; none of its sale objects is fitted, and the quantile rows summarize nineteen never-fitted quantile regressions per contrast by their 5th–35th and 65th–95th percentile means. Panel B's feed row imposes the model's one-unit pressure reversal on the estimated normal-state feed gap, the one outcome never used in estimation; its devaluation cells (cohorts 27 and earlier, the paper's definition) are joint tests entangled with the fitted devaluation aggregates, and the final row quantifies the devaluation selection shift, which the model matches only through a calibrated shifter. Panel C's four contrasts, re-estimated from the current data under the paper's specifications, are listed in Appendix B's text with coefficients and standard errors; the check assumes stated demand rises with realized surplus. Panel D re-estimates the model four times, each time dropping a block of fitted moments, and predicts the dropped block; the preference rotation is partly mechanical. Every data cell is re-derived from the analysis dataset by the exhibit's build scripts.

Three further checks bear on the model's premises. Starting animal quality is balanced across the selection and contract contrasts (six contrasts, all within 1.2 standard errors of zero), which rules out the possibility that groups simply began with different animals; random post-assignment shocks to animal development remain, but they are orthogonal to contract assignment by design. Equity-only borrowers tilt toward the traits a stewardship interpretation suggests (more education, higher digit-span recall and reciprocity), while survey measures of debt aversion do not predict the group, and compensation shares among bad-outcome cases are close to equal across groups.

Against these, preference-by-contract interactions offer suggestive evidence against treating debt aversion and stewardship as fully independent within the indifferent group (the default interaction is −0.09, SE 0.04). Because several checks share samples or auxiliary assumptions, we read them as correlated clusters of evidence rather than independent tests, and we interpret the results as convergent but incomplete support for the model's selection, incentive, and demand mechanisms.

D Reconciliation with the Pre-Analysis Plan

The experiment was registered on the AEA RCT Registry as trial AEARCTR-0009797 on July 22, 2022, before data collection began, and the analysis plan was accepted as a Stage 1 registered report at the *Journal of Development Economics* (proposal August 2022, revised October 2023). This appendix reconciles the registered plan, in its revised form, with the analysis in the paper. Table D1 maps each planned element to its implementation and location. The rest of the appendix reports the feasible prespecified analyses that do not appear in the body of the paper and explains the deviations that need more than a table row. Analyses developed after registration, including the devaluation analysis in Section 5 and the model in Section 6, are labeled as added.

D.1 Analysis sample

The plan analyzes every randomized client. The main tables restrict to the 24 cohorts financed after the devaluation period, cohort 28 onward, which hold 727 of the 1,731 randomized clients and 336 of the 838 takers. Section 2.5 gives the reason: the 2023–2024 devaluations changed the relative price of the two contracts in mid-study, and Section 5 treats the exposed cohorts as a separate experiment. The full sample enters with devaluation interactions in Table 7 and in Appendix Tables A7, A8, and D4, and Appendix Table A9 moves the cutoff from cohort 26 to 29.

D.2 Attrition

Attrition is minimal at both follow-ups. Of the 1,901 randomized clients outside the pilot cohorts, 99.1 percent completed the 7-month survey and 98.8 percent the 13-month survey. Completion does not differ by treatment assignment within either preference group (the largest coefficient is one percentage point), and joint tests of treatment-by-baseline-characteristic interactions do not reject (p-values from 0.48 to 0.80; Table D2). Because the response differentials are at most half a percentage point, the Lee (2009) bounds for 13-month income in Table D3 sit tightly around the unadjusted differences.

D.3 Baseline characteristics by preference group

Equations (2) and (3) of the plan carry cohort and baseline-enumerator fixed effects, and Table 1 includes both. The enumerator effects matter for the joint test. With cohort effects alone the fifteen characteristics jointly predict who is equity-only ($p = 0.05$); with the enumerator effects added they do not ($p = 0.30$), because the enumerator who conducted the baseline interview explains as much of the equity-only choice as the cohort does, and the two together leave 368 cells of about five clients. The differences in education, digit span, and quantitative risk tolerance are unchanged or larger under the plan's specification; the differences in the qualitative risk measure and in reciprocity, significant with cohort effects alone, are not.

The plan's comparison group also contains applicants willing to take only debt, whom Equation (2) folds into the group that applies for debt and Equation (3) carries as a separate indicator. These 219 applicants completed the baseline survey and were not enrolled, so they have no cohort; Appendix Table D9 reports the three-group comparison with baseline-month fixed effects in place of cohort effects, alongside the enumerator effects. Debt-only applicants are the most educated and most male of the three groups, with three more years of schooling than the indifferent group and 18 percentage points fewer women. Against the plan's comparison group the equity-only education

advantage halves, to about half a year and insignificant, and the joint test does not reject at p = 0.32 (Appendix Table D9). Against the indifferent group, the comparison that holds the contract fixed as the outcome analysis does, the pattern of Table 1 stands.

D.4 Census refusals and the religion prompt

The plan committed to recording how many census households declined for religious reasons and then revised their answer once told that the products are permissible. Two features of the census form matter for reading the counts. First, the screening question asked only whether the household would like to hear about an opportunity to expand its flock; financing, the lender, and the contracts had not been mentioned, and the reasons offered were practical ones. Second, religion was not among those reasons: the item was a note by the enumerator that the household had raised religion on its own, followed by the Dar Al-Ifta prompt. It was a precaution against households declining on the assumption that the offer was an interest-bearing loan, and for the same reason the products were introduced at consent as *musharaka* and in-kind financing rather than as loans. Of the 3,558 households that declined at the screen, 2,970 were probed (the item entered the form after the census's first three days). Among them, 51 percent cited input costs, 25 percent no place to keep animals, 18 percent the risk of losing them, and 24 percent gave a reason of their own (most often that they already had enough animals or did not want the project); enumerators noted a religious remark for 44, or 1.5 percent, all of whom were told that both products carry Dar Al-Ifta approval and asked again, and one agreed to continue. Of the 310 financial decision-makers who heard the introduction, 97 declined to participate. The most common reasons were again input costs, space, and the risk of loss; two mentioned religion, five said they did not like loans, and neither of the two changed after the prompt. These counts show that few households turned the offer away on religious grounds when it was first put to them. They do not measure how religion shapes the choice between the two contracts, which the demand results in Section 3 address.

D.5 Income measurement and the sale-proceeds probe

The 7-month survey asks for total income in the previous month: the respondent's own, or the main earner's when the respondent reports no income source (Table 5, notes). Partway through the fieldwork the field team noticed that takers interviewed just after selling the financed animals reported no rise in income, and from 9 August 2024 the survey asked directly whether the reported figure included the proceeds from the ABWA animals. Appendix Table D11 reports who was asked and what they answered. Of the 368 takers asked, 332 (90 percent) said the figure did not include the proceeds. The total income measure in Table 5, column 1, therefore adds the borrower's computed contractual share of the sale to the survey figure, except for takers who said the figure already included the proceeds, whose column 1 is the figure as reported.

Coverage differs by sample. In the non-devaluation cohorts, the primary sample, 326 of the 336 takers (97 percent) were asked and 11 percent of those asked (35 takers) said the figure included the proceeds. All primary-sample interviews took place after the question was added; of the ten takers not asked, nine had no income figure to ask about and one has a missing answer. The devaluation-period cohorts were surveyed between March 2023 and October 2024, and 459 of their 502 takers (91 percent) were interviewed before the question was added; 42 were asked and one said yes. The share is added for nearly every devaluation-period taker, so a devaluation-period taker who did count the proceeds as income has them counted twice; if the primary sample's rate carries over, that is about one taker in ten. No full-sample table uses this measure. The monitoring

test in Appendix Table D5 and the 13-month comparisons in Tables D4 and D3 use the survey figure alone, and Table 7 does not include income.

D.6 Thirteen-month outcomes

The 13-month survey was designed to capture persistence and any effects on subsequent borrowing. The plan designates the 7-month outcomes as the primary analysis, anticipating that longer-term impacts would be muted once the six-month contract ended, and the regulatory delay that prevented a second financing round (Section 2.3) removed the subsequent borrowing margin entirely. The paper's outcome analysis accordingly centers on the 7-month survey, which covers the full production cycle. Table D4 reports intent-to-treat effects on the outcomes the 13-month survey does carry: household income and the psychological module.

Two results follow. First, income differences at 13 months are small and statistically insignificant; the equity-only point estimate is −227 EGP (SE 326) against a control mean of roughly 3,800. This is the expected pattern rather than a reversal. The 7-month gains are concentrated in proceeds from the financed animals (Table 5, column 1 versus column 2), and no second cycle followed, so there is no channel through which those gains would recur at 13 months. Second, the psychological costs for equity-only borrowers do not fade. At 13 months, financing raises self-reported stress by 10.6 points (SE 4.2), the GAD-2 score by 0.39 (SE 0.18), and the share above the GAD-2 screening threshold by 10.4 percentage points (SE 5.2). Each estimate is larger than its 7-month counterpart, and each survives the multiple-testing adjustment in Table D6. Effects for the indifferent group remain near zero under either contract, so the reading in Section 4, that the psychological cost attaches to taking on a formal obligation rather than to the contract form, carries through; the caveat is that the cost now looks persistent rather than transitory.

D.7 Monitoring under equity versus debt

Equation (6) of the plan compares monitoring effects across the two contracts directly, with the equality of the equity and debt monitoring interactions as the test of interest. Table D5 implements it: each outcome is regressed on a contract main effect and separate monitoring indicators for the equity and debt arms, across the plan's four primary outcomes (total income, total livestock revenue, product-specific livestock revenue, and delinquency) plus the adverse-outcome indicator. The within-arm monitoring effects are small throughout, and the equality of the equity and debt monitoring effects is never rejected (p-values from 0.17 to 0.84). This matches Section 4.5: light-touch monitoring did not measurably move outcomes under either contract.

D.8 Multiple-hypothesis testing

The plan commits to Anderson (2008) sharpened q-values for its families of secondary outcomes, with the four primary outcomes reported under unadjusted inference; the original submission additionally proposed adjusting the Hypothesis 1 characteristic comparisons. Table D6 reports q-values within family for the secondary outcomes the paper analyzes, for the characteristic comparisons, and for the mechanism measures, each under the specification of the corresponding paper table. Adjustment changes little. Every comparison significant at the five percent level keeps a q-value below 0.10, including the baseline education, cognitive, and reciprocity differences behind the advantageous-selection result and the contract effect on stated preference for equity. Four estimates significant only at the ten percent level move above the 0.10 threshold: the qualita-

tive risk-tolerance difference in Table 1 (q of 0.18) and the three 7-month psychological effects for equity-only borrowers (q of 0.12), which the paper already treats as suggestive. Q-values for the outside-financing family are computed within family and reported with its analysis in Appendix Table A21; none falls below 0.10.

D.9 Double post-lasso controls

Equation (4) of the plan specifies double post-lasso selection of baseline controls. The paper instead uses design fixed effects with baseline controls chosen ex ante: data-driven selection adds little in samples of this size, and fixing the controls in advance removes any scope for specification search. We report the prespecified procedure as robustness. Table D7 re-estimates each headline coefficient with double selection over the plan's baseline characteristic list, keeping the design fixed effects. No coefficient changes sign. Two estimates move across conventional significance thresholds: the contract effect on feed costs gains a 10 percent star (−86.93 to −91.40), and the debt effect on income moves from the 5 to the 10 percent level (1,269 to 1,102). Among the animal outcomes the largest movement is in the selection effect on deaths, from −0.11 to −0.13.

D.10 Winsorization

The plan specified winsorizing outcomes at the 99th percentile. The paper winsorizes the survey-based outcomes at the top 1 percent and reports the administrative outcomes unwinsorized, since the repayment ledger and sale records are complete and their extreme values are real transactions. Table D8 applies the literal rule to the continuous administrative outcomes of Table 3 and re-estimates the same specifications. No coefficient changes sign or crosses a conventional significance threshold; the largest movement is in the contract effect on total sale value, from −1,385 to −1,360 EGP, and the net-revenue estimates are identical.

D.11 Vet-observed measures

The plan's first secondary family is what the veterinarians observed at monitoring visits: animal health, borrower neglect, and feed quality. Two visit logs exist, kept by the J-PAL veterinarian and by the ABWA veterinarian; they cover different visits, and where both cover a borrower their scores are essentially uncorrelated. Among the 371 monitored takers, 346 have at least one recorded visit, none of the unmonitored takers do, and the neglect flag appears twice, so it cannot be tested. Appendix Table D10 compares the veterinarian's health score and the share of visits with feed rated good across preference groups and across contracts among monitored takers in the non-devaluation cohorts, from each log and from both together. On the J-PAL veterinarian's scores, the animals of equity-only borrowers score about a third of a point above those of equity-indifferent borrowers, on a ten-point scale with a mean near 6.3, and those of equity-indifferent borrowers a quarter point below those of debt borrowers, the ordering of the adverse-outcome results in Table 2; the ABWA log shows no differences, and the combined measure is small and insignificant. With 20 to 53 borrowers per group (45 to 92 per regression), these estimates are suggestive at most.

D.12 Deviations from implementation

Two deviations reflect implementation rather than analysis choices. First, the plan proposed the borrower's realized goat-versus-sheep mix as a mechanism for contract effects on risk-taking. In practice the product standardized to sheep: 2 of 838 takers with recorded animal deliveries received

any goats, so no actual-choice variation exists, and the hypothetical project-scope elicitation in Appendix Table A4 is the feasible substitute. Second, the plan intended the veterinary visits to identify overt negligence that could trigger repossession and a fee. The vets did record a structured animal-condition category that includes neglect, and flagged 3 of the 385 financed borrowers with an assessment across all cohorts (2 of the 346 in the analysis sample), so the penalty was operative on paper and essentially never invoked; with so few cases there is no test of the negligence channel.

Table D1: Reconciliation of the Pre-Analysis Plan and Implemented Analyses

Planned analysis	Status	Location or explanation
Design and sample		
Randomization balance	As prespecified	Appendix Table A1
Randomization strata: stated preference and gender	Modified	Randomization was stratified by cohort and stated preference. Gender entered the strata only in the first two cohorts (21 clients); with cohorts of a few dozen clients split across up to five arms, a second stratifier would have left many strata too small to balance, so it was dropped. Strata fixed effects follow the randomization files; gender is a balance covariate (Appendix Table A1), and Appendix Table A15 finds no heterogeneity by gender
Analysis sample for borrower outcomes: all randomized cohorts	Modified	The main tables restrict to the 24 cohorts financed after the devaluation period (cohort 28 onward: 727 of the 1,731 randomized clients and 336 of the 838 takers), because the 2023–2024 devaluations changed the contracts' relative prices in mid-study (Section 2.5). The full sample enters with devaluation interactions in Table 7 and Appendix Tables A7, A8, and D4; Appendix Table A9 varies the cutoff from 26 to 29

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Planned enrollment of 1,875 farmers (revised plan; 1,125 offered funding at a forecast 80 percent take-up)	As prespecified; analyzed sample modified	1,901 clients were randomized outside the pilot cohorts against the 1,875 target, and 1,884 of them completed the consented midline survey. The plan fixed the bank's funding envelope at roughly 900 contracts, and with take-up above the 80 percent forecast (86 to 92 percent by arm over the full implementation; take-up by period and arm is in Appendix Table A2), that envelope was exhausted after 1,050 offers rather than the planned 1,125; 906 contracts were financed. The realized offer share was 55 rather than 60 percent because more applicants than planned were equity-only (52 versus 40 percent of enrollees): the lender chose to enroll them rather than turn interested applicants away, and offer ratios were reduced to stay within the envelope. The analyzed sample of 1,731 excludes one loan officer's fraudulent later cohorts; Appendix Table A14 varies the exclusion rule
"Neither" preference group	Clarification	Eq. 3 anticipates four preference groups, including "neither"; in the implemented consent flow, clients enrolled by choosing equity-only, debt-only, or both, so declining both products meant declining the study, and no baseline record exists for this group. Debt-only applicants were surveyed, and a subset was randomized to receive debt for credibility, but per the plan they are excluded from the analysis samples (Section 2.2)
Attrition tests and Lee bounds	Reported here	Tables D2 and D3; the bounds are for the 13-month income comparison, and completion exceeds 98 percent at both follow-ups

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Census: decliners who revise after the religion prompt (plan footnote 13)	Reported here	Of the 3,558 census households that declined the opportunity, enumerators recorded a religious reason for 44 of the 2,970 probed (the probe entered the form after the first three days); all 44 were told that both products carry Dar Al-Ifta approval and were asked again, and one revised. Two of the 97 who declined study consent gave a religious reason, and neither revised (Section 2.4)
Hypothesis 1: demand and selection		
Stacked equity/debt application regression (Eq. 1), and its take-up version (plan footnote 24)	Equivalent implementation	Section 3 reports preference-group shares in both samples, which carry the same comparison; the stacked coefficient adds no separate information. In the census, 86 of the 196 eligible households chose equity only, 59 either product, and 51 debt only; none enrolled while declining both. The take-up version appears as take-up rates by arm and in Appendix Table A2

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Baseline characteristics by preference group (Eqs. 2 and 3)	Modified	Table 1 compares equity-only with indifferent applicants, with cohort and baseline-enumerator fixed effects as the plan specifies: column 2 implements Eq. 3 for the two enrolled groups and columns 3 and 4 implement Eq. 2. Two departures. First, the plan's comparison group also contains applicants willing to take only debt, who completed the baseline survey but were not enrolled (Section 2.2); Appendix Table D9 adds them, with baseline-month fixed effects in place of cohort effects because they were never randomized. Second, of the plan's listed characteristics, age, years raising small livestock, monthly livestock profit, and the livestock share of income appear by experimental arm in Appendix Table A1 rather than by preference group, and the table adds reciprocity, anxiety, stress, last-month income, and livestock ownership
Flexible (multinomial) prediction of preference groups	Not separately reported	Table 1's characteristic-by-characteristic comparison and joint test cover the same ground
Sharpened q-values for secondary families and characteristic comparisons	Reported here	Table D6; the characteristic-comparison adjustment was proposed in the original submission and retained here. The hypothetical-scope comparisons (Appendix Table A4) and the heterogeneity tables are reported with unadjusted p-values
Hypothesis 2: borrower outcomes		

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Primary outcomes: total income, total livestock revenue, product-specific livestock revenue (price and weight), days delinquent	Modified in two definitions	Total income is last-month income (the respondent's own, or the main earner's when the respondent reports none; Table 5, notes) plus the borrower's computed share of the financed animals' sale, added unless the respondent said the reported figure already included the proceeds, as 35 of the 326 primary-sample takers asked did (Appendix Table D11; Table 5, column 1, with column 2 the survey figure alone). Total livestock revenue is not reported: the survey's monthly livestock-revenue question captures the household's regular livestock activity, which is zero for most borrowers and does not pick up the one-off sale of the financed animals, so the administrative sale value is the only revenue measure that reaches the financed animals (Section 2.3). Product-specific revenue, price, and weight are in Table 3; days delinquent, continuous and at five thresholds, in Appendix Table A10
Mechanism outcomes: grazing hours, feed spending, high-quality feed type, goat share	Partially reported	Grazing hours and feed spending in Table 4; the goat share is infeasible (see below). Feed type was recorded at the 7-month survey but is not reported
Selection effect (Eq. 5): equity-only versus indifferent under equity, cohort fixed effects, test $\beta_1 = \beta_2 - \beta_3$	As prespecified	The "Selection Effect" row of Table 5 and of Appendix Table A8 reports the prespecified test. Product outcomes exist only for takers, so Tables 2–4 compare equity takers across preference groups directly, the comparison the plan's Hypothesis 2 describes

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Contract effect: the outcome specification restricted to the indifferent group	As prespecified	Panel B of Tables 2–4 (takers) and of Table 5 (intent to treat, with the Wald test). The plan's text points to "equation 6" for this comparison; that is the monitoring specification, and the outcome specification is the one implemented
ANCOVA with double post-lasso controls, Huber–White standard errors, and a Wald test of equity = debt (Eq. 4)	Modified; robustness reported here	The paper uses design fixed effects with the baseline value of the outcome as the control, imputed with a missing indicator (numerically the same as the plan's zero-and-indicator rule), and robust standard errors throughout. The equity-versus-debt test is the "Contract Effect" row of Table 5 and Appendix Table A8. Table D7 shows the prespecified selection procedure leaves every headline coefficient essentially unchanged
Treatment-on-the-treated via randomized-offer IV	Not separately reported	Take-up is 86–92 percent across arms over the full implementation, so ITT and TOT effects differ by at most a factor of 1.16; product outcomes exist only for takers
Pooled equity-versus-control comparison	Not separately reported	Named in the PAP's power discussion rather than as a hypothesis; the preference-stratified estimates in Table 5 are strictly more informative
13-month outcomes	Reported here	Table D4; the plan designates the 7-month outcomes as primary, and the lender could not offer the planned second financing round (Section 2.3), so effects on subsequent borrowing are not identified
Psychological outcomes at both follow-ups	As prespecified	7-month: Appendix Table A8; 13-month: Table D4
Secondary family: product revealed preference (satisfaction, preferred product, would take each product now, amount)	As prespecified	Table 6 and Appendix Table A4, Panel B; q-values in Table D6

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Secondary family: hypothetical project scope	As prespecified	Appendix Table A4; the intended sheep/goat mix appears with the balance table
Secondary family: outside financing (MFIs, banks, family, informal)	Reported	Appendix Table A21 and the accompanying footnote in Section 4; the survey asked the module of one third of respondents, with entry balanced across arms
Distributional analysis (variance equality, quantile regressions)	Reported	Quantile treatment effects in Appendix Figure A3; variance-equality tests in Appendix Table A12 and the accompanying footnote in Section 4
Winsorize outcomes at the 99th percentile	Modified; robustness reported here	Survey-based outcomes winsorized at the top 1 percent; administrative outcomes left unwinsorized by design. The literal rule applied to administrative outcomes changes nothing (Table D8)
Actual goat-versus-sheep asset choice	Infeasible	The implemented product standardized to sheep: 2 of 838 takers with recorded deliveries received any goats; hypothetical project scope appears in Appendix Table A4
Generic machine-learning heterogeneity	Not implemented	Targeted heterogeneity by the salient baseline traits appears in Appendix Tables A15–A18; the generic exercise is underpowered at these sample sizes
Hypothesis 3: monitoring		
Monitoring effects with contract equality test (Eq. 6)	Reported here; modified	Section 4.5 and Appendix Table A7 report within-arm effects; Table D5 adds the prespecified equality test, estimated on takers in all cohorts with strata fixed effects and without the plan's post-lasso controls, on the plan's four outcome families plus the adverse-outcome indicator; sale weight is not among its outcomes

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Table D1 (continued)

Planned analysis	Status	Location or explanation
Secondary family: vet-observed measures (animal health, neglect, feed quality)	Partially infeasible; two measures reported here	Recorded only at monitoring visits, so the sample is the monitored takers: 346 of 371 have at least one assessment, and none of the unmonitored takers do. The neglect flag has no variation to test the penalty channel (2 flags among the 346; 3 among the 385 financed borrowers with an assessment across all cohorts). Animal health (the vet's 1–10 score) and feed quality are compared by contract in Appendix Table D10, from each of the two visit logs and from both together
Analyses added after the plan		
Devaluation analysis (Section 5)	Added	Motivated by the unforeseen 2023–2024 currency devaluations
Structural pricing model (Section 6)	Added	Developed after data collection

Notes: Status labels follow the disclosure conventions in the pre-analysis-plan literature: "as prespecified" (implemented as planned), "modified" (implemented with a documented change), "infeasible" (could not be implemented, with the reason), "not separately reported" (a justified presentation decision), and "added" (developed after registration). Analyses added after registration are exploratory in the usual sense; their inference does not carry the same protection against specification search as the prespecified results.

Table D2: Survey Completion by Treatment Arm

	(1) Completed 7-Month Survey	(2) Completed 13-Month Survey
Panel A: Equity Only		
Equity	0.000	0.002
	(0.006)	(0.006)
Control Mean	0.993	0.991
Joint p-value (interactions)	0.475	0.643
N	981	981
Panel B: Indifferent		
Equity	0.007	-0.003
	(0.009)	(0.010)
Debt	0.010	-0.003
	(0.009)	(0.010)
Control Mean	0.983	0.987
Joint p-value (interactions)	0.802	0.723
N	920	920

Notes: Sample is all randomized individuals excluding pilot cohorts and the separately randomized debt-only group. Completion is an indicator for appearing in the consented survey data at each follow-up. The joint p-value tests all interactions of treatment indicators with six baseline characteristics (education, gender, cognitive score, risk tolerance, winsorized last-month income, livestock ownership). All regressions include strata fixed effects. Robust standard errors in parentheses.

Table D3: Lee Bounds for 13-Month Income

	Treat. Response	Comp. Response	Unadjusted Difference	Lower	Upper
Equity Only: Equity vs. Control	0.993	0.989	-564***	-629	-549
			(190)		
Indifferent: Equity vs. Control	0.984	0.987	167	156	213
			(247)		
Indifferent: Debt vs. Control	0.984	0.987	70	59	116
			(233)		
Indifferent: Equity vs. Debt	0.984	0.984	97	86	141
			(249)		

Notes: Lee (2009) bounds for 13-month last-month household income (excluding ABWA animal-sale revenue, winsorized at the top 1%). The observed outcome distribution of the higher-response group is trimmed from the top (lower bound) or bottom (upper bound) by the response-rate differential; trimming is by observation count because the winsorized upper tail is a mass point. The unadjusted difference is the difference in means among respondents, with a robust standard error in parentheses. Sample is all randomized individuals excluding pilot cohorts and the separately randomized debt-only group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D4: 13-Month Borrower Outcomes: Income and Psychological Well-Being

	(1) Total Income Last Month	(2) Stress Level (0 to 100)	(3) GAD-2 Score	(4) GAD-2 Above Threshold	(5) Financial Worry
Panel A: Equity Only					
Equity	-226.84	10.55**	0.39**	0.10**	0.18
	(325.91)	(4.17)	(0.18)	(0.05)	(0.13)
Equity X Deval	-511.52	-9.95*	-0.53**	-0.15**	-0.12
	(396.04)	(5.55)	(0.23)	(0.07)	(0.17)
Control Mean	3804.8	48.02	2.042	0.401	1.454
N	866	851	856	856	856
Panel B: Indifferent					
Equity	296.25	3.75	0.28	0.02	0.04
	(452.97)	(5.28)	(0.22)	(0.07)	(0.16)
Debt	-5.72	-1.97	0.04	0.03	0.18
	(418.84)	(5.20)	(0.22)	(0.06)	(0.16)
Equity X Deval	-224.53	-6.78	-0.33	-0.07	0.01
	(531.26)	(6.92)	(0.29)	(0.09)	(0.21)
Debt X Deval	399.63	3.06	0.05	-0.02	-0.13
	(487.95)	(6.81)	(0.29)	(0.09)	(0.21)
Control Mean	3353.0	51.48	2.054	0.405	1.502
N	813	788	793	793	793

Notes: Outcomes are measured in the 13-month (endline) survey. Column 1 is last-month total household income excluding ABWA animal-sale revenue, winsorized at the top 1%, and controls for baseline income (imputed, with a missing indicator) as in Table 5. Columns 2–5 follow the corresponding 7-month specifications in Appendix Table A8 and control for the baseline value of the outcome. Devaluation indicator equals 1 if the contract could have spanned either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). All regressions include strata fixed effects. Robust standard errors in parentheses.

Table D5: Monitoring Effects Under Equity and Debt (PAP Specification)

Outcome	Equity Monitoring	Debt Monitoring	Difference (Eq. − Debt)	Equality p-value	N
Any Delinquency 30+ Days	0.015	0.010	0.006	0.840	815
	(0.013)	(0.026)	(0.029)		
Any Death or Forced Sale	-0.026	0.033	-0.059	0.348	815
	(0.034)	(0.053)	(0.063)		
Total Value at Sale	-87.26	-603.46	516.20	0.468	815
	(404.25)	(584.36)	(711.11)		
Value/KG at Sale	0.86	-3.00	3.86	0.166	815
	(1.53)	(2.33)	(2.78)		
Total Income Last Month	440.96	653.71	-212.75	0.686	788
	(340.03)	(414.86)	(525.27)		

Notes: Each row is a separate regression of the outcome on a contract main effect (equity) and separate monitoring indicators for the equity and debt arms, following the monitoring specification in the pre-analysis plan. Sample is all takers in the equity and debt arms. The difference column reports the equity-monitoring effect minus the debt-monitoring effect; the p-value tests their equality. All regressions include strata fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D6: Sharpened Q-Values for Secondary Outcomes and Characteristic Comparisons

Family	Outcome	Coeff.	SE	p-value	Sharpened q-value
13-month: EO, equity					
	Stress level (13m)	10.55	4.17	0.012	0.062
	GAD-2 score (13m)	0.39	0.18	0.029	0.062
	GAD-2 above threshold (13m)	0.10	0.05	0.045	0.064
	Financial worry (13m)	0.18	0.13	0.168	0.092
	Total income last month (13m)	-226.84	325.91	0.487	0.242
13-month: IND, debt					
	Financial worry (13m)	0.18	0.16	0.273	1.000
	GAD-2 above threshold (13m)	0.03	0.06	0.594	1.000
	Stress level (13m)	-1.97	5.20	0.705	1.000
	GAD-2 score (13m)	0.04	0.22	0.846	1.000
	Total income last month (13m)	-5.72	418.84	0.989	1.000
13-month: IND, equity					
	GAD-2 score (13m)	0.28	0.22	0.198	1.000
	Stress level (13m)	3.75	5.28	0.478	1.000
	Total income last month (13m)	296.25	452.97	0.513	1.000
	GAD-2 above threshold (13m)	0.02	0.07	0.714	1.000
	Financial worry (13m)	0.04	0.16	0.829	1.000
H1 characteristics					
	Years of Education	1.01	0.39	0.010	0.106
	Digit Span Recall	0.20	0.08	0.013	0.106
	Risk Tolerance (Quantitative)	-0.95	0.50	0.057	0.332
	Stress (In The Moment)	3.32	2.02	0.100	0.431
	Debt Stigma	0.28	0.23	0.226	0.987
	Pos. Reciprocity: Thank You gift (EGP amt)	0.08	0.07	0.286	1.000
	Debt Aversion	0.19	0.22	0.394	1.000
	Previously Applied For Any Loans?	0.03	0.03	0.414	1.000
	Last-Month Income (1,000 EGP)	-0.14	0.22	0.525	1.000
	Owns Goats or Sheep?	0.01	0.02	0.707	1.000
	GAD-2 Above Threshold	0.01	0.02	0.733	1.000
	Female	-0.01	0.03	0.782	1.000
	Risk Tolerance (Qualitative)	0.04	0.19	0.843	1.000
	Muslim	0.00	0.01	0.872	1.000
	Prior Livestock-Sharing Arrangement	-0.00	0.03	0.927	1.000
Inputs: contract					
	Feed costs	-86.93	54.16	0.110	1.000
	Input costs / value	74.75	75.25	0.322	1.000
	Grazing hours	-0.96	2.06	0.643	1.000

Continued on next page

Table D6 (continued)

Family	Outcome	Coeff.	SE	p-value	Sharpened q-value
	Medication costs	8.02	23.06	0.728	1.000
	Total input costs	-33.58	96.73	0.729	1.000
Inputs: selection					
	Grazing hours	3.32	2.16	0.126	1.000
	Medication costs	-24.36	24.51	0.322	1.000
	Total input costs	31.65	96.68	0.744	1.000
	Input costs / value	23.98	84.54	0.777	1.000
	Feed costs	-5.90	48.60	0.904	1.000
Psych 7-month: EO, equity					
	GAD-2 score	0.32	0.17	0.055	0.116
	Financial worry	0.23	0.13	0.077	0.116
	Stress level	7.46	4.23	0.078	0.116
	GAD-2 above threshold	0.05	0.05	0.292	0.116
Psych 7-month: IND, debt					
	GAD-2 above threshold	0.09	0.07	0.149	0.767
	Financial worry	0.16	0.16	0.342	0.767
	GAD-2 score	0.19	0.23	0.397	0.767
	Stress level	4.20	5.37	0.434	0.767
Psych 7-month: IND, equity					
	GAD-2 above threshold	0.05	0.07	0.449	1.000
	Financial worry	0.05	0.16	0.753	1.000
	GAD-2 score	0.06	0.23	0.781	1.000
	Stress level	1.38	5.47	0.800	1.000
Satisfaction: contract					
	Prefer equity next time	0.27	0.06	0.000	0.001
	Prefer debt next time	-0.16	0.04	0.000	0.001
	Prefer neither next time	-0.10	0.07	0.129	0.112
	Satisfaction	0.33	0.24	0.166	0.112
	Would repeat	0.08	0.05	0.167	0.112
Satisfaction: selection					
	Prefer debt next time	0.02	0.03	0.403	1.000
	Prefer equity next time	-0.05	0.07	0.490	1.000
	Prefer neither next time	0.03	0.07	0.710	1.000
	Would repeat	0.02	0.06	0.745	1.000
	Satisfaction	-0.04	0.25	0.867	1.000

Notes: Anderson (2008) sharpened two-stage q-values, computed within each family. Families group related outcomes for a given experimental comparison; specifications match the corresponding paper tables. The plan's four primary outcomes (total income, total livestock revenue, product-specific livestock revenue with its price and weight components, and days delinquent) are reported with unadjusted inference per the plan and are not included here.

Table D7: Headline Coefficients Under Double Post-Lasso Control Selection

	Paper Specification	Double Post-Lasso	Controls Selected	N
Total Deaths (selection)	-0.11**	-0.13**	24	225
	(0.05)	(0.05)		
Total Deaths (contract)	0.04	0.04	24	210
	(0.05)	(0.05)		
Total Forced Sales (selection)	-0.12**	-0.11**	25	225
	(0.05)	(0.05)		
Total Forced Sales (contract)	0.13**	0.13**	24	210
	(0.05)	(0.05)		
Any Delinquency 30+ (selection)	0.03	0.02	24	225
	(0.02)	(0.02)		
Any Delinquency 30+ (contract)	-0.07***	-0.07***	24	210
	(0.03)	(0.03)		
Default (selection)	0.01	0.01	24	225
	(0.01)	(0.01)		
Default (contract)	-0.06***	-0.06***	24	210
	(0.02)	(0.02)		
Total Value at Sale (selection)	699.36	861.72	24	225
	(708.31)	(713.64)		
Total Value at Sale (contract)	-1,384.94**	-1,440.70**	24	210
	(664.70)	(666.94)		
Net Revenue (selection)	713.50	868.26	24	225
	(704.91)	(711.58)		
Net Revenue (contract)	-1,422.86**	-1,471.00**	24	210
	(662.98)	(666.19)		
Bank Revenue Share (selection)	53.07	43.10	24	225
	(182.59)	(184.54)		
Bank Revenue Share (contract)	-1,486.60***	-1,489.34***	24	210
	(132.59)	(133.19)		
Feed Costs (selection)	-5.90	-4.72	24	218
	(48.60)	(49.32)		
Feed Costs (contract)	-86.93	-91.40*	24	207
	(54.16)	(54.43)		
Income (incl. ABWA): EO, equity	1,071.64**	1,058.57**	26	387
	(423.59)	(433.24)		
Income (incl. ABWA): IND, equity	1,735.28***	1,542.39***	27	327
	(485.45)	(486.28)		
Income (incl. ABWA): IND, debt	917.56*	903.73*	27	327
	(495.64)	(479.92)		

Notes: Each row compares the paper's specification with double post-lasso control selection (PAP Equation 4) for the same coefficient and sample. Candidate controls are the pre-analysis plan's baseline characteristic list (mean-imputed, plus missing indicators); design fixed effects (cohort for selection comparisons, strata otherwise) are always retained. Product outcomes restrict to takers in the non-devaluation cohorts; income rows use the intent-to-treat samples. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D8: Administrative Outcomes Winsorized at the 99th Percentile

	(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	689	3	2	713
	(707)	(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	-1,360**	-5	-4*	-1,423**
	(661)	(4)	(2)	(663)
Debt-Indiff Mean	23,653	131	181	2,956
N	210	210	210	210

Notes: The pre-analysis plan specified winsorizing outcomes at the 99th percentile. The paper winsorizes survey-based outcomes at the top 1 percent and leaves administrative outcomes unwinsorized by design. This table applies the literal rule to the continuous administrative outcomes of Table 3 and re-runs the same specifications. Repayment outcomes are binary indicators, so winsorization leaves them unchanged. The revenue and margin outcomes are EGP levels rather than ratios. Applying the same top-1 percent rule to them caps eight or nine observations each, none of which falls in either estimation frame, so the Table 3 coefficients are unchanged. Sample restricted to non-devaluation cohorts. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses.

Table D9: Baseline Characteristics by Preference Group, Including Debt-Only Applicants

	Group means and differences			Multivariate (LPM of equity-only)	
	Indifferent	Equity-Only minus Indiff.	Debt-Only minus Indiff.	vs. Indifferent	vs. Indifferent and Debt-Only
	(1)	(2)	(3)	(4)	(5)
Education (Years)	6.44	1.09***	3.00***	0.003	0.001
	[6.35]	(0.38)	(0.57)	(0.003)	(0.002)
Cognition (Digital Span Recall)	2.43	0.19**	0.29**	0.015	0.015
	[1.48]	(0.08)	(0.11)	(0.010)	(0.009)
Risk Tolerance (Quantitative)	6.66	-1.02**	0.17	-0.003**	-0.003**
	[9.48]	(0.50)	(0.80)	(0.001)	(0.001)
Risk Tolerance (Qualitative)	5.76	0.08	0.30	0.002	0.001
	[3.67]	(0.18)	(0.29)	(0.003)	(0.003)
Positive Reciprocity (0-6)	5.09	0.07	-0.01	0.008	0.006
	[1.39]	(0.07)	(0.11)	(0.009)	(0.009)
Debt Aversion (1-10)	6.10	0.20	-0.14	0.002	0.002
	[4.10]	(0.22)	(0.34)	(0.003)	(0.003)
Debt Stigma (1-10)	3.83	0.23	-0.27	0.002	0.003
	[4.32]	(0.22)	(0.34)	(0.003)	(0.003)
GAD-2 (Anxiety) $\geq$ 3	0.26	-0.03	-0.03	-0.027	-0.027
	[0.44]	(0.03)	(0.05)	(0.036)	(0.034)
Stress (0-100)	33.08	3.57*	4.73	0.000	0.000
	[37.60]	(2.00)	(3.38)	(0.000)	(0.000)
Female	0.65	-0.00	-0.18***	0.001	0.040
	[0.48]	(0.03)	(0.04)	(0.027)	(0.025)
Muslim	0.98	-0.00	-0.03**	-0.087	0.000
	[0.14]	(0.01)	(0.02)	(0.080)	(0.075)
Income Last Month (1,000 EGP)	3.59	-0.12	0.13	-0.003	-0.001
	[4.01]	(0.21)	(0.32)	(0.003)	(0.003)
Owns Sheep or Goats	0.19	0.00	0.01	0.039	0.025
	[0.39]	(0.02)	(0.03)	(0.034)	(0.031)
Prior Equity Experience	0.17	-0.01	-0.05	-0.007	0.002
	[0.38]	(0.02)	(0.03)	(0.038)	(0.037)
Prior Formal Loan Experience	0.33	0.02	0.03	0.024	0.024
	[0.47]	(0.03)	(0.04)	(0.031)	(0.028)
p-value (Joint Test)				0.130	0.321
N	841	890	219	1,714	1,933

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

Notes: The sample is the experimental sample of Table 1 plus the 219 applicants who were willing to take only debt and were therefore not enrolled. Column (1) shows means for the indifferent group, with standard deviations in brackets. Columns (2) and (3) report the equity-only minus indifferent and the debt-only minus indifferent difference in each row variable, in the variable's own units, from a regression of the variable on indicators for the two groups (pre-analysis plan Equation 3). Columns (4) and (5) report coefficients from a linear probability model of the equity-only indicator on all row variables jointly; the comparison group is the indifferent group in column (4), as in Table 1, and everyone who applied for debt, the indifferent and debt-only groups together, in column (5) (pre-analysis plan Equation 2). Debt-only applicants were never randomized and have no cohort, so every regression includes baseline-enumerator and baseline-month fixed effects in place of the cohort and enumerator fixed effects of Table 1. Income is in thousands of EGP, winsorized at the top 1 percent. In the joint regressions, missing values are imputed with means and a missing indicator. Robust standard errors in parentheses.

Table D10: Veterinarian-Observed Animal Condition and Feed Quality by Contract

	Vet score (1–10)			Share of visits, feed rated good		Share of visits, animals not good
	J-PAL log	ABWA log	Both logs	J-PAL log	Both logs	Both logs
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)						
Equity-Only	0.36**	0.04	0.17	0.10	0.06	-0.02
	(0.17)	(0.13)	(0.17)	(0.10)	(0.07)	(0.02)
Equity-Indiff Mean	6.31	9.02	7.27	0.18	0.12	0.02
N	81	45	92	81	92	92
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)						
Equity-Indifferent	-0.26*	-0.09	0.05	-0.10	-0.06	0.00
	(0.14)	(0.11)	(0.19)	(0.09)	(0.08)	(0.03)
Debt-Indiff Mean	6.61	9.00	7.22	0.23	0.16	0.02
N	71	41	81	71	81	81

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

Notes: Sample is monitored takers in the non-devaluation cohorts with at least one recorded veterinary visit in the named log. Each borrower's outcome is the mean over their recorded visits: in the J-PAL veterinarian's log, in the ABWA veterinarian's log, or in both logs together. The vet score is the veterinarian's 1 to 10 rating of the animals' condition. Feed rated good is the top category of a three-point scale; the ABWA veterinarian did not use the top category in these cohorts, so that column is omitted. Animals not good is the share of visits at which the animals were recorded as not in good condition, from neglect or natural causes. Panel A includes cohort fixed effects. Panel B includes strata fixed effects. Robust standard errors in parentheses. Coverage: of the 371 monitored takers in all cohorts, the ABWA veterinarian's log holds dated visits for 176 (cohorts 4 to 45, from November 2022), the J-PAL veterinarian's for 307 (cohorts 9 to 51, from August 2023), 133 appear in both and 21 in neither; 346 have an assessment (rating, condition, or feed quality) in either log. In the non-devaluation cohorts the ABWA log holds assessments for 66 of 148, the J-PAL log for 120, and either log for 134.

Table D11: 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.