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

Kareem Haggag and Adam Osman

Preliminary - Do Not Cite

March 27, 2026

Abstract

Firms often cite financing constraints as key barriers, yet lenders rarely innovate beyond debt products. While theory suggests debt may dominate equity due to adverse selection and moral hazard, these mechanisms are difficult to isolate empirically. We design and test a microequity product with Egyptian livestock farmers, providing animals and splitting sales proceeds at a fixed rate. We market this product alongside a flexible debt contract. Using a two-stage RCT, 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—evidence of moral hazard. A set of currency devaluations eliminated this negative effect, demonstrating that moral hazard varies with context. 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

*Haggag: Anderson School of Management, University of California-Los Angeles, and NBER; Osman: Department of Economics, University of Illinois Urbana-Champaign; aosman@illinois.edu. We thank AR Nagy, Mohamed Omar, Noha Adel, Mohamed Ghandour, Yahia Ashraf, Andrew Ghattas, Ziad Sabbah, Menna Enan, Mohamed Sabry, Youngjoo Jung, Ronald Cueva, Amr Mostafa, and Yasmine Eid for research support. We appreciate support from the Sawiris Foundation including Laila Hosny, Mays Abouthegab, Samy Abdelrehim, Rehab Moghazy, Mohamed Moussa, and Noura Selim. We are grateful to Gharad Bryan, Dean Karlan, Asim Khwaja, and Muhammad Meki for valuable discussions and suggestions, and to seminar participants at the Federal Reserve Bank of Minneapolis, London School of Economics, Oxford University, University of Nebraska, and University of Southern California for their helpful feedback. Part of this work was completed during Haggag's visit to the Opportunity & Inclusive Growth Institute at the Minneapolis Fed.

1 Introduction

Capital allocation is an essential function of financial markets and contributes to aggregate productivity and growth (Hsieh and Klenow, 2009). Debt instruments are the primary method small and medium enterprises (SMEs) use to raise capital for their business (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 typically preferable over 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?

Theoretically, equity 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. Even conditional on selection, moral hazard may induce borrowers to reduce effort once their downside risk is mitigated. 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? Separating out the selection and contract effects of equity contracts versus debt contracts is challenging.[3] To overcome this, we implement a multi-staged randomized experiment that allows us to measure each mechanism on its own. Working with livestock farmers in rural Egypt—a sector that supports the livelihoods of approximately 1.3 billion people globally—we design

[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, recent work confirms that market frictions have substantial real effects tied to capital structure: 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.

an 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. 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 expand 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 simply composed of the poorest or most constrained households: relative to those willing to take either contract, equity-only borrowers are modestly more educated, perform better on a brief cognitive test, differ on measures of reciprocity and risk preferences, and 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—agnostic as to whether driven by observables or unobservables—affects performance. Holding fixed the contract type, we find evidence of *advantageous* selection into equity. Among borrowers who receive the equity contract, those who would accept only equity have substantially fewer bad project outcomes—such as animal death or forced early sale—than those who are indifferent between contracts, and they exhibit equal or better performance on realized net revenues and input use. In our setting, the borrowers who are willing to take equity but not debt look, if anything, like relatively higher types.

Third, holding fixed selection, we find that equity underperforms debt along several key margins, consistent with moral hazard. Among borrowers ex-ante willing to accept either contract, those randomly assigned to equity experience more adverse livestock outcomes than those assigned to debt, invest less in purchased feed, and generate lower net revenues for the lender. From the lender's perspective, the debt contract produces substantially higher expected revenues per financed project than the equity contract, even

[4]Livestock rearing is a sector of primary economic importance for the global poor. Approximately 1.3 billion people depend on livestock value chains for their livelihoods (Food and Agriculture Organization, 2018), including an estimated 60% of rural households in developing countries (Pica-Ciamarra et al., 2011).

though the two contracts finance the same asset. A randomized monitoring intervention is ultimately both underpowered and likely too light-touch to mitigate these effects. These results suggest that the insurance embedded in equity meaningfully weakens incentives relative to debt.

Turning to borrower outcomes, we find that both contracts meaningfully raise short-run income relative to the pure control group, though income gains do not translate into uniform improvements in psychological well-being. Relative to the control group, financing worsens self-reported mental health for borrowers who would only accept equity, while we estimate near-zero effects on mental health for borrowers indifferent between contracts. This suggests that the act of taking on any formal financial obligation—rather than the specific contract form—may be the key driver of these short-run psychological costs for the "equity-only" group.

A further feature of our setting is that the experiment spans two large currency devaluations, which sharply change the relative payoffs of the two contracts. Because the debt contract specifies a fixed nominal repayment, devaluation lowers the real burden of debt and generates a windfall for borrowers with existing loans. Under equity, by contrast, nominal revenues rise with the overall price level, so that the lender's share increases while borrowers' margins tighten. We show that these shocks alter both the behavior of borrowers and the relative performance of equity and debt. In particular, among borrowers exposed to this highly inflationary period, effort and project performance improve under equity, and much of the resulting gain accrues to the lender. The combination of our baseline results and these shock-induced changes underscores that the severity of moral hazard and the sustainability of equity contracts depend on the macroeconomic environment and the real value of contractual obligations.

Taken together, our findings paint a nuanced picture of micro-equity. On the one hand, equity contracts appear to relax important financial and psychological 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 also brings with it psychological costs and exposure to the macroeconomic and contractual environment.

Our findings contribute to several literatures. A first contribution is to research on microfinance and contract design. A large evaluation literature documents modest average effects of standard microcredit on firm growth and household welfare (e.g. Banerjee et al., 2015), though recent work shows that impacts are concentrated among upper-tail borrowers and households with prior business experience (e.g. Banerjee et al., 2019; Crépon et al., 2024), and that altering repayment timing and flexibility can meaningfully affect investment and default (e.g. Barboni and Agarwal, 2023; Battaglia et al., 2024). We complement this literature by studying a more radical departure from fixed-repayment debt: replacing fixed obligations with a fully state-contingent equity contract that shares realized project returns. Our experimental design compares this equity product to a flexible, asset-based debt contract that already incorporates many recent innovations, and allows us to separately measure selection and contract effects using rich data on project-level outcomes and borrower welfare. In this sense, our results also complement recent work on equity-like and performance-contingent contracts for small firms (e.g. de Mel et al., 2019; Cordaro et al., 2022; Meki, 2024; Meki and Quinn, 2024). 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 provides 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 one of the first comprehensive field tests 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 contexts (Burchardi et al., 2019; Herbst and Hendren, 2024).

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.

The remainder of the paper is organized as follows. Section 2 describes the local context, the implementing institution, and the design of the equity and debt products. Section 2.3 lays out the experimental design and data. Section 3 presents our estimates of demand for equity versus debt and of selection into each contract. Section 4 reports the contract and selection effects of equity versus debt on project outcomes, borrower income, and mental health, and discusses the monitoring treatment. Section 5 analyzes how the currency devaluations reshape the relative performance of equity and debt. Section 6 discusses the implications for contract design and the role of equity in microfinance, and concludes.

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 possess significant 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 fundamentally mismatched with the cash flow of livestock fattening, which often requires a several month investment period before revenues are realized.

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. With a combined population of over 9 million, agriculture dominates the local economy, and Assiut and Minya historically rank among the poorest governorates in the country. Second, small farmers are underserved by the formal financial system since lenders typically require monthly repayment which does not match the timing of agricultural activities and cashflows. 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 regarding paying interest on debt ("*Riba*" in Arabic) remain strong. While state religious authorities permit banking interest, many low-income households view conventional fixed-interest debt as religiously suspect or socially stigmatizing. 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 and preferred over debt.

2.1 Institutional Partner

To implement these contracts at scale, we partnered with the Assiut Businesswomen's Association (ABWA). Established in 2000, ABWA is a nonprofit civil association with a mandate to promote local economic development, specifically targeting women and marginalized groups. The organization operates a network of 13 branch offices throughout the governorate and had over 5,000 active clients at the time of the study. As ABWA doesn't exclusively serve women, they compose roughly 65% of the clients in our sample.

Our partnership was motivated by ABWA's interest in reaching segments of the population who had rejected their standard loan products. The organization recognized that their existing portfolio—primarily cash loans for trade activities—was not serving the agricultural base of the region effectively. The experiment was therefore designed not as a standalone pilot, but as an overlay onto their existing infrastructure, utilizing their loan officers to screen clients and facilitate asset delivery and sales.

2.2 *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.[5] Both products also included mandatory insurance for non-negligent animal death, assessed by a veterinarian. 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. Crucially, this contract shares downside risk. If the animals are sold at a loss, the lender absorbs one-third of that financial loss. This structure provides implicit insurance, potentially encouraging 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.[6,7]

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.

2.3 *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.

[5]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.

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

[7]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.

2.3.1 *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. 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), but would only receive financing if they had indicated willingness to accept the assigned product. Thus, 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.

2.3.2 *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 has 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 in both the equity and debt arms to control for any pure reminder or information effects of veterinary contact.[8]

Comparing outcomes across monitoring conditions within each contract type allows us to jointly estimate the extent of moral hazard and the effectiveness of light-touch monitoring. Comparing the monitoring effect across contract types (equity vs. debt) provides

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

a test of whether moral hazard is differentially important under equity, as theory would predict.

2.3.3 Randomization

Randomization was conducted at the individual level in batches, stratified by stated preferences and gender, using Stata. 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.[9]

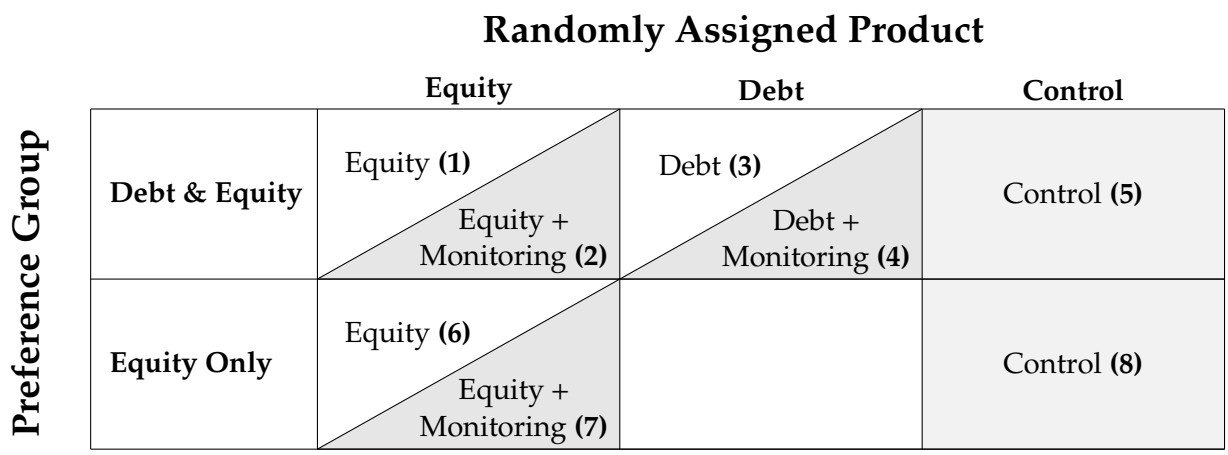
This design produces eight distinct experimental cells (Figure 1), enabling several comparisons of interest. First, we can estimate the causal effect of each contract type on outcomes by comparing treated clients to their respective control groups within preference strata. Second, we can isolate selection 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 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.

2.4 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 the applicant homes and implement and in-person baseline surveys prior to randomization. These surveys collected information on demographics, household composition, prior experience with livestock and finance, and behavioral preferences. In addition to questions about risk aversion we included measures of debt aversion ("If I need money, I try to avoid taking a loan, even if I know I can pay it back") and debt stigma ("It is embarrassing if someone else knows I have loans from a bank"). Cognitive ability was assessed

[9]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 as 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.

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.

Third, we 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. For clients in the monitoring arm who were visited by the veterinarians we have recorded assessments of animal health, feed quality, and any evidence of borrower neglect during each visit.

Finally, we collected data on client beliefs and hypothetical choices. At baseline, we elicited beliefs about market prices for livestock and asked clients to report their ideal project scope (financing amount and mix of sheep vs. goats) under each contract type.

These measures allow us to examine whether contract structure affects intended investment decisions at the extensive and intensive margins.

2.5 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 the financing opportunity we asked them a few follow up questions about why.

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. If we had only relied on recruiting individuals through the normal channels a lender uses, we may miss individuals who eschew these lenders for any of a number of important reasons, including debt aversion and religious concerns. By approaching households directly, we capture a more complete picture of latent demand and can more fully assess selection into equity versus debt.

2.6 Devaluations

The experiment took place during a time with elevated macroeconomic uncertainty in Egypt. Russia's invasion of Ukraine in February 2022 proved an immediate catalyst: Egypt imported roughly 80% of its wheat from Russia and Ukraine, and the resulting disruption to global grain markets caused food costs to spike sharply for households already operating on thin margins. 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.

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.

Because these macro swings can confound clean comparisons, our main analysis in

Section 4 focuses on borrowers who take up financing after the final devaluation, when exchange rates and inflation dynamics are more stable. In Section 5 we report how the borrow behavior differs during the devaluation window relative to the main analysis.

3 Demand and Sample Characteristics

3.1 Differences in Demand for Equity and Debt

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 refer to these individuals as "indifferent" throughout the paper. In Column 2 we report information about those who applied for the equity product only. We do this by showing the difference in their characteristics relative to those who are applied for both products (the "indifferent"). This set up allows us to more easily describe how equity-only individuals differ relative to those who are open to both equity and debt.

Column 1 shows that on average those who applied for both products has 6.4 years of schooling, 65% are women, and 18% already own sheep or goats. Average income in the previous month was around 4400 EGP, which is approximately $100 USD.

As described above we also collected several different behavioral measures. This includes measures or 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, 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, have higher levels of positive reciprocity and report higher levels of risk tolerance on the quantitative elicitation (but lower tolerance on the qualitative elicitation).[10] 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. The joint test of coefficients has a p-

[10]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, are more susceptible to response heuristics (particularly among lower cognitive ability participants) suggesting confusion, and can exhibit opposite-signed correlations with auxiliary variables (such as the implied field behavior) despite being positively correlated with each other. It is thus somewhat reassuring that the quantitative risk tolerance measure correlates in the expected direction with demand for equity-only.

value of 0.015, showing that the groups are statistically different based on their observable characteristics.

Interestingly, there are several baseline characteristics that do not differ despite our prior expectations. For example, while the equity only group have 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 while the two groups are clearly selected differently as a whole, it is not cleanly along the dimensions one may expect.

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.

Overall about two thirds of the sample are female with an average age of 40 years. Educational attainment is modest with an average of around 7 years of schooling. The vast majority (97%) are Muslim. At baseline, around 20% of respondents own goats or sheep, with about 3 years of experience raising small livestock. Monthly profits from livestock average 583 EGP among those with any livestock income, though livestock represents only 18% of total household income on average.

Regarding contract preferences, 45% of eligible respondents expressed interest in equity only, while 55% indicated willingness to accept either product. Interestingly when those in the 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. Behavioral measures reveal moderate levels of debt stigma (about 4 on a 0–10 scale) and debt aversion (mean of 6.2), with considerable heterogeneity across individuals.

4 Contract and Selection Effects

Our experimental design allows us to compare different experimental groups in ways that will allow us to estimate how differential selection into the products, and the contract structure of the financing itself affects 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 individuals who expressed "equity-only" preferences to those who expressed a willingness to take both equity and debt, conditional on being allocated to the equity treatment (i.e. the "equity-indifferent" group). We do this using the following regression:

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

In this regression we restrict the sample to only those randomized into the equity treatment arms. The only differences between these groups are how they selected into the project. We regress a binary that is equal to 1 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, where we regress a binary that is equal to 1 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.

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

4.1 Animal Deaths & Repayment

Table 2 which 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 95% take up in the debt group (see Appendix Tables A2 & A3).[11]

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 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.[12] 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 animals deaths and 0.13 more forced sales, the later being more than three times as much as the debt group. This provides evidence of moral hazard— the equity contract is leading borrowers to lower effort, or take more risk taking, because of the risk-sharing structure of the contract.

Columns 3 & 4 show how this impacts high-level measures of repayment—delinquency

[11] If our analysis included everyone in treatment, including those that didn't 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.

[12] 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.

and default. We see limited amounts of each overall, with slightly higher but statistically insignificant differences in Panel A. 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.[13]. Overall, these results provide evidence of *advantageous* selection in equity and moral hazard in performance. The following tables will allow us to explore the mechanisms through which the outcomes manifest in more detail.

4.2 Animal Production Outcomes

Careful data collection allows us to dig deeper into 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 700EGP 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% 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 1,385EGP *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 breaks 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.

[13] An alternative explanation is differential loan officer monitoring. While we did not collect an administrative measure of monitoring at sale, a proxy is whether the client reported that the loan officer was present for weighing the animals (which typically, but not always, happened during sale). Appendix Table A14 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.

On the other hand, we find that those in the equity indifferent group have lighter animals with lower quality, receiving lower value per kilogram.

Finally Columns 5 & 6 show how the revenues are split between the lender and borrower. The equity product is much more lucrative for the borrower, and hence less lucrative for the lender. The lender makes 238EGP more on the equity-only borrowers relative to the equity indifferent borrowers, a 43% increase, but they make 2251EGP less on the equity-indifferent borrowers relative to the debt borrowers, an 80% decrease.

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, consistent with the outcomes in Table 3. 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 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 strong impacts found on animal deaths, it is clear that effort changes dramatically for the different groups. 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. Panel A compares those who expressed equity-only preferences and where allocated to treatment with those that were allocated to control. Column 1 looks at their total income last month, which increased by 1429EGP, a 36% increase relative to control. These data come from our 7-month survey, which was timed to make sure we can pick up the additional profits the borrowers got from the sale of the sheep.

There are two main considerations when interpreting this large increase. First, 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 6% monthly increase in income, which is welcome

but much more modest. Second, it's important to consider if income from other sources may have changed, in case the financing led to substitution away from other income generating activities. Column 2 reports no change in income from non-ABWA related sources, meaning that the sheep did not lead to significant substitution of income from other sources.[14] 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 1605EGP larger, a 42% increase. Again, this would average out to 7% monthly over the course of the financing. Those in the debt group had a much smaller increase in come of 337EGP, a 9% increase relative to control, which would only average out to 1.5% monthly. This is because, as shown in Table 3 while the debt group sold their animals for the greatest average value, the bank share of the profits was much larger for the debt borrowers relative to the equity borrowers.

4.4 Product Satisfaction

Table 6 reports on borrower's 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 medium levels of satisfaction, the equity-indifferent group reports an average of 2.8, equity-only is also 2.8 while the debt group is 2.5, all of which are statistically equivalent. When looking at repeat borrowing, we see a similar pattern. The debt group is least interested in repeating, while the equity groups are more likely but statistically equivalent.

These results mitigate concerns that equity demand simply reflects unfamiliarity or misunderstanding of the 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

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

and debt built a strong preference for equity, with them being significantly more likely to prefer equity the next time around.

4.5 Monitoring

As described in Section 2.3.2 we cross-cut a monitoring intervention in each of the main experimental cells. This allows us to assess the combination of the existence of moral hazard and the ability to mitigate it through active monitoring of the sample. Appendix Table A4 estimates the impacts of the monitoring treatment on primary outcomes of interest. We find no evidence that the monitoring arm led to any statistically or economically significant differences across 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 type of monitoring was too light touch. We 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 sufficient to ensure that borrowers took their task seriously, leaving the veterinarian visits as redundant.

5 Devaluation

As mentioned above, the project began during a period of macroeconomic uncertainty, with three currency devaluations in the first twelve months. In Table 7 we consider how product performance differs for the earlier cohorts whose contracts spanned this highly inflationary period. Critically, a devaluation changes the relative incentives of the two products: since interest rates were fixed and the equity revenue share proportion was also fixed, the devaluation made the debt product cheaper for the borrower and the equity product more expensive.[15]

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

[15] 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.

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 acting in a way consistent with increased effort aimed at maximizing livestock productivity.

The rest of columns show a similar story, whereas during the non-devaluation period the overall sales price was lower for the equity-indifferent group, this difference is erased 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. 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.

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

6 Conclusion

Financial markets for small and informal firms are characterized by persistent frictions that limit capital access and distort contract choice. Debt dominates small-business lending not necessarily because it is the efficient contract, but because the theoretical barriers to equity—adverse selection and moral hazard—are widely assumed to be prohibitive. This paper provides the first field experimental evidence that simultaneously identifies both mechanisms, and shows that their severity is neither fixed nor uniformly discouraging for equity.

Our results reveal both promise and a clear challenge for equity.. On the demand side, nearly half of eligible borrowers in our setting would accept equity but not debt, and these equity-only borrowers exhibit advantageous selection: they experience fewer adverse livestock outcomes and generate equal or higher revenues than borrowers willing

to accept either contract. This finding directly challenges the conventional presumption that state-contingent contracts attract lower-quality borrowers, and suggests that equity may expand financial inclusion by drawing in a population that debt markets leave behind. On the supply side, the picture is more cautionary. Holding selection fixed, equity causes a meaningful reduction in borrower effort and lender revenues relative to debt, consistent with moral hazard. Light-touch monitoring is insufficient to close this gap, pointing to the need for more structural solutions—such as performance-based pricing or tighter revenue verification—if equity is to be deployed at scale without sacrificing lender sustainability.

We also show that these frictions respond to the macroeconomic environment. The currency devaluations that occurred during our study period changed the relative cost of equity and debt in ways that were plausibly exogenous to individual behavior, and we document that borrowers responded accordingly: effort and project performance under equity rose sharply in the devaluation period, largely erasing the moral hazard gap observed in normal times. This context-dependence has immediate policy relevance. It implies that assessments of equity contracts based on a single macroeconomic environment may generalize poorly, and that lenders considering equity products should account for how the real value of contractual obligations varies with inflation and exchange rate dynamics.

These findings raise a natural design question. Attracting more equity-only borrowers expands the pool of high-performing clients, but cheapening equity relative to debt exacerbates moral hazard among the indifferent. How lenders should price and structure equity contracts to balance inclusion against incentives remains an open question, and one that our devaluation results suggest is sensitive to the broader economic environment. Equity-like arrangements exist across many contexts, from venture capital to sharecropping to Islamic finance, but their performance varies with the institutional setting. Our results reinforce this point: the same contract produced quite different outcomes depending on the macroeconomic backdrop.

Capital structure matters for small firms, just as it does for large ones. The challenge for policy and product design is to build the complementary institutions that allow equity's advantages to be realized while mitigating its costs.

References

Amar, Moty, Ariely, Dan, Ayal, Shahar, Cryder, Cynthia E. and Rick, Scott I. (2011). 'Winning the Battle but Losing the War: The Psychology of Debt Management', *Journal of Marketing Research* 48(SPL), 38–50.

Banerjee, Abhijit, Breza, Emily, Duflo, Esther and Kinnan, Cynthia. (2019), Can Microfinance Unlock a Poverty Trap for Some Entrepreneurs?, NBER Working Paper 26346, National Bureau of Economic Research. Revised 2024.

Banerjee, Abhijit, Karlan, Dean and Zinman, Jonathan. (2015). 'Six Randomized Evaluations of Microcredit: Introduction and Further Steps', *American Economic Journal: Applied Economics* 7(1), 1–21.

Barboni, Giorgia and Agarwal, Parul. (2023), How Do Flexible Microfinance Contracts Improve Repayment Rates and Business Outcomes? Experimental Evidence from India. Working paper.

Battaglia, Marianna, Gulesci, Selim and Madestam, Andreas. (2024). 'Repayment Flexibility and Risk Taking: Experimental Evidence from Credit Contracts', *Review of Economic Studies* 91(5), 2635–2675.

Beck, Thorsten, Demirgüç-Kunt, Asli and Maksimovic, Vojislav. (2008). 'Financing patterns around the world: Are small firms different?', *Journal of Financial Economics* 89(3), 467–487.

Burchardi, Konrad B., Gulesci, Selim, Lerva, Benedetta and Sulaiman, Munshi. (2019). 'Moral Hazard: Experimental Evidence from Tenancy Contracts', *Quarterly Journal of Economics* 134(1), 281–347.

Caetano, Gregorio, Palacios, Miguel and Patrinos, Harry A. (2019). 'Measuring Aversion to Debt: An Experiment Among Student Loan Candidates', *Journal of Family and Economic Issues* 40(1), 117–131.

Chapman, Jonathan, Ortoleva, Pietro, Snowberg, Erik, Yariv, Leeat and Camerer, Colin. (2025), Reassessing Qualitative Self-Assessments and Experimental Validation. Working Paper.

Cordaro, Francesco, Fafchamps, Marcel, Mayer, Colin, Meki, Muhammad, Quinn, Simon and Roll, Kate. (2022), Microequity and Mutuality: Experimental Evidence on Credit with Performance-Contingent Repayment, NBER Working Paper 30411, National Bureau of Economic Research.

Crépon, Bruno, Komi, Mohamed El and Osman, Adam. (2024). 'Is It Who You Are or What You Get? Comparing the Impacts of Loans and Grants for Microenterprise Development', *American Economic Journal: Applied Economics* 16(1), 286–313.

de Mel, Suresh, McKenzie, David and Woodruff, Christopher. (2019), Micro-Equity for Microenterprises, Policy Research Working Paper 8799, World Bank.

Eswaran, Mukesh and Kotwal, Ashok. (1985). 'A theory of contractual structure in agriculture', *American Economic Review* 75(3), 352–367.

Food and Agriculture Organization. (2018), *World Livestock: Transforming the livestock sector through the Sustainable Development Goals*, FAO, Rome.

Friedman, Milton. (1955). 'The role of government in education', *Economics and the public interest* pp. 123–144.

Ghatak, Maitreesh and Pandey, Priyanka. (2000). 'Contract Choice in Agriculture with Joint Moral Hazard in Effort and Risk', *Journal of Development Economics* 63(2), 303–326.

Herbst, Daniel and Hendren, Nathaniel. (2024). 'Opportunity Unraveled: Private Information and the Missing Markets for Financing Human Capital', *American Economic Review* 114(7), 2024–2072.

Hsieh, Chang-Tai and Klenow, Peter J. (2009). 'Misallocation and Manufacturing TFP in China and India', *Quarterly Journal of Economics* 124(4), 1403–1448.

Kaur, Supreet, Mullainathan, Sendhil, Oh, Suanna and Schilbach, Frank. (2024). 'Do Financial Concerns Make Workers Less Productive?', *Quarterly Journal of Economics* . Forthcoming; earlier version NBER Working Paper No. 28338.

Martínez-Marquina, Alejandro and Shi, Mike. (2024). 'The Opportunity Cost of Debt Aversion', *American Economic Review* 114(4), 1140–1172.

Meki, Muhammad. (2023). 'Levelling the debt–equity playing field: Evidence from Belgium', *European Economic Review* 151, 104305.

Meki, Muhammad. (2024), Small Firm Investment under Uncertainty: The Role of Equity Finance. Manuscript.

Meki, Muhammad and Quinn, Simon. (2024). 'Microequity: some thoughts for an emerging research agenda', *Oxford Review of Economic Policy* 40(1), 176–188.

Modigliani, Franco and Miller, Merton H. (1958). 'The Cost of Capital, Corporation Finance and the Theory of Investment', *American Economic Review* 48(3), 261–297.

Peter, Alessandra. (2025), Equity Frictions and Firm Ownership, NBER Working Paper 34301, National Bureau of Economic Research.

Petersen, Mitchell A and Rajan, Raghuram G. (1994). 'The benefits of lending relationships: Evidence from small business data', *The Journal of Finance* 49(1), 3–37.

Pica-Ciamarra, Ugo, Tasciotti, Luca, Otte, Joachim and Zezza, Alberto. (2011), Livestock Assets, Livestock Income and Rural Households: Cross-Country Evidence from Household Surveys, Esa working paper no. 11-17, Food and Agriculture Organization, Rome.

Robb, Alicia M and Robinson, David T. (2014). 'The capital structure decisions of new firms', *The Review of Financial Studies* 27(1), 153–179.

Stiglitz, Joseph E. (1974). 'Incentives and Risk Sharing in Sharecropping', *Review of Economic Studies* 41(2), 219–255.

Townsend, Robert M. (1979). 'Optimal Contracts and Competitive Markets with Costly State Verification', *Journal of Economic Theory* 21(2), 265–293.

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	0.91**	0.003	-0.001
	(6.35)	(0.37)	(0.003)	(0.009)
Cognition (Digital Span Recall)	2.43	0.21***	0.018*	0.063*
	(1.48)	(0.07)	(0.010)	(0.035)
Risk Tolerance (Quantitative)	6.66	-0.65	-0.002*	-0.012**
	(9.48)	(0.46)	(0.001)	(0.006)
Risk Tolerance (Qualitative)	5.76	0.36*	0.005	0.011
	(3.67)	(0.19)	(0.003)	(0.013)
Positive Reciprocity (0-6)	5.09	0.17**	0.019**	0.020
	(1.39)	(0.07)	(0.009)	(0.038)
Debt Aversion (1-10)	6.10	0.24	0.003	-0.001
	(4.10)	(0.21)	(0.003)	(0.010)
Debt Stigma (1-10)	3.83	0.20	0.002	0.010
	(4.32)	(0.22)	(0.003)	(0.010)
GAD-2 (Anxiety) ≥ 3	0.26	0.01	0.019	-0.127
	(0.44)	(0.02)	(0.029)	(0.097)
Stress (0-100)	33.08	0.42	-0.000	-0.001
	(37.60)	(1.95)	(0.000)	(0.001)
Female	0.65	0.00	0.005	0.205*
	(0.48)	(0.02)	(0.027)	(0.108)
Muslim	0.98	0.00	0.030	-0.057
	(0.14)	(0.01)	(0.093)	(0.168)
Income Last Month	4,286.43	-528.95	-0.000**	-0.000
	(12,543.63)	(551.25)	(0.000)	(0.000)
Owns Sheep or Goats	0.19	0.01	0.040	-0.031
	(0.39)	(0.02)	(0.033)	(0.117)
Prior Equity Experience	0.17	-0.01	-0.028	-0.049
	(0.38)	(0.02)	(0.039)	(0.188)
Prior Formal Loan Experience	0.33	0.03	0.041	-0.264**
	(0.47)	(0.03)	(0.031)	(0.130)
p-value (Joint Test)			0.015	0.030
N			1,716	145

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

Notes: Column (1) shows means (SD in parentheses) for the indifferent group. Column (2) shows coefficients (SE in parentheses) from regressions of each variable on equity-only indicator. Columns (3) and (4) show coefficients from a regression of equity-only on all variables. For joint test regressions, missing values are imputed with means. Experiment sample regressions include cohort fixed effects.

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**	-0.06**	-0.06***
	(0.05)	(0.05)	(0.02)	(0.02)
Debt-Indiff Mean	0.13	0.05	0.07	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 = not fully paid within 30 days of maturity. Default = left at least 5% of death-adjusted obligation unpaid. See Appendix Table A6 for sensitivity to different 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 Shares

	(1) Total Value at Sale	(2) Weight at Sale	(3) Value/KG at Sale	(4) Net Revenue	(5) Bank Share of Revenues	(6) Borrower Share of Revenues
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)						
Equity-Only	699	3	2	713	238	476
	(708)	(4)	(2)	(705)	(235)	(470)
Equity-Indiff Mean	22,372	126	178	1,651	550	1,101
N	225	225	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)						
Equity-Indifferent	-1,385**	-5	-4*	-1,423**	-2,251***	828
	(665)	(4)	(2)	(663)	(184)	(543)
Debt-Indiff Mean	23,678	131	181	2,956	2,797	159
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). 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	1,429***	40	23	-205***	12
	(521)	(419)	(57)	(78)	(280)
Control Mean	4,000	4,000	100	293	3,204
N	387	387	387	387	387
Panel B: Indifferent (Equity vs Debt)					
Equity	1,605**	518	57	159	2
	(633)	(480)	(36)	(131)	(335)
Debt	337	-40	89	173	-406
	(637)	(423)	(57)	(133)	(346)
Control Mean	3,799	3,799	12	148	3,323
N	327	327	327	327	327
Selection Effect (Equity-Only - Equity-Indif)	0.85	0.44	0.64	0.02**	0.92
Contract Effect (Equity-Indif - Debt-Indif)	0.09*	0.23	0.60	0.92	0.25

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

Notes: Sample restricted to non-devaluation cohorts (cohort>=28). Columns 1 and 2 report income in the *last* month, with column 1 adding on the borrower revenue share from ABWA animals (net of bank repayment). The survey questions for the components (Columns 3-5) simply ask about *each* month. Columns 1 and 2 are elicited for the household as a whole, whereas the components are asked of the respondent and the household head (we sum these). 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.

Table 7: Devaluation Effects on Selected Outcomes

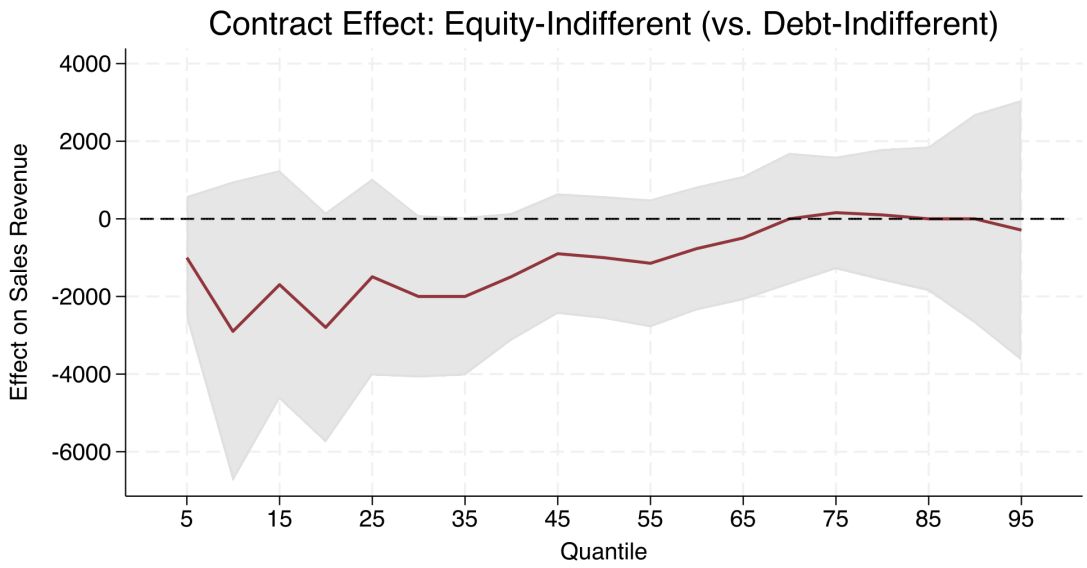
	Bad Outcomes		Good Outcomes		Inputs		Satisfaction	
	(1) Deaths/ Forced Sales	(2) Default	(3) Sale Value	(4) Value per KG	(5) Livestock Costs	(6) Grazing Hours	(7) Satisfaction	(8) Repeat?
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)								
Equity-Only	-0.23***	0.01	699.36	1.82	31.65	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

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

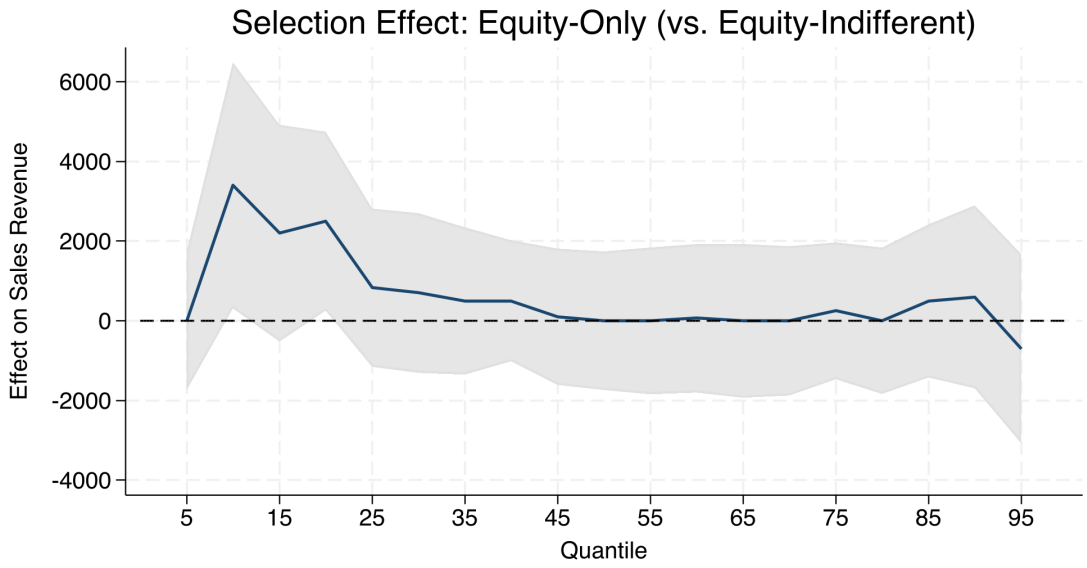
Notes: Devaluation indicator equals 1 if the contract could have spanned either the 2023 or 2024 devaluation episodes (i.e., those in cohorts 27 and earlier). Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

A Appendix Figures and Tables

Figure A1: 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		0.972	0.328

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

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: 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	0.04	-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	-0.01	-0.01	-687.70	-3.57
	(0.05)	(0.08)	(910.87)	(2.83)
Monitoring X Deval	0.05	0.09	26.85	0.31
	(0.05)	(0.12)	(1209.42)	(4.56)
No Monitor Mean	0.06	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 A5: 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 A6: 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	0.07	0.08**	0.03	0.02	0.03*	6.51***
	(0.05)	(0.03)	(0.02)	(0.02)	(0.01)	(2.46)
Equity-Indiff Mean	0.09	0.02	0.01	0.01	0.00	1.98
N	225	225	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)						
Equity-Indifferent	-0.01	-0.05*	-0.06**	-0.06**	-0.06***	-8.51***
	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(3.09)
Debt-Indiff Mean	0.10	0.07	0.07	0.07	0.06	11.87
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 A7: Default Robustness: Different Thresholds

	(1) Default ≥ 1%	(2) Default ≥ 5%	(3) Default ≥ 10%	(4) Default Percent
Panel A: Selection Effect (Equity-Only vs. Equity-Indifferent)				
Equity-Only	0.05*	0.01	0.00	0.21
	(0.02)	(0.01)	(0.00)	(0.14)
Equity-Indiff Mean	0.01	0.00	0.00	0.05
N	225	225	225	225
Panel B: Contract Effect (Equity-Indifferent vs. Debt-Indifferent)				
Equity-Indifferent	-0.05**	-0.06***	-0.04**	-0.74**
	(0.02)	(0.02)	(0.02)	(0.32)
Debt-Indiff Mean	0.06	0.06	0.05	0.90
N	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 2-3 use progressively higher thresholds for the share of obligation left unpaid. Column 4 is the continuous percent of obligation unpaid (0 for non-defaulters). Panel A includes cohort fixed effects. Panel B includes strata fixed effects.

Table A8: 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

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

Notes: Sample restricted to non-devaluation cohorts. 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. Panel A includes cohort FE. Panel B includes strata FE.

Table A9: 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 A10: 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 A11: 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 A12: 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 A13: 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 A14: 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.