Revenue-Based Financing*

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Abstract

We study revenue-based financing, an emerging capital source for small firms in low- and middle-income countries. Using transaction-level data from a South African payment platform, we show firms that take financing process 16% less revenue through the platform than observably similar non-takers after eight months, slowing repayment. Two natural experiments show this reflects moral hazard from firms diverting revenue and adverse selection. Repayment improves when firms use the platform's other services (e.g., inventory management tools), and screening improves with longer histories and repeat financing. Our results highlight the frictions with flexible repayment models in developing economies, and how providers mitigate them.

JEL codes: D21, D22, G20, G23, O16

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1 Introduction

Small firms are central to employment and growth in low- and middle-income countries, but many face severe credit constraints due to information and enforcement frictions (Ayyagari *et al.*, 2011; Karlan and Morduch, 2010; Djankov *et al.*, 2008). As developing economies transition from cash to digital payments, new models of financing have emerged that may help address these constraints. One prominent example is revenue-based financing offered by technology platforms (e.g., payment processors, e-commerce platforms), which automatically deduct a fixed share of daily on-platform revenue until a set amount is repaid.[1]

Some features of revenue-based financing are promising. In settings with high enforcement costs, contracting over all firm revenues is often infeasible, and automatic repayment through digital technologies offers a scalable solution. The contract also shifts risk to the financier, as repayment speed changes with revenue, which may encourage investment (Cordaro *et al.*, 2025; Meki, 2025).[2] Yet this flexibility introduces contracting challenges: the contract is attractive to firms expecting low future revenue, and borrowers can divert sales off the platform to delay repayment. How severe is this adverse selection and moral hazard in practice, and what can providers do to mitigate these challenges?

In this paper, we study revenue-based financing at scale using data on over 100 million transactions from a South African financial technology platform. We develop a conceptual framework that highlights the key frictions in these contracts and maps them to the data. Empirically, we show that these frictions are large, and that firms who take financing divert sales off the platform when substitutes are cheap. However, providers can limit both adverse selection and moral hazard from diversion using information and incentives related to repeated interactions. Our findings offer direct evidence on the frictions in flexible finance models and insights into addressing them.

We organize our analysis with a conceptual framework. In environments with costly enforcement and private information, revenue-based financing is vulnerable to moral hazard from revenue hiding and to adverse selection. The framework predicts that limiting both, by increasing the cost of hiding and screening effectively, is necessary to sustain a lending equilibrium. It also shows that the ex-post revenue gap between observably similar firms that do and do not take financing ("takers" versus "non-takers") can be decomposed into three components: adverse selection, moral hazard from revenue hiding, and the causal effect of

[1] See, for example, PrestaClip in Mexico, PayMongo Capital in the Philippines, Velocity in India, Kopo Kopo in Kenya, and PagBank in Brazil, among many others. The model has also attracted attention from international organizations, including the World Bank (e.g., Weis and Kebede, 2024; Weis and Alibhai, 2025).

[2] We discuss the contract in more detail in Section 2. This contract differs from providers in high-income countries, such as Square, Toast, and Shopify in the US, which impose a repayment floor if sales fall below a certain threshold. Such a floor may be difficult to enforce in many developing and emerging markets.

financing on takers. The first two components reduce the observed revenue of takers.

We begin our empirical analysis by estimating this revenue gap between takers and non-takers on the platform. Eight months after receiving financing, takers process 16% less on-platform revenue than matched non-takers, consistent with moral hazard and adverse selection. This gap is primarily driven by an intensive-margin decline in takers' revenue relative to non-takers, rather than by higher default; even conditional on survival, takers' revenue is 15% lower than the matched control. We show that this gap is robust across a range of regression and fully flexible machine learning estimation methods.

Next, we test the framework's predictions about the determinants of this gap. The framework predicts that moral hazard from revenue hiding will increase when substitutes become cheaper. We test this by exploiting a shock to a rival processor's pricing in response to funding by the World Bank Group to "make digital payments systems more affordable." We use a triple-difference design identified from variation between takers and non-takers across regions and time.[3] The funding shock for the competitor led to a 10% relative decrease in takers' transactions, consistent with hiding when diversion becomes cheaper.

Two additional pieces of evidence support the framework's prediction that takers hide revenue when low cost substitutes are available. First, using a triple-difference design comparing online and in-person sales of takers and non-takers over time, we find online sales—easiest to divert because they require no new hardware—are more likely to disappear after an advance. Second, we show that takers with a larger share of transactions in cash perform worse than similar non-takers, suggesting some firms may shift card sales into cash.

Whereas lower hiding costs increase diversion, the framework also predicts that providers can mitigate moral hazard by raising hiding costs. We present evidence consistent with this prediction. Among firms that use the Platform's add-on services (such as accounting and inventory management tools) the revenue gap between takers and non-takers falls by nearly half. This suggests that "sticky" features that increase the cost of hiding help limit moral hazard. We also conduct a back-of-the-envelope exercise that shows the cost of hiding for the average taker is at least 45,000 ZAR, slightly above monthly revenue. This high cost suggests that technology platforms can meaningfully limit diversion in a developing economy.

We then examine the framework's second key friction, adverse selection, and how providers can limit it. We present quasi-experimental evidence that a longer history of transactions improves screening. Specifically, due to a temporary system error, businesses that joined the platform after March 2022 and met minimum activity requirements were only offered an advance after six months on the platform, instead of the usual three. After this change,

[3] As we discuss in Section 5.1, this design, therefore, accounts for any time-region specific shocks under the plausible assumption that any potential confounds would affect takers and non-takers similarly.

total revenue in the eight months post-advance for takers was 5% higher, controlling for pre-advance revenue, showing that information from additional interactions improved screening. We also show that repeat advances outperform first advances even conditional on time on platform, consistent with lenders continuing to learn about borrowers from repeat financing.

Finally, guided by our framework, we decompose the revenue gap and find suggestive evidence that revenue-based financing supports business growth. We use estimates from our two natural experiments: the temporary delay in advance offers suggests that adverse selection accounts for about 60% of the gap in the short run, while conservative estimates of moral hazard from the rival price cut more than explain the remaining 40%. Since these forces together are larger than the gap, the implied causal effect is positive. Assuming takers hide one-tenth of their revenue, the positive causal effect is an 8% revenue gain. Consistent with this, takers with small initial geographic footprints were more likely than similar non-takers to expand across locations over the subsequent eight months.

This paper makes three main contributions. First, we provide large-scale evidence on revenue-based financing, an emerging product in many developing economies where small firms have traditionally faced credit constraints. Most closely related to our setting is Rishabh and Schäublin (2021), who study sales-linked loans in India and document revenue hiding immediately after disbursement among some repeat borrowers. Relative to this work, our framework clarifies and measures frictions in revenue-based financing, beyond strategic default for a subset of firms.[4] We also present evidence on how a technology provider sustains the contract despite revenue hiding and adverse selection. In doing so, we contribute to a literature on technology and credit access in developing economies, which has emphasized improved screening (Björkegren and Grissen, 2018; Hau *et al.*, 2019; Alok *et al.*, 2024; Chioda *et al.*, 2024; Ghosh *et al.*, 2025), stronger enforcement (Gertler *et al.*, 2024), and faster, lower-cost transactions (Suri *et al.*, 2021; Cramer *et al.*, 2024).[5] Our results provide an example of both the promise and limits of technology to ease credit constraints.[6]

Second, we show the challenges of implementing flexible, performance-contingent contracts in developing economies, and provide evidence on when these frictions can be mitigated. While small firms in low and middle-income countries often earn high returns on capital (e.g., De Mel *et al.*, 2008; McKenzie and Woodruff, 2008), lending interventions

[4] Understanding these issues is important, in part, because they help set equilibrium prices, and recent work suggests borrowers may not always receive favorable terms on digital credit (Brailovskaya *et al.*, 2024).

[5] More broadly, we contribute to strands of literature on household finance and development (reviewed by Badarinza *et al.*, 2019), the effects of digital payments on consumers and firms (see e.g., Jack and Suri, 2014; Breza *et al.*, 2020; Higgins, 2024; Agarwal *et al.*, 2025), and the rise of FinTech lending in both developed and developing markets (for overviews, see Berg *et al.*, 2022; Agarwal and Zhang, 2020; Allen *et al.*, 2021).

[6] Our paper also provides empirical evidence to support anecdotal accounts of individuals concealing digital revenue in low- and middle-income countries. See examples from Ghana, Kenya, Uganda, and Brazil.

have achieved limited success (Banerjee *et al.*, 2015). This has motivated the exploration of equity-like performance-contingent contracts (e.g., Ayayi, 2012; De Mel *et al.*, 2019; Meki and Quinn, 2024; Cordaro *et al.*, 2025).[7] For example, Meki (2025) highlights the potential of debt–equity hybrid contracts that cap financiers' upside, similar to the contracts we study. Much of this work emphasizes borrower benefits, using theory or relatively small-scale RCTs, but realizing these benefits at scale depends on whether providers can overcome moral hazard and adverse selection generated by contingent contracts.[8] Our work provides practical insights into how and when performance-contingent contracts may scale.

Third, methodologically, we show how data on both takers and non-takers, combined with the observables available to the decision maker, can be used to test for asymmetric information in selection markets. We demonstrate how natural experiments can decompose the difference in outcomes between takers and non-takers into moral hazard, adverse selection, and the causal effects of take-up. In doing so, we extend prior approaches that measure asymmetric information using price variation (Einav *et al.*, 2010; Einav and Finkelstein, 2011), administrative or survey data (Herbst and Hendren, 2024), and field experiments (Karlan and Zinman, 2009; Brune *et al.*, 2022; Jack *et al.*, 2023).

2 Background & Data

2.1 Institutional Details

Setting. Our data comes from a major South African financial technology platform ("the Platform"). The Platform primarily offers card processing machines and online payment interfaces to small businesses, currently processing over $2 billion USD ($5 billion USD purchasing power parity) in transactions per year for 250,000 active businesses and 25 million consumers.[9] These businesses are estimated to represent 10% of all South African small and medium-sized enterprises (SMEs), including a large number of unregistered informal firms in both urban and rural areas.[10] The Platform also offers a revenue-based financing "capital advance" product that borrowers repay with their daily processed payments. Since 2018, the

[7] A number of works discuss different forms of repayment flexibility (e.g., Karlan and Mullainathan, 2007; Field *et al.*, 2013; Brune *et al.*, 2022; Barboni and Agarwal, 2023; Battaglia *et al.*, 2024).

[8] See, for example, literatures on costly state verification (e.g., Townsend, 1979; Webb, 1992; Winton, 1995; Bond and Crocker, 1997) and equity-like financing in a variety of settings (e.g., Friedman, 1955; Leland and Pyle, 1977; Alfaro and Kanczuk, 2005; de Silva, 2023; Herbst and Hendren, 2024).

[9] The Platform takes 2-3% of each transaction before depositing the rest into business bank accounts.

[10] Estimates of the total number of SMEs in South Africa vary widely. While tax and registration data from 2016 pointed to only around 250,000 small businesses *total*, this excludes a large informal sector in which firms remain unregistered and bypass taxation (Small Business Institute, 2019). By other estimates, the number of total SMEs is over 2 million (OECD, 2022; United Nations, 2023).

Platform has issued over 65,000 advances and re-advances.

In general, a large majority of South African SMEs are self-funded and many face financing challenges. In one survey, firms ranked financing as their second most important obstacle, behind only a national electricity crisis (World Bank, 2021).[11] Another survey reported that only 2% of SMEs rely on bank financing (SME South Africa, 2018). South Africa is representative of many low- and middle-income countries where SME funding gaps remain pervasive: formal SMEs in developing countries face an annual financing gap of $5.2 trillion, roughly 1.4 times current global SME lending (World Bank, 2017).

Revenue-based financing, if successful, has the potential to help close this gap in small business credit. These contracts are often offered by technology companies—such as payment processors or e-commerce platforms—that have access to sales history and can automatically deduct repayments. In principle, this design can ease both information and enforcement frictions that hinder traditional lending in low- and middle-income countries.

Contract Details. The Platform's advance is a typical form of revenue-based financing in emerging markets. Offers consist of a principal, a factor rate, and a charge rate. If the business takes the offer, the Platform deposits the principal within one business day. The Platform then automatically deducts a share of each future transaction (the charge rate) that goes through their platform until the principal $\times$ factor rate is paid off. There is no fixed term, and repayment is quicker if revenue increases and slower if revenue falls. There are no additional fees or interest for slower repayment, and the Platform only deducts from the revenues they process. Two partner companies, who observe past transactions and time on platform only, provide the capital and set the contract terms. Advance offers are extended to small businesses that processed transactions for at least three months on the platform and met minimum activity requirements.[12]

For illustration, consider an advance with a $2000 principal, a factor rate of 1.3, and a charge rate of 20%. When the offer is accepted, $2000 will be directly deposited into the business's account. To repay the advance, the Platform then deducts 20% off each transaction they process until these deductions sum to $\$2000 \times 1.3 = \$2600$. If the business processes $1500 in transactions per month, the $2600 is paid over 8.7 months. If, instead, the business processes $1000 in transactions per month, the same $2600 is paid over 13 months, lowering the implicit APR. We provide summary statistics on the terms of the contract in Section 4.

[11] Among exiting SMEs, 22% said their challenges were due to "problems getting financing" (United Nations, 2023). Given South Africa's rolling blackouts (Kozak, 2023; Clarke *et al.*, 2025), financing has also become important for firms to invest in backup power solutions (e.g., generators and solar systems).

[12] As of July 2023, these requirements were a minimum of 18 card transactions over the last 90 days and a monthly turnover rate of more than 3,000 ZAR per month.

Contract Design & Frictions. In settings with high enforcement costs, contracting over all revenues of small firms is often infeasible. Automatic repayment from platform revenues offers a scalable solution, but it also introduces moral hazard: borrowers may divert sales off-platform to reduce repayment. In practice, the extent of this diversion is unclear. Contracts usually prohibit diversion, shifting to alternative processors can be costly, and using cash is limited by many consumers' preference for cards.[13] Partial diversion also only lowers the implicit APR, which requires financial sophistication that some small firms may lack.[14]

Additionally, the contract raises adverse selection concerns, as businesses with lower revenues repay more slowly. In settings with few alternative financing sources, however, it is unclear ex-ante whether low-revenue firms would disproportionately select advances. Meki (2025), for example, shows in experiments in Kenya that hybrid debt-equity contracts, similar to revenue-based financing, attracted higher take-up and led to more profitable investments than alternative models. This suggests flexible contracts can draw in high-potential firms.

We explore these potential frictions by comparing observably similar advance takers and non-takers.[15] The lack of random variation in contract terms limits our ability to speak to the effects of pricing and other features, so we take the terms—which are typical across developing economy contexts—as given, and focus on the motivating questions above.[16]

2.2 Data & Summary Statistics

Data Overview. Our data cover all transactions for both takers and non-takers, including the amount, time, location, and payment method (e.g., card processor, online) of the transaction. We also use data on platform activity (e.g., clicks in an inventory management tool), along with business characteristics including location, industry, and owner demographics. Finally, we observe all advances, including principal, pricing, and repayments. We restrict the sample to advances made from June 2020 onward (to exclude the strictest COVID-19 lockdown) with at least 12 months of post-advance outcomes by May 2024.

Advance Summary Statistics. Table 1 provides summary statistics on the 15,375 first advances and 23,879 repeat/re-advances in this sample.[17] Advance takers are generally small,

[13] Only around 30% of consumers are considered especially cash reliant in South Africa (University of Pretoria and SBV Services, 2023). See Higgins (2024) for evidence from another emerging economy, Mexico.

[14] Small firms often forgo profitable opportunities (e.g., Gertler *et al.*, 2025; Banerjee *et al.*, 2022; Bruhn *et al.*, 2018), and financial literacy, as measured by Nanziri and Olckers (2019) is only 40% in South Africa.

[15] A key advantage in our setting is that the offers are based on observables available to us. Therefore, differences between takers and non-takers reflect asymmetric information from the platform's perspective.

[16] As noted in Footnote 2, these contracts differ in high-income countries with fewer enforcement frictions.

[17] Additional advances can be taken out after fully paying off previous advances (repeat advance) or *before* fully paying off previous advances, adding to the outstanding amount due (re-advance). We will generally

consumer-facing businesses and entrepreneurs (e.g., hair salons, food trucks).[18] First-time takers have an average of 120,000 in South African Rand ($\sim\$7,000$ USD) in sales over the prior three months.[19] The average principal of first advances is around one month's worth of processed revenue. Combined with the average charge rate of 20% and factor rate of 1.3, this implies an estimated repayment period of 7-8 months.[20] On average, first-time capital takers paid 1.06 times their original principal, or 1.05 and 1.03 when each payment is discounted at annualized rates of 5% and 15% respectively. Panel B shows that repeat advances have higher repayment rates after one year: 1.24 times the original principal on average.

Figure 1 shows the outcomes of advance takers one year later. 22% of first-time takers have an open advance but no transactions in the last 30 or more days, nearly 1.5 times the rate of repeat takers (15%). For both groups, despite the 7-8 month average estimated repayment period, a sizeable majority have some open advance one year later.

Predictors of Advance Performance. We construct two measures of performance. A business is classified as *defaulting* if it has no transactions (and therefore no payments) in the eighth month after taking an advance but still has an outstanding balance.[21] For non-defaulters, *conditional revenue* is total transactions over the subsequent eight months.

Table 2 shows that time on platform and pre-advance transaction volume strongly predict these outcomes. Columns (1) and (2)—which control for owner demographics, quarter, and advance characteristics—show that for first advances, an additional year on the platform corresponds to a 3.7 percentage point decrease in default and a 3.2% increase in conditional revenue. A doubling of pre-advance volume corresponds to an 82% increase in conditional revenue. Greater pre-advance volatility predicts default but not conditional revenue. Columns (3) and (4) suggest that similar patterns hold across repeat advances, and that first advances are more likely to default.

refer to both as repeat advances in the text.

[18] Appendix Table G.1 summarizes the advance takers by industry and sub-industry. The two most common sub-industries are "Beauty Salon/Spa" and "Bar/Club/Wine Farm."

[19] By purchasing power parity, 120,000 ZAR is roughly $16,000 USD.

[20] The implicit APR, if sales from the last three months stay constant, is around 80%. To understand this, a factor rate of 1.3 over eight months implies a lump sum repayment APR of around 40%. However, because repayment is *daily*, early payments are made at a much higher effective APR. This roughly doubles the APR again (for related discussion, see Stango and Zinman, 2009).

[21] Appendix Figure F.1 provides hazard plots.

3 Conceptual Framework

We present a stylized model to clarify the frictions in revenue-based financing and motivate our empirical analysis. Two challenges are central: moral hazard, where borrowers may hide revenue ex post, and adverse selection, where borrowers have private information about future revenue ex ante. We show how these frictions contribute to the revenue gap between takers and non-takers, which we estimate in Section 4. Proofs are in Appendix A.

3.1 Setup

A risk-neutral lender may offer small businesses "advances" of the form (η, L). If accepted, businesses receive L in period 1 and are expected to repay η share of their revenue in period 2.[22] A small business i will have revenue $Y_i(1)$ when they take an advance and $Y_i(0)$ when they do not. This can be viewed as a potential outcomes framework in which businesses who take an advance if offered are "compliers" and those who do not are "never takers."

Business Types. Businesses differ in their potential revenue outcomes, $Y_i(0)$ and $Y_i(1)$. The latter is shaped by their investment opportunities μ_i. The lender observes only a set of ex-ante characteristics, X, which provides information on these outcomes. To start, we assume that conditional on observables there are two types:

- "Bad" (B) types with no investment opportunities: $Y_i(0) = 0, \mu_i = 0$
- "Good" (G) types with investment opportunities: $Y_i(0) = y; \mu_i = \mu_X$

The lender cannot distinguish between types, but knows the mix of G and B types conditional on observable characteristics X: $P(G|X) = p$ and $P(B|X) = 1 - p$. As businesses with characteristics X are ex-ante identical, the lender must offer the same advance (if any) to all businesses with the same X.

Enforcement Frictions. If an advance is extended, the lender must monitor the borrower's revenue and enforce repayment. Since full enforcement is often prohibitively costly in low- and middle-income countries (LMICs), the lender can only observe and collect a fraction $v(c)$ of revenue, where $c < \infty$ is the borrower's cost of hiding. This cost is shaped

[22] This two-period framework abstracts from the Platform's full dynamics where the financier captures a share of payments until a fixed threshold is met. However, our stylized contract is enough to capture the two main features of revenue-based financing. First, the amount repaid is in practice often lower than the threshold since transactions may fall to zero. Second, when revenue falls the *present value* of repayments falls even if the payment threshold does not change, lowering the time-discounted cost of the contract.

both by factors outside the lender's control (such as the availability of substitutes) and by the lender's ability to improve enforcement technology.

Business's Problem. Businesses choose whether to accept the advance (η, L). If they accept, they can either invest or "consume" it. Denote $v^{\dagger}(c)$ as optimal hiding if the business makes the investment, and $v^{\bullet}(c)$ as optimal hiding if the business consumes the advance. The cost of hiding share $1-v$ of revenue is $c(1-v)^2$ and discounting occurs at rate r. Then, businesses choose to take the advance iff:

$$\max\{\underbrace{(1-\eta v^{\dagger})(\mu_i + Y_i(0)) - c(1-v^{\dagger})^2}_{\text{Invest }(I)}, \underbrace{(1-\eta v^{\bullet})Y_i(0) + L(1+r) - c(1-v^{\bullet})^2}_{\text{Consume }(C)}\} > Y_i(0). \tag{1}$$

Bad types will always choose to take the advance to consume L. Good types may choose to invest when η is low and μ_i is high. Thus, there is both positive selection (the contract is attractive for businesses with better investment opportunities μ_i) and negative selection (the contract is attractive for businesses with low $Y_i(0)$).

Lender's Problem. The lender chooses η and, for simplicity, leaves L fixed. Lender profits for a set of observationally equivalent borrowers with characteristics X is:

$$\pi_X = \arg\max_{\eta} \ \eta\mathbb{E}[v^* Y(1)|X, Taker] - L(1+r). \tag{2}$$

where v^* is the optimal amount of hiding chosen by the business. From the business's problem in Equation 1, $v^* = v^{\dagger}$ and $Y_i(1) = Y_i(0) + \mu_i$ if returns to investment exceed returns to consuming the advance, and $v^* = v^{\bullet}$ and $Y_i(1) = Y_i(0)$ otherwise. Assume lenders make zero profits in equilibrium, which pins down η for each X. If lenders cannot make non-negative profits for any $0 < \eta < 1$, no advances will be offered. Intuitively, for any X in which revenue-based financing is offered, good types must cross-subsidize the bad types.

In our baseline model, the borrower faces no uncertainty, so the repayment flexibility offered by the contract does not affect risk and investment. In Appendix B, we show that in a model with uncertainty, revenue-sharing—which moves risk from the borrower to the lender—can attract new risk-averse investors to make positive NPV investments.

3.2 Framework Predictions

3.2.1 Asymmetric Information Impedes Revenue-Based Financing

Proposition 1 formalizes the frictions lenders must overcome to sustain revenue-based financing in an environment with hidden types and enforcement frictions.

Proposition 1. *The following frictions may cause revenue-based financing to unravel:*

1. *Revenue hiding: When c decreases, η weakly increases. Additionally, there exists a $\bar{c}$ such that for all $c < \bar{c}$, revenue-based financing is impossible.*
2. *Poor screening: When p decreases, η weakly increases. Additionally, there exists a $\bar{p}$ such that for all $p < \bar{p}$, revenue-based financing is impossible.*

Proposition 1 makes two intuitive predictions about asymmetric information frictions. First, revenue-based financing incentivizes revenue hiding; if hiding costs c are low, contracts may unravel. Second, revenue-based financing can attract bad types since repayment is state-contingent; better screening (higher p) improves contract performance.

The proposition also highlights two levers that digital payment providers can use to mitigate asymmetric information. First, raising the cost of hiding can deter diversion. While automatic deductions limit hiding, firms can still switch to cash or rival processors. If a technology provider is valuable outside of financing (e.g., because alternatives are inconvenient or add-on services are valuable), the cost of hiding may increase. Second, improving screening can reduce adverse selection. Payment providers observe detailed revenue and past repayment data. In contexts with informal and unbanked businesses, this data may substantially improve screening. We explore these levers in Section 6.

3.2.2 Asymmetric Information Leads to Revenue "Gap"

How do we empirically test for asymmetric information frictions? Proposition 1 is challenging to bring to the data for two reasons. First, researchers see no more than lenders, and cannot neatly classify firms into types characterized by p. Second, the cost of hiding, c, is not directly measurable. To overcome these challenges, we allow for arbitrary relationships between observables and business outcomes and define the following three objects:

Definition 1. *Let moral hazard from revenue hiding (MH), adverse selection (AS), and the causal effect of financing on takers (CE) be defined in the following way:*

$$MH \equiv \mathbb{E}[(1 - v)Y(1)|Taker] \tag{3}$$

$$AS \equiv \mathbb{E}[Y(0)|\textit{Non-Taker}] - \mathbb{E}[Y(0)|\textit{Taker}] \tag{4}$$

$$CE \equiv \mathbb{E}[Y(1) - Y(0)|\textit{Taker}] \tag{5}$$

Equation 3, which defines moral hazard from revenue hiding, captures the quantitative effect of hiding on the ex-post revenue that the financier can observe and collect.[23] It is shaped by c and defined over advance takers, as only takers have an incentive to hide. Equation 4, defining adverse selection, captures the effect of businesses with lower average future revenue differentially selecting into advances.[24] Equation 5, defining the causal effect of financing on takers, is shaped by the investment opportunities available to takers. In a potential outcomes framework, CE is the average treatment effect on the treated (ATT).

Revenue Gap This framework provides a straightforward tie between the model's frictions and ex-post difference in revenue between observably identical businesses that do and do not take advances. We call this object the "Gap" in revenue. The Gap is measurable because our data includes the observables, X, available to the Platform, as well as the processed transactions for both advance takers and *non-takers*. Proposition 2 shows that the Gap can be decomposed into the three forces in Definition 1.

Proposition 2. *Let X be the set of characteristics observed by the financier (and econometrician). Define:*

$$Gap \equiv \int \bigg(\mathbb{E}[Y(0)|X = x, \textit{Non-Taker}] - \mathbb{E}[vY(1)|X = x, Taker]\bigg) \cdot f(x|Taker)dx. \tag{6}$$

Then:

$$Gap = \int (AS - CE + MH)|(X = x) \cdot f(x|Taker)dx. \tag{7}$$

Proposition 2 implies that when advances are randomly assigned and $v = 1$, the Gap provides an estimate of the (opposite signed) causal effect of financing on takers (ATT). However, asymmetric information invalidates this interpretation. In particular, a large (positive) Gap provides evidence for the existence of moral hazard, adverse selection, and/or negative causal effects. With non-negative causal effects, the Gap bounds adverse selection and moral hazard from below. This decomposition can be generalized for other selection markets.[25]

[23] An alternative form of moral hazard associated with equity contracts arises in a principal-agent setting where managers reduce effort. However, in revenue-based financing contracts, the owner retains full control of business revenues after the advance is paid off, reducing the potential for a negative impact on effort. Formally, shirking would decrease $Y(1)$, appearing in both the causal effect and moral hazard terms.

[24] For simplicity, in Section 3.1 adverse selection arose from private information about "types." Empirically, adverse selection could also arise from other differences in unobservables (e.g., financial sophistication).

[25] In our setting we can parameterize moral hazard using v. With other forms of moral hazard, one can

3.2.3 Empirical Tests of Propositions 1 & 2

In the remainder of the paper, we bring our framework to the data and test its key predictions. First, we test Proposition 2 in Section 4, documenting a substantial Gap between takers and non-takers. Second, we test Proposition 1, which predicts that changes in c (the cost of hiding) and p (adverse selection) determine the extent of asymmetric information. Section 5 shows that the increased availability of payment alternatives, which lowers c, decreases the performance of advances, as the model predicts. Section 6 shows how providers can mitigate asymmetric information by raising c with valuable add-on features, and raising p by extending screening periods. Third, Section 7 uses Equation 7 to decompose the Gap and estimate CE, the effects of revenue-based financing on businesses.

4 Empirical Revenue Gap Between Takers and Non-Takers

We estimate the revenue gap between takers of revenue-based financing and observably similar non-takers, as motivated by Proposition 2. Takers have significantly lower transaction volume on the Platform after receiving financing, consistent with asymmetric information.

Matching Estimator. In our baseline analysis, we match each first-time advance-taking business to their nearest "control" non-taker.[26] In particular, for each taker, we find a non-taker in the same month and industry who met the minimum advance eligibility requirements and is closest in terms of time on platform and transaction amount in the month before the advance (according to normalized Euclidean distance).[27] The average difference in post-advance revenue between takers and their matched control businesses then provides an estimate of the Gap in Equation 6.

Figure 2 shows a time-series plot of the average on-platform revenue of takers and the matched control. The two appear similar before the advance but then begin to diverge after the advance, consistent with adverse selection and moral hazard. By month eight, the average revenue of the takers is 16.3% lower than the matched control.

The average outcomes in Figure 2 can be driven downward by intensive margin revenue decreases or exits from the platform. Panel (A) of Figure 3 separates out the former by limiting to matched pairs where both the taker and control business transacted in the eighth

define $Y(1)$ as the decision maker's outcome of interest (instead of $vY(1)$) and let the Gap be the sum of our AS and $-CE$ terms, with the latter including moral hazard. The total Gap can be estimated in any data that includes outcomes for takers and non-takers, as well as the observables available to the decision maker.

[26] We show results with $K = 1$. Results are similar when averaging across larger sets of K neighbors.

[27] Appendix Table G.2 summarizes the sample of takers and matched control firms in our baseline matching analysis, showing the two are generally well-balanced across matched and unmatched observables.

month. The advance takers' average revenue is 15.2% lower than the matched control in month eight, only slightly smaller than the unconditional gap in Figure 2. Accordingly, Panel (B) shows that advance takers are only 4% (0.8 percentage points) more likely to disappear. These results suggest that "running away"—taking the advance with the intent to close or default—is relatively less important than the decrease in intensive margin revenue.

Additional Analyses. We conduct robustness tests to confirm our interpretation. Columns (5) and (6) of Table 2 show, in a regression framework, that takers have lower conditional revenue than non-takers, but default at similar rates.[28] Appendix E shows that panel regression and machine learning approaches provide similar results to our matching approach. Appendix Figure F.2 shows the Gap still exists among businesses with revenue more than five times larger than the eligibility threshold, suggesting it is not driven by businesses manipulating revenue to become eligible.

Our results show that asymmetric information is central to revenue-based financing, consistent with our model. To scale revenue-based financing, providers must reduce these frictions. The next sections analyze the sources of asymmetric information and potential mitigants.

5 Empirical Evidence of Revenue Hiding

Proposition 1 suggests that takers will divert revenue off the Platform when it becomes cheaper to do so, widening the Gap between takers and non-takers. In this section we test this prediction. We present three pieces of evidence that borrowers hide using both other processors and cash, and that this hiding responds to the availability of substitutes.

5.1 Processor Competition Reduces Advance Performance

When a competing payment processor lowers its prices, the cost of hiding falls. Exploiting a shock to a rival's pricing, we show that firms respond by shifting transactions off-platform.

Background. We use a natural experiment involving a primary rival ("the Competitor") of the Platform. The Competitor offers similar payment processing products. Whereas the Platform operates out of Cape Town, the Competitor started in Durban and primarily

[28] For this analysis, we include all eligible non-takers, assigning them a placebo advance date one year after joining the platform, with outcomes measured over the subsequent eight months. One year is approximately the median time on the platform before a first advance for takers. Our results do not significantly change when using other counterfactual times on the platform. The magnitude of this result is *cumulative over eight months*, which is similar to the effect over this horizon in Figure 2.

operated in the surrounding region before expanding. In March 2021, the Competitor's parent company received \$15 million USD from a World Bank Group member to "make digital payments systems more affordable."[29] Accordingly, they cut the price of their flagship product by more than 50% over the next six months, as shown in Appendix Figure F.3.

As the Competitor reduced its prices, it became cheaper for the Platform's advance takers in areas around Durban to shift transactions to another processor.[30] Proposition 1 suggests this reduction in hiding costs, c, would worsen repayment. Empirically, if advance takers shift revenue to rival processors, one would expect a greater post-advance decline in the number of transactions after price cuts among the Platform's users in Durban relative to users around Cape Town. This effect should also be concentrated in takers, not non-takers.

Empirical Design. To test this prediction, we estimate a triple-difference design comparing takers and non-takers across Durban versus Cape Town, before and after the price cuts. This design accounts for any region–time shocks, which we would plausibly expect to affect takers and non-takers similarly.[31] Appendix Figure F.4 shows the underlying variation: Panel (a) compares takers across regions, and Panel (b) does the same for non-takers.[32] The contrast between the two series provides the identifying variation. Empirically:

$$\begin{aligned} Y_{i,t} = \alpha_0 + &\alpha_1 \text{Post}_t + \alpha_2 \text{Taker}_i + \alpha_3 D_i + \kappa_1(\text{Post}_t \times \text{Taker}_i) + \\ &\kappa_2(\text{Post}_t \times D_i) + \kappa_3(\text{Taker}_i \times D_i) + \beta(\text{Post}_t \times \text{Taker}_i \times D_i) + \mathbf{X}_{i,t} + \epsilon_{i,t} \end{aligned} \tag{8}$$

For business i in time t, $Y_{i,t}$ is the ratio of average monthly transactions in the eight months after to the three months before the advance. Post_t is an indicator for after the price change. Taker_i is an indicator for whether firm i took an advance and D_i for whether firm i is located in Kwazulu-Natal/Eastern Cape (Durban region) versus Western/Northern Cape (Cape Town region). $\mathbf{X}_{it}$ includes industry-by-month fixed effects,[33] pre-advance transaction volume, and months since first joining the platform. To show trends pre- and post-event, we

[29] See the archived press release here.

[30] While it is possible that businesses outside of Durban may have also substituted, the Competitor's advertising and processing machine distribution was not (initially) targeting other areas. Additionally, any substitution to the Competitor in the Cape Town area will only bias our results toward zero.

[31] Of particular relevance is the unrest in July 2021, which began in Durban and spread nationally (see e.g., Harding, 2021). Because our design compares takers and non-takers within a region, any riot-related effects are differenced out under the plausible assumption that both groups were similarly affected.

[32] Seasonality drives much of the raw fluctuations in both series: in Q3, for instance, the numerator of the outcome includes the December holidays, whereas in Q1 the denominator does. We include industry-by-month fixed effects to account for any seasonal differences within quarter across taker status and province.

[33] For ease of exposition, Equation 8 shows time fixed effects explicitly. In practice, these are absorbed by the industry-by-month terms.

estimate a (quarterly) dynamic version of Equation 8.[34]

Results. Figure 4 shows that the triple-difference coefficient, β, falls by roughly 10 percentage points following the rival's price cuts, implying a larger contraction in takers' post/pre transaction ratio in Durban, relative to Cape Town and relative to non-takers. The effect moves closer to zero as the Competitor expands throughout South Africa in 2022. Our results are consistent with revenue hiding caused by the Competitor's price drop, and suggest that takers divert transactions when hiding becomes cheaper.

5.2 Takers' Online Sales Disappear After an Advance

Online sales can be shifted off-platform more easily than in-person payments. Consistent with firms hiding, online sales are more likely to disappear after an advance.

Background. As noted in Section 2.1, the Platform processes transactions not only through physical card machines, but also online gateways (e.g., plugins to Wix and WordPress). Prior to taking an advance, 17.9% of takers have any online transactions, and for these firms, online sales average 14.4% of revenue. These online transactions are subject to repayment deductions. They are also cheaper to shift entirely off the Platform: firms do not need to acquire new hardware and can sign up for alternative providers with no sign-up fees.[35] Proposition 1 suggests these lower hiding costs, c, would make online transactions more likely to disappear.

Empirical Design. We test whether firms hide by diverting online sales to other providers. Specifically, we track the presence of online and in-person transactions over time for a sample of advance takers and non-takers with online transactions preceding the (actual or placebo) advance date.[36] The identifying variation then comes from three comparisons: within-firm changes before versus after the advance, across online versus offline transactions, and between advance takers and non-takers. Formally this is,

$$Y_{i,j,t} = \mu_i + \alpha_1 \text{Post}_t + \alpha_2 C_j + \kappa_1 (\text{Post}_t \times \text{Taker}_i)$$

[34] Appendix Table G.3 shows estimates from the two-period model in Equation 8. The note for Figure 4 provides the full dynamic specification. We include one observation for each taker in the quarter of their first advance and observations for each non-taker in all quarters in which they were eligible.

[35] See, for example, Peach Payments and PayFast. Firms may also divert some in-person sales to cash (see Section 5.3). However, some consumers strongly prefer card payments (see, e.g., University of Pretoria and SBV Services, 2023) making full diversion less feasible. This suggests that cash hiding will be more important on the intensive margin than extensive margin, and we focus on the latter here.

[36] We include businesses who had online transactions three or more months before the actual or placebo advance date. We include non-takers as described in Footnote 28.

$$+\kappa_2(\text{Post}_t \times C_j) + \kappa_3(\text{Taker}_i \times C_j) + \beta(\text{Post}_t \times \text{Taker}_i \times C_j) + \epsilon_{i,j,t}. \quad (9)$$

For business i, $Y_{i,j,t}$ is an indicator for whether the business has transactions of type j (online or in-person card) in time t. Post_t is an indicator for post-advance, Taker_i is an indicator for whether firm i took an advance, and C_j for whether the transaction type is online. μ_i is a business fixed effect. To show pre- and post-trends we estimate β by month.[37]

Results. Figure 5 shows that β falls after the advance, consistent with firms diverting online sales to other providers. The effect is gradual at first, but three months later takers are about four percentage points less likely to record online transactions, relative to their offline activity and to non-takers. Appendix Figure F.5 shows the underlying difference-in-differences patterns: the effect is driven by disappearing online transactions, while the probability of in-person transactions does not substantially fall.[38] The disappearance of online transactions shows how takers disproportionately hide sales that are easiest to shift.

5.3 Cash Usage Prior to Advance Predicts Poorer Performance

Takers with higher self-reported cash shares pre-advance process less in subsequent months than similar non-takers, consistent with firms hiding by shifting some sales to cash.

Background. Firms may hide transactions from the Platform by asking customers to pay in cash. While we cannot observe this hiding directly, some firms that use the Platform's accounting tools self report cash sales. Appendix Figure F.6 shows that, among these firms, the share of sales in cash strongly correlates with province-level survey data, suggesting this measure captures real variation in the ease of cash use across firms.

Empirical Design. We look at a sample of taker and non-taker firms who report cash usage in the three months prior to an (actual or placebo) advance.[39] We test whether takers who use more cash ex ante perform worse relative to similar non-takers using the design:

$$Y_{i,t} = \beta_0 + \beta_1 \text{Taker}_i + \beta_2 \text{CashUsage}_i + \beta_3 \text{Taker}_i \times \text{CashUsage}_i + \mathbf{X}_i' \boldsymbol{\beta}_4 + T_t + \epsilon_{i,t}. \quad (10)$$

[37]Appendix Table G.4 shows estimates from the two-period model in Equation 9. The note for Figure 5 provides the full dynamic specification.

[38]In-person transactions not disappearing is consistent with the lack of an extensive-margin response in Panel (B) of Figure 3.

[39]As in Section 5.2, we include all eligible non-takers, assigning them a placebo advance date one year after joining the platform.

For advance i given in quarter t, $Y_{i,t}$ is either default or log conditional revenue. Taker_i is an indicator for whether firm i took an advance and CashUsage_i is the share of the firm's revenue that is in cash the quarter prior to the advance. $\mathbf{X}_i'$ is a vector of characteristics related to platform use and transactions in the three months before disbursement, as well as additional business characteristics. T_t are quarter fixed effects.

Results. Columns (1) and (2) of Table 3 show that takers who use cash are no more likely to default but generate significantly lower revenue post-advance than similar non-takers. In particular, a ten percentage-point increase in a firm's cash share corresponds to a 1.3% decline in on-platform revenue post advance relative to non-takers. This pattern matches diversion into cash rather than exit, and suggests that takers who can more easily switch some customers to cash do so.

5.4 Additional Evidence & Discussion

Appendix C presents further evidence of revenue hiding. First, takers reduce the number of days they use the Platform's machine after taking an advance, consistent with diversion in the presence of fixed daily usage fees. Second, larger transactions are disproportionately more likely to disappear, consistent with the fact that shifting the largest payments off the Platform is the most effective way (per transaction) to extend the duration of an advance. Third, in a small survey of Platform users in metropolitan Cape Town, 42% reported having another card machine (e.g., as a backup), suggesting that diversion is feasible.[40]

Taken together, our results suggest that firms hide revenue using both alternative processors and cash, and that they do so when hiding is cheaper—either because rival methods are less costly or because consumers are more willing to switch to cash. This moral hazard from hiding is one contributor to the revenue gap documented in Section 4.

6 Mitigating Asymmetric Information

Proposition 1 suggests that providers of revenue-based financing can mitigate asymmetric information by raising the cost of hiding and improving screening. In this section, we examine how they do so in practice. We show that "sticky" platform features reduce diversion, longer histories and repeat financing improve screening, and implicit hiding costs inferred from firm behavior are relatively large. Together, these results suggest that design features can

[40] Appendix C.3 provides additional details on the survey, introduced in Clarke *et al.* (2025).

meaningfully limit moral hazard and adverse selection, offering practical lessons for the expansion of flexible finance in low- and middle-income countries.

6.1 Raise Hiding Costs: "Sticky" Platform Features

Proposition 1 suggests that providers can reduce moral hazard by raising the cost of hiding, c. One potential way to do so is through "sticky" platform features, that businesses value outside the advance itself. For example, a platform that bundles free accounting or performance metrics with payment processing makes diversion more costly: hiding would disrupt accounting records and require finding both a new card machine and accounting system. By contrast, firms that only use payment processing may have many substitutes.[41]

We test this idea by comparing takers and non-takers that use the Platform's add-on services, including invoicing tools and a "manage" tab for inventory, customer, and staff tracking. We interact taker status with add-on usage, an approach that helps address the concern that add-on usage simply reflects selection (i.e., better businesses are more likely to adopt these tools).[42] Columns (3) and (4) of Table 3 show that add-on usage does not predict default, but strongly predicts higher revenue for takers relative to non-takers. That is, add-ons narrow the revenue gap, consistent with higher switching costs limiting diversion. In particular, the interaction term in Column (4) implies that firms that use add-ons have a revenue gap that is 4.7 percentage points smaller (43% of the full gap).[43]

As an additional test, we examine usage of a feature that exports sales to a CSV, often adopted by firms that use third-party accounting systems instead of the Platform's accounting tools. If takers that use external accounting are differentially better performers, exporting sales should *positively* predict performance; but if the feature lowers hiding costs, it should *negatively* predict performance for takers relative to non-takers. Column (5) shows that having exported sales positively predicts default for takers relative to non-takers—a *negative* impact on performance. Taken together, our results suggest that platforms can mitigate ex-post moral hazard with "sticky" features that increase the costs of diversion.

[41] An additional example is an e-commerce platform with a large consumer base. A business that diverts off-platform risks losing access to these customers, so in effect the customer base itself becomes a "sticky" feature that raises the cost of hiding.

[42] We include non-takers as described in Footnote 28.

[43] Since the add-on and export features were only introduced in February 2022 and June 2021, respectively, the sample is smaller than in Table 2. However, the size of the overall Gap remains similar.

6.2 Improve Screening: Wait Longer Before Financing

Proposition 1 highlights the importance of reducing adverse selection for revenue-based financing. Two pieces of prior observational evidence highlight the ways different data can help. First, Table 2 shows that time on platform, pre-advance transaction volume, and volatility strongly predict performance, indicating that past digital sales information is informative. Second, Section 6.1 shows that ex-ante add-on usage also predicts performance, suggesting that broader measures of digital engagement may further aid screening.

One interpretation of these results is that platforms can benefit from observing a longer history of activity before financing, as it reveals information about business quality. Appendix Figure F.9 supports this, showing that time on platform reduces the Gap between takers and non-takers. However, another interpretation of the same results is that "bad types" demand financing as soon as they are eligible. Such selection would exist regardless of the informational value of more transactions.

Background & Empirical Design. To test whether waiting longer causally mitigates adverse selection, we use a natural experiment that delayed advance offers. Due to a temporary system error, businesses who joined the platform between March 20, 2022 and September 1, 2022 and met the minimum activity requirements were not offered an advance until six months after joining, instead of the usual three. Figure 6 shows the variation introduced by the initial offer change.[44] The top panel shows that around 10% of businesses who joined before the change and were eligible to take an advance in month three did so before month six. Due to the system error, there were no early takers after the change.

We ask whether these delayed offers improved screening. We use advances made within 12 months of joining and the event-study design:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \text{AfterCut}_i + \mathbf{X}_i' \boldsymbol{\beta}_3 + \text{Month}_t + \epsilon_{i,t}. \tag{11}$$

Here, $Y_{i,t}$ is cumulative total revenue eight months post advance, $Y_{i,t-1}$ is revenue in the quarter prior to taking an advance, and $\mathbf{X}_i$ are demographic fixed effects. The coefficient of interest is β_2 on AfterCut_i, which is an indicator for whether the business joined the platform after the March 20, 2022 cutoff date.[45]

[44] We focus on the initial change because the fix was implemented in a way that led to many businesses receiving offers at different times on platform, resulting in gradual rather than sharp variation.

[45] Our baseline analysis is in levels, as it allows us to combine both extensive (default) and intensive margin effects. In Appendix Table G.6, we separate the two margins. For both the wider sample and those within six weeks of the cutoff, conditional revenue increases and default decreases after the change. This suggests both margins contribute to our result in Table 4, though neither effect alone is statistically significant.

Results. Columns (1) and (2) of Table 4 shows results for businesses that joined between September 2020 and August 2022, controlling for seasonality with Month_t fixed effects. With our tightest demographic controls, the cumulative eight-month post-advance revenue of takers is 13,000 ZAR higher after the cutoff than before, a 5% increase in revenue per month relative to the pre-period average. To rule out other trends in advance performance over time, Columns (3) and (4) focus only on businesses that joined the platform in the six weeks immediately before and after the cutoff. The effect size remains similar.[46]

To further ensure that our results are not driven by confounding time trends, Appendix Table G.7 compares the sales of businesses who joined the platform in the six weeks on either side of the cutoff and *did not take advances*. The sales of pre- and post-change non-takers are similar, suggesting that changing economic conditions cannot explain our results. Instead, our results are consistent with the additional information from six months of interactions improving screening, mitigating adverse selection.

6.3 Repeat Financing

A long literature underscores the importance of repeat interactions, firm-bank relationships, and "learning by lending", especially for small businesses (e.g., Petersen and Rajan, 1994; Berlin and Mester, 1999; Berger *et al.*, 2005; Botsch and Vanasco, 2019; Fuchs *et al.*, 2022). Repeat financing may mitigate frictions both by improving screening (raising p) and by increasing firms' incentives to maintain access to future capital (raising c). Consistent with the former, Table 1 shows that repeat advances have higher average returns than first-time advances. Yet since repeat borrowers have longer platform histories, it is also possible providers learn nothing from advance performance beyond that longer history.

We test this possibility using a sample of first-time and repeat advances. Column (3) of Table 2 shows that even conditional on observables, first advances are 4.3 percentage points more likely to default than repeat advances. Figure 7 regresses default on weeks on platform separately for first, second, and third or later advances. Defaults fall steeply with time on platform in all groups; however, conditional on time on platform, the level of default still decreases monotonically with plan number. This suggests that advance repayment itself generates information beyond time on platform.

[46] To understand the differences between takers before and after the cutoff, in Appendix Table G.5 we compare ex-ante characteristics of takers. Intuitively, the post-change takers had spent more time on platform and had somewhat higher revenue. However, they do not appear significantly different in terms of location, owner demographics, and industry.

6.4 Estimates of Implicit Hiding Costs

We conclude this section with a back-of-the-envelope estimate of c, the costs of hiding. That revenue-based financing has not unraveled in this setting implies that there are positive costs to diversion. Although hiding costs cannot be directly observed, if businesses are optimizing, the shape of the marginal benefit curve (i.e. "gains from hiding") places bounds on the marginal cost curve. Appendix D details our method for using this intuition to estimate c.

For the average business, the total cost of hiding needed to justify shifting 16% of revenue—the full size of the gap from Section 4—is 45,000 ZAR. This estimated cost is large, slightly higher than average monthly revenue of advance takers and substantially higher than the 2,000 ZAR cost of buying a rival's machine.

These hiding costs can be interpreted as a revealed preference measure of the value firms place on platform services, including "sticky" add-ons (Section 6.1) and access to repeat advances (Section 6.3). Other forces such as detection risk or moral considerations may also play a role in mitigating asymmetric information. The high implied costs suggest that revenue hiding can be meaningfully mitigated in developing financial markets.

7 Revenue-Based Financing and Business Growth

This section uses our empirical framework to explore the effects of revenue-based financing on business growth. Proposition 2 shows that moral hazard and adverse selection widen the Gap, while positive causal effects narrow it. Estimates of the Gap and evidence from natural experiments in Sections 5.1 and 6.2 suggest that moral hazard and adverse selection can *more than explain* the Gap, implying positive causal effects. Consistent with this, takers are more likely to expand their geographic footprint than similar non-takers.

Overview. Our procedure for bounding the causal effect has four steps. First, we describe a hypothetical experiment that identifies adverse selection. Second, we discuss the assumptions necessary to use our temporary delay natural experiment in Section 6.2 instead of the hypothetical experiment. Third, we produce a short-run estimate of adverse selection under these assumptions. Fourth, we use our revenue hiding estimates from Section 5 to decompose the short-run Gap into its three components.

Step 1: Ideal Experiment. Consider an experiment in which eligible businesses are randomly assigned into two groups. Suppose one group receives offers and the other does not. The observed revenue of takers in the offered group will change due to revenue hiding and the causal effect of financing. Adverse selection shows up "equally" across the two groups as they are randomly assigned, and can be differenced out. Proposition 3 formalizes this intuition (proof in Appendix A).

Proposition 3. *If businesses are randomly assigned offers, the expected difference in reported (on-platform) revenue between those without offers and with offers is* $\mathbb{P}(Taker) \times (MH - CE)$.

If the difference in reported revenue between the two groups, the probability of uptake, and the Gap is observed, adverse selection can be identified using $AS = Gap - (MH - CE)$.

Step 2: Natural Experiment. Following the intuition of the ideal experiment, we use variation from the natural experiment described in Section 6.2—in which eligible businesses were given offers later—to identify adverse selection in the three months post-advance. Businesses who joined the Platform just before March 20, 2022, are in an "offer-receiving group" in their third to sixth months on the Platform, while those joining just after the cutoff form a "non-receiving group." The arbitrary cutoff can be seen as quasi-randomly assigning businesses to either group which will allow us to apply Proposition 3.

The necessary identifying assumption is that businesses on either side of the cutoff have the same counterfactual distributions of $Y|X$ and $\mathbb{P}(\text{Taker}|X)$. That is, if businesses who joined in the months after March 20 were instead offered financing at three months, their uptake rate and expected revenue would be the same as observably similar businesses who joined before March 20. Appendix Table G.8 shows that the ex-ante characteristics of businesses on either side of the cutoff are generally well-balanced, providing support for this assumption.[47] Since businesses after the cutoff may have taken advances after month six, we look only at "short-run" three-month outcomes.[48]

Step 3: Estimating Short-Run Adverse Selection. Following Proposition 3, we estimate the difference in revenue between those with and without offers, the probability of uptake when offered, and the short-run (three-month) Gap.

We first estimate the difference in revenue between those offered and not with:

$$Y_{i,t} = \alpha_1 Y_{i,t-1} + \alpha_2 \text{Offered}_i + \mathbf{X}_i' \boldsymbol{\alpha}_3 + \text{Month}_t + \epsilon_{i,t}. \tag{12}$$

$Y_{i,t}$ is observed three-month revenue, $Y_{i,t-1}$ is revenue lagged by one quarter, $\mathbf{X}_i$ are industry fixed effects, and Month_t are month fixed effects. The coefficient of interest is α_2 on the

[47] To capture businesses that would have received an offer, we only include those who met the minimum eligibility criteria. To address seasonality, we again use two full years of data—with businesses who joined from September 2020 to August 2022—and include controls for month-of-year fixed effects.

[48] For businesses who were offered at the end of month 3 and took in months 4-6, we use the quarter before and after the advance. For businesses who were offered at the end of month 3 but did not take before month 6 and businesses who were not offered at month 3, we use the first and second quarters on the platform.

indicator Offered$_i$, since $MH - CE = -\alpha_2/\mathbb{P}(\text{Taker})$. Column (3) of Table 5 shows that with industry fixed effects α_2 is around -300. It is worth emphasizing, however, that the standard error on this estimate is large and that our analysis should be seen as providing directional guidance rather than a sharp point estimate. Dividing by the share of those offered that are takers, 11%, gives $MH - CE \approx 2{,}600$ ZAR.[49]

To estimate adverse selection, we subtract the short-run Gap from $MH - CE$. The short-run Gap conditional on observables is identified with:[50]

$$Y_{i,t} = \gamma_1 Y_{i,t-1} + \gamma_2 \text{Taker}_i + \mathbf{X}_i' \boldsymbol{\gamma}_3 + \text{Month}_t + \epsilon_{i,t}. \tag{13}$$

Variables are defined similarly to Equation 12. Table 5, Columns (2) and (4) show γ_2 (short-run Gap) estimates of around 6,400 ZAR. Subtracting $MH - CE$ implies adverse selection of around 3,800 ZAR, roughly the size of 60% of the Gap.

Step 4: Estimating the Full Decomposition. Figure 8 illustrates what different assumptions about the overall level of revenue hiding, v, imply for the causal effect. For any $v < 0.976$, the average causal effect of the advance for takers is positive. In scenarios where businesses hide a greater share of revenue, the implied size of the causal effect increases.

Is $v < 0.976$ reasonable? In Section 5, we showed that in response to a competitor's price drop, transactions fell by approximately 10% for new takers (Section 5.1).[51] Conservatively applying this 10% estimate to all firms implies $v \approx 0.9$, well below the 0.976 threshold for positive effects. At $v = 0.9$, the short-term causal effect is a revenue gain of around 7,800 ZAR, an 8% increase in quarterly revenue relative to pre-advance for the average taker.

Additional Evidence of Growth. To further explore effects on businesses, we use transaction locations from payment processor devices to study geographic expansion. Using the same matched controls as in Figure 2, we compare takers and non-takers. Figure 9 shows

[49] Alternatively, we can make in-sample predictions with the regression $\text{Taker}_i = \text{Month}_t + \sigma_0 Y_{i,t-1} + \sigma_1 X_i$. The average of α_2 divided by $\widehat{\text{Taker}}_i$ yields an estimate for $MH - CE$. This method gives nearly identical results, consistent with observables having little predictive power on whether an individual is a taker. Another alternative is instrumenting for whether a business is an early taker with the side of the cutoff they joined on. Because there are no "always takers", scaling by the share of takers can be seen as recovering the IV estimates (the two may differ slightly due to controls in the 1st stage). Appendix Table G.9 shows IV estimates. The estimate in Column (2), which corresponds to the reduced form in Table 5, Column (3), is very similar to our baseline estimate of $MH - CE$.

[50] This Gap differs from the Gap estimated in Section 4 and Appendix E because we only include businesses that (1) were eligible for an advance after their third month on the platform, (2) took advances in their fourth through sixth months, and (3) took an advance in the 18 months before the March 20, 2022 cutoff.

[51] Our hiding estimates from Section 5.1 should be seen as conservative, as it assumes no hiding in the Competitor's region after the price drop and no hiding in either region before the price drop.

that takers with a small geographic footprint before an advance were more likely to expand their footprint over the following eight months than similar non-taker control firms.

Discussion of Effects. The causal effect size we document aligns with two strands of work in development and finance. One strand shows high returns to capital among small businesses in developing economies, consistent with underinvestment due to financial frictions; for example, McKenzie and Woodruff (2008) find monthly returns of 20–33% in Mexico. Another emphasizes how risk associated with debt may deter investment; for instance, Cordaro *et al.* (2025) report an ITT of 170% for a hybrid debt–equity contract in Kenya versus 59% for debt. In our setting, revenue-based financing with digital payments plausibly works through both channels, relaxing frictions while providing partial insurance.

8 Conclusion

Digital payments are rapidly transforming consumer-firm interactions in low- and middle-income countries. By creating verifiable sales histories and enabling automatic repayment, this transition may open new opportunities for small-firm financing. Revenue-based financing offered by payment platforms shows both the promise and the limits of this model: technology may ease information and enforcement frictions while offering repayment flexibility, but asymmetric information remains. Our results quantify this asymmetric information, and identify the conditions under which this model of financing can scale.

Using transaction-level data from a major South African payment processor, we find that after eight months, firms that take financing earn 16% less through the processor than observably similar non-takers. We show revenue hiding plays a role: when a rival processor cuts prices, repayment falls, consistent with sales diversion. Firms also appear to shift online transactions and move transactions to cash. Providers can counteract these frictions with "sticky add-ons" (valuable services tied to repayment), delayed offers, and repeat financing. Finally, we present evidence that moral hazard and adverse selection are more than enough to explain the initial 16% gap, providing suggestive evidence of positive causal effects.

Revenue-based financing is expanding globally, in both developed and developing economies. Our work focuses on low- and middle-income countries, where it offers larger potential benefits—providing flexible capital where it has traditionally been difficult to do so—but also faces larger challenges. For example, one of the key challenges we document, revenue-hiding, could be avoidable in settings where providers can more easily track businesses and collect repayments. Studying how emerging technologies and policies, including open banking, continue to reshape this landscape is an important direction for future work.

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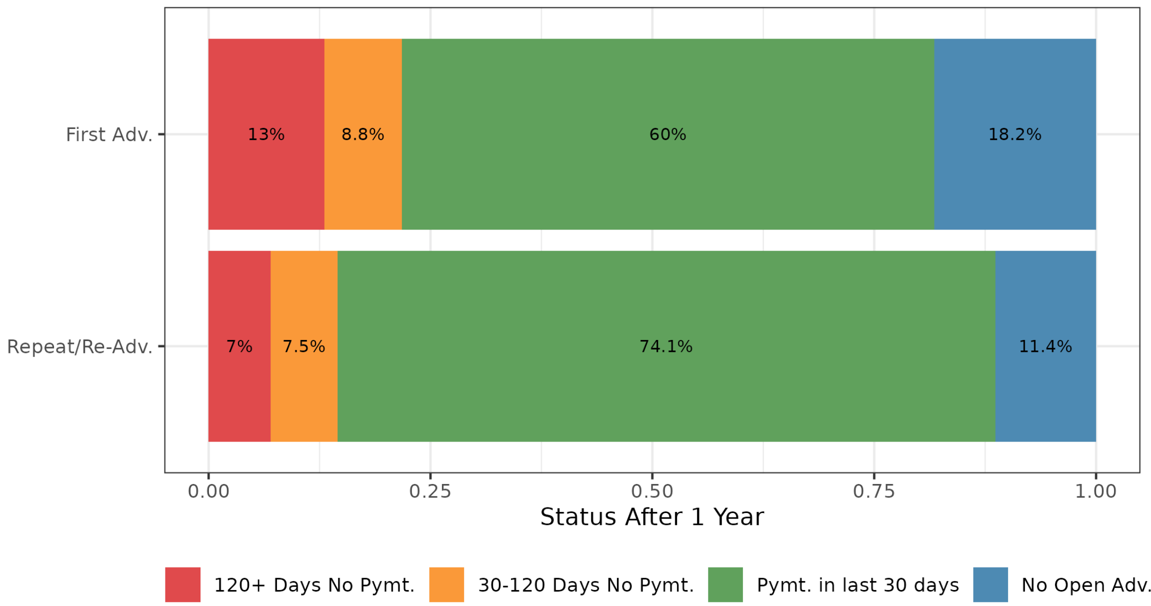
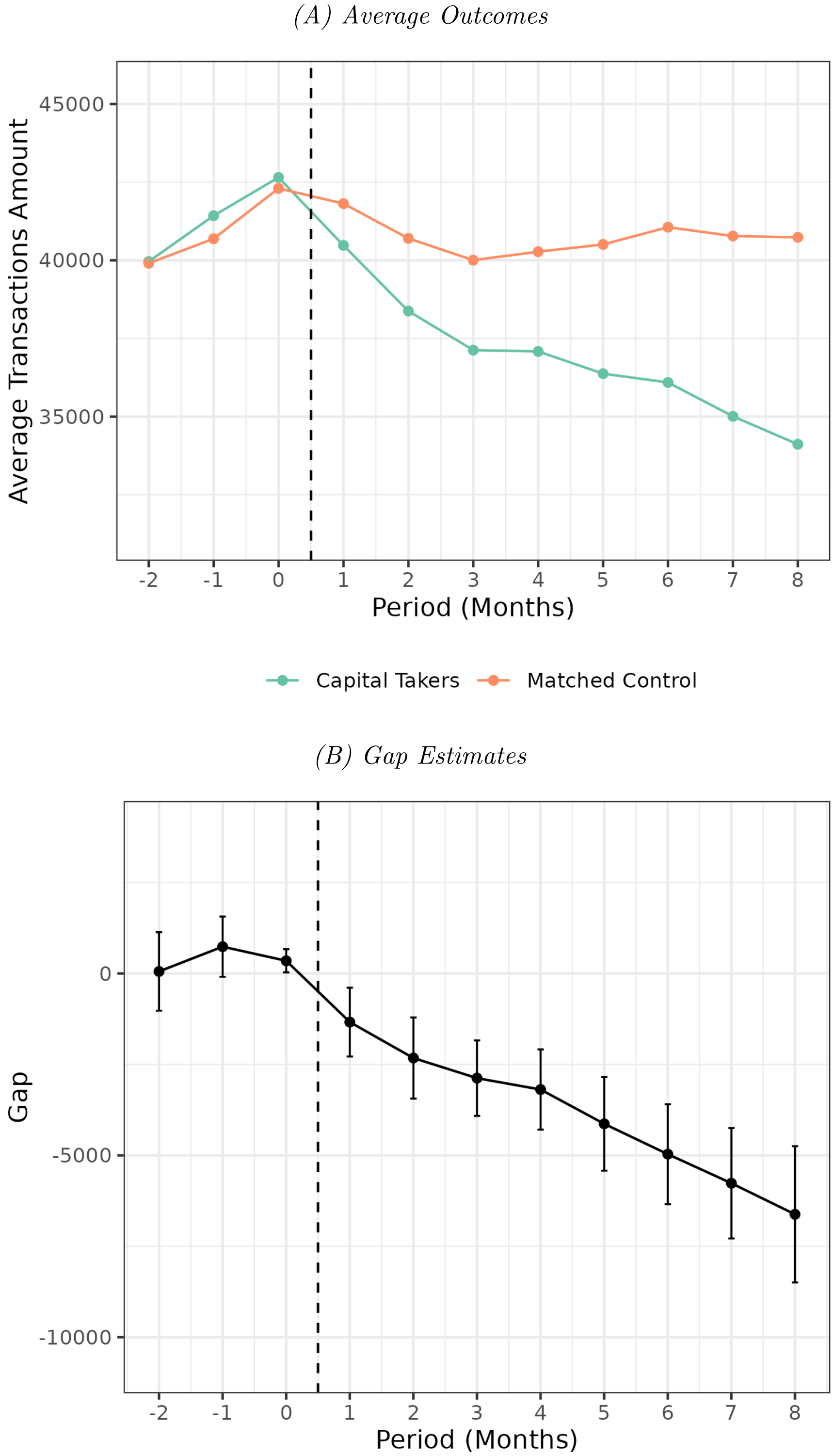
Figures

Figure 1: *Advance Taker One-Year Outcomes*



Note: Figure shows outcomes for first-time and repeat advance takers, respectively, one year after taking an advance. Outcomes shown are for *any* advance the business has one year later.

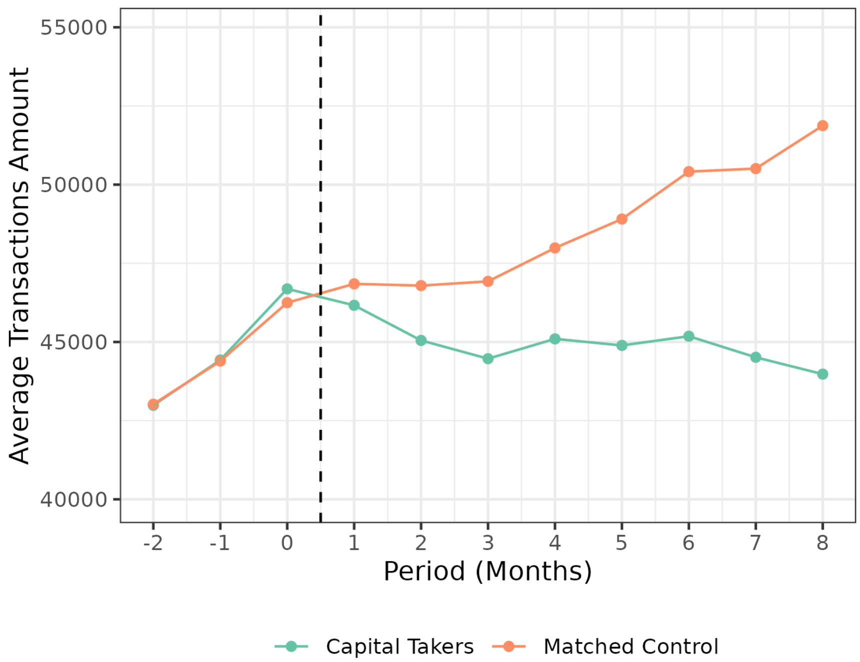
Figure 2: *Gap Between Capital Takers and Non-Takers*



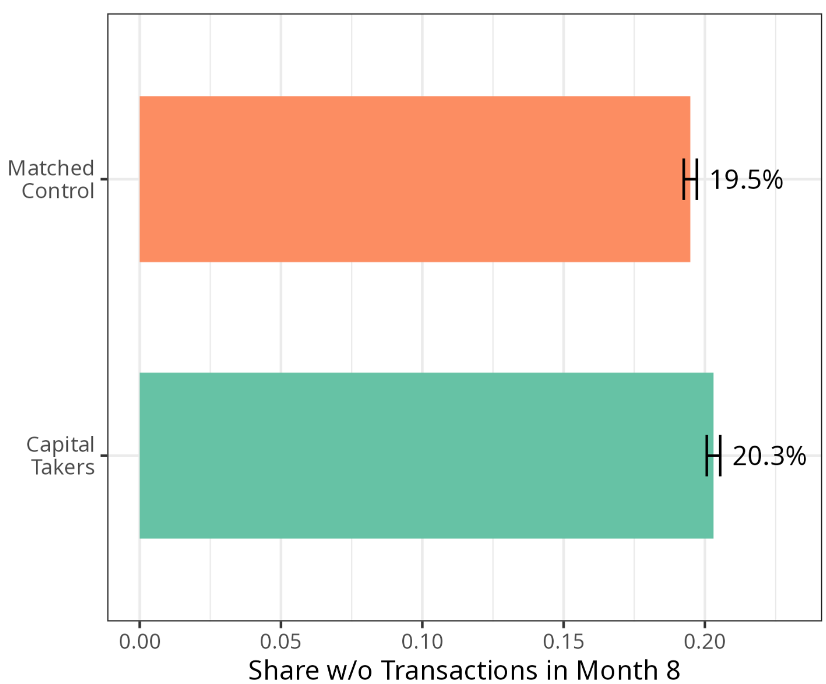
Note: Figure shows the average monthly transactions of capital takers and matched control businesses. Advances were taken in month 0. Each taker is matched to a control business in the same month and industry with the smallest Euclidean distance according to (normalized) time on platform and transaction amount in month 0. Panel A shows the average transactions amount of capital takers and the matched control group. Panel B displays the difference between groups. Bars display the 95% confidence intervals with Abadie and Imbens (2006) adjusted standard errors.

Figure 3: *Gap Primarily Driven by "Intensive Margin"*

(A) Average Outcomes Conditional on "Living"

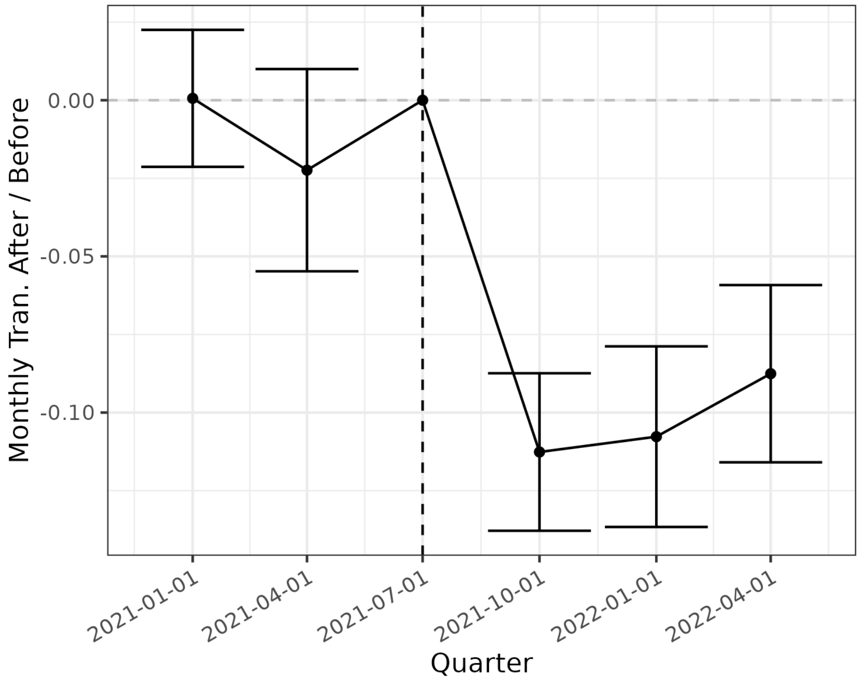


(B) Share "Disappearing"



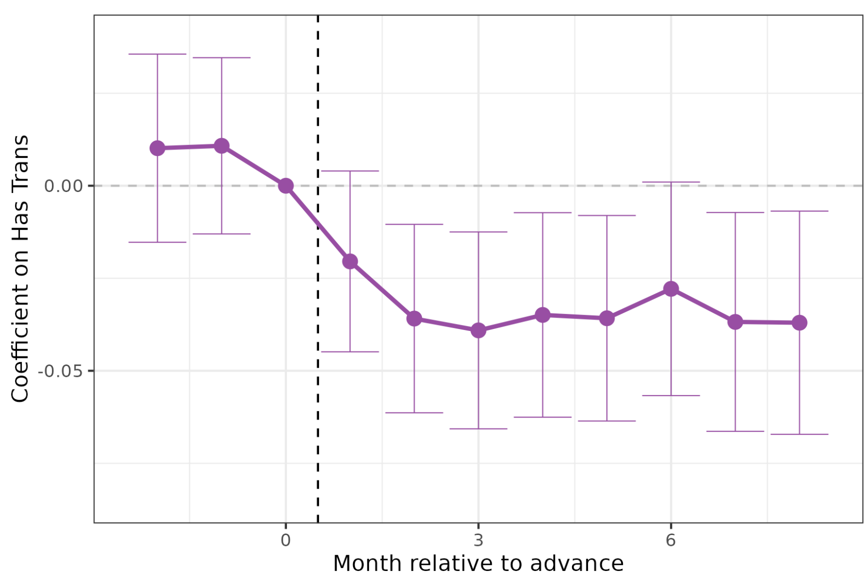
Note: Figure shows the intensive and extensive margin contributions to Figure 2. Panel A shows the average monthly transactions of capital takers and matched control firms as in Figure 2, but selected only from pairs in which both are transacting on the platform in the eighth month after the advance. Panel B shows the share of each set of business in Figure 2 that do not transact on the platform in the eighth month.

Figure 4: *Post-Capital Transactions Decrease When Rival Lowers Price*



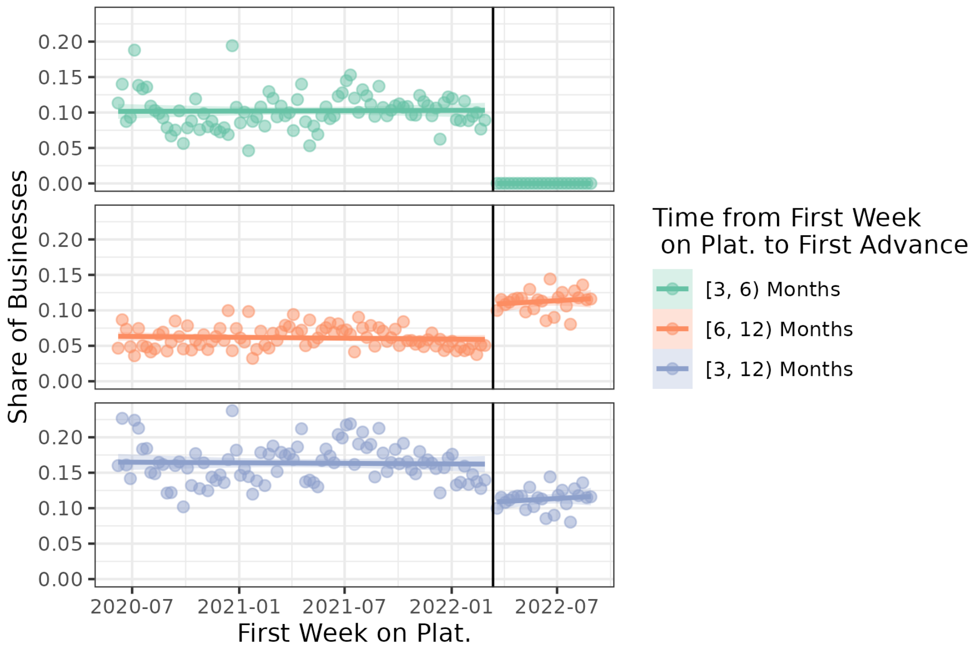
Note: Figure shows a quarterly version of the triple-differences specification in Equation 8. The estimating equation is: $Y_{i,t} = \alpha + \alpha_0 \text{Taker}_i + \alpha_1 D_i + \alpha_2(\text{Taker}_i \times D_i) + \sum_{\tau \neq 3} [\kappa_\tau T_\tau + \delta_\tau(T_\tau \times \text{Taker}_i) + \gamma_\tau(T_\tau \times D_i) + \beta_\tau(T_\tau \times \text{Taker}_i \times D_i)] + \mathbf{X}_{it} + \epsilon_{i,t}$. The plot shows β_τ. T_τ are quarterly dummies. The quarter of the Competitor's price drop is July 2021 (Figure F.3). Standard errors are clustered by business and by province-quarter. Bars display 95% confidence intervals.

Figure 5: *Taker Online Transactions Disappear After Advance*



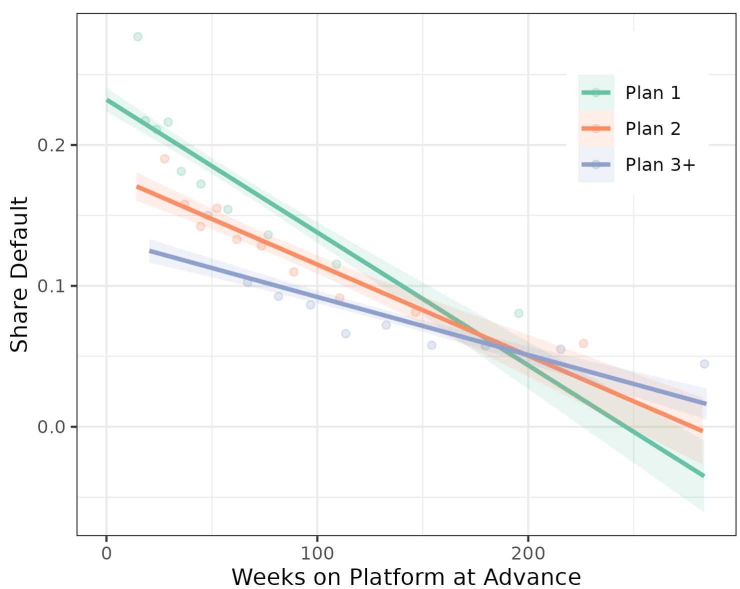
Note: Figure shows a monthly version of the triple-differences specification in Equation 9. The estimating equation is: $Y_{i,j,t} = \mu_i + \alpha_1 C_j + \alpha_2(\text{Taker}_i \times C_j) + \sum_{\tau \neq 0} [\kappa_\tau T_\tau + \delta_\tau(T_\tau \times \text{Taker}_i) + \gamma_\tau(T_\tau \times C_j) + \beta_\tau(T_\tau \times \text{Taker}_i \times C_j)] + \epsilon_{i,j,t}$. The plot shows β_τ. T_τ are monthly dummies. Advances were taken in month zero. Standard errors are clustered by business. Bars display 95% confidence intervals.

Figure 6: *Time to First Advance, Around Policy Change*



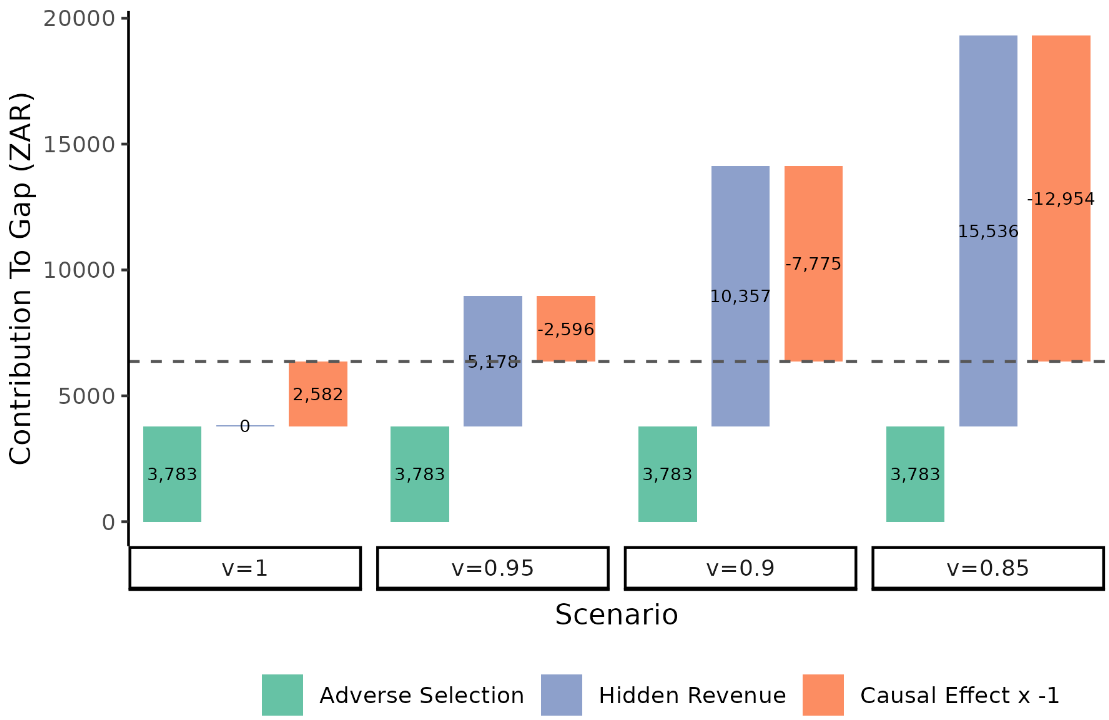
Note: Figure shows, by first week on the platform, the share of businesses who met the minimum transaction eligibility criteria in month three that took an advance within different time frames. From top to bottom, the panels show the share of businesses who took an advance in months 3-5, 6-11, and 3-11, respectively. The week of the policy change described in Section 6.2 is excluded.

Figure 7: *Share "Disappearing" by Advance Number and Time on Platform at Advance*



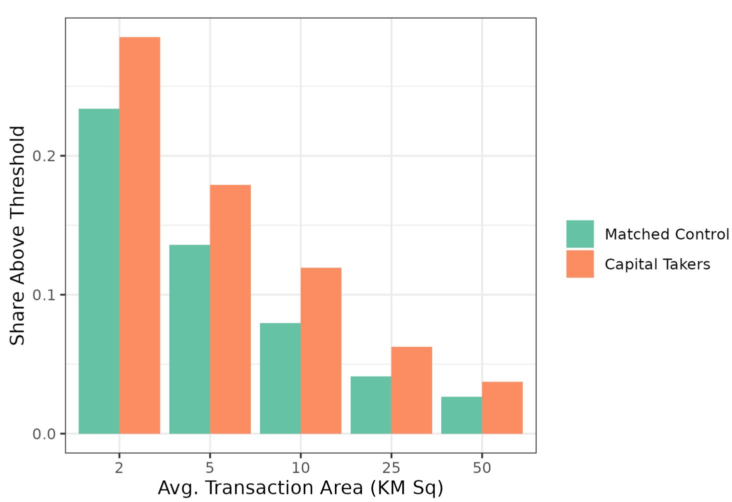
Note: Figure shows the share of businesses that default against the number of weeks the business had been on the platform, split by advance number. Default is whether the business has an open advance and no transactions 8 months after the start of the advance. Weeks on platform is calculated from the date of first transaction on the platform to the date of receiving the advance.

Figure 8: *Decomposition Scenarios*



Note: Figure shows the contributions of adverse selection, moral hazard, and causal effects to the overall Gap—as each is defined in Equations 3–5—under various revenue hiding scenarios. The estimates are for the three-month post-advance revenue of businesses who took advances in their first six months on the platform. The dashed grey line displays the three-month gap. Our methodology for constructing these estimates is described in Section 7. As defined in Section 3, v is the share of revenue the financier can observe.

Figure 9: *Business Expansion for Small Capital Takers vs Non-Takers*



Note: Figure shows how transaction-level footprints change for businesses who were geographically small in the pre-period. Transaction locations come from each business's payment processing devices. The sample is capital takers and matched control businesses in Figure 2, but only from pairs in which both businesses have average monthly transaction locations within a 1km^2 bounding rectangle in the pre-period (N = 4,354). Thresholds are for bounding rectangles of the average monthly transaction locations in the post-period.

Tables

Table 1: *Summary of Primary Sample*

Panel A: 1st Advances					
Var	Mean	SD	p25	p50	p75
Prior Weeks on Platform	58.57	53.77	23.22	38.34	72.66
Sales Amount in Prior 3mo (ZAR)	118,319	240,084	28,220	57,109	123,698
Sales N in Prior 3mo	513.78	1003.53	94	217	535
Principal Amt. (ZAR)	37,830	63,376	8,000	17,000	40,000
Charge Rate (%)	19.55	6.46	17	22	23.19
Factor Rate	1.28	0.04	1.27	1.3	1.3
Est. Repayment Period (Months)	7.66	2.17	6	8	9
1 yr. Amt. Paid / Princip.	1.06	1.01	1.01	1.26	1.3
Discounted (5%) Amt. Paid 1 yr. / Princip.	1.05	0.99	0.99	1.25	1.28
Discounted (15%) Amt. Paid 1 yr. / Princip.	1.03	0.95	0.96	1.22	1.25
Panel B: Repeat / Re-Advances					
Var	Mean	SD	p25	p50	p75
Prior Weeks on Platform	115.72	69.41	60.77	98.23	156.36
Sales Amount in Prior 3mo (ZAR)	162,301	265,253	46,716	89,626	179,401
Sales N in Prior 3mo	694.87	1297.49	123.5	302	766
Principal Amt. (ZAR)	46,277	79,277	11,500	21,000	47,250
Charge Rate (%)	20.53	5.45	19	23	23
Factor Rate	1.37	0.16	1.3	1.31	1.44
Est. Repayment Period (Months)	8.53	2.13	8	8	9
1 yr. Amt. Paid / Princip.	1.24	1.55	1.23	1.3	1.42
Discounted (5%) Amt. Paid 1 yr. / Princip.	1.23	1.55	1.21	1.29	1.4
Discounted (15%) Amt. Paid 1 yr. / Princip.	1.2	1.53	1.19	1.26	1.37
Nth Advance	3.6	2.01	2	3	4

Note: Table presents summary statistics describing characteristics of first and repeat advances. The sample includes advances made from June 2020 until May 2023 (so outcomes for at least 12 months are observable as of May 2024). When discounting repayments, we assume an annual rate of return of x_a (5% or 15%) and compound interest payments daily with a daily discount rate of $x_d = (1 + x_a)^{\frac{1}{365}} - 1$. Then, daily repayments are discounted by $\frac{1}{(1+x_d)^t}$ where t is the number of days since the advance was opened. We use a daily discount rate as repayments are collected at the end of each day. Section 2.1 provides more details on the structure of advances.

Table 2: *Predictors of Advance Performance*

	(1)	(2)	(3)	(4)	(5)	(6)
	Default	Log Total Amt. 8 Months	Default	Log Total Amt. 8 Months	Default (8mo)	Log Total Amt. 8 Months
Years on Platform	-0.037***	0.032***	-0.025***	0.019***	-0.037***	0.039***
	(0.0064)	(0.0039)	(0.0039)	(0.0033)	(0.0055)	(0.0049)
Log Amt. -3 Months	-0.0070	0.86***	-0.0098*	0.88***	-0.018***	0.91***
	(0.0058)	(0.043)	(0.0031)	(0.020)	(0.0030)	(0.0095)
Relative Sd.	0.089***	-0.055	0.071***	-0.11*	0.16***	-0.17***
	(0.0086)	(0.042)	(0.0076)	(0.036)	(0.0055)	(0.036)
First Plan			0.043***	0.0039		
			(0.0021)	(0.0093)		
Taker					0.015*	-0.081***
					(0.0054)	(0.015)
Sample	Takers, First Plans	Takers, First Plans	Takers, All Plans	Takers, All Plans	All, First Plans	All, First Plans
Demographic FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Advance Controls	Yes	Yes	Yes	Yes	No	No
Observations	17763	14633	46740	40829	49651	39618
Adjusted R^2	0.036	0.68	0.044	0.73	0.074	0.72

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows regressions of transaction characteristics on advance performance. Each observation is an advance. The dependent variable "Log Total Amt. 8 Months" is log total transaction amount within 8 months post advance, conditional on no default. In Columns (1) and (3), "Default" is whether the advance taker has an open advance and no transactions in the 8th month post-advance. Since we include non-takers in Columns (5)-(6) (following Footnote 28), "Default (8mo)" is whether a business has no transactions in the 8th month post-advance (real or placebo). "Years on Platform" is years between date of first transaction on the platform to the advance being given. "Log Amt. -3 Months" is log total transactions three months before the advance. "Relative Sd." is the standard deviation of weekly transactions amounts, divided by the mean, in the three months before the advance. "First plan" is an indicator for whether the advance was a first advance. Additional controls include: demographic fixed effects (industry, business type, citizenship, location classification, province), quarter by year fixed effects, and advance controls (principal, charge rate, factor rate). Standard errors are clustered at the industry level.

Table 3: *Cash, Feature Usage, and Advance Performance*

	(1) Default (8mo)	(2) Log Total Amt. 8 Months	(3) Default (8mo)	(4) Log Total Amt. 8 Months	(5) Default (8mo)	(6) Log Total Amt. 8 Months
Taker	0.0069	-0.0089	0.013^{*}	-0.11^{***}	0.011^{+}	-0.093^{***}
	(0.015)	(0.016)	(0.0046)	(0.018)	(0.0049)	(0.018)
Share Cash	0.057	0.20^{**}				
	(0.047)	(0.045)				
Taker X Share Cash	-0.038	-0.14^{*}				
	(0.082)	(0.055)				
Uses Add-Ons			0.0064	-0.0087		
			(0.012)	(0.025)		
Taker X Uses Add-Ons			-0.0047	0.047^{*}		
			(0.015)	(0.019)		
Exported Sales					-0.027^{*}	0.046
					(0.011)	(0.037)
Taker X Exported Sales					0.041^{+}	0.033
					(0.022)	(0.057)
Sample	All, First Plans	All, First Plans	All, First Plans	All, First Plans	All, First Plans	All, First Plans
Demographic FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter X Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Past Revenue Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7989	6160	29995	23728	39894	31660
Adjusted R^2	0.060	0.72	0.082	0.72	0.078	0.72

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows regressions of various measures on advance performance. Each observation is an advance. Every column includes non-takers, following Footnote 28. "Default (8mo)" is whether a business has no transactions in the 8th month post-advance (real or placebo). "Log Total Amt. 8 Months" is log total transaction amount within 8 months post advance, conditional on no default. "Share Cash" is the share of transactions that are in cash in the quarter pre-advance. The sample only includes those who use cash in the pre-period. "Uses Add-Ons" is whether, in the month before the advance, the business opened the manage tab to track staff, customers, and inventory, or creates invoices. "Exported sales" is whether, in the month before the advance, the business exported its sales history to a CSV. Past Revenue Controls are "Years on Platform", "Log Amt. -3 Months", and "Relative Sd." from Table 2. All other independent variables are defined in Table 2. Standard errors are clustered at the industry level.

Table 4: *Total Revenue Post-Advance Relative to Pre-Advance, Around Policy Change*

	Dependent Variable: Amt. 8 Months Post-Advance			
	(1)	(2)	(3)	(4)
Amt. -3 Months	1.82***	1.80***	1.89***	1.87***
	(0.10)	(0.10)	(0.18)	(0.19)
After Cutoff	10061.11*	13227.54*	10731.81**	13805.67**
	(4359.27)	(4696.06)	(2514.95)	(4164.53)
Sample	Full	Full	Near Cutoff	Near Cutoff
Month of Year FE	Yes	Yes	No	No
Demographic FE	Industry	All	Industry	All
Observations	6963	6963	1038	1038
Adjusted R^2	0.571	0.574	0.591	0.599

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows results from regression 11. Each observation is a business. The dependent variable is total transactions amount in the eight months after taking a first advance. After cutoff is an indicator for whether the business joined the platform after the March 20, 2022 cutoff described in Section 6.2. Columns (1) and (2) include all businesses who joined the platform between September 2020 and August 2022 and took an advance in 12 months. Columns (3) and (4) further filter to those who joined in six weeks on either side of the cutoff. Columns (1) and (3) include industry fixed effects. Columns (2) and (4) include demographic fixed effects described in Table 2. Standard errors are clustered at the industry level.

Table 5: *Decomposition Regressions*

	Dependent Variable: Amt. 3 Months Post			
	(1)	(2)	(3)	(4)
Amt. -3 Months	0.92***	0.87***	0.92***	0.87***
	(0.05)	(0.07)	(0.05)	(0.07)
Offered	−293.64		−289.25	
	(1385.10)		(1373.93)	
Taker		-6438.96^{+}		-6365.71^{+}
		(3204.19)		(3268.69)
Sample	Full	Offered Only	Full	Offered Only
Month of Year FE	Yes	Yes	Yes	Yes
Demographic FE	Industry	Industry	All	All
Adjusted R^2	0.682	0.672	0.682	0.672
Observations	42341	30452	42341	30452

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows results from regressions to decompose the short-term Gap (Section 7). Columns (1) and (3) show results from Equation 12 over all businesses who joined the platform between September 2020 and August 2022. Columns (2) and (4) show results from Equation 13, for those who joined the platform before the March 20, 2022 cutoff. Columns (1) and (2) include industry fixed effects. Columns (3) and (4) include demographic fixed effects described in Table 2. Standard errors are clustered at the industry level.

Appendix

A Proofs

Proof of Proposition 1

Proof. Since the bad types will always choose to take the advance and consume it, sustaining revenue-based financing depends on good types taking the advance and making an investment. If, instead, good types consume the advance, profits from this type will be:

$$\pi|G = \eta v^{\bullet} y - L(1+r).$$

Starting from Equation 1,

$$\begin{aligned} & 0 < -\eta v^{\bullet} y + L(1+r) - c(1-v^{\bullet})^2 = -(\pi|G) - c(1-v^{\bullet})^2 \\ \Rightarrow & \pi|G + c(1-v^{\bullet})^2 < 0 \\ \Rightarrow & \pi|G < 0 \end{aligned}$$

since the cost of hiding is positive. Thus, good types need to take the advance and make an investment for any advances to be offered. We can therefore limit to cases where this is true to understand lender and borrower behavior when revenue-based financing might be possible. To pin down η, we need $v^{\dagger}$ for the good types. The borrower's FOC is:

$$-\eta(y+\mu_X) + 2c(1-v^{\dagger}) = 0 \Rightarrow v^{\dagger} = 1 - \frac{\eta(y+\mu_X)}{2c}.$$

if $c \geq \frac{\eta(y+\mu_X)}{2}$ or 0 otherwise. However, in the corner case the lender makes a loss (business hides everything), which again makes revenue-based financing impossible. In the non-corner case, the lender's profit function is:

$$\pi_X = p\eta v^{\dagger}(y+\mu_X) - L(1+r) = p\left[\eta - \frac{\eta^2(y+\mu_X)}{2c}\right](y+\mu_X) - L(1+r).$$

Imposing the zero profit condition and solving for η gives:

$$\eta = \frac{c \pm \sqrt{c^2 - \frac{2cL(1+r)}{p}}}{y+\mu_X} \Rightarrow \eta^* = \frac{c - \sqrt{c^2 - \frac{2cL(1+r)}{p}}}{y+\mu_X}$$

where, since $\eta < 1$, we want to take the smaller root (otherwise when $c \to \infty$, $\eta \to \infty$). Consequently,

$$\frac{\partial \eta^*}{\partial c} \propto 1 - \frac{c - \frac{L(1+r)}{p}}{\sqrt{c^2 - \frac{2cL(1+r)}{p}}} = 1 - \frac{\sqrt{(c - \frac{L(1+r)}{p})^2}}{\sqrt{c^2 - \frac{2cL(1+r)}{p}}} = 1 - \frac{\sqrt{c^2 - \frac{2cL(1+r)}{p} + \frac{L^2(1+r)^2}{p^2}}}{\sqrt{c^2 - \frac{2cL(1+r)}{p}}} < 0$$

$$\frac{\partial \eta^*}{\partial p} \propto - \left[\frac{\frac{cL(1+r)}{p^2}}{\sqrt{c^2 - \frac{2cL(1+r)}{p}}} \right] < 0$$

as wanted.

For revenue-based financing to be possible for a given c and p, there must be an η smaller than 1 for which the lender makes non-negative profits (as $\eta > 1$ implies the borrower pays back *more* than their revenue). The result that revenue-based financing is impossible for $c < \bar{c}$ and $p < \bar{p}$ comes from the fact that if c and p are too small, such an η does not exist which allows the lender to break-even. In particular, since η^* is decreasing in p and c, the constraint $\eta < 1$ will bind when p and c are small.

Here are two examples of a possible $\bar{p}$ and $\bar{c}$. If $p < \frac{L(1+r)}{y+\mu_X}$, no $\eta < 1$ will make π_X non-negative:

$$\pi_X = p\eta v^\dagger(y + \mu_X) - L(1+r) \leq p(y + \mu_X) - L(1+r) < 0.$$

Additionally, if $c \leq \frac{2L(1+r)}{p}$, π_X has no roots and since π_X is concave in η, this implies that no η with non-negative profits is possible. $\square$

Proof of Proposition 2

Proof. Proposition 2 follows from linearity of expectation. In particular:

$$\begin{aligned} MH + AS - CE =& \mathbb{E}[(1-v)Y(1)|\text{Taker}] + \\ & \mathbb{E}[Y(0)|\text{Non-Taker}] - \mathbb{E}[Y(0)|\text{Taker}] - \mathbb{E}[Y(1) - Y(0)|\text{Taker}] \\ =& \mathbb{E}[Y(0)|\text{Non-Taker}] - \mathbb{E}[vY(1)|\text{Taker}] \end{aligned}$$

Thus,

$$MH + AS - CE|(X = x) = \mathbb{E}[Y(0)|X = x, \text{Non-Taker}] - \mathbb{E}[vY(1)|X = x, \text{Taker}]$$

as wanted. $\square$

Proof of Proposition 3

Consider two groups that are randomly assigned, but Group 1 receives offers and Group 2 does not. The difference in expected reported revenue Y_{obs} between the two groups is $\mathbb{E}[Y_{obs}|\text{Group 2}] - \mathbb{E}[Y_{obs}|\text{Group 1}]$. Notice that:

$$\mathbb{E}[Y_{obs}|\text{Group 2}] = \mathbb{E}[Y(0)|\text{Taker}] \cdot \mathbb{P}(\text{Taker}) + \mathbb{E}[Y(0)|\text{Non-Taker}] \cdot \mathbb{P}(\text{Non-Taker})$$
$$\mathbb{E}[Y_{obs}|\text{Group 1}] = \mathbb{E}[vY(1)|\text{Taker}] \cdot \mathbb{P}(\text{Taker}) + \mathbb{E}[Y(0)|\text{Non-Taker}] \cdot \mathbb{P}(\text{Non-Taker})$$

where we can remove the conditioning on group because the groups are randomly assigned. In the first equation, "Taker" refers to "would take if offered." Thus,

$$\begin{aligned}\mathbb{E}[Y_{obs}|\text{Group 2}] - \mathbb{E}[Y_{obs}|\text{Group 1}] =& \mathbb{E}[Y(0)|\text{Taker}] \cdot \mathbb{P}(\text{Taker}) - \mathbb{E}[vY(1)|\text{Taker}] \cdot \mathbb{P}(\text{Taker}) \\ =& \mathbb{P}(\text{Taker}) \cdot (-CE + MH)\end{aligned}$$

where we have used the definitions of CE and MH from Equations 3–5.

B Simple Model with Risk-Sharing

Businesses can undertake a risky investment project of cost L. Conditional on undertaking the project, they earn a stochastic revenue payoff $\tilde{y} \sim \mathcal{N}(\mu, \sigma^2)$. If they decide to not undertake the investment they have a fixed revenue y. Businesses are risk averse with CARA utility over wealth so that $E[u(W)] = \frac{-\exp(-AW)}{W}$. Assume $\mu > L$ so there is a risk-neutral benefit to investing. Lenders are risk-neutral and make zero profits. There are two types of contracts available to businesses to finance their investment: debt, in which borrowers receive and repay L, and a revenue-sharing contract, in which borrowers repay a share, η, of their revenue after investment.

Proposition 4. *Let $\overline{y}_d$ and $\overline{y}_r$ denote thresholds such that for all $y < \overline{y}_d$ the debt contract is preferred over no investment and for all $y < \overline{y}_r$ revenue-based financing is preferred over no investment. Then, $\overline{y}_d < \overline{y}_r$. If $\overline{y}_r \leq y$, neither financing contract is taken and no investment occurs.*

Intuitively, revenue-sharing contracts move risk to the lender, decreasing the variance of the investment payoff for the borrower. Less is paid back when revenue is low, more is paid back when revenue is high. This can attract new risk-averse investors to accept revenue-based financing (so $\overline{y}_d < \overline{y}_r$).

Proof of Proposition 4

Proof. A firm will invest under a debt contract iff:

$$\mathbb{E}[u(\tilde{y} - L)] \geq u(y) \Rightarrow \mu - L - \frac{A}{2}\sigma^2 \geq y.$$

Similarly, a firm will invest under a revenue-based financing contract iff:

$$\mathbb{E}[u((1-\eta)\tilde{y})] \geq u(y) \Rightarrow (1-\eta)\mu - \frac{A}{2}(1-\eta)^2\sigma^2 \geq y.$$

As lenders are perfectly competitive, the η offered will be given by $\eta\mu = L \Rightarrow \eta = \frac{L}{\mu} < 1$. Thus, the above expression can be written as:

$$\mu - L - \frac{A}{2}(1 - L/\mu)^2\sigma^2 \geq y.$$

As $L/\mu < 1$, this implies that the threshold y for taking revenue-based financing is lower. □

C Additional Evidence of Hiding

In this Appendix, we present additional evidence supporting the view that businesses hide revenue, and offer insight into how they do so.

C.1 "Disappearing" Days

One logistically straightforward way businesses might hide transactions is by setting a particular card machine on certain days. Additionally, many payment processors in South Africa charge small fixed daily settlement fees which create an incentive to hide revenue by using one processor per day. Appendix Figure F.7 shows examples of businesses where this behavior may be occurring. For these businesses, daily transactions are rarely zero before taking an advance, but are often zero afterward. In some cases, the volatility of transactions also falls, consistent with shifting a block of sales to another processor, or "controlling" usage.

In addition to these illustrative examples, we also more systematically test whether takers' transactions are more likely to "disappear" for full days. Appendix Table G.10 shows that, among businesses who frequently used the machine, the number of days takers use their machine post-advance is more likely to fall compared to similar non-takers.

C.2 Transaction Size

Moving the largest transactions off the Platform is the most effective way (per transaction) to extend advance duration, suggesting larger transactions could be shifted to other processors. Appendix Figure F.8 shows that, indeed, large transactions more sharply decline after an advance. This result is unchanged when residualizing on business revenues, suggesting that within-business variation, rather than across-business variation, drives our result. While the decline in large transactions post-advance is suggestive of hiding, it could also be driven by takers having private information about large future sales.

C.3 Survey of Small Firms

We use results from a survey of small firms introduced in Clarke *et al.* (2025). The October 2024 survey collected data from 300 SMEs in the Cape Town metropolitan area, including firms not on the Platform. The firms surveyed were on average slightly larger than those on the Platform, as noted in Clarke *et al.* (2025), and $N = 69$ had a Platform card machine.

Two results suggest that some business are able to easily divert a portion of their transactions to cash and other processors. First, among respondents who had a card machine from the Platform, only 41% said their sales are "mostly cards." The other responses were "evenly split between card and cash" (23%); "mostly EFT" (20%); "mostly cash" (14%); and "mostly SnapScan/Zapper" (1%). Second, 42% of respondents with a Platform machine reported having at least one additional card machine from a different company. In open-ended responses, respondents discussed having multiple machines as a backup, especially for network issues during electricity outages, a common occurrence during our sample period.

D Additional Details on Hiding Costs Estimates

In this appendix, we use our estimates to provide back-of-the-envelope guidance on the cost of hiding. We present our main results in Section 6.4.

The key intuition of our analysis is that, although hiding costs cannot be directly observed, if businesses are optimizing, the shape of the marginal benefit curve (i.e. "gains from hiding") will place bounds on the marginal cost curve. Define $h \equiv 100 \cdot (1 - v)$ as the percent of revenue hidden and $g(h)$ as the present-value gain from hiding h% of transactions each day. In particular, $g(h^*)$ is the difference in the net present value of repayments between $h = 0$ and $h = h^*$. $g'(h)$ is the marginal benefit of hiding. Let $c(h)$ be the "cost" of hiding, and $c'(h)$ is the cost of moving an additional percentage point of revenue off the platform. If businesses are optimizing and there is an interior solution, $g'(h^*) = c'(h^*)$.

We can observe the gains from hiding an additional percent of revenue directly from the repayment schedule conditional on a discount rate. For our exercise, we used average advance characteristics, an annual discount rate of $r = 30\%$, and constant daily revenues.[52] Appendix Figure F.12 shows the corresponding $g(h)$ and $g'(h)$ curves, which implies that the marginal cost of moving an additional 1% of transactions each day is $c'(10) = g'(10) \approx 42$ ZAR. Intuitively, $g'(h)$ is steep because the discount rate has a non-linear effect on payments far into the future, so the marginal benefit of hiding is increasing in the percentage hidden (i.e., a "duration effect").

How does this translate into the cost of shifting? If the optimal amount of hiding, h^*, is 10%, then $c'(h)$ must be below $g'(h)$ for $h < 10$. For $h > 10$, we need $\int_{10}^{100} c'(h)dh > \int_{10}^{100} g'(h)dh \Rightarrow c(100) - c(10) > g(100) - g(10)$, otherwise businesses would hide all of their revenue.[53] For example, if marginal costs rose faster than marginal benefits for $h > 10$, then $h^* = 10$ could be possible. Consequently, the total cost of shifting to justify $h^* = 10$ (and not all transactions) is bounded below by $g(100) - g(10) \approx 45{,}000$ ZAR.

E Alternative Approaches to Estimating the Gap

In this Appendix, we present two alternative methods for estimating the Gap between capital takers and non-takers, in addition to the matching approach detailed in Section 4.

Panel Regression Approach

We use a panel of business-by-quarter observations for every business that ever met the minimum advance eligibility requirements. To estimate the Gap we run a regression of the form:

$$Y_{i,t} = \delta_{c(i),t} + X_i + \beta_0 \text{Taker}_{i,t} + \beta_1 \text{Taker}_{i,t-1} + ... + \beta_8 \text{Taker}_{i,t-8}. \tag{E.1}$$

Here, $Y_{i,t}$ is the revenue of business i in month t; $\delta_{c(i),t}$ are cohort (first month on platform) by time fixed effects; and X_i are industry fixed effects. The indicators $\text{Taker}_{i,k}$ equal one when a business took a first advance in month k.

[52] We chose 30% based on several articles that value small businesses in practice (e.g., Mercer Capital) and academic work showing that the discount rate for small businesses should be fairly high, due to idiosyncratic risk. See, for example, Trevino (1997) and Jagannathan *et al.* (2016). We convert the annual discount rate to a daily discount rate because repayments are collected at the end of each day. We also assume that daily revenue is constant, given by 103,570/90 (Figure 8). Average advance characteristics are: factor rate of 1.3, charge rate of 20%, and principal of 37,830 (Table 1).

[53] In Figure F.12 we assume two functional forms for $c(h)$. If $c(h)$ was cubic and $c'(h)$ was in the form ah^2 starting at $(0, 0)$ and passing through $(10, g'(h))$, then $h = 10$ could be optimal. However, quadratic costs and $c'(h) = bh$ (passing through $(0, 0)$ and $(10, g'(h))$ would not work because $c(100) - c(10) \ll g(100) - g(10)$.

Intuitively, β_0, the coefficient on taker $\text{Taker}_{i,t}$, will be positive. This is because, while our sample includes only businesses that were eligible at *some* point, in any given month many will not be eligible. A decline in the coefficients β_1 through β_8 then captures a differential decline in revenue of advance takers, providing an estimate of the Gap. Figure F.10 shows that such a differential decline exists, consistent with the existence of the Gap. The difference between the highest and lowest coefficients is around 6,000, a magnitude roughly equal to our baseline result in Figure 2.

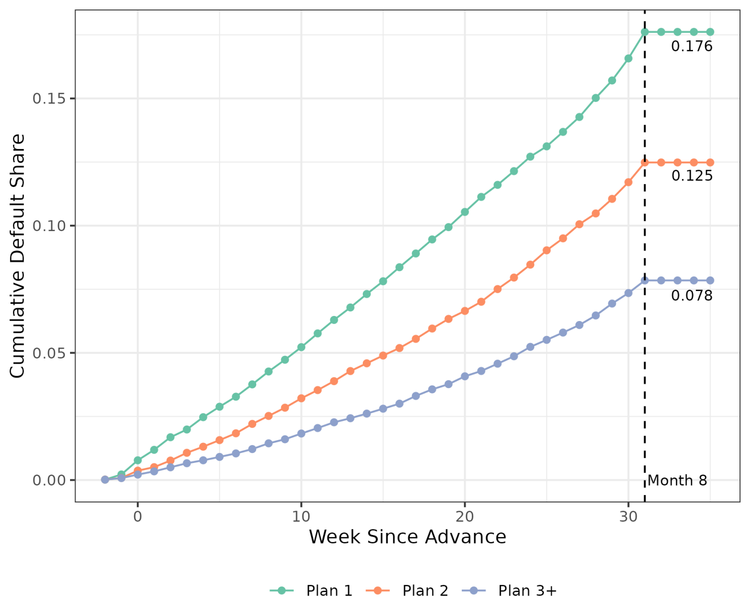
Machine Learning Approach

We use a panel of business-by-quarter observations for non-advance-taking businesses combined with observations for each advance taker in the quarter of their advance. We use each business-by-quarter observation to train random forests to predict the revenue of each taker and non-taker in the next eight months.[54] For each model we use revenue and transaction months in the prior three months, months since joining the platform, month, industry, and a taker indicator as predictors. We then use each model to make revenue predictions for the capital takers and, counterfactually, the capital takers if they did not take an advance. Figure F.11 shows the resulting estimates. The Gap between the in-sample (solid green line) and counterfactual (solid orange line) predictions is around 4,000 ZAR, roughly two-thirds of the magnitude of our baseline result in Figure 2.

[54] The algorithm allows us to find non-linear relationships between variables, without overfitting, by aggregating mean predictions from a number of regression trees generated over sample subsets of both observations and input variables. See Breiman (2001).

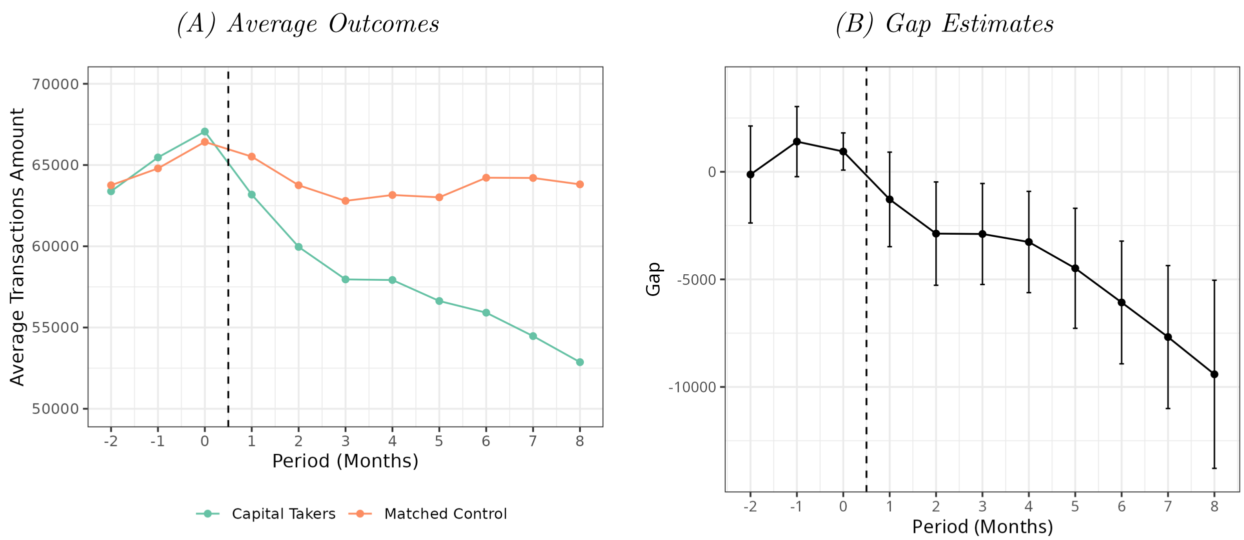
F Additional Figures

Figure F.1: *Hazard Plot for 8 Month Default*



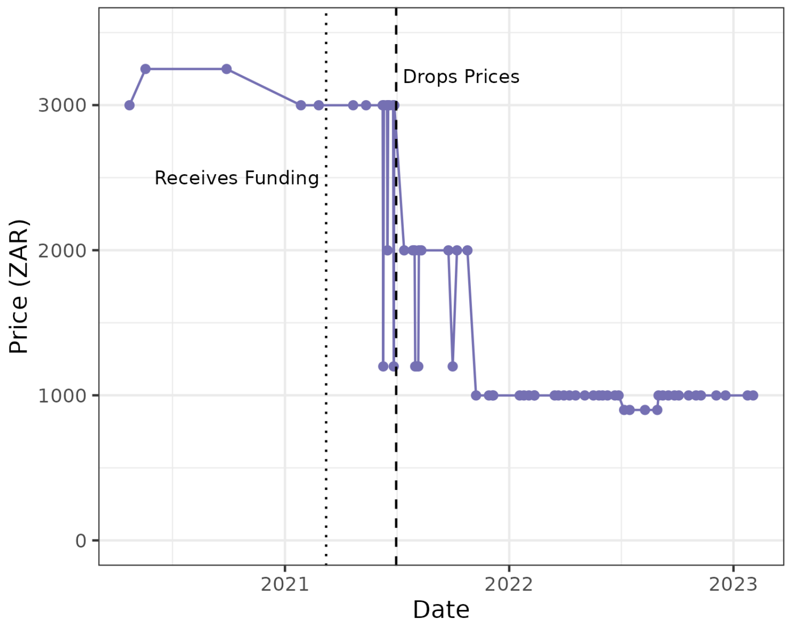
Note: Figure shows a hazard plot of default. Default is whether the business associated with the advance has an open advance and no transactions 8 months after the start of the advance. The cumulative default share is the fraction of businesses that had last transacted x weeks since the advance or earlier.

Figure F.2: *Gap Between Capital Takers and Non-Takers: Pre-Period Revenue* $> 50k$ *ZAR*



Note: Figure shows average monthly transactions of capital takers and matched control, limited to businesses with total revenue in periods -2, -1, and 0 over 50,000 ZAR (5X the minimum advance eligibility criteria). The matching was done using the same methodology as in Figure 2. Advances were taken in month 0. Bars display the 95% confidence intervals with Abadie and Imbens (2006) adjusted standard errors.

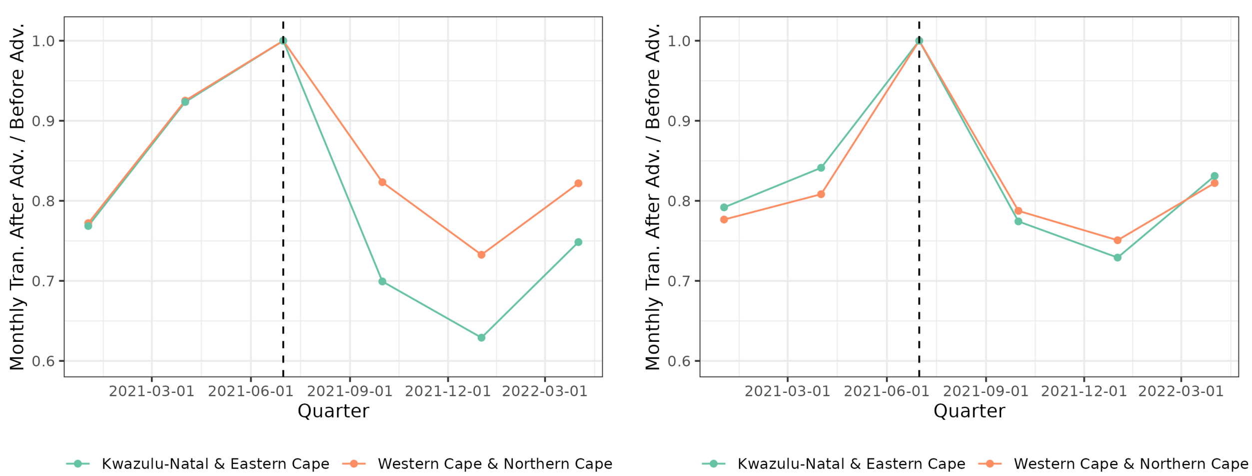
Figure F.3: *Rival Pricing Over Time*



Note: Figure shows the up-front price of the Competitor's flagship product over time. Observations are archived pages from the Internet Archive and the Competitor's Facebook posts.

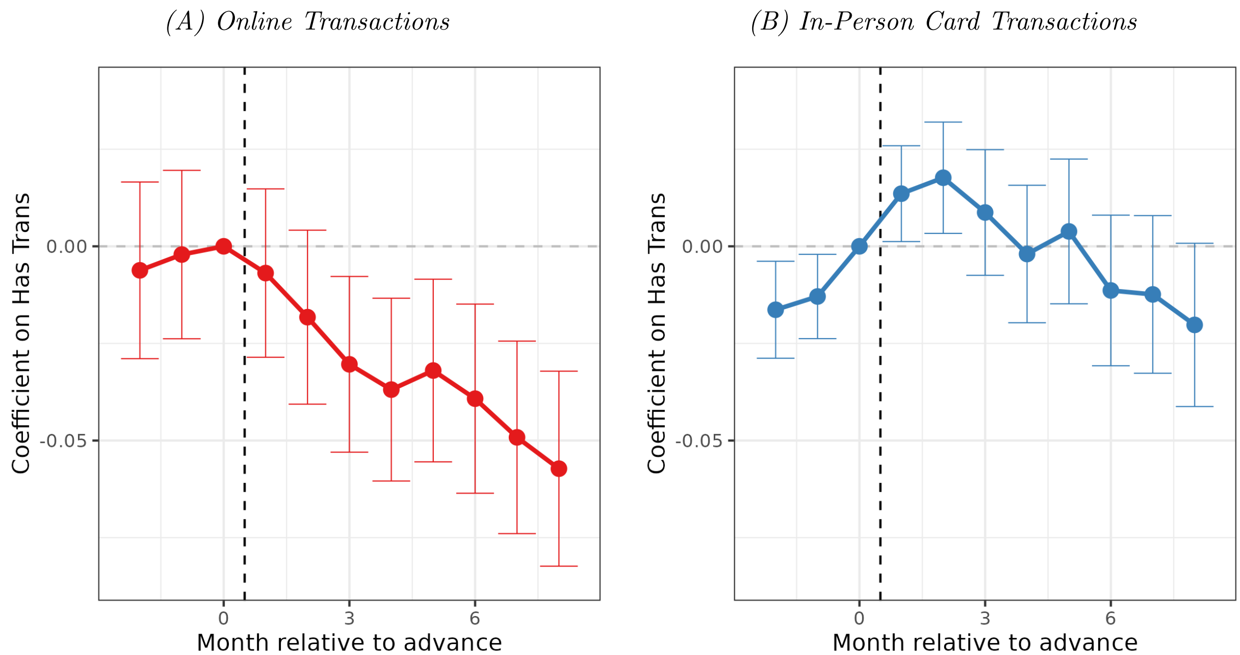
Figure F.4: *Post-Capital Transactions When Rival Lowers Price: Raw Averages*

(A) Capital Takers *(B) Non-Takers*



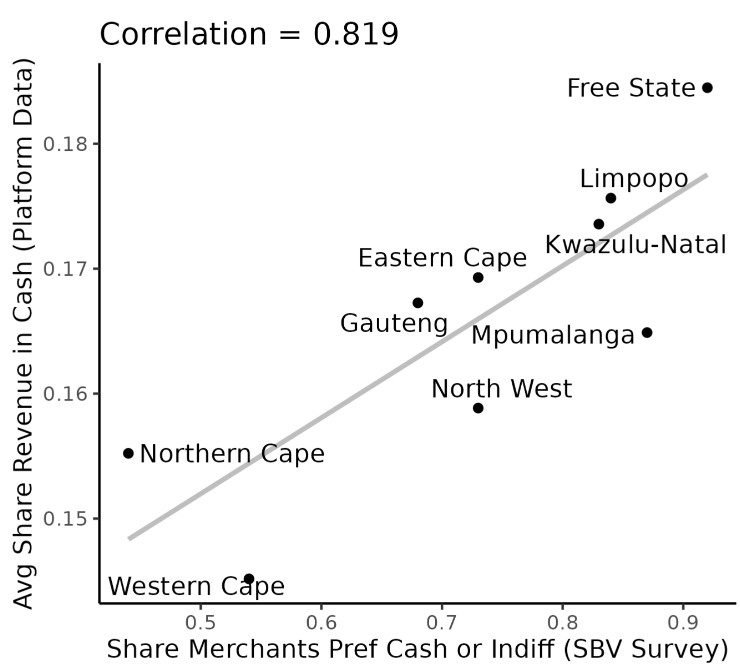
Note: Figure shows averages of Y_{it} from Equation 8 by province. All series are normalized to 1 in the month of the price change. This figure illustrates the raw variation underlying the estimates in Figure 4.

Figure F.5: *Takers vs Non-Takers Online and In-Person Transactions After Advance*



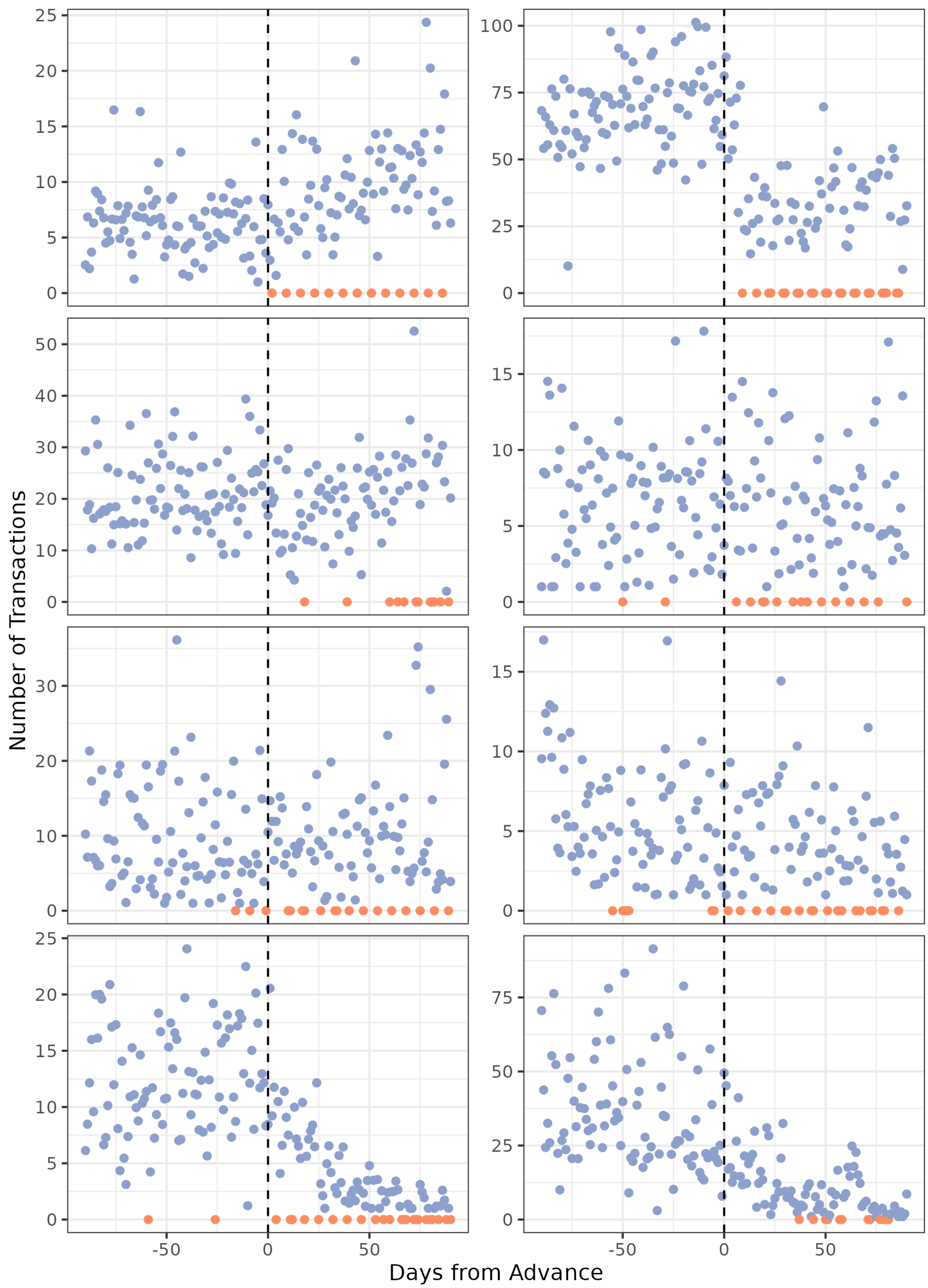
Note: Figure shows the two difference-in-differences estimates underlying the estimates in Figure 5. Panel (A) includes only online transactions, and exploits variation pre vs. post-advance and for takers vs. non-takers. Panel (B) is the same as Panel (A) but includes only in-person card transactions.

Figure F.6: *Cash Usage by Province: Platform Data versus Survey Data*



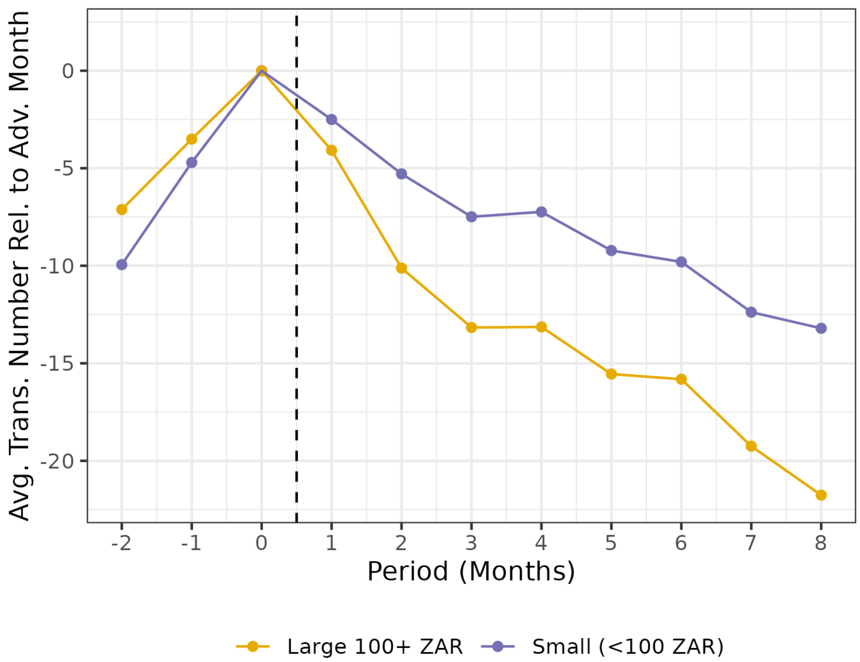
Note: Figure shows the correlation between the average share of Platform business revenue in cash (among businesses that report some cash use) and province-level survey data on cash preferences from SBV (University of Pretoria and SBV Services, 2023). The SBV measure is $1 -$ Share Digital in Image 21.

Figure F.7: *Examples of Business "Disappearing" on Certain Days*



Note: Figure shows examples of businesses around a first advance. Each plot shows one business. Each point is a day. Orange points indicate days with zero transactions. Small amounts of noise were added to each plot to preserve anonymity.

Figure F.8: *Capital Taker Revenue by Transaction Size*

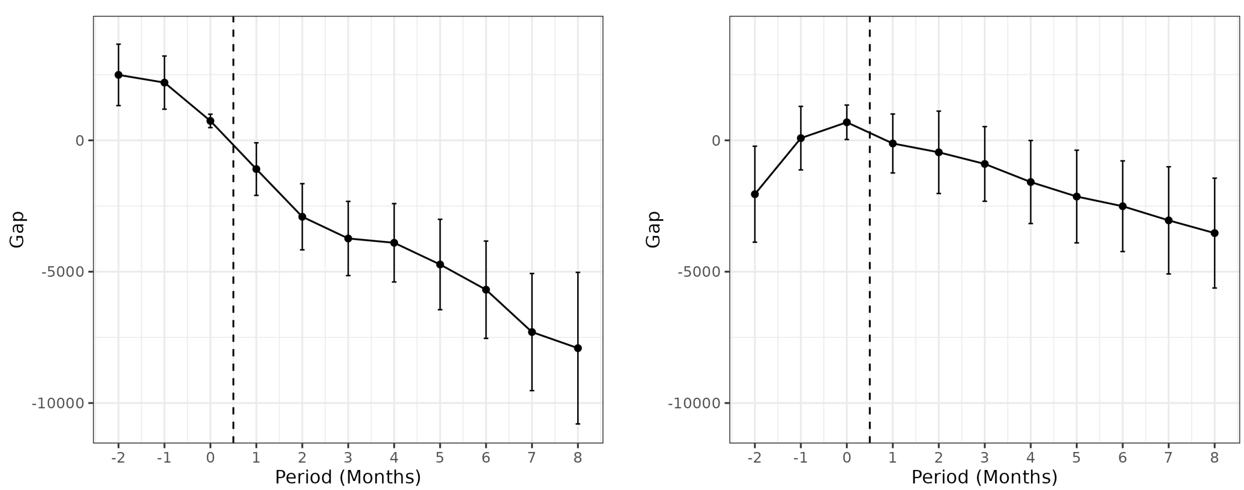


Note: Figure shows the average monthly number of transactions of capital takers by transaction size. Advances were taken in month 0. Both series are normalized to 0 in month 0. By purchasing power parity, 100 ZAR is approximately 15 USD.

Figure F.9: *Gap Between Capital Takers and Non-Takers by Time on Platform at Advance*

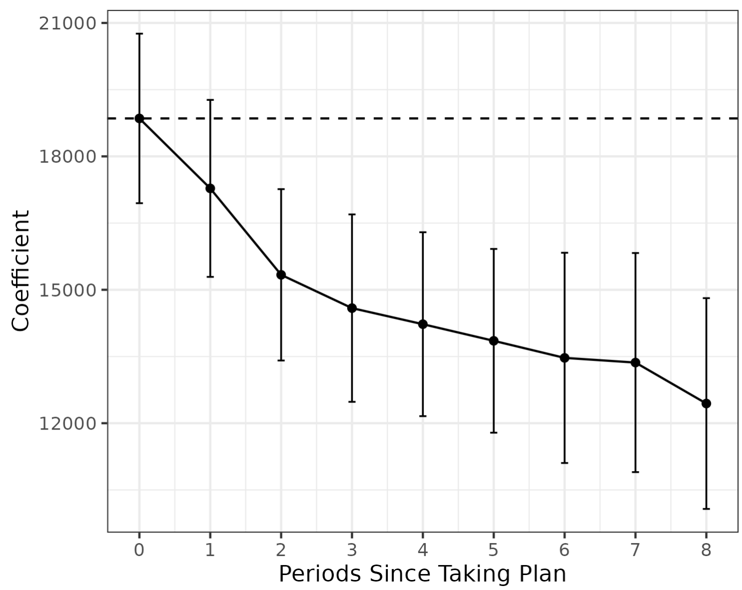
(A) 9 or Fewer Months on Platform

(B) More than 9 Months on Platform



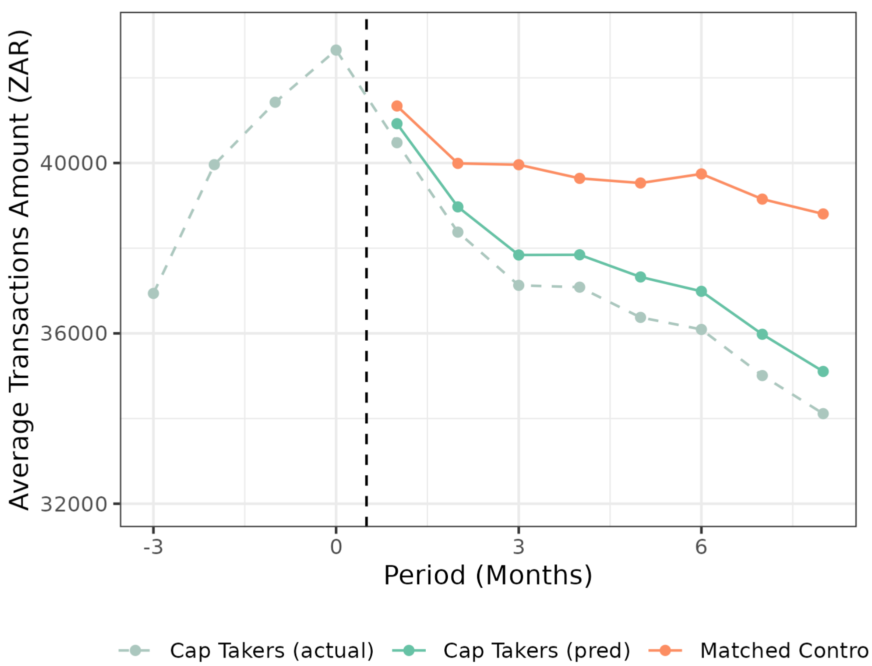
Note: Figure shows average monthly transactions of capital takers and matched control businesses split by time-on-platform. Panel (A) only includes businesses that had 9 or fewer months on the platform, panel (B) only includes businesses that had more than 9 months on the platform. The matching was done using the same methodology as in Figure 2. Advances were taken in month 0. Bars display the 95% confidence intervals with Abadie and Imbens (2006) adjusted standard errors.

Figure F.10: *Gap Between Capital Takers and Non-Takers - Panel Regression*



Note: Figure shows estimates of β_0 through β_8 from regression Equation E.1.

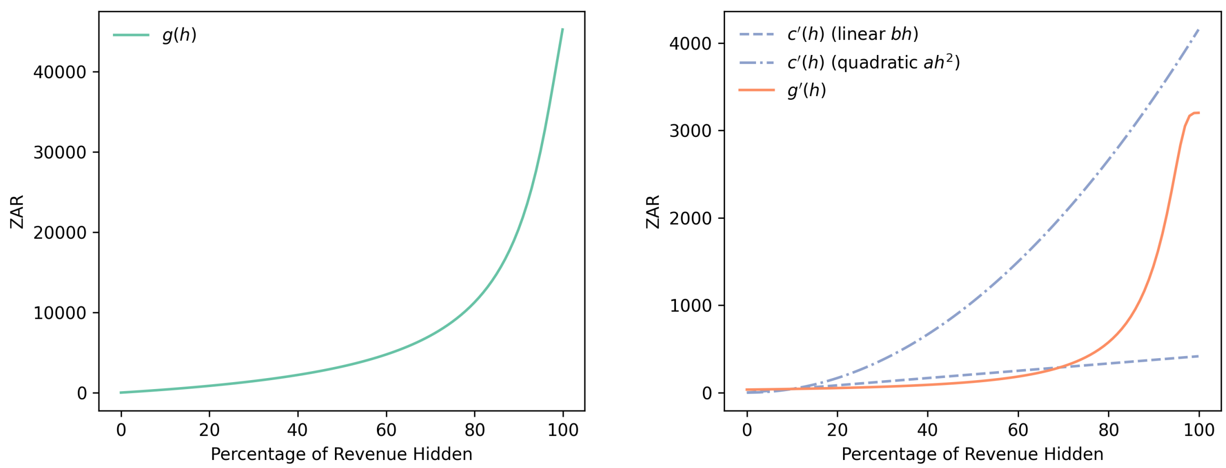
Figure F.11: *Gap Between Capital Takers and Non-Takers - Random Forest*



Note: Figure shows average actual and predicted monthly transactions of first time capital takers. Advances were taken in month 0. As described in Appendix E, random forest models were trained to predict the revenue of each taker and non-taker over eight months. The dashed green line shows the actual average monthly transactions amount of takers. The solid green line shows the average in-sample predicted amount using a random forest model. The solid orange line shows the average predicted amount using the covariates of the takers and the models trained on the *non-takers*.

Figure F.12: *Simulation for Valuing the Processor*

(A) Gain from Hiding *(B) Marginal Cost vs. Marginal Gain from Hiding*



Note: Figure shows the gains from hiding, potential marginal cost curves, and marginal benefit curve for the average advance. Our method is described in Appendix D. Panel (A) plots $g(h)$, the difference in the NPV of repayments between hiding 0% of revenue and h% of revenue. Panel (B) plots $g'(h)$ and two example marginal cost curves. a and b in the marginal cost curves are determined by both curves starting at $(0, 0)$ and passing through $(10, g'(10))$.

G Additional Tables

Table G.1: *Summary of Advance Takers by Sub-Industry*

Industry	Sub-Industry	N	Median Amount Sales 3mo (ZAR)	Median N Sales 3mo
Food, drink and hospitality	Bakery	502	75,292	508.5
Food, drink and hospitality	Bar/Club/Wine Farm	3,733	91,773	682
Food, drink and hospitality	Café	2,043	79,190	791
Food, drink and hospitality	Caterer	820	62,244	484
Food, drink and hospitality	Food truck / cart	1,770	63,775	535
Food, drink and hospitality	Restaurant	2,139	104,384	645
Food, drink and hospitality	Other	3,221	69,851	524
Healthcare, beauty and fitness	Beauty salon/Spa	4,005	49,983	112
Healthcare, beauty and fitness	Dentist/Orthodontist	101	72,941	84
Healthcare, beauty and fitness	Hair salon / barber shop	2,851	61,712	148
Healthcare, beauty and fitness	Medical & Health Services	785	75,699	135
Healthcare, beauty and fitness	Recreation/Sports	176	104,355	267
Healthcare, beauty and fitness	Sport & Fitness	132	68,447	259.5
Home and repair	Automotive services	972	114,822	113.5
Home and repair	Cleaning/Laundry Services	542	65,794	316
Home and repair	Computer Services	181	52,170	88
Home and repair	Other	274	112,709	117.5
Leisure and entertainment	Events	272	81,304	433
Leisure and entertainment	Ticketing	407	82,150	466
Online	Online Retail	247	96,613	160
Personal services	Education/tutor	159	70,778	72
Personal services	Other	1,015	72,983	155
Professional services	Photography/Art/Design	374	55,860	129.5
Professional services	Other	1,452	76,431	167
Retail	Antiques & Restorations	105	88,115	143
Retail	Art Dealer/Gallery	157	95,925	143
Retail	Automotive Parts	545	124,510	168
Retail	Card Shop/Gifts/Souvenirs	377	63,621	195
Retail	Clothing and Accessories	1,776	80,905	176
Retail	Craft market	830	63,733	193
Retail	Electronics/CPU Games	341	104,468	198
Retail	Food/Beverage/Grocery	1,349	86,492	532
Retail	Furniture / Home goods	421	109,875	101
Retail	Hardware shop	157	135,463	177
Retail	Jewelry and watches	112	53,050	139
Retail	Pet store	262	98,829	276
Retail	Other	1,611	89,693	248
Transportation	Taxi/Limo	121	46,320	81
Transportation	Other	200	98,772	111
Travel and tourism	Bed and Breakfast	348	78,496	82
Travel and tourism	Other	126	125,192	116

Note: Table presents summary statistics describing the businesses who take advances. The sample is the businesses associated with each advance in Table 1. Sales are in the three months prior to taking an advance. Table includes only sub-industries with > 100 businesses.

Table G.2: *Summary of Taker and Matched Control Samples*

Measure	Perfect Match	NN Match	Takers	Control
Month	X			
Industry	X			
Months on Platform		X	13.7	13.7
Pre-Period 3mo Revenue		X	123,234	121,454
Business is Rural			0.25	0.24
Owner is SA Citizen			0.93	0.93
Business is Sole Prop.			0.55	0.52

Note: Table presents summaries of the taker and matched control samples used in Section 4. The columns "Perfect Match" and "NN Match" indicate whether control firms were chosen by perfect or nearest neighbor matching on each measure. Rows with no "X" were not explicitly matched on in the matching procedure. The columns "Takers" and "Control" show means for each sample. The mean revenue of takers slightly differs from Table 1 which includes only businesses for which we can see 12 months of outcomes (instead of eight).

Table G.3: *Rival Price Drop: Two Period Triple-Diff*

	(1)	(2)
After Cut	−0.103	
	(0.088)	
Taker	−0.037	−0.022
	(0.048)	(0.057)
Affected Area	−0.031	−0.037
	(0.021)	(0.023)
After Cut X Taker	−0.036	−0.042
	(0.051)	(0.061)
After Cut X Affected Area	−0.029	−0.023
	(0.023)	(0.024)
Taker X Affected Area	0.098*	0.075*
	(0.026)	(0.019)
After Cut X Taker X Affected Area	−0.119**	−0.092**
	(0.027)	(0.020)
Log Amt. -3 Months		−0.050**
		(0.010)
Months on Plat FE	No	Yes
Month X Industry FE	No	Yes
R2 Adj.	0.002	0.010
Observations	189246	189246

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows estimates from Equation 8, as described in Section 5.1. Column (2) includes fixed effects for months on platform at the time of advance, and for industry-by-advance-month.

Table G.4: *Has Online & In-Person Transactions: Two Period Diffs*

	(1)	(2)	(3)
After Cut	−0.047***	−0.106***	−0.106***
	(0.005)	(0.005)	(0.005)
After Cut X Taker	−0.031***	0.009	0.009
	(0.008)	(0.007)	(0.007)
Is Online			−0.511***
			(0.009)
After Cut X Is Online			0.059***
			(0.006)
Is Online X Taker			0.011
			(0.013)
After Cut X Taker X Is Online			−0.040***
			(0.009)
Sample	Online Trans	In-Person Trans	Both Trans
Business FE	Yes	Yes	Yes
R2 Adj.	0.584	0.518	0.435
Observations	65175	65175	130350

Note: Table shows estimates from the triple-difference design in Equation 9 and corresponding diff-in-diffs, as described in Section 5.2. Column (1) includes only online transactions. Column (2) includes only in-person card transactions. Column (3) displays the triple difference estimates.

Table G.5: *Characteristics of Takers, Joining 6-weeks Pre- and Post-Change*

Measure	Pre-Change Takers	Post-Change Takers
Months on Platform	6.14	8.35
Pre-Period 3mo Revenue	91,579	95,036
Business is Rural	0.28	0.32
Owner is SA Citizen	0.92	0.9
Business is Sole Prop.	0.49	0.57
Industry = Food & Drink	0.48	0.49
Industry = Retail	0.23	0.2
Industry = Health & Beauty	0.1	0.14
Industry = Other	0.1	0.14

Note: Table presents summaries of characteristics of businesses that joined the platform within six weeks of the March 20, 2022 cutoff (Section 6.2) and took an advance in 12 months. This is the sample used in Columns (3) and (4) of Table 4.

Table G.6: *Advance Performance Around Policy Change: Robustness*

	(1) Default	(2) Log Total Amt. 8 Months	(3) Default	(4) Log Total Amt. 8 Months
Log Amt. -3 Months	-0.037^{**}	0.969^{***}	-0.016^{+}	0.897^{***}
	(0.008)	(0.027)	(0.007)	(0.059)
After Cutoff	-0.005	0.031	-0.006	0.047
	(0.017)	(0.039)	(0.024)	(0.050)
Sample	Full	Full, No Default	Near Cutoff	Near Cutoff, No Default
Month of Year FE	Yes	Yes	No	No
Demographic FE	All	All	All	All
R2 Adj.	0.017	0.467	0.005	0.396
Observations	5592	6963	831	1038

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table presents results from robustness specifications of Table 4. Each observation is a business. The dependent variable "default" in Columns (1) and (3) is whether the advance taker has an open advance and no transactions in the 8th month post-advance. The dependent variable in Columns (2) and (4) is log of total transaction amounts within 8 months post-advance, conditional on no default. All columns include demographic fixed effects described in Table 2. Standard errors are clustered at the industry level.

Table G.7: *Sales of Non-Takers, Joining 6-weeks Pre- and Post-Change*

Measure	Pre-Change Non-Takers	Post-Change Non-Takers
Sales Amt. Q1 (ZAR)	23,813	21,749
Sales N Q1	93	88
Sales Amt. Q2 (ZAR)	21,435	20,637
Sales N Q2	85	89
Sales Amt. Q3 (ZAR)	22,776	22,661
Sales N Q3	91	89
Sales Amt. Q4 (ZAR)	23,618	20,975
Sales N Q4	84	87

Note: Table summarizes sales of businesses that joined the platform within six weeks of the March 20, 2022 cutoff (Section 6.2) and did not take an advance in their first year. Rows show the sales amount and number of transactions in each of the first four quarters on the platform. "Pre-Change Non-Takers" displays means for businesses who joined in the six weeks prior and "Post-Change Non-Takers" displays means for businesses who joined in the six weeks after.

Table G.8: *Characteristics of Businesses Before and After Change, Decomposition Sample*

Measure	Pre-Change (Offered)	Post-Change (Not Offered)
Pre-Period 3mo Revenue	94,234	98,096
Business is Rural	0.25	0.26
Owner is SA Citizen	0.93	0.94
Business is Sole Prop.	0.5	0.49
Industry = Food & Drink	0.4	0.43
Industry = Retail	0.22	0.21
Industry = Health & Beauty	0.17	0.14
Industry = Other	0.17	0.14

Note: Table presents summaries of characteristics of businesses that joined the platform between September 2020 and August 2022. This is the sample used in Columns (1) and (3) in Table 5.

Table G.9: *Decomposition Regressions: IV*

	Dependent Variable: Amt. 3 Months	
	(1)	(2)
Taker	−2316.30	−2285.95
	(11120.95)	(11052.30)
Amt. -3 Months	0.92***	0.92***
	(0.05)	(0.05)
IV for Taker	Offered	Offered
Sample	Full	Full
Month of Year FE	Yes	Yes
Demographic FE	Industry	All
Adjusted R^2	0.682	0.682
Observations	42341	42341

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows results from IV regressions to decompose the short-term Gap, as described in Section 7. Columns (1) and (3) of Table 5 correspond to the "reduced form" regressions of the instrument (whether the businesses joined before or after the March 20, 2022 cutoff) on the outcome in Columns (1) and (2) in this table. The coefficient on "Taker" (whether the business took an advance) provide a direct estimate of $MH - CE$.

Table G.10: *Extensive Margin Usage (Days/Week)*

	(1) Share <5 Days 3m Post	(2) Share <5 Days 8m Post	(3) Avg Days 3m Post	(4) Avg Days 8m Post
Taker	0.023*	0.060***	-0.058*	-0.18***
	(0.0074)	(0.0046)	(0.022)	(0.021)
Avg Days 3m Pre			0.92***	0.89***
			(0.015)	(0.019)
Sample	All, First Plans	All, First Plans	All, First Plans	All, First Plans
Demographic FE	Yes	Yes	Yes	Yes
Quarter X Year FE	Yes	Yes	Yes	Yes
Past Revenue Controls	Yes	Yes	Yes	Yes
Observations	12784	12784	12784	12784
Adjusted R^2	0.057	0.060	0.30	0.26

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows regressions of taker on number of days per week the card machine was used. Each observation is an advance. Every column includes non-takers, following Footnote 28. We condition on businesses who had transactions in the 8th month post advance and were using the card machine on average more than five days per week in the three months pre-advance. "Pre" refers to the months pre-advance, "Post" refers to the months post-advance. "Share $<$ 5 Days" is whether a business transacts on average less than five days per week. "Avg Days" is the average number of days per week transacted. Past Revenue Controls are "Years on Platform", "Log Amt. -3 Months", and "Relative Sd." from Table 2. All other independent variables are defined in Table 2. Standard errors are clustered at the industry level.

Table G.11: *Province-Level Survey Cash Measure on Advance Performance*

	(1) Default (8mo)	(2) Log Total Amt. 8 Months	(3) Default (8mo)	(4) Log Total Amt. 8 Months
Taker	0.015*	-0.081***	0.023	-0.11**
	(0.0054)	(0.015)	(0.018)	(0.036)
Survey Cash			0.043**	-0.15*
			(0.012)	(0.048)
Taker X Survey Cash			-0.012	0.047
			(0.023)	(0.045)
Sample	All, First Plans	All, First Plans	All, First Plans	All, First Plans
Province FE	Yes	Yes	No	No
Demographic FE	Yes	Yes	Yes	Yes
Quarter X Year FE	Yes	Yes	Yes	Yes
Past Revenue Controls	Yes	Yes	Yes	Yes
Observations	49651	39618	49642	39612
Adjusted R^2	0.074	0.72	0.073	0.71

Standard errors in parentheses

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Table shows regressions of various measures of advance performance. Columns (1) and (2) are the same as Columns (5) and (6) in Table 2. Columns (3) and (4) use a province-level survey measure of merchants' cash usage from SBV (University of Pretoria and SBV Services, 2023). The SBV measure is $1 -$ Share Digital in Image 21.