

# FINTECH & FINANCIAL FRICTIONS: THE RISE OF REVENUE-BASED FINANCING\*

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## Abstract

We use transaction-level data from a major payment processor to study FinTech-provided small business “revenue-based financing.” After eight months, payments through the processor are 16% lower for businesses who take financing offers than observably similar non-takers, driven by moral hazard from revenue hiding and adverse selection. Two natural experiments suggest FinTech platforms’ non-lending interactions with small businesses—e.g., payment processing and inventory management—can limit both hiding and selection. By tying repayment to the continued use of non-lending products, FinTechs can mitigate enforcement and monitoring frictions. Our results help explain the rise of FinTech-provided revenue-based financing.

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

**Keywords:** small business lending, moral hazard, adverse selection, FinTech

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Revenue-based financing—which ties repayment schedules to future revenue—has become an increasingly important source of capital for small businesses in the U.S. and globally, with FinTech providers driving much of the growth.<sup>1</sup> Despite this rise, relatively little evidence exists on FinTechs’ use of these contracts and whether these providers can overcome the frictions that have historically made lending to small businesses at scale challenging. A handful of recent works focus on FinTech lenders’ access to alternative data to screen ex-ante,<sup>2</sup> but many providers are also broader platforms that tie repayment to the continued use of their non-lending products ex-post.<sup>3</sup> This connection raises the possibility that FinTech platforms’ non-lending interactions can help mitigate ex-post enforcement and monitoring frictions. For example, an e-commerce firm not only has data on past sales, but may also automatically deduct repayments from future sales on its platform.

In this paper, we use theory and transaction-level data from a major financial technology platform to study two questions. First, how do FinTech platforms’ non-lending interactions with small businesses affect classic contracting frictions in lending? Second, under what circumstances will these platforms be more or less successful in mitigating these frictions? These questions are important for evaluating the long-term potential of FinTech platforms as small business lenders and the extent to which they can more efficiently provision capital than traditional lenders. In answering them, we also speak to the effects of FinTech-provided revenue-based financing on small and medium business entrepreneurs in this setting.

We organize our analysis with a conceptual framework of revenue-based financing in which borrowers can hide revenue ex-post and have private information about future revenue ex-ante. The model generates two predictions on when and how new technology may help mitigate these frictions. First, the sustainability of financing depends on whether the FinTech platform and its environment can increase the cost of diverting transactions. The more non-lending products increase this cost (e.g., by being unsubstitutable and tied to repayment), the more they can reduce ex-post moral hazard. Second, the sustainability of these contracts depends on the extent to which the FinTech platforms’ interactions aid in screening. The more non-lending products provide predictive information (e.g., from a longer history of transactions), the more they can reduce ex-ante adverse selection. Limiting both ex-post and ex-ante frictions is necessary for sustaining a lending equilibrium: either alone can cause the market to unravel.

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<sup>1</sup>For one overview, see Rush (2021). See examples from [Square](#), [Toast](#), [Shopify](#), and [Velocity](#) among many others. Dixit (2018) focuses on revenue-based financing from payment processors, in particular.

<sup>2</sup>See, for example, Hau *et al.* (2019); Berg *et al.* (2020); Ghosh *et al.* (2021); Balyuk (2023).

<sup>3</sup>Berg *et al.* (2022) describe a rise in lending from “BigTechs, telecommunication firms, E-commerce firms, and payment firms.” Cornelli *et al.* (2020) estimate that volume from FinTechs narrowly focused on lending (e.g., marketplace lenders) roughly *halved* from 2017 to 2019, even as total tech lending rose from 600 billion to 795 billion. Dresner *et al.* (2022) provides an overview of the related concept of “embedded finance.”

We explore the framework’s two predictions using data on over 100 million transactions from a South African financial technology platform. The platform processes payments and offers small businesses revenue-based financing “capital advances.” When a business takes an advance, the platform deducts a constant share of their daily transactions until the principal plus a fixed fee is repaid. An increase in processed revenue results in faster repayment, while a drop leads to slower or no repayment. This contract moves some risk from the business to the financier, as there are no additional interest charges if revenue falls and no required repayment if the business fails.<sup>4</sup>

Our data includes processed transactions for advance takers and *non-takers*, allowing us to compare these businesses and explore the model’s frictions. We show that the ex-post revenue “gap” between the takers and observably similar non-takers can be decomposed into three components: adverse selection, moral hazard in revenue hiding, and the causal effect of the advance on takers. Eight months after an advance, the revenue of takers is 16.3% lower than observably similar non-takers, consistent with the existence of asymmetric information. This gap is primarily driven by an intensive margin decrease in the revenue of takers relative to non-takers, rather than by takers being more likely to leave the platform; even conditional on no default, the average revenue for takers is 15.2% lower than the matched control.

Next, to explore our framework’s first prediction, we show how changes in non-lending interactions impact advance repayment through hiding costs. We do so by using a shock to a rival processor’s (non-lending) pricing in response to funding from a World Bank Group member to “make digital payments systems more affordable.” We use a difference-in-differences design that compares takers in areas where the rival does and does not operate, looking at pre- vs post-advance revenue before versus after the shock. The funding shock for the competitor led to a relative decrease in takers’ transactions of 10–15%, providing evidence that hiding responds to the cost of diverting transactions to alternative processors. We also show that the usage of two platform features, opening a “manage” tab and exporting sales, are predictive of advance performance. This suggests platforms can improve repayment with “sticky” features (e.g., add-ons, higher quality machines) which make it more costly to hide.

We then explore our second prediction, which implies FinTech platforms’ non-lending interactions with small businesses can limit hidden information challenges. We first note that data from non-lending platform usage—pre-advance transaction volume and volatility, time on the platform, and “sticky” feature usage—are all strongly predictive of advance performance, conditional on business and advance characteristics. This predictive power supports the idea that non-lending interactions can be used to screen for both business quality

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<sup>4</sup>We discuss the contract in greater detail in Section 1.

and hiding costs.<sup>5</sup> We then provide quasi-experimental evidence that a longer history of non-lending interactions can improve screening. Specifically, due to a temporary system error, businesses that joined the platform after March 2022 and met minimum activity requirements were not offered an advance for six months, instead of the usual three. After this change, total revenue in the eight months post-advance for takers was 5% higher, controlling for pre-advance revenue, consistent with information from additional interactions improving screening. We also show repeat advances are less likely to default than first-time advances, suggesting these repeated interactions provide additional information as well.<sup>6</sup>

The ability to mitigate financial frictions may allow FinTechs to expand credit access or enable revenue-linked repayment flexibility that encourages entrepreneurial risk-taking.<sup>7</sup> To understand the possibility of such positive effects on small businesses in this setting, we empirically decompose the revenue gap between takers and observably similar non-takers. This allows us to provide suggestive evidence on the size of the causal effect on takers. In particular, variation from the temporary delay in offers identifies short-run adverse selection under an assumption of counterfactual similarity between businesses who joined the platform before and after the error. Our point estimates imply adverse selection is roughly 60% the size of the gap, but is noisily estimated. If we additionally assume businesses shift 10% of their post-advance revenue (the magnitude of response to the rival price drop),<sup>8</sup> our results imply a three-month positive causal effect equal to an 8% increase in revenue relative to the pre-advance average. Consistent with this result, we show 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.

Finally, we discuss what our findings imply about the cost of revenue hiding, a key force that sustains lending in our framework. Our back-of-the-envelope calculations suggest that if takers hide 10% of revenue, the additional cost of hiding all transactions for the average taker is at least 45,000 ZAR, slightly higher than their monthly revenue. This relatively high cost suggests businesses find different platforms to be imperfect substitutes, or that other factors—such as fear of being “caught” or moral considerations—play an important role.

Our work builds on a recent literature studying the rise of FinTech lending (for overviews, see Berg *et al.*, 2022; Agarwal and Zhang, 2020; Allen *et al.*, 2021).<sup>9</sup> Using UCC filings,

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<sup>5</sup>The latter can be seen as mitigating a form of “adverse selection on moral hazard.”

<sup>6</sup>Practically, it supports the notion that FinTech platforms can “learn-by-lending” to limit adverse selection (Botsch and Vanasco, 2019).

<sup>7</sup>See, for example, the discussion of Battaglia *et al.* (2023) below.

<sup>8</sup>As discussed in Section 6, the size of shifting is inversely related to the causal effect. Thus a smaller shifting estimate provides a more conservative estimate of the causal effect. The magnitude of the post-period response bounds post-period hiding from below (because businesses cannot negatively hide).

<sup>9</sup>Chen *et al.* (2017) and Schneider *et al.* (2020) discuss the decline in bank small business lending.

Gopal and Schnabl (2022) show loan originations from FinTechs increased almost 40-fold from around 2,000 loans in 2006 to over 78,000 by 2016 in the US.<sup>10</sup> Globally, Cornelli *et al.* (2020) estimates that “FinTech” and “BigTech” firms lent 795 billion USD in 2019, up from 20 billion in 2013.<sup>11</sup> Other works quantify the increased presence of FinTechs in mortgage lending (Buchak *et al.*, 2018; Jagtiani *et al.*, 2021) and consumer lending (Berg *et al.*, 2020; Ziegler *et al.*, 2021).

We speak directly to recent papers that have offered at least three types of explanations for the growth of FinTech lending. First, FinTechs may more easily acquire customers by reducing search costs or processing applications faster (e.g., Fuster *et al.*, 2019; Liu *et al.*, 2022). Second, FinTechs may have regulatory advantages (e.g., Buchak *et al.*, 2018; Braggion *et al.*, 2023). Third, FinTechs may use technology to limit classic financing frictions in lending (e.g., Frost *et al.*, 2019; Ghosh *et al.*, 2021). Berg *et al.* (2022) note that evidence on the third force, “a significant competitive advantage over banks in terms of screening and monitoring”, is limited. This paper provides evidence—and a precise explanation of how—this force is important for the rise of FinTech-provided revenue-based financing. In doing so, we help explain how FinTech platforms compete with banks’ informational advantages (Diamond, 1991; Rajan, 1992) and the extent to which they overcome long-documented small firm financing frictions (see Nanda and Phillips, 2022, for one overview).<sup>12</sup>

Our setting is similar to Rishabh and Schäublin (2021), who study sales-linked loans in India and find evidence of revenue hiding immediately after disbursement for certain repeat borrowers. Relative to this work, our framework clarifies how FinTechs’ non-lending activities interact with financial frictions beyond strategic default. We also provide broad evidence of asymmetric information (rather than for a subset of repeat borrowers) and decompose this into ex-ante and ex-post frictions using two natural experiments.

We add to a literature in development economics that suggests credit constraints and risk aversion prevent small firms in developing countries from making positive NPV investments, motivating contracts with flexible repayment and risk-sharing.<sup>13</sup> In a field experi-

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<sup>10</sup>Dixit (2018) describes the rise of payment processors as small business lenders in the US, noting that from 2015-2018 PayPal and Square originated over 5 billion USD. Payment processors may also have better pricing than earlier “merchant cash advance” providers, suggesting they could have an edge.

<sup>11</sup>While there is no single definition, Berg *et al.* (2022) define FinTech financing as “the use of technology to provide lending products.” Gopal and Schnabl (2022) define FinTechs as entities that “lend online and do not take deposits.” Our use of “FinTech” follows these expansive definitions, including “BigTech” firms that have a core business unrelated to finance.

<sup>12</sup>Some non-FinTech platforms offer revenue-based financing (e.g., [Karmen](#), [Uncapped](#)), generally to larger businesses. These lenders also often focus on businesses with verifiable revenue from digital payments, consistent with the importance of the ex-ante and ex-post informational channels we discuss.

<sup>13</sup>In the field, these contracts have been tested using financial technology. See, for example, Cordaro *et al.* (2022) who notes that they “leverage innovations in technology and digital finance that improve the observability of microenterprise performance.”

ment, Battaglia *et al.* (2023) show that the option to delay up to two repayments during a 12-month loan cycle substantially improved business outcomes, through an insurance mechanism. The FinTech-provided revenue-based financing we study offers a degree of repayment flexibility tied to sales, an example of “state-contingent” contracts at scale. Our work provides insights into how and when technology providers can mitigate classic information and incentive frictions associated with such contracts.<sup>14</sup>

We also build on classic literatures studying the benefits of revenue-sharing contracts in vertical markets such as franchising (e.g., Lafontaine and Shaw, 1999; Dana and Spier, 2001; Mortimer, 2008)<sup>15</sup> and the lending advantages of trade credit providers (e.g., Smith, 1987; Biais and Gollier, 1997; Wilner, 2000; Burkart and Ellingsen, 2004; Cunat, 2007). Our results suggest that technology-based non-lending interactions could have similar benefits and advantages in small-business lending while also aiding enforcement.

Finally, we add to prior works that estimate the prevalence of asymmetric information using price variation (Einav *et al.*, 2010; Einav and Finkelstein, 2011), observational administrative and survey data (Hendren, 2013; Herbst and Hendren, 2024), or field experiments (Karlan and Zinman, 2009). We show how, in selection markets, data that includes outcomes for takers and non-takers, as well as the observables available to the decision maker, can be used to quantify asymmetric information net a causal effect. We also provide an example of how natural experiments can decompose this object.

The rest of the paper is structured as follows. Section 1 describes the setting and platform’s revenue-based financing contract. Section 2 presents the conceptual framework. Section 3 introduces the data and provides evidence of financing frictions. We explore the framework’s predictions of moral hazard in Section 4 and adverse selection in Section 5, showing that non-lending interactions help mitigate both frictions. Section 6 provides suggestive evidence of the effects of revenue-based financing for businesses. Section 7 estimates the cost of hiding transactions. Section 8 concludes.

## 1 Setting

Our data comes from a major South African financial technology platform (“the Platform”). The Platform primarily offers processing machines and online interfaces to small businesses, currently processing over \$2 billion USD (\$5 billion USD purchasing power parity) in trans-

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<sup>14</sup>In this way, our work also speaks to 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).

<sup>15</sup>In particular, Dana and Spier (2001) suggest that distributor-retailer revenue-sharing contracts are sustained by distributors selling cassettes and *information systems* to video stores, allowing them to monitor.

actions per year for 250,000 active users. These users are estimated to represent 10% of all South African small and medium-sized enterprises (SMEs).<sup>16</sup> The Platform takes a small percentage (2-3%) of each transaction before depositing the rest into business bank accounts.

In addition to processing transactions, the Platform offers a revenue-based financing “capital advance” product, that borrowers repay with their daily processed payments. Since 2018, the 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 reported obtaining financing as their second most important obstacle, behind only a national electricity crisis (World Bank, 2021).<sup>17</sup> Another survey reported that only 2% of SMEs rely on bank financing (SME South Africa, 2018). The lack of traditional financing suggests contracting frictions are important in this context.

The Platform’s advance offers consist of a principal, a factor rate, and a charge rate. If the business takes the offer, the Platform deposits the principal amount within one business day. The Platform then automatically takes 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 has a claim on the revenues they process. Two partner companies, who observe past transactions and time on platform only, provide the capital and set the contract terms.

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 3.

Advance offers are extended to small businesses that processed transactions for at least three months and met minimum activity requirements.<sup>18</sup> A key advantage in our setting is that both the minimum activity threshold and the offer contract features are based on

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<sup>16</sup>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).

<sup>17</sup>Among exiting SMEs, 22% said they had challenges due to “problems getting financing” (United Nations, 2023). Financing has also become important for firms to invest in backup power solutions (e.g., generators and solar systems), given South Africa’s rolling blackouts (Kozak, 2023; Clarke *et al.*, 2024).

<sup>18</sup>As of July 2023, these requirements were a minimum of 18 card transactions over the last 90 days and a monthly turnover of more than 3,000 ZAR per month.

observables available to us. Therefore, we can estimate asymmetric information by comparing observably similar advance takers and non-takers. The lack of random variation in contract features limits our ability to speak directly on the effects of pricing, so in our empirical analyses, we take the contract features as given and focus on the motivating questions above.

## 2 Conceptual Framework

We present a stylized model of small business financing that helps explain the link between non-lending interactions and frictions in lending. Non-lending interactions help limit revenue hiding ex-post and hidden information ex-ante, both of which are necessary to sustain revenue-based financing in the model. We also show how moral hazard in hiding and adverse selection contribute to the empirical difference in revenue between takers and non-takers, allowing us to explore the model’s forces in the data. All proofs are in Appendix A.

### 2.1 Setup

A risk-neutral lender may offer small businesses “advances” of the form  $(\eta, L)$ . If accepted, small businesses receive  $L$  in period 1 and must repay a share  $\eta$  of their revenue in period 2.<sup>19</sup> 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 small businesses who take an advance if offered are seen as “compliers” and those who do not as “never takers.”

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

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

The lender cannot distinguish between types, but knows the mix of good ( $G$ ) and bad ( $B$ ) types conditional on observable characteristics  $X$ :  $P(G|X) = p$  and  $P(B|X) = 1 - p$ .

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<sup>19</sup>This two-period framework abstracts the Platform’s full dynamics in which the financier captures a share of payments until a fixed threshold is met. However, our stylized contract still captures the two central forces of the contract. First, as the Platform does not hold a claim on the business if it never reaches the repayment threshold, the amount owed is in practice often lower than the threshold. 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.

As businesses with characteristics  $X$  are ex-ante identical, the lender must offer the same advance (if any) to all businesses with characteristics  $X$ .

**Revenue Hiding** If an advance is extended in the first period, the lender faces two related challenges in the second period. First, they must enforce the contract, collecting repayments from borrowers. Second, because repayment is tied to revenue, they must monitor their borrowers' revenue. Borrowers, by contrast, want to hide revenue to avoid repayment. To capture this, the lender can only observe and collect from a fraction  $v(c)$  of a borrower's revenue, where  $c$  is the borrowers' cost of hiding.

**Business's Problem** Businesses choose whether to accept the advance  $(\eta, L)$ . If they take the advance 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  $v$  is  $c(1-v)^2$  and discounting occurs at rate  $r$ . Then, businesses choose to take the advance iff:

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

Note that bad types will always choose to take the advance to consume  $L$ .<sup>20</sup> Good types may choose to invest when  $\eta$  is low and  $\mu_i$  is high. Thus, there is both positive selection (the contract is attractive for businesses with better investment opportunities  $\mu_i$ ) and negative selection (the contract is attractive for businesses with low  $Y_i(0)$ ).

**Lender's Problem** The lender chooses  $\eta$  (which governs the degree of revenue sharing) and for simplicity, leaves  $L$  fixed. Lender profits for a set of observationally equivalent borrowers with characteristics  $X$  is:

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

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  $\eta$  for each  $X$ . If lenders cannot make non-negative profits for any  $0 < \eta < 1$ , no advances will be offered. Intuitively, for any  $X$

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<sup>20</sup>The "bad type" business receives 0 when either investing or not taking the advance, but receives  $L$  in period 1 if they take the advance and consume it.

in which revenue-based financing is offered, good types must cross-subsidize the bad types (since lenders always make a loss on bad types).

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

## 2.2 Non-Lending Interactions and Financial Frictions

Non-lending interactions may affect the model in two ways. First, they may increase the “cost” of hiding revenue,  $c$ . Second, they may increase  $p$  within sets of observables. Proposition 1 formalizes how each effect helps FinTech platforms sustain revenue-based financing.

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

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

Proposition 1 makes two predictions about the circumstances under which FinTech platforms’ non-lending interactions with small businesses may mitigate financial frictions. First, due to a reduction in ex-post revenue hiding, contracts should perform better when a FinTech platform can increase the borrower’s “hiding costs”,  $c$ . Second, due to a reduction in ex-ante adverse selection, financing contracts should perform better when the platform’s non-lending activities provide information on a borrower’s future revenue.

To understand the first effect, note that hiding payments from a FinTech platform often requires losing access to the platform’s non-lending features. For example, if repayment is automatically deducted from sales on an e-commerce website, a business must find customers off the website to divert revenue. If repayment is through a payment processor, the business must switch to cash or buy an alternative processor. The strength of this channel depends on the non-lending features’ value. In particular, when they are not substitutable (e.g., due to inconvenient alternatives or a superior product) the cost of hiding increases.

The second effect captures the idea that non-lending interactions generate observables predictive of future business performance. For instance, data on a small business’s prior e-commerce sales provide a verifiable source of information that can be used to predict future sales. With more information (e.g., from more features or a longer history of interactions), the ability of the lender to partition businesses into groups with high  $p$  increases. The positive

effects of non-lending interactions on both  $c$  and  $p$  help sustain revenue-based financing, as either being too low can cause unraveling.

Proposition 1 also suggests, more broadly, that technologies that lower the cost of receiving accurate (ex-ante and ex-post) signals about borrowers increase the feasibility of small business lending. FinTechs’ interactions can be seen as an innovation that lowers the cost of verifying business quality at scale, somewhat analogous to the long-documented informational advantages of bank loans over other debt sources (Diamond, 1991). In a similar spirit to Proposition 1, if the financier’s verification costs are too high, they will not lend.

## 2.3 General Framework and Revenue “Gap” Decomposition

We now generalize the framework, showing how moral hazard from revenue hiding and adverse selection appear in an empirical revenue gap between observably similar takers and non-takers. This generalization helps us explore financing frictions in the data.

There are two challenges with bringing Proposition 1 to the data. First, it is practically and conceptually difficult to classify businesses into two types that can be characterized by  $p$ . Second, the cost of hiding  $c$  is not a directly measurable object. 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:*

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

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

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

Equation 3, which defines moral hazard from revenue hiding, captures the quantitative effect of hiding on the ex-post revenue collectible and observable to the financier.<sup>21</sup> 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 quantitative effect of businesses with private information about future revenue differentially selecting into advances. Equation 5, defining the causal effect of financing on takers, is shaped by the investment opportunities available to takers. In

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<sup>21</sup>An alternative form of moral hazard associated with equity contracts arises in a principal-agent setting where managers reduce effort, lowering the investment returns. However, unlike a pure equity contract, in our empirical setting 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.

a potential outcomes framework,  $CE$  is the average treatment effect on the treated (ATT).

**Revenue Gap** Our general empirical 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. Because our data includes the observables,  $X$ , available to the Platform, as well as the processed transactions for advance takers and *non-takers*, we can directly identify the Gap in the data.<sup>22</sup> 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:*

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

*Then:*

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

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.<sup>23</sup>

In the remainder of this paper, we conduct four empirical exercises motivated by our framework. We (1) show a substantial Gap exists and, given this, (2) explore the predictions of Proposition 1 using both observational evidence and two natural experiments. To understand the (causal) effects of revenue-based financing, we (3) bound the objects in Definition 1. Finally, we (4) discuss what our estimates imply about hiding costs,  $c$ , in this setting.

### 3 Summary Statistics & Revenue Gap

This section summarizes the data we use to explore our conceptual framework empirically. We show that non-lending interactions predict advance performance, but there is also a

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<sup>22</sup>We describe our procedures for doing so and our estimates in Section 3.3.

<sup>23</sup>In our setting we can parameterize moral hazard using  $v$ . With other forms of moral hazard, one can 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.

substantial revenue gap between takers and observably similar non-takers, providing evidence of financing frictions (Proposition 2).

### 3.1 Data & Summary Statistics

Our data comes from a South African financial technology company that processes payments and offers capital advances, as described in Section 1. The data includes all transaction-level payments for both advance takers and non-takers, including the size, amount, time, and location of transactions. We also observe all information on advances, including the principal amount, pricing components, and repayments. Finally, we have business-level information including location, industry, platform feature usage, and owner demographics. We focus on advances made from June 2020 onwards (to exclude the highest level of COVID-19 lockdown) and advances where we can observe 12 months of post-advance outcomes as of May 2024.

Table 1 provides summary statistics on the 15,375 first advances and 23,879 repeat/re-advances in this sample.<sup>24</sup> Advance takers are generally small, consumer-facing businesses and entrepreneurs (e.g., hair salons, food trucks).<sup>25</sup> First advance takers have an average of 120,000 in South African Rand ( $\sim$ \$7,000 USD) in sales over the prior three months.<sup>26</sup> The average principal of first advances is around one month’s worth of 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.<sup>27</sup> 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.<sup>28</sup> 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.

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<sup>24</sup>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 refer to both as repeat advances in the text.

<sup>25</sup>Appendix Table E.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.”

<sup>26</sup>By purchasing power parity, 120,000 ZAR is roughly \$16,000 USD.

<sup>27</sup>The implicit APR, if sales from the last three months stay constant, is around 80%. To understand this, note first that 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).

<sup>28</sup>Repayment is daily and begins immediately, resulting in many payments with little discounting.

## 3.2 Measures & Predictors of Repayment

We test the relationship between ex-ante observables and two outcome measures of advance performance. First, we say a business has *defaulted* if they have no transactions (and therefore no payments) in the eighth month after taking an advance, but still have an open advance.<sup>29</sup> Second, for businesses that did not default, *conditional revenue* is the sum of revenue over the eight months after taking the advance. We begin by exploring the predictability of advance performance and regress each measure on ex-ante observables with:

$$Y_{i,t} = \mathbf{T}'_i\boldsymbol{\beta}_1 + \beta_2\text{First}_i + \mathbf{X}'_i\boldsymbol{\beta}_3 + \delta_t + \epsilon_{i,t}. \quad (8)$$

For advance  $i$  given in quarter  $t$ ,  $Y_{i,t}$  is either default or log conditional revenue.  $\mathbf{T}_i$  is a vector of characteristics related to platform use and transactions in the three months before disbursement.  $\text{First}_i$  is an indicator for whether the advance is a first advance.  $\mathbf{X}_i$  is a vector of business and advance characteristics.<sup>30</sup>  $\delta_t$  are quarter by year fixed effects.

Table 2 shows that both time on platform and transaction volume before the advance are strong predictors of default and conditional revenue.<sup>31</sup> Our preferred specifications in Columns (3) and (4) (which control for owner demographics, quarter, and advance characteristics) show that for first advances, an additional year on the platform corresponds to a 3.5 percentage point decrease in default and a 3.6% increase in conditional revenue. A doubling of transaction volume in the three months prior to receiving an advance corresponds to a 87% increase in conditional revenue. Table 2 also shows that a measure of ex-ante “stability” or volatility—the weekly relative standard deviation of transactions in the months prior—positively predicts default.<sup>32</sup> Each of these results provide suggestive evidence that non-lending interactions through payment processing generate observables predictive of repayment. We explore this further in Section 5. Columns (5) and (6) also suggest that these results hold when looking across both first-time and repeat advances, but that first advances are more likely to default than repeat advances.

<sup>29</sup>Appendix Figure D.1 provides hazard plots of this measure over eight months. We use eight months since it is the median estimated repayment period in Table 1.

<sup>30</sup>Business characteristics: Fixed effects for industry, business type, owner citizenship, location classification, and province. Advance characteristics: linear controls for principal, charge rate, and factor rate.

<sup>31</sup>The sample in this analysis is slightly larger than in Section 3.1 because we use all advances for which we can observe eight months post-advance—rather than 12 months post-advance—as of May 2024.

<sup>32</sup>Note that this measure predicts default, but not conditional revenue. This is consistent with ex-ante volatility predicting ex-post volatility (i.e., “stability” or the frequency of negative shocks). Ex-post volatility would make a business more likely to default, but conditional on staying alive, not predict performance.

### 3.3 Gap Estimation

To understand the importance of the forces in our conceptual framework, we next estimate the Gap in revenue between advance takers and observably similar non-takers. In our baseline analysis, we match each first-time advance-taking business to their nearest “control” non-taker.<sup>33</sup> 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).<sup>34</sup> 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 revenue of takers and the matched control. The two appear similar before the advance but then begin to diverge after the advance, consistent with the existence of adverse selection and moral hazard.<sup>35</sup> By month eight, the average revenue of the takers is 16.3% lower than the matched control. In the Appendix, we conduct additional robustness tests to confirm our interpretation. Appendix C shows that panel regression and machine learning approaches to estimation provide similar results to our matching approach. Appendix Figure D.2 shows that the Gap exists among businesses with revenue more than five times larger than the eligibility threshold, suggesting the Gap is not driven by businesses manipulating revenue to become eligible.

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 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% more likely to disappear. Interestingly, 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.

The existence of a substantial Gap provides evidence that our framework’s frictions, arising from revenue hiding and adverse selection, are important in this setting. Given this, we next use both observational evidence and natural experiments to explore the extent to which FinTechs’ non-lending interactions affect each of the two frictions.

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<sup>33</sup>We show results with  $K = 1$ , but these results are very similar when averaging across larger sets of  $K$  matched neighbors.

<sup>34</sup>Appendix Table E.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.

<sup>35</sup>Here we use individual monthly outcomes, analogous to letting  $Y(1)$  be a vector of post-period outcomes.

## 4 FinTech Platforms & Ex-Post Revenue Hiding

Do advance takers reduce the cost of financing by “hiding” revenue using alternative processors or cash? If so, when will a FinTech be better or worse at mitigating this friction? In this section, we provide evidence of hiding and show that it increases when borrowers can more cheaply substitute to other payment processors. We also show that the usage of certain add-on platform features predicts repayment. Our results suggest that non-lending features can mitigate financial frictions when they increase the costs of hiding transactions.

### 4.1 Hiding & Non-Lending Product Competition

We begin by exploring whether advance performance responds to competition in the Platform’s primary product, payment processing. Unlike its competitors, the Platform charges no fixed or “daily settlement” fees for processing—only a per transaction fee—which makes their product relatively cheaper for smaller transactions. This, combined with the fact that moving the largest transactions off the Platform is the most effective way (per transaction) to extend advance duration, suggests large transactions could be shifted to other processors. Appendix Figure D.3 shows that, indeed, large transactions more sharply decline after an advance.<sup>36</sup> 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.

To overcome potential confounders and explore the possibility of non-lending features having causal effects on financing, we use a natural experiment involving a primary rival (“the Competitor”) of the Platform. The Competitor offers similar payment processing products. Whereas the Platform we study operates out of Cape Town, the Competitor started in Durban and primarily operated in the surrounding region before expanding recently. 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.”<sup>37</sup> Accordingly, they cut the price of their flagship product by more than 50% over the next six months, as shown in Appendix Figure D.4.

As the Competitor reduced its prices, it became cheaper for the Platform’s advance takers in areas around Durban (where the Competitor operated) to substitute transactions to another processor.<sup>38</sup> We exploit this shock to explore the effects of non-lending product

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<sup>36</sup>This result is unchanged when residualizing on business revenues, suggesting that within-business variation, rather than across-business variation, drives our result.

<sup>37</sup>See the archived press release [here](#).

<sup>38</sup>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.

substitutability. 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 their counterparts around Cape Town. To test this empirically, we employ a difference-in-differences regression framework with the following specification:

$$Y_{i,t} = \alpha_1 D_i + \sum_{t \neq 3} \beta_t (D_i \times \text{Quarter}_t) + \mathbf{X}_i' \boldsymbol{\alpha}_3 + \text{Quarter}_t + \epsilon_{i,t}. \quad (9)$$

For business  $i$  who took a first advance in quarter  $t$ ,  $Y_{it}$  is the ratio of the average monthly number of transactions in the eight months following the advance over three months before the advance.  $D_i$  is an indicator for whether borrower  $i$  is in the Kwazulu-Natal and Eastern Cape provinces around Durban or the Western and Northern Cape provinces around Cape Town.  $\mathbf{X}_i$  are controls for pre-advance transaction amount and industry.

Our identification comes from differential changes in takers’ post-advance revenue *relative* to their own pre-advance revenue. This design allows us to separate advance-related hiding from switches to the Competitor independent of the advance. To see this, consider a business that took an advance in February of 2022. Because the competitor’s price change happened in mid- to late-2021, if the business’s response was entirely driven by a response to pricing (independent of an advance) there should be no effect on their pre- vs post-advance revenue.

Figure 4 shows that  $\beta_t$  decreases after the price cut, consistent with revenue hiding caused by the Competitor’s price drop. Borrowers exposed to the price drop experienced a 10–15 percentage point decrease in transactions on the Platform.<sup>39</sup>  $\beta_t$  increases as the Competitor expands throughout South Africa in 2022. In line with the mechanism suggested in Appendix Figure D.3, Appendix Figure D.6 shows that the same difference-in-differences specification appears somewhat stronger for large transactions. Our results suggest that advance takers hide revenue, at least in part, by moving transactions to rival processors. Furthermore, the substitutability of payment processing affects the importance of this financial friction.

## 4.2 Hiding & “Sticky” Features

In addition to price, the cost of using an alternative processor is determined by any other factor that limits substitutability. “Sticky” features (e.g., add-ons, a higher-quality machine, better customer support) that provide value to businesses *outside* the advance, then, have the potential to reduce moral hazard. Consider, for example, a business that uses a processor’s accounting platform which is automatically tied to transactions. Shifting transactions would

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<sup>39</sup>Appendix Figure D.5 shows the corresponding raw averages of  $R_{it}$ . The counterfactual mean is close to one in the post period, which implies that a 10 percentage point decrease is approximately a 10% decrease in transactions on the Platform. Our estimate of  $\alpha_1$  is 0.043.

disrupt their accounting, as only a fraction of their transactions would be recorded. Moving transactions entirely off the platform would require an entirely new accounting system.

We test this intuition by focusing on two interactions with the platform: (1) opening a “manage” tab on the platform app that allows businesses to track their inventory, customers, and staff; and (2) clicking a button to export sales history to a CSV file. The first feature proxies for the extent to which a business values the platform’s non-processing features. Regressing usage of this feature on advance performance may pick up the effects of “sticky” features, but may also pick up a confounding relationship between feature usage and business quality. To separate the impact of the former, we use the second feature, exporting a CSV, which is commonly used by businesses with third-party management tools. Accordingly, businesses will have a *higher* cost of shifting when they use the manage feature to track their business, and a *lower* cost of shifting if they export sales and do this tracking externally.

We add usage of these features to Equation 8 in Section 3.2 to test their relationship with advance performance. Columns (1) and (2) in Table 3 show that among takers, the “manage” measure is negatively predictive of default and positively predictive of conditional revenue.<sup>40</sup> In particular, pre-advance usage of the feature predicts a 2.1 percentage point decrease in default probability and a 5.8% increase in transaction amounts conditional on no default. It is possible, however, that those who use the feature are better performing businesses in general. We use the second measure, exporting sales, to address this concern. If businesses that use external accounting are better performers, exporting sales should *positively* predict performance. But if this feature decreases the cost of shifting, exporting sales should *negatively* predict performance for takers compared to non-takers. To test this, in Columns (3) and (4) we include non-takers and interact usage of the export feature with being a taker.<sup>41</sup> The interaction on default is significantly positive, which predicts a *negative* impact on performance, consistent with decreased shifting costs rather than selection.

In sum, our results in this section suggest FinTech platforms can mitigate ex-post financial frictions with non-lending features that are not substitutable. These features, like payment processing, increase the cost to small businesses of hiding revenue from repayment. Proposition 1 highlights that these costs help FinTechs sustain revenue-based financing. Next, we turn to the Proposition 1’s second prediction on ex-ante information frictions.

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<sup>40</sup>The manage and export features we analyze were introduced in February 2022 and June 2021, respectively, reducing the sample sizes for these analyses.

<sup>41</sup>To include non-takers, we use eight months of outcomes starting one year after they joined the platform (as if they counterfactually took an advance at this time). One year is approximately the median time on the platform before an advance for takers. Our results do not significantly change when using other counterfactual times on the platform.

## 5 FinTech Platforms & Ex-Ante Adverse Selection

Even without the moral hazard discussed in the prior section, Proposition 1 illustrates how adverse selection can lead revenue-based financing contracts to unravel. This section explores factors that shape FinTech platforms’ ability to mitigate this challenge. We first revisit relevant results from previous sections before presenting quasi-experimental evidence that a longer history of non-lending interactions can improve screening. We also provide evidence that repeated financing interactions can help alleviate adverse selection.

### 5.1 Selection & Non-Lending Interactions: Observational Evidence

Two prior pieces of evidence support the notion that non-lending interactions improve screening and help mitigate adverse selection. First, Table 2 shows that three measures from non-lending interactions—time on platform, pre-advance transaction volume, and volatility—predict default. Columns (1) and (2) include only these predictors, while Columns (3) and (4) add many controls for business characteristics, advance characteristics, and quarter-by-year fixed effects. The large number of additional controls increases the adjusted  $R^2$  by no more than 50%, consistent with measures linked to digital payments and platform usage having meaningful predictive power.

Second, Section 4.2 shows that businesses’ ex-ante usage of certain “sticky” non-lending features (e.g., accounting service add-ons) predicts future business performance. This result suggests that screening on specific FinTech platform feature usage can reduce selection. Importantly, this is true regardless of whether usage picks up business quality or propensity to revenue-hide. This can be seen as lowering “adverse selection on moral hazard”, in which the financier directly screens out businesses with a lower cost of shifting.

### 5.2 Selection & Non-Lending Interactions: Causal Evidence

We use a temporary system error that caused a delay in advance offers to show that a longer history of transactions allows the Platform to better predict repayment. This provides causal evidence that additional information from non-lending interactions can improve screening.

One possible interpretation of our results in Section 5.1 is that platforms benefit from observing a longer history of payments before offering financing, as it reveals information about business quality and reduces adverse selection. Appendix Figure D.7 supports this, showing that time on platform not only predicts future revenue unconditionally, but also reduces the Gap between takers and non-takers. However, another interpretation of these results is that the observational differences may be due to “bad types” demanding financing

as soon as they are eligible. Such selection would exist regardless of the informational value of non-lending interactions.

To test this, we use a natural experiment that delayed advance offers to businesses on the Platform. In particular, 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.<sup>42</sup> Figure 5 shows the variation introduced by the initial offer change.<sup>43</sup> The top panel shows 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 are no early takers after the change. But the second panel shows that the number of advances made between months six and twelve increased, suggesting demand was pushed to later months. The final panel shows this increase in demand did not fully catch up by month twelve: the share of takers drops from 16% to 11%.

We next ask “do the delayed offers improve screening?” We test this using advances made within 12 months of joining and the regression:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \text{AfterCut}_i + \mathbf{X}'_i \boldsymbol{\beta}_3 + \text{Month}_t + \epsilon_{i,t}. \quad (10)$$

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  $\beta_2$  on  $\text{AfterCut}_i$  which is an indicator for whether the business joined the platform after the March 20, 2022 cutoff date.<sup>44</sup> Columns (1) and (2) of Table 4 shows results for businesses that joined between September 2020 and August 2022, controlling for seasonality with  $\text{Month}_t$  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 that this result is driven by 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.<sup>45</sup>

<sup>42</sup>A small number of new very high-revenue businesses continued to receive advance offers throughout this period. We exclude these businesses from these analyses.

<sup>43</sup>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.

<sup>44</sup>Our baseline analysis is in levels, as it allows us to combine both extensive (default) and intensive margin effects. In Appendix Table E.4, 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.

<sup>45</sup>To understand the differences between takers before and after the cutoff that drive this result, in Appendix Table E.3 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

To further ensure that our results are not driven by confounding time trends (e.g., from changing economic conditions), Appendix Table E.5 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. This highlights how FinTech platforms can mitigate ex-ante frictions with data from non-lending interactions.

### 5.3 Selection & Repeat Financing

A long literature underscores the importance of repeated interactions and firm-bank relationships in lending, especially for small businesses (e.g., Petersen and Rajan, 1994; Berlin and Mester, 1999; Berger *et al.*, 2005). A potential implication is that FinTechs could limit adverse selection by following a “learning by lending” approach in which they start small and offer more credit over time (e.g., Botsch and Vanasco, 2019; Fuchs *et al.*, 2022). Indeed, Table 1 shows that repeat advances have higher average returns than first-time advances, as discussed in Section 3. However, since repeat borrowers have usually been on the platform for longer, it is also possible that FinTechs learn nothing additional from repeat advance performance, beyond a longer history of transactions.

We test this possibility by using a sample of first-time and repeat advances and modifying Equation 8 to include an indicator for initial advance. Column (5) of Table 2 shows that even conditional on observables, first plans are 4.3 percentage points more likely to default than repeat advances. In Figure 6 we regress default on weeks on platform separately for first, second, and third or later advances. The downward slopes across all three groups show default decreases steeply with time on platform; however, the level of default decreases monotonically with plan number suggesting repeat advances provide additional information beyond time on the platform.

## 6 Effects of Revenue-Based Financing on Businesses

In this section, we use estimates of the Gap—the ex-post difference in revenue between takers and observably similar non-takers—and the natural experiments described in Sections 4.1 and 5.2 to provide suggestive evidence of the effects of revenue-based financing. Our results are consistent with the causal effect on takers being positive. This highlights the importance of FinTech lenders’ ability to mitigate financial frictions, potentially enabling broader, more

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different in terms of location, owner demographics, and industry.

affordable, and flexible credit access.

Our procedure for estimating the causal effect in Equation 5 involves four steps, following the framework provided by Section 2.3. First, we describe a hypothetical experiment that identifies the combined size of moral hazard and the causal effect. Proposition 2 implies subtracting this quantity from an estimate of the Gap identifies adverse selection. Second, we discuss the identifying assumptions necessary to use our natural experiment in Section 5.2 instead of the hypothetical experiment. Third, we produce a short-run (three months post-advance) estimate of adverse selection under these assumptions. Fourth, we use back-of-the-envelope estimates on the overall level of revenue hiding—informed by our natural experiment in Section 4.1—to fully decompose the short-run Gap into its three components.

### **Ideal Experiment**

Consider an experiment in which one randomly assigns eligible businesses into two groups. If neither group receives advance offers, their expected revenues will be the same. Instead, suppose one group receives offers and the other does not. While the observed revenue of non-takers in each group will not change, the observed revenue of takers in the offered group will change due to revenue hiding and the causal effect of financing on true revenue. Adverse selection shows up “equally” across the two groups as they are randomly assigned. Proposition 3 formalizes this intuition (proof in Appendix A).

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

If the difference in reported revenue between the two groups, the probability of uptake, and the Gap is observed, adverse selection is identified from the Gap minus  $(MH - CE)$ .

### **Natural Experiment**

Following the intuition of the ideal experiment, we use variation from the natural experiment described in Section 5.2—in which eligible businesses were given offers six months after joining the Platform instead of three months—to identify adverse selection in the three months post-advance. To map the natural experiment to the ideal one, observe that 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. Thus, comparing the revenues of the groups can provide an estimate of moral hazard and causal effects, as Proposition 3 describes.

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 rate of uptake and expected revenue would be the same as observably similar businesses who joined before March 20. To capture only businesses that would have been eligible to receive an advance if not for the cutoff, we only include those who met the minimum transaction eligibility criteria. Appendix Table E.6 shows that the ex-ante characteristics of such businesses on either side of the cutoff are generally well-balanced, providing support for this assumption. However, the identifying assumption would also be violated by seasonality in revenue. To address this, we again use two full years of data—with businesses who joined from September 2020 to August 2022—and always include controls for month-of-year fixed effects. Since businesses after the cutoff may have taken advances after month six, we look only at “short-run” three-month outcomes.<sup>46</sup>

### 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 (or three-month) Gap.

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

$$Y_{i,t} = \alpha_1 Y_{i,t-1} + \alpha_2 \text{Offered}_i + \mathbf{X}'_i \alpha_3 + \text{Month}_t + \epsilon_{i,t}. \quad (11)$$

$Y_{i,t}$  is observed three-month revenue,  $Y_{i,t-1}$  is revenue lagged by one quarter,  $\mathbf{X}_i$  are industry fixed effects, and  $\text{Month}_t$  are month fixed effects. The coefficient of interest is  $\alpha_2$  on the indicator  $\text{Offered}_i$  since  $MH - CE = -\alpha_2 / \mathbb{P}(\text{Taker})$ . Column (3) of Table 5 shows that with industry fixed effects  $\alpha_2$  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, 11.1%, gives  $ME - CE \approx 2,600 \text{ ZAR}$ .<sup>47,48</sup>

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<sup>46</sup>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.

<sup>47</sup>Alternatively, we can make in-sample predictions using the logistic regression  $\text{Taker}_i = \text{Month}_t + \sigma_0 Y_{i,t-1} + \sigma_1 X_i$ . Then the average of  $\alpha_2$  divided by  $\text{Taker}_i$  yields an estimate for  $MH - CE$ . This method gives nearly identical results, consistent with the observables having little predictive power on whether an individual is a taker.

<sup>48</sup>Our exercise can alternatively be viewed as instrumenting for whether a business is an early taker with the side of the cutoff they joined on. Columns (1) and (3) of Table 5 can be seen, then, as reduced form regressions of the instrument on the outcome. Because there are no “always takers”, scaling by the share of takers can be seen as recovering the IV estimates (the two will may differ slightly due to controls in the

To estimate adverse selection, we subtract the short-run Gap from  $ME - CE$ . The short-run Gap conditional on observables is identified with:<sup>49</sup>

$$Y_{i,t} = \gamma_1 Y_{i,t-1} + \gamma_2 \text{Taker}_i + \mathbf{X}'_i \gamma_3 + \text{Month}_t + \epsilon_{i,t}. \quad (12)$$

Variables are defined similarly to Equation 11. Table 5, Columns (2) and (4) show  $\gamma_2$  (short-run Gap) estimates of around 6,400 ZAR. Subtracting  $ME - CE$  implies adverse selection of around 3,800 ZAR, roughly the size of 60% of the Gap.

### Estimating the Full Decomposition

Our ability to separate moral hazard from the causal effect is limited by the fact that the Platform cannot directly observe revenue hiding. However, Section 4.1 shows that in response to increased competition transactions fell by 10–15%. We use this range as a back-of-the-envelope estimate of  $1 - v$ .<sup>50</sup> Assuming an identical  $v$  for all businesses,  $MH|(X = x) = (1 - v)\mathbb{E}[Y(1)|\text{Taker}, X = x]$ . Then,  $CE|(X = x)$  is the Gap less adverse selection and moral hazard.

Figure 7 illustrates what different assumptions about the overall level of revenue hiding  $v$  imply about the magnitude of the causal effect. For any  $v < 0.976$ , the causal effect of the advance for takers is positive. In scenarios in which businesses hide a greater share of revenue, the implied size of the causal effect increases. For example, if advance takers hide 10% of their transactions, the short-term causal effect is a revenue increase of around 7,800 ZAR, an 8% increase in quarterly revenue relative to pre-advance for the average taker.

### Additional Evidence of Positive Effects

To further explore effects on businesses beyond the first three months, we use transaction-level location data (from a business’s payment processor devices) and ask whether capital takers are more likely to expand their geographic footprint than non-takers. Using the same matched control as in Figure 2, Figure 8 shows that takers with a small geographic

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1st stage). Appendix Table E.7 shows IV estimates. The estimate in column (2), which corresponds to the reduced form in column (3) in Table 5, is very similar to our baseline estimate of  $MH - CE$ .

<sup>49</sup>This Gap differs from the Gap estimated in Section 3.3 and Appendix C because we only include businesses that (1) were eligible for an advance at the end of their third month on the platform, (2) took advances in their fourth through sixth month on platform, and (3) took an advance in the 18 months before the March 20, 2022 cutoff.

<sup>50</sup>Lower estimates of hiding result in smaller estimates of the causal effect. Our use of the hiding estimates from Section 4.1 is 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. It is also conservative in that it assumes transactions of all sizes are equally likely to be hidden, whereas Appendix Figure D.6 provides evidence that larger transactions are more likely to be hidden.

footprint before an advance were more likely to expand their footprint over the following eight months than similar non-taker control firms. If FinTech-provided revenue-based financing has positive effects in our setting, this could come through increased credit access in general and/or businesses valuing the flexibility and risk-sharing offered by revenue-based financing in particular (see, e.g., Battaglia *et al.*, 2023, and Appendix B). Either explanation would be consistent with businesses using capital to invest and expand. These results highlight the potential benefits of FinTech platforms being able to mitigate financial frictions.

## 7 Revealed Preference Measure of Hiding Costs

In this last section, we discuss what our estimates imply about the cost of hiding transactions, a key force for sustaining revenue-based financing (Proposition 1) and a revealed preference measure of how valuable businesses find the Platform. In prior sections, we presented evidence that businesses hide transactions to slow repayment and respond to incentives to do so; however, Figure 3 shows little intentional extensive margin default, suggesting businesses place high value, at least implicitly, on the Platform. We now use our previous estimates to provide back-of-the-envelope guidance on the cost of shifting.

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 costliness 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 let small businesses have an annual discount rate of  $r = 30\%$  and constant daily revenues.<sup>51</sup> We take the characteristics of the average advance.<sup>52</sup> Appendix Figure D.10 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

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<sup>51</sup>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 7).

<sup>52</sup>These are: factor rate of 1.3, charge rate of 20%, and principal of 37,830 (Table 1).

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.<sup>53</sup> 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.

This cost of shifting, 45,000 ZAR, is relatively large, slightly higher than average monthly revenue of advance takers. Since the cost of switching to a competitor processor after the price drop described in Section 4.1 is 2,000 ZAR, this suggests customers may find different platforms to be imperfect substitutes (e.g., because of machine quality, customer support, switching costs, or “sticky” add-on features as discussed in Section 4.2). Other factors, such as fear of being “caught” or moral considerations, may also play an important role.

## 8 Conclusion

FinTech-provided revenue-based financing has become increasingly common in both the US and globally. Our paper provides evidence that FinTech platforms’ non-lending interactions help mitigate both ex-ante (information) and ex-post (enforcement and monitoring) financing frictions in small business lending, helping to explain this rise.

We use data on over 100 million transactions from a South African FinTech platform that processes payments and offers revenue-based financing. We find that payments through the processor are 16% lower for businesses who take financing than observably similar non-takers (the “Gap”). The comparatively lower revenue of the takers is consistent with the existence of ex-post moral hazard in revenue-hiding and ex-ante adverse selection.

To explore if and how FinTech platforms mitigate these frictions, we use two natural experiments. First, a price shock to a competing payment processor changed the substitutability of the platform’s core payment-processing product. While repayment is tied to the usage of the product, the increase in its substitutability reduced the costs of giving up that usage. Consistent with this, for takers who were exposed to the price shock, post- vs pre-advance transactions fell by an additional 10-15% relative to a control group. Second, a temporary delay in advance offers led the platform to have six months, rather than three

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<sup>53</sup>In Figure D.10 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  $c(100) - c(10) \ll g(100) - g(10)$ .

months, of non-lending information to screen. After this change, total revenue in the eight months post-advance for takers was 5% higher than before the change.

Our results add evidence to a recent literature studying the rise of FinTech lenders. The finding that non-lending interactions add value for screening supports a handful of works highlighting FinTechs' potential informational advantages ex-ante. However, we also show that by tying repayment to the continued usage of their products, FinTechs are well-suited to mitigate enforcement and monitoring frictions ex-post. Our framework and results suggest that these effects on both ex-ante *and* ex-post frictions are important for explaining the rise of FinTech-provided revenue-based financing.

Mitigating financial frictions may allow FinTech lenders to expand credit access or sustain revenue-sharing contracts that entrepreneurs value. Both possibilities would aid small firms and support economic growth, especially in developing economies (see e.g., Woodruff, 2018). We provide suggestive evidence of such positive effects on small businesses in our setting by quantitatively decomposing the Gap into moral hazard from revenue hiding, adverse selection, and the causal effect on takers. We also show that takers with a small geographic footprint were more likely to expand their footprint after an advance. Providing further evidence on the effect of FinTech lenders in different contexts, and the mechanisms through which these effects operate, remains an important area for future work.

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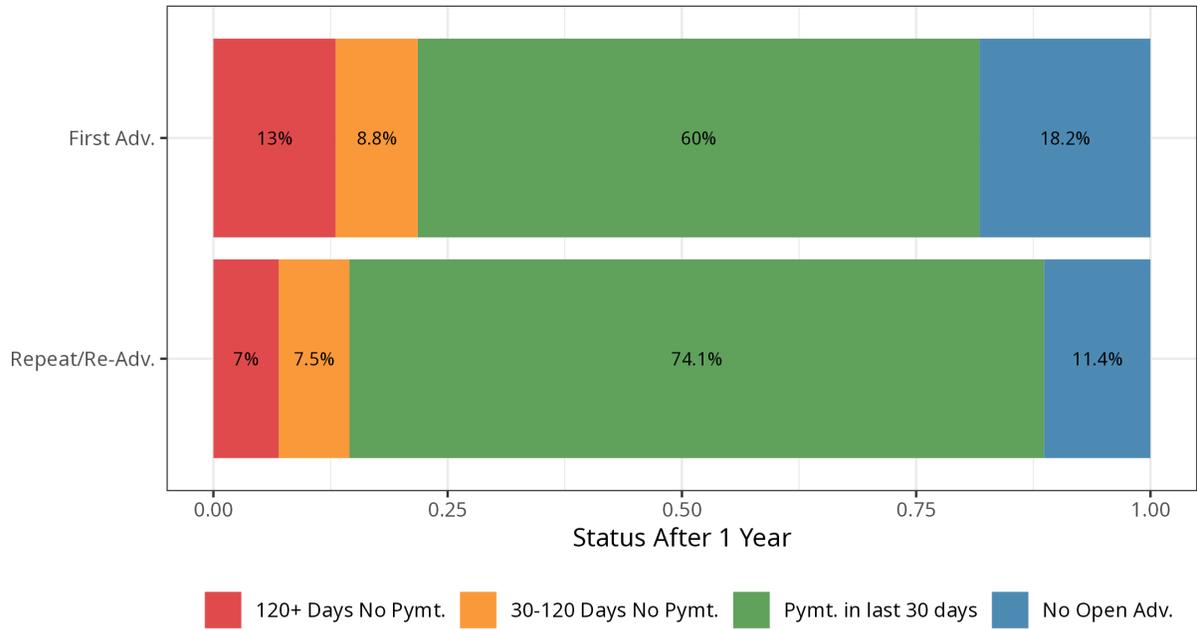
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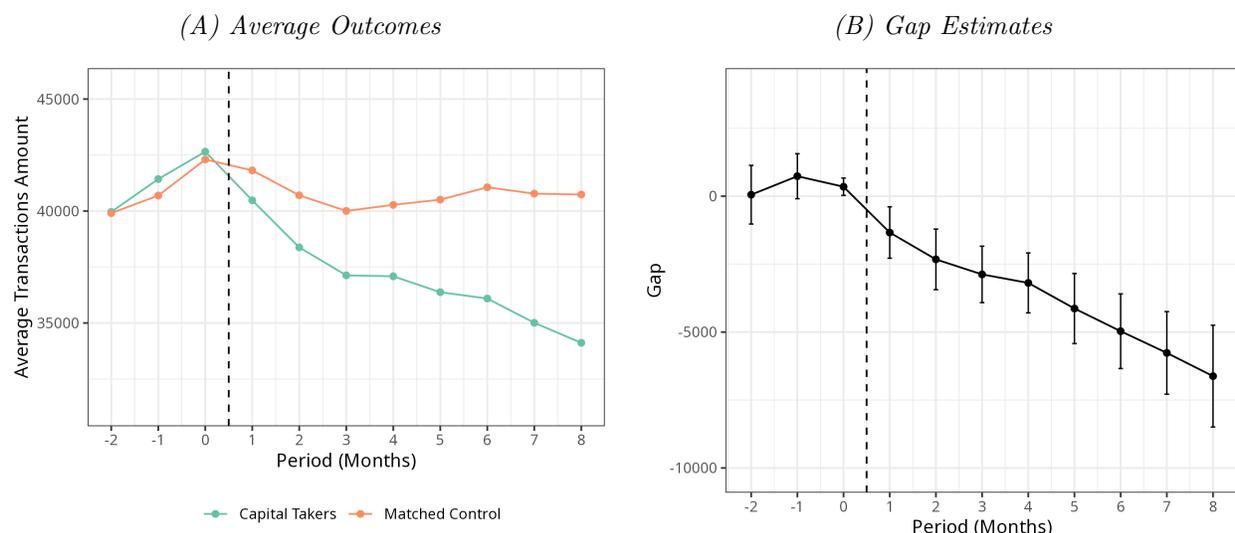
# Figures

**Figure 1:** *Advance Taker One-Year Outcomes*



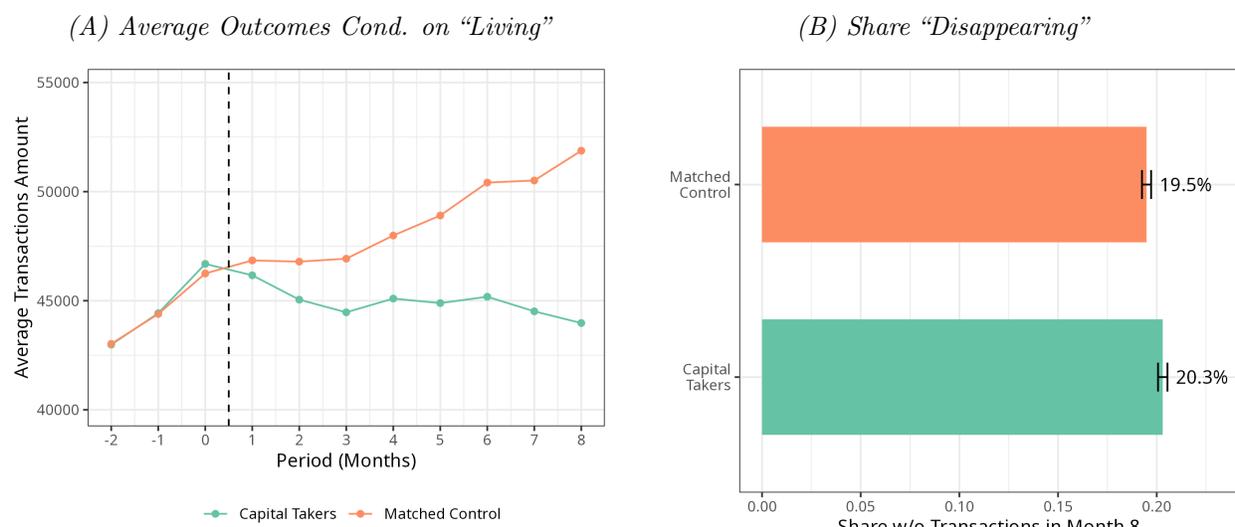
**Note:** Figure shows outcomes for first-time and repeat advance takers, respectively, one year after taking an advance. Outcomes shown are for *any* advance the business has one year later.

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



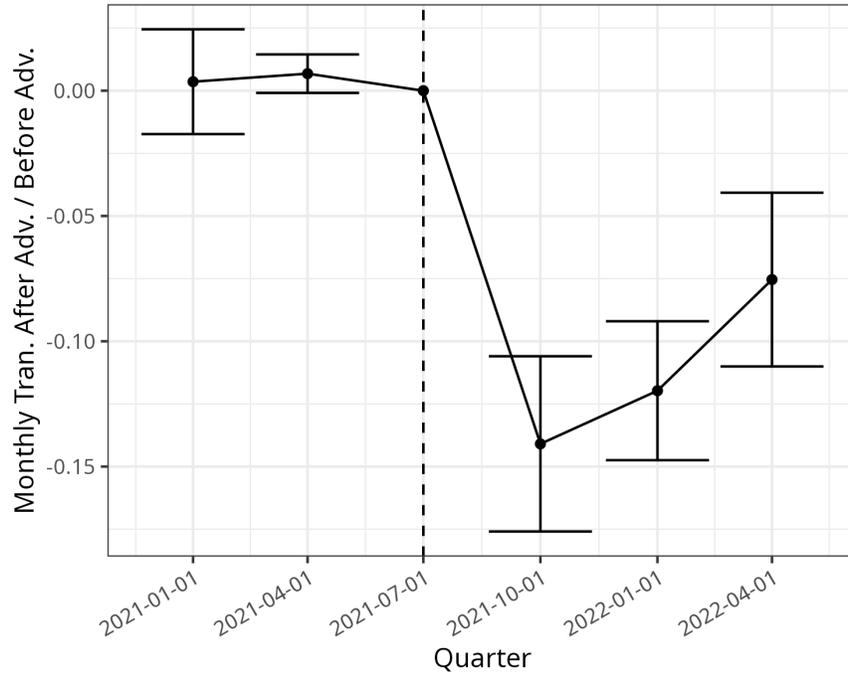
**Note:** Figure shows the average monthly transactions of capital takers and matched control businesses. Advances were taken in month 0. Each taker is matched to a control business in the same month and industry with the smallest Euclidean distance according to (normalized) time on platform and transaction 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”*



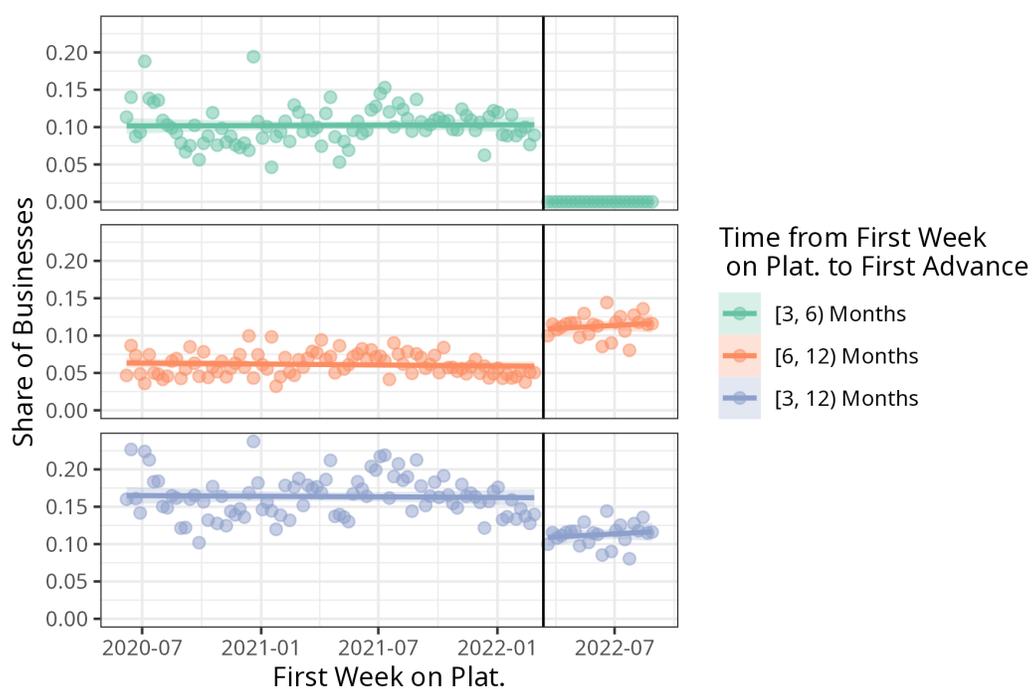
**Note:** Figure shows the intensive and extensive margin contributions to Figure 2. Panel A shows the average monthly transactions of capital takers and matched control firms as in Figure 2, but selected only from pairs in which both are transacting on the platform in the eighth month after the advance. 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 When Rival Lowers Price*



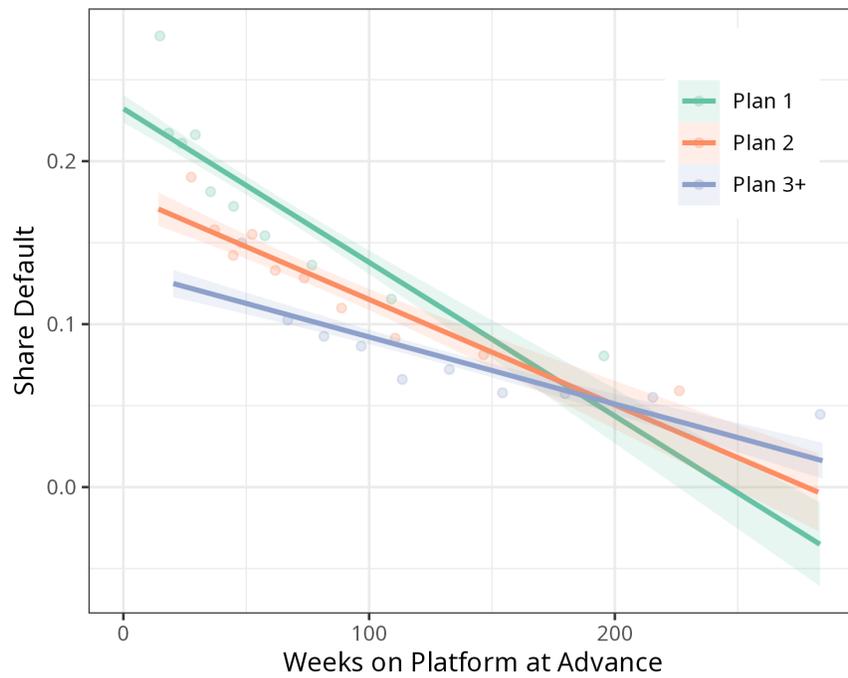
**Note:** Figure shows estimates of  $\beta_t$  from the difference-in-differences specification in Equation 9. The quarter of the Competitor's price drop is July 2021 (see Figure D.4). Bars display 95% confidence intervals.

**Figure 5:** *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 5.2 is excluded.

**Figure 6:** Share “Disappearing” by Advance Number and Time on Platform at Advance



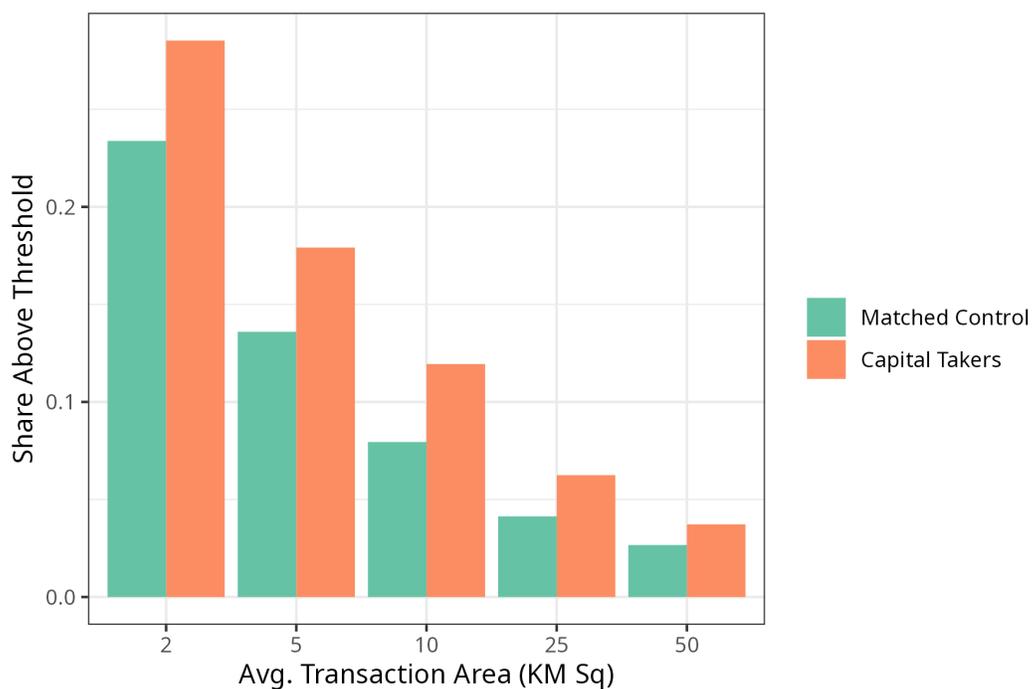
**Note:** Figure shows the share of businesses that default against the number of weeks the business had been on the platform, split by advance number. Default is whether the business has an open advance and no transactions 8 months after the start of the advance. Weeks on platform is calculated from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

**Figure 7: 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 6. As defined in Section 2,  $v$  is the share of revenue the financier can observe. Each scenario applies a given  $v$  to all advance takers.

**Figure 8:** *Business Expansion for Small Capital Takers vs Non-Takers*



**Note:** Figure shows businesses with small transaction-level location footprints in the pre-period by the share that exceed average monthly post-period thresholds. Transaction locations come from the payment processing devices a business uses. The sample is the capital takers and matched control businesses in Figure 2, but selected only from pairs in which both businesses have average monthly transaction locations within a  $1\text{km}^2$  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 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	Log Total Amt. 8 Months
Years on Platform	-0.044*** (0.0056)	0.041*** (0.0077)	-0.035*** (0.0056)	0.036*** (0.0068)	-0.026*** (0.0035)	0.023** (0.0072)
Log Amt. -3 Months	-0.022* (0.0088)	0.92*** (0.022)	-0.017+ (0.0078)	0.87*** (0.053)	-0.015** (0.0037)	0.88*** (0.029)
Relative Sd.	0.068** (0.020)	0.036 (0.036)	0.075*** (0.0100)	0.049 (0.042)	0.070*** (0.0036)	-0.014 (0.058)
First Plan					0.043*** (0.0032)	-0.0091 (0.012)
Sample	First Plans	First Plans, No Default	First Plans	First Plans, No Default	All Plans	All Plans, No Default
Demographic FE	No	No	Yes	Yes	Yes	Yes
Quarter X Year FE	No	No	Yes	Yes	Yes	Yes
Advance Controls	No	No	Yes	Yes	Yes	Yes
Observations	10983	9013	10983	9013	26344	22907
Adjusted $R^2$	0.024	0.66	0.036	0.67	0.048	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. The dependent variable “Default” in columns (1), (3), and (5) is whether the advance taker has an open advance and no transactions in the 8th month post-advance. The dependent variable in columns (2), (4), and (6) is log of total transaction amounts within 8 months post advance, conditional on no default. Years on platform is calculated from the date of first transaction on the platform to the advance being given. “Log Amt. -3 Months” is the log of total transactions three months before the advance was given. Relative standard deviation is the standard deviation of weekly transactions amounts, divided by the mean, in the three months before the advance was given. First plan is an indicator for whether the advance was the first advance taken by the business. Columns (3)–(5) 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:** *Feature Usage and Advance Performance*

	(1)	(2)	(3)	(4)
	Default	Log Total Amt. 8 Months	Default (8mo)	Log Total Amt. 8 Months
Manage Button	-0.021* (0.0090)	0.058* (0.019)		
Exported Sales	0.026+ (0.014)	0.026 (0.032)	-0.027 (0.018)	0.072+ (0.037)
Years on Platform	-0.023*** (0.0022)	0.017+ (0.0091)	-0.032*** (0.0060)	0.039** (0.0085)
Log Amt. -3 Months	-0.017* (0.0060)	0.89*** (0.013)	-0.020* (0.0065)	0.90*** (0.016)
Relative Sd.	0.060*** (0.0087)	-0.055 (0.046)	0.16*** (0.0076)	-0.11+ (0.051)
First Plan	0.034* (0.013)	-0.00095 (0.023)		
Taker			0.0097+ (0.0045)	-0.087*** (0.013)
Taker × Exported Sales			0.065* (0.026)	0.016 (0.061)
Sample	Takers, All Plans	Takers, All Plans	All, First Plans	All, First Plans
Demographic FE	Yes	Yes	Yes	Yes
Quarter X Year FE	Yes	Yes	Yes	Yes
Advance Controls	Yes	Yes	No	No
Observations	7938	7011	15682	12652
Adjusted $R^2$	0.045	0.74	0.066	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. The dependent variable “Default” in column (1) is whether the advance taker has an open advance and no transactions in the 8th month post-advance. In column (2) it is log of total transaction amounts within 8 months post advance, conditional on no default. In columns (3) and (4) we include non-takers, using their outcomes starting one year after they joined the platform, as if they counterfactually took an advance after one year. Default and Log Total Amt. 8 Months in columns (3) and (4) are then defined similarly to (1) and (2), but with a sample that includes non-takers. The measure “manage button” is whether the business opened the manage tab to track staff, customers, and inventory. The measure “exported sales” is whether the business exported its sales history to a CSV. 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*** (0.10)	1.80*** (0.10)	1.89*** (0.18)	1.87*** (0.19)
After Cutoff	10061.11* (4359.27)	13227.54* (4696.06)	10731.81** (2514.95)	13805.67** (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 10. Each observation is a business. The dependent variable is the 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 5.2. Columns (1) and (2) include all businesses who joined the platform between September 2020 and August 2022 and took an advance in 12 months. The sample in columns (3) and (4) further filters 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*** (0.05)	0.87*** (0.07)	0.92*** (0.05)	0.87*** (0.07)
Offered	-293.64 (1385.10)		-289.25 (1373.93)	
Taker		-6438.96+ (3204.19)		-6365.71+ (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, as described in Section 6. Columns (1) and (3) show results from equation 11 over all businesses who joined the platform between September 2020 and August 2022. Columns (2) and (4) show results from equation 12, 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  $\eta$ , 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  $\eta$  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  $c \rightarrow \infty$ ,  $\eta \rightarrow \infty$ ). Consequently,

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

as wanted.

For revenue-based financing to be possible for a given  $c$  and  $p$ , there must be an  $\eta$  smaller than 1 for which the lender makes non-negative profits (as  $\eta > 1$  implies the borrower pays back *more* than their revenue). The results 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  $\eta$  does not exist which allows the lender to break-even. In particular, since  $\eta^*$  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  $\pi_X$  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  $c \leq \frac{2L(1+r)}{p}$ ,  $\pi_X$  has no roots and since  $\pi_X$  is concave in  $\eta$ , this implies that no  $\eta$  with non-negative profits is possible. □

## Proof of Proposition 2

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

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

Thus,

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

as wanted.

□

### 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:

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

where we can remove the conditioning on group because the groups are randomly assigned. The first equation comes from the fact that “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  $y$ .<sup>54</sup> Businesses are risk averse with CARA utility over revenue so that  $E[u(\tilde{y})] = \mu - \gamma\sigma^2$ . Assume  $\mu > L \geq u(y)$  so there is a risk-neutral benefit to investing. However, if the borrower’s risk-aversion  $\gamma$  is high enough, this can prevent some positive NPV investments from being made. 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,  $\eta$ , of their revenue after investment.

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

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<sup>54</sup>We assume that  $\mu \gg 0$  so the probability that  $\tilde{y} < 0$  is negligible.

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  $\bar{y}_d < \bar{y}_r$ ).

#### Proof of Proposition 4

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

$$\mu - \gamma\sigma^2 - L \geq u(y).$$

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

$$(1 - \eta) \cdot \mu - (1 - \eta)^2 \gamma\sigma^2 \geq u(y).$$

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

$$\mu - L - (1 - L/\mu)^2 \gamma\sigma^2 \geq u(y).$$

As  $\eta < 1$ , this implies that the threshold  $y$  for taking revenue-based financing is lower.  $\square$

## C 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 3.3.

### 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:

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

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  $k$ .

Intuitively,  $\beta_0$ , 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  $\beta_1$  through  $\beta_8$  then captures a differential decline in revenue of advance takers, providing an estimate of the Gap. Figure D.8 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**

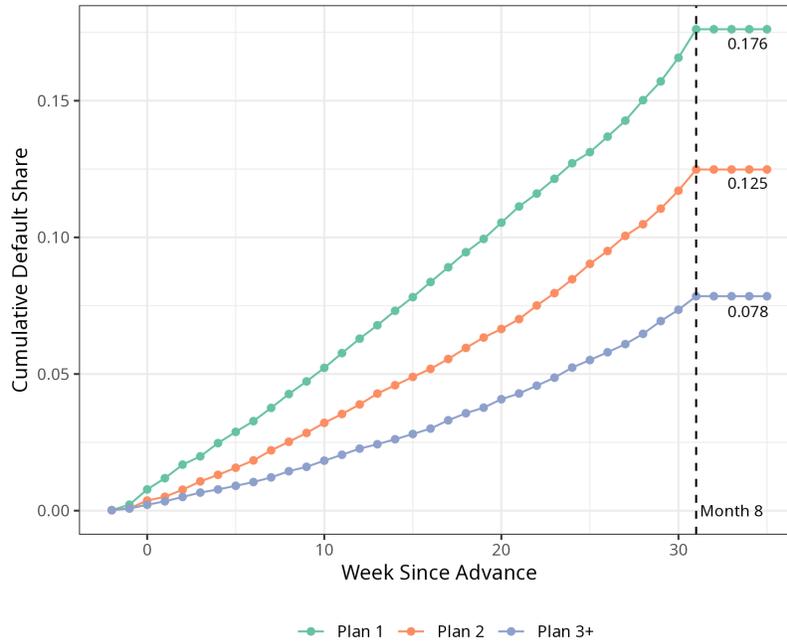
We use a panel of business-by-quarter observations for non-advance-taking businesses combined with observations for each advance taker in the quarter of their advance. We use each business-by-quarter observation to train random forests to predict the revenue of each taker and non-taker in the next eight months.<sup>55</sup> 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 D.9 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.

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<sup>55</sup>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).

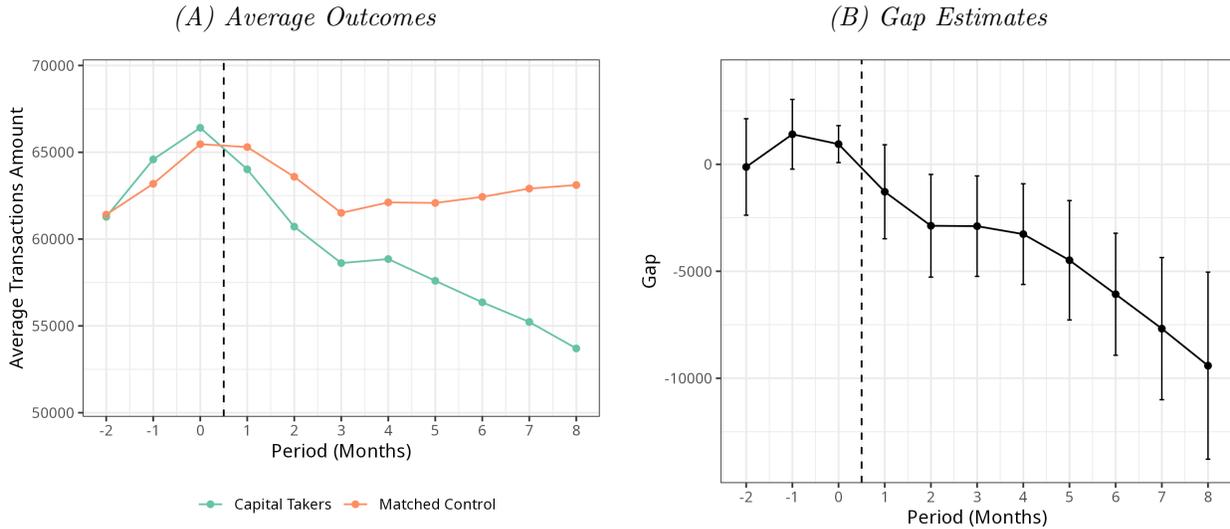
## D Additional Figures

Figure D.1: Hazard Plot for 8 Month Default



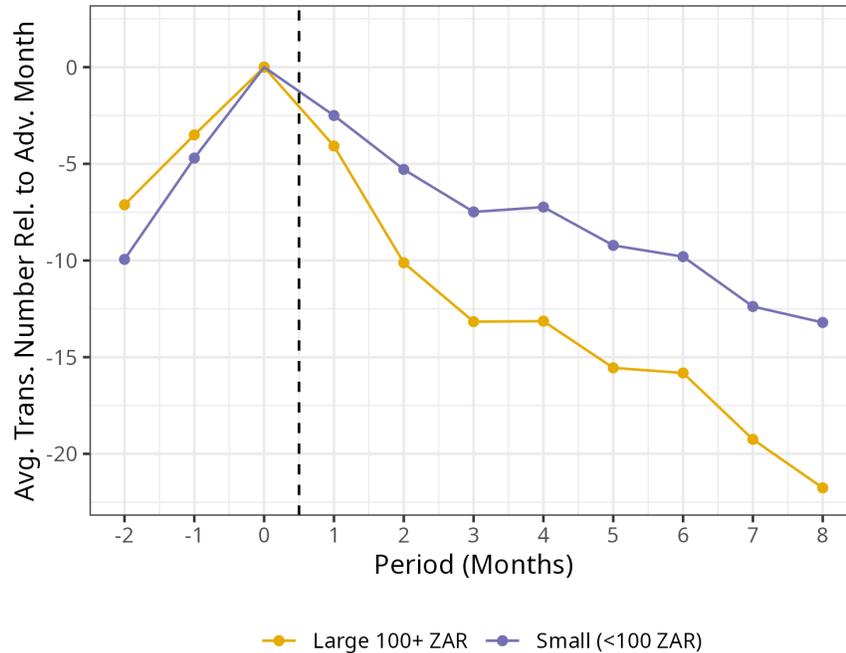
**Note:** Figure shows a hazard plot of default. Default is whether the business associated with the advance has an open advance and no transactions 8 months after the start of the advance. The cumulative default share is the fraction of businesses that had last transacted  $x$  weeks since the advance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

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



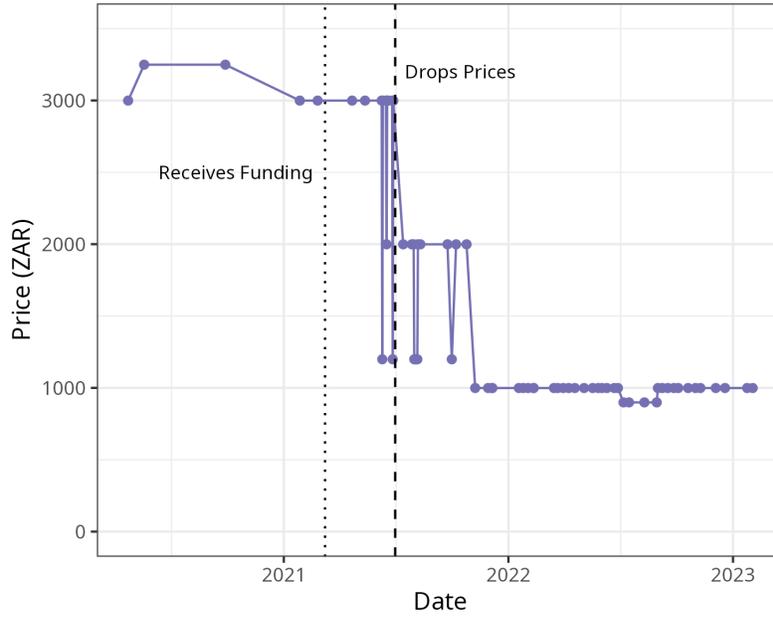
**Note:** Figure shows average monthly transactions of capital takers and matched control businesses, limited to businesses with total revenue in periods -2, -1, and 0 over 50,000 ZAR (5X the minimum eligibility criteria for an advance). The matching was done using the same methodology as in Figure 2.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

**Figure D.3:** *Capital Taker Revenue by Transaction Size*



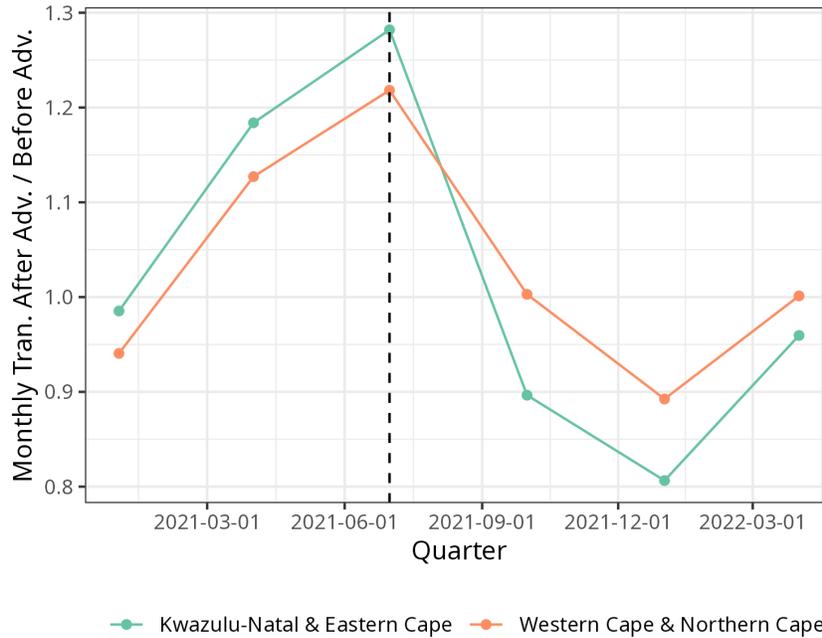
**Note:** Figure shows the average monthly number of transactions of capital takers by transaction size. Advances were taken in month 0. Both series are centered at 0. By purchasing power parity, 100 ZAR is approximately 15 USD.

**Figure D.4:** *Rival Pricing Over Time*



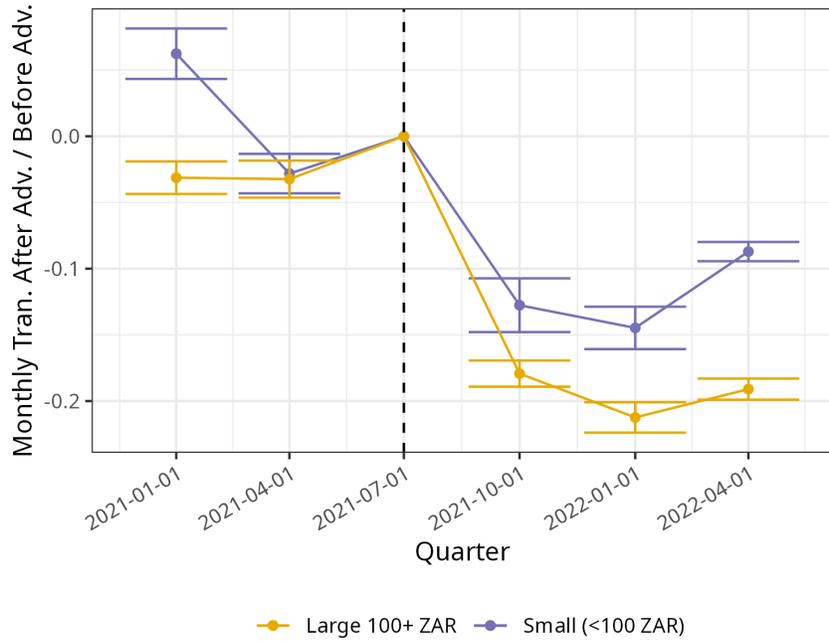
**Note:** Figure shows the up-front price of the Competitor’s flagship product over time. Observations are archived pages from the Internet Archive and the Competitor’s Facebook posts.

**Figure D.5:** *Post-Capital Transactions When Rival Lowers Price: Raw Averages*



**Note:** Figure shows averages of  $Y_{it}$  from Equation 9 by province group.

**Figure D.6:** *Post-Capital Transactions When Rival Lowers Price: By Transaction Size*

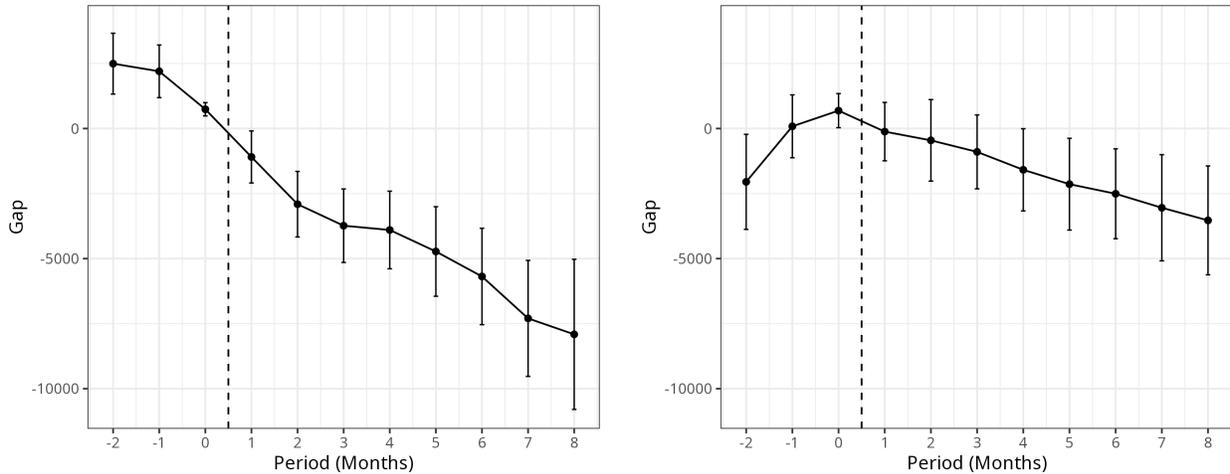


**Note:** Figure shows estimates of  $\beta_t$  from Equation 9 separately for large and small transactions. Bars display 95% confidence intervals. The figure includes only businesses who had more than five transactions of each size in the quarter before taking the advance. The top two percent of outcomes have been winsorized.

**Figure D.7:** *Gap Between Capital Takers and Non-Takers by Time on Platform at Advance*

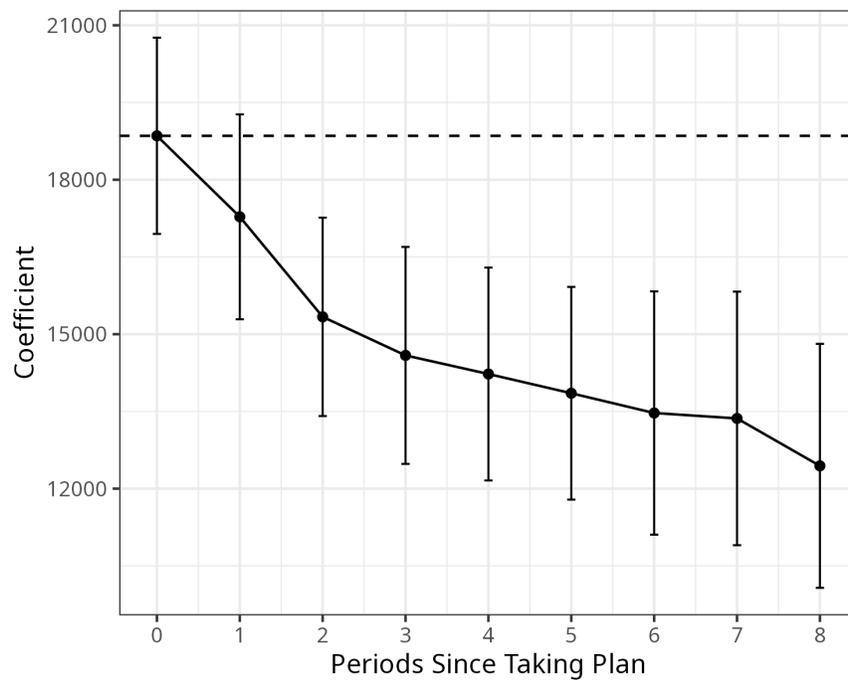
(A) *9 or Fewer Months on Platform*

(B) *More than 9 Months on Platform*



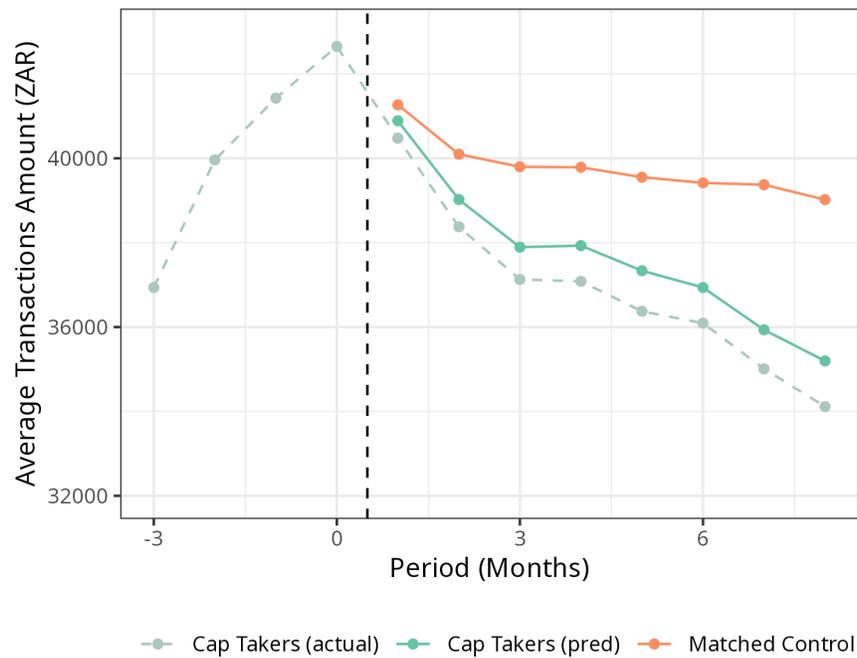
**Note:** Figure shows average monthly transactions of capital takers and matched control businesses split by time-on-platform. Panel (A) only includes businesses that had 9 or fewer months on the platform, panel (B) only includes businesses that had more than 9 months on the platform. The matching was 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 D.8:** *Gap Between Capital Takers and Non-Takers - Panel Regression*



**Note:** Figure shows estimates of  $\beta_0$  through  $\beta_8$  from regression Equation C.1.

**Figure D.9:** *Gap Between Capital Takers and Non-Takers - Random Forest*

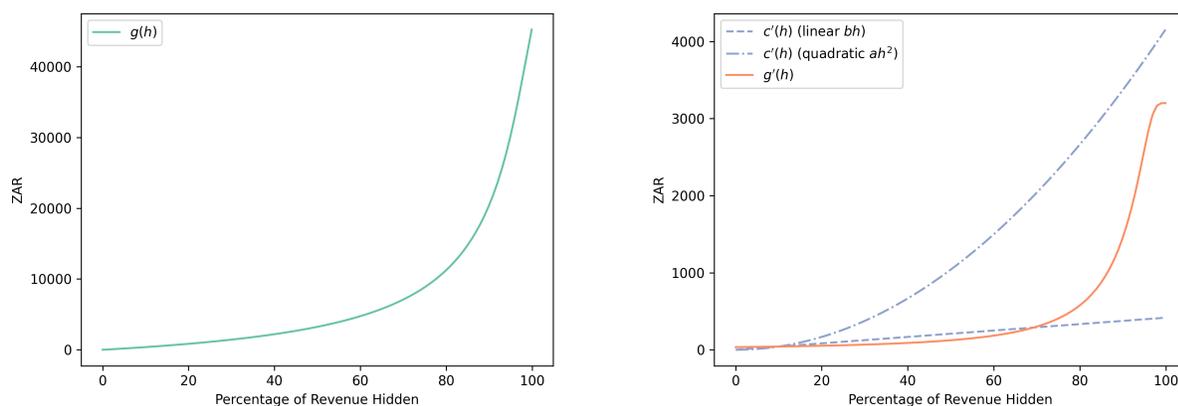


**Note:** Figure shows the average actual and predicted monthly transactions of first time capital takers. Advances were taken in month 0. As described in Appendix C, random forest models were trained to predict the revenue of each taker and non-taker over eight months. The dashed green line shows the 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 D.10:** *Simulation for Valuing the Processor*

(A) *Gain from Hiding*

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



**Note:** Figure shows the gains from hiding, marginal cost curve, and marginal benefit curve for the average advance. We assume that daily revenue is constant, given by  $103,570/90$  as in Figure 7. We assume an annual discount rate of  $r = 30\%$ , which implies a daily discount rate of  $x_d = (1.3)^{\frac{1}{365}} - 1$ . 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 assume a factor rate of 1.3, charge rate of 20%, and principal of 37,830 (Table 1). 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. Linear of the form  $bh$  and quadratic of the form  $ah^2$ , where  $a$  and  $b$  are determined by the fact that both curves start at  $(0,0)$  and must pass through  $(10, g'(10))$ .

## E Additional Tables

**Table E.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 E.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 (ZAR)		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 3.3. 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 E.3:** *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 (ZAR)	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 5.2) and took an advance in 12 months. This is the sample used in columns (3) and (4) of Table 4.

**Table E.4:** *Advance Performance Around Policy Change: Robustness*

	(1)	(2)	(3)	(4)
	Default	Log Total Amt. 8 Months	Default	Log Total Amt. 8 Months
Log Amt. -3 Months	-0.037** (0.008)	0.969*** (0.027)	-0.016+ (0.007)	0.897*** (0.059)
After Cutoff	-0.005 (0.017)	0.031 (0.039)	-0.006 (0.024)	0.047 (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 E.5:** *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 5.2) 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 week after.

**Table E.6:** *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 E.7:** *Decomposition Regressions: IV*

	Dependent Variable: Amt. 3 Months	
	(1)	(2)
Taker	-2316.30 (11120.95)	-2285.95 (11052.30)
Amt. -3 Months	0.92*** (0.05)	0.92*** (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 6. 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  $MH - CE$ .