# Can Instant Payments Fuel Credit Card Expansion?*

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**Abstract**

We study how digital payments affect consumer credit through the information they generate. Using Brazilian credit registry and transaction-level data on Pix, credit and debit cards, we show that Pix users are more likely to receive credit cards, higher limits, and lower interest rates from the bank observing their payments. Pix data improve banks' predictions of future default and expand credit within their customer base. Digital payment expansion is also associated with greater credit-card borrowing. Interchange fees and rewards remain unchanged, ruling out pricing and competition explanations. Finally, we estimate a structural model quantifying information acquisition, payment demand, pricing incentives, and counterfactual market designs.

*Keywords*: Digital payments, credit cards, data sharing, Pix

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# 1 Introduction

Payment technologies are typically viewed as competing ways to execute the same transaction. A new low-cost payment method should therefore take transactions away from incumbent methods and intensify competition among payment providers (Rochet and Tirole (2002)). Yet modern payment systems do more than transfer funds: they also generate information (Parlour et al. (2022); Ghosh et al. (2026)). When payments are conducted through a financial institution, that institution observes a stream of transactions that can reveal information about the customer's income, expenditures, liquidity, and financial behavior. This information may be particularly valuable for credit cards, which combine a payment function with access to unsecured consumer credit and expose issuers to uncertainty about borrower risk. A new payment technology can therefore have an effect that runs in the opposite direction from simple payment substitution: by generating information, it may expand the supply of credit through the card product offered by the financial institution that observes the payments.

The interaction between payments and credit is becoming increasingly important with the rapid adoption of instant payment systems around the world. Systems such as UPI in India, Pix in Brazil, and FedNow in the United States provide low-cost, real-time alternatives to traditional payment methods, changing both how consumers transact and which financial institutions observe their payment activity. We study these effects using the introduction of Pix, Brazil's instant-payment system, in the fourth quarter of 2020. Pix provides a particularly useful setting because it was introduced rapidly and quickly became a widely used alternative to existing retail payment methods. Consumers access Pix through accounts at financial institutions, allowing the institution through which a customer transacts to observe the customer's payment activity. At the same time, Pix competes directly with existing payment instruments, including debit and credit cards. The perceived substitution has also entered policy debate: the Trump administration opened a trade inquiry into Pix, alleging unfair competition with U.S. credit-card op-

erators because it bypasses traditional networks such as Visa and Mastercard.[1] This setting allows us to ask whether a new payment technology simply reallocates transactions across payment methods or whether the information it generates also affects banks' lending decisions.

To study how instant payments affect both payment choices and credit provision, we exploit a unique combination of administrative data that allows us to observe the two sides of this interaction. We combine detailed regulatory data from credit-card issuers and acquirers with the household credit registry linked to individual-level Pix transactions, merchant payment records, and household and merchant surveys. These data allow us to trace how the introduction of Pix changes the use of competing payment methods and, crucially, to identify the bank that observes each consumer's Pix activity and compare its lending decisions with those of other banks. We find that the introduction of Pix is followed by a substantial expansion in card adoption and borrowing. Our evidence points to an information channel: consumers are more likely to obtain a credit card and receive more credit from the bank that observes their Pix activity, while the effects are substantially weaker at other lenders. Pix activity also improves the bank's ability to predict future default, and the expansion in credit occurs without an increase in borrower risk. Using technical disruptions to the Pix network as an instrument, we show that greater Pix activity leads banks to expand credit, further supporting a causal interpretation. At the same time, rewards and interchange fees remain largely unchanged, making increased competition for payments an unlikely explanation for the expansion in credit cards. Finally, we develop a structural model of payment choice and credit provision that rationalizes these findings and allows us to quantify the implications of instant payment systems for payment use, information acquisition, and consumer credit.

We begin our analysis by documenting that after the arrival of Pix, credit-card use expands substantially rather than declines. Survey evidence shows that by 2023, 83% of households use Pix, yet the fraction using credit cards increases from 40.3% in 2019 to

[1]See here.

48%. Debit-card usage, in contrast, falls from 47% to 44%. The administrative issuer data show an even sharper divergence. Following the introduction of Pix, the number of active credit cards rises steadily and is approximately 126% above its pre-Pix level by the end of 2022, while credit-card transaction volume rises by approximately 171%. Debit-card activity exhibits no comparable break. Moreover, spending per active credit card changes relatively little, implying that the expansion occurs primarily through the issuance and adoption of additional cards rather than more intensive spending on existing cards.

Credit cards also increasingly serve their distinctive role as a source of financing (Agarwal et al. (2018); Drechsler et al. (2025)). The amount of credit-card spending that is financed—either rolled over or paid through installments—rises sharply following the introduction of Pix. Financed volume is roughly 152% above its pre-Pix level within two quarters and approximately 569% higher by mid-2022. Installment use increases as well. Thus, Pix does not simply coexist with credit cards as another means of payment. Its introduction is followed by an expansion of the credit component of the credit-card relationship.

A natural explanation is that Pix increases competition for payments, inducing card issuers to make their products more attractive. Our evidence does not support this mechanism. Rewards expenditures per BRL of credit-card spending show no discernible increase around the introduction of Pix, and neither does interchange income. More detailed acquirer data similarly show no broad increase in interchange fees in the segments in which Pix is most relevant. Merchant behavior also works against a simple demand-side explanation: merchants frequently offer discounts for cash and Pix and therefore actively encourage consumers to use alternatives to credit cards. The expansion of credit cards despite these incentives, together with the absence of a meaningful rewards response, suggests that the key effect of Pix operates outside the pricing of the payment service itself.

Another alternative is that banks mechanically cross-sell credit cards to customers

who hold a Pix account, without using the transactions to learn about them. We distinguish account access from transaction information by restricting the sample to consumers who use Pix through multiple banks in the same month. Each bank can cross-sell, but the amount of payment activity it observes differs. Comparing banks within the same consumer-month, a larger share of the consumer's Pix value at a bank is associated with higher credit limits and higher balances at that bank. Thus, the existence of a Pix relationship alone cannot explain the results: the intensity of bank-specific payment information matters for credit provision.

We show that the expansion instead reflects the information generated by Pix transactions. Our empirical design exploits a distinctive feature of the data: for the same consumer and month, we can identify the bank that observes the consumer's Pix activity and compare its credit decisions with those of banks that do not observe that activity. We link individual-bank-month Pix transactions to Brazil's credit registry, which contains credit-card relationships, balances, committed limits, interest rates, and defaults. Individual-by-month fixed effects absorb any change in a consumer's income, credit demand, risk, or general propensity to use digital payments that is common across the consumer's lenders. Identification therefore comes from comparing how different banks treat the same borrower at the same point in time depending on whether the bank observes the borrower's Pix transactions.

The provider-specific pattern is pronounced. At the extensive margin, greater Pix activity at a bank is associated with a substantially higher probability that the same bank issues the customer a credit card. Pix activity conducted through other banks does not generate a comparable increase in card issuance. Conditional on an existing credit-card relationship, greater Pix activity at the card-issuing bank is associated with higher credit limits and balances and lower interest rates, while the corresponding credit-quantity effects are absent at banks that do not observe the customer's Pix activity. Importantly, the additional credit is not accompanied by higher default. If anything, default declines at the informed bank. The combination of greater credit provision and no deterioration

in repayment is difficult to reconcile with indiscriminate credit expansion and instead suggests that payment information allows banks to identify borrowers to whom they can profitably extend more credit.

We provide several pieces of evidence that the mechanism is improved information rather than an omitted change in borrower demand. First, we examine how the mapping between credit decisions and subsequent repayment changes across periods and Pix-usage groups. The default-prediction tests focus on the bank observing Pix, while the residual-correlation tests also compare own and other banks. After Pix is introduced, credit limits become more strongly related to borrowers' subsequent default outcomes, and the explanatory power of observed credit terms and borrower characteristics for future default increases. This improvement is particularly strong among consumers with greater Pix usage. We obtain complementary evidence from residual-correlation tests following Crawford et al. (2018): among high-Pix borrowers, residual limits at the bank observing Pix are more negatively correlated with borrowers' future default than residual limits at other banks. At own banks, this correlation is also more negative for high- than low-Pix borrowers, whereas other banks show no corresponding pattern. These conditional associations are consistent with banks using payment information when setting limits, although a negative own-bank correlation is already present before Pix.

Second, we exploit temporary disruptions in the Pix network to address concerns about the endogeneity of consumers' Pix usage. Sender-bank Pix timeouts generate plausibly exogenous variation in both the intensity of Pix use and its allocation across banks, while being unlikely to reflect contemporaneous changes in borrower credit demand or creditworthiness. We use this variation in two complementary instrumental-variable designs. At the extensive margin, individuals exposed to timeouts reallocate a larger share of their Pix activity toward banks that do not experience disruptions. We use this induced reallocation to test whether greater Pix activity observed by a bank increases the likelihood that the consumer subsequently obtains a credit card from that bank. A 10 percentage point increase in predicted Pix share raises the probability of

holding a card from the bank by approximately 1.7 percentage points after three months and 2.0 percentage points after six months. At the intensive margin, timeout-induced increases in Pix usage lead to higher credit limits and balances at the bank observing the payments and lower default, while we find no comparable pattern at other lenders. Together, these results make reverse causality—for example, consumers directing more Pix activity toward a bank because that bank has already expanded their credit—a less plausible explanation for the main findings.

Additional tests reinforce the information interpretation. The increase in credit persists beyond the contemporaneous relationship: local projections show that higher Pix usage predicts greater subsequent credit at the Pix bank, including unsecured credit, without a corresponding increase at other lenders or a rise in future defaults. The results are also not driven by banks jointly opening Pix and credit-card products. Excluding relationships in which Pix and credit cards begin at the same time leaves the main effects intact. Finally, the effect of Pix on credit provision is stronger when the bank has a shorter pre-existing relationship with the borrower. Payment data are therefore most valuable precisely where the lender begins with less information about the customer.

Our empirical findings reveal two forces: Pix is a payment substitute, and once available, households move transactions away from cash, debit, and credit cards. However, Pix is also an information production technology that impacts credit pricing and competitive lender interaction. Our reduced-form results show the net effect of these two forces, but do not necessarily imply that the same quantitative effect would hold in other settings, or how the results would change under alternate rules governing the production of, or access to, data affiliated with Pix transactions. To separate these mechanisms and quantify their importance in equilibrium, we build and estimate a structural model of household payment and credit choices with lender competition.

In our model, households have an unobserved type that simultaneously determines their demand for liquidity and default risk. In a logit-style demand system, households first choose a bundle of payment and credit products, and then choose how to pay for

each transaction that arrives. Conditional on using a credit card, they decide whether to pay in full or borrow, and may or may not default when borrowing. Card issuers receive signals of borrower type and set signal-contingent rates. In equilibrium, these rates account for how they impact product adoption, card use, borrowing, and importantly, borrower selection. We introduce Pix as both a competing payment technology and a source of information: A household's Pix activity gives its bank a more precise signal of its type, allowing the bank to set prices with more information. Thus, Pix acts as both a substitute for payments and a complement in the provision of credit.

We estimate the model using data on product adoption, payment-method use, interest rates, and default, together with variation in household characteristics, payment acceptance, and lender costs. In future versions of the paper, we use the estimated model to decompose Pix's effects as coming from its role as a direct payment substitute and from its role as an information production technology. We also evaluate other plausible information environments, including a regime in which Pix data is unavailable to lenders (e.g., as if Pix were run entirely by the government or by a siloed division of the bank), and regime in which Pix information is shared more broadly. This allows us to study how data access affects interest rates, credit use, default, and more broadly how the effects of a government-run payments system spill over into the private payments system.

Taken together, the results point to a form of complementarity between payments and lending. Pix competes with cards as a transaction technology, but it simultaneously generates information that can strengthen the credit relationship between a consumer and the bank observing those transactions. This distinction is important because it changes how one should think about competition between payment instruments. A payment innovation need not reduce the use of an incumbent financial product even when it substitutes for that product's transaction function. If the incumbent product bundles payments with credit, information generated by the new technology can expand demand or supply along the credit dimension and offset or even reverse the direct substitution effect.

We contribute to several strands of the literature. First, we contribute to the literature on payment systems and payment choice. A large theoretical and empirical literature studies the two-sided nature of card markets and how network effects, merchant acceptance, interchange fees, and rewards shape the adoption and use of payment instruments (Rochet and Tirole (2002); Rysman (2007); Rysman and Wright (2014); Bedre-Defolie and Calvano (2013); Wang (2023); Egan et al. (2026)). We add new evidence showing that the introduction of a low-cost instant payment method need not be followed by substitution away from credit cards. Around the launch of Pix, credit card issuance, transaction volume, and borrowing increase, even though interchange fees and rewards remain largely unchanged. These associations show that payment instruments can be complements when payment activity supports a separate function of cards–access to unsecured credit.

Our paper also relates to the growing literature on mobile, cashless, and instant payments. Existing work studies how these technologies affect risk sharing, consumption, financial inclusion, and the adoption of electronic payments (Jack and Suri (2014); Suri and Jack (2016); Chodorow-Reich et al. (2020); Crouzet et al. (2023); Higgins (2024); Ouyang (2026); Dubey and Purnanandam (2023)).[2] Closely related papers use the introduction and diffusion of Pix to study payment-technology adoption, deposit competition, and bank liquidity and risk taking (Mariani et al. (2023); Sarkisyan (2024); Liang et al. (2024); Sampaio and Ornelas (2024); Ding et al. (2025)). We document a different consequence of instant payment systems: a universally available payment rail can reshape the incumbent card market and strengthen the connection between payment activity and consumer credit. Rather than operating only through substitution across payment methods or changes in payment-market pricing, Pix generates information within the bank–customer relationship that helps explain why credit card use expands alongside instant payments.

[2]A broader literature studies how FinTech development affects banks. For example, Buchak et al. (2018); Erel and Liebersohn (2022); Ghosh et al. (2026); Chava et al. (2021); Di Maggio and Yao (2021); Gopal and Schnabl (2022); Beaumont et al. (2022); Yannelis and Zhang (2023), among others.

Second, we contribute to the literature on credit cards and consumer borrowing. Existing work shows that changes in credit limits and interest rates affect borrowing and consumption (Gross and Souleles (2002); Agarwal et al. (2018)), and studies how consumers allocate borrowing across cards and respond to differences in card pricing (Ponce et al. (2017); Stango and Zinman (2016); Gelman and Roussanov (2024)). Closely related, Bian et al. (2023) show that digital wallets and buy-now-pay-later products can expand access to consumer credit, while Benetton and Buchak (2024) study revolving credit supplied through business credit cards. Other papers examine how regulation, rewards, default risk, market power, and information sharing shape unsecured credit supply and the economics of credit card lending (Agarwal et al. (2015); Han et al. (2018); Agarwal et al. (2022); Nelson (2025); Dempsey and Ionescu (2026); Drechsler et al. (2025); Ausubel (1991); Calem and Mester (1995); Berlin and Mester (2004); Herkenhoff and Raveendranathan (2025); Guttman-Kenney and Shahidinejad (2023)). We add new evidence that digital payment activity affects both access to credit cards and subsequent borrowing: consumers receive cards, higher limits, and better terms from the bank that observes their Pix transactions, increase their card balances, and do not default more frequently. Our findings therefore connect the payment and borrowing functions of credit cards by showing that information produced through an instant payment system can expand unsecured credit without a change in card rewards or interchange fees.

Third, we contribute to the literature on information production and sharing in credit markets. Traditional work studies the distinction between informed and arm's-length finance, the value of lending relationships, and how sharing borrower information affects credit-market outcomes (Rajan (1992); Pagano and Jappelli (1993); Petersen and Rajan (1994); Doblas-Madrid and Minetti (2013); Liberti and Petersen (2019)).[3] A growing literature examines how digital footprints, payment flows, and borrower-controlled transaction data affect screening and competition between banks and FinTech lenders

[3]Recent work has also evaluated the importance of financial data (Farboodi et al. (2025); Liu et al. (2025); Farboodi and Veldkamp (2026)).

(Berg et al. (2020); Parlour et al. (2022); He et al. (2023); Babina et al. (2025); Nam (2023); Ghosh et al. (2026)). We add new evidence that routine payment activity creates proprietary information within a bank–customer relationship. Banks that observe a consumer's Pix transactions predict future defaults more accurately and expand credit-card access while offering higher limits and better terms; banks that do not observe the same consumer's Pix activity do not respond similarly. These findings show that payment data can reduce information frictions even when the payment technology is universally available, because the information it generates remains specific to the intermediary that observes the underlying transactions.

The rest of the paper is organized as follows. Section 2 describes our data and empirical strategy. Section 3 shows the main empirical results. Section 4 introduces a structural model of payment-credit complementarities. Section 5 contains additional results and robustness tests. Section 6 concludes.

# 2 Data and empirical strategy

In this section, we describe our main data sources, summary statistics, and identification strategy.

## *2.1 Data and summary statistics*

We combine four administrative data sources that describe the Brazilian payment and consumer-credit markets at complementary levels of aggregation: regulatory reports collected by the Central Bank of Brazil (BCB) from card issuers and acquirers, the BCB's Credit Information System (SCR) linked to individual-bank-month Pix transactions, and merchant-level payment records, collected by the Card Receivables Registry - Núclea. Together, these data allow us to observe payment activity, card pricing, and credit outcomes while distinguishing the bank that observes a consumer's Pix activity from other banks.

The issuer data come from the BCB's Document 6308, which combines the *CONCEMIS* and *LUCREMIS* reports. At the issuer-quarter level, they provide issued and active cards, transaction values, interchange income, and rewards expenses. The acquirer data come from the BCB's Document 6334, specifically the *INTERCAM* report, and measure interchange rates, associated fee amounts and transaction counts, and installments by acquirer, network, card type, capture channel, and merchant segment. In these reports, we observe interchange fees and rewards separately by card type, form of capture, payment company, and number of installments. They also report merchant discount rates separately for the 15 largest firms and for the remaining firms combined within each industry.

The SCR provides monthly borrower-bank credit records, including balances, committed credit limits, effective interest rates, delinquency, and self-reported income. It also identifies the borrower's location. We construct the interest-rate outcome using the reported annual revolving credit-card rate (SCR category 0204) for records with positive exposure, to avoid conflating pricing with changes in the composition of credit products. We refer to this measure simply as the interest rate throughout the paper and tables. Matching SCR to borrower and bank identifiers in the Pix records yields the value and number of transfers observed by each (issuer-)bank for each individual-month. We also turn to merchants payment records, consolidating monthly inflows of Pix, credit-card, and debit-card values and transaction counts, as well as identifying merchants location. These data allow us to characterize payment substitution and construct payment-method shares using the transactions observed in merchant, bank and Pix data, aggregating outcomes geographically when needed.

We also use two waves of BCB household and merchant payment surveys, conducted before Pix in 2019 and after its introduction in 2023. The surveys provide direct evidence on payment acceptance, consumer payment choices, and merchants' discounts across payment methods. We use these responses to guide the empirical analysis and to discipline the structural model, while the administrative data provide the main evidence

on payment behavior and credit outcomes.

Table 1 summarizes the main variables in these four non-survey sources over two-year windows before and after the launch of Pix. Panels A and B compare 2018Q4–2020Q3 with 2020Q4–2022Q3, while Panels C and D compare November 2018–October 2020 with November 2020–October 2022. The table reports pooled means and standard deviations; monetary values are nominal BRL in the units stated in each row. The entity counts show broad and growing coverage in each source, from 39 to 43 issuers, 16 to 20 acquirers, 9,850 to 11,615 borrowers, and 97,087 to 101,575 merchants.

The summary statistics describe several facts that guide the empirical analysis. In the issuer data, the average number of issued credit cards rises from 3.2 to 4.7 million and average credit-card transaction value rises from R$6.2 to R$8.0 billion. At the same time, issuer rewards expenses remain at 0.2 percent of transaction value, and the average interchange rate reported by acquirers remains at 1.7 percent. Average installments rise from 5.9 to 6.4. This broad stability in card pricing, alongside the expansion of credit-card activity and installment use, motivates the analysis of whether Pix affects credit through the information it generates rather than through changed rewards or interchange incentives. The detailed acquirer records also reveal substantial heterogeneity across transactions, as reflected in their large standard deviations.

The borrower-bank and merchant panels show the rapid emergence of Pix after its launch. Average monthly Pix activity in the registry rises from zero to R$0.6 thousand and 1.6 transactions per borrower-bank-month. Over the same windows, average credit limits rise, while average balances and defaults decline. Merchant data likewise show the appearance of substantial Pix activity, alongside higher average credit- and debit-card payment values. These are descriptive patterns; the fixed-effects specifications below isolate the within-individual and within-bank relationships central to our empirical design.

Table 1: Summary Statistics: Main Datasets

| | Before Pix launch | | After Pix launch | |
|---|---|---|---|---|
| | Mean | Std. dev. | Mean | Std. dev. |
| Panel A: Credit-card issuers | | | | |
| Credit cards issued (millions) | 3.2 | 5.8 | 4.7 | 8.5 |
| Active credit cards (millions) | 2.2 | 4.2 | 2.8 | 5.6 |
| Credit-card transaction value (bn. BRL) | 6.2 | 14.5 | 8.0 | 17.7 |
| Interchange income (% of transaction value) | 47.9 | 262.4 | 50.4 | 230.0 |
| Rewards expense (% of transaction value) | 0.2 | 0.4 | 0.2 | 0.3 |
| Number of observed issuers | 39 | | 43 | |
| Panel B: Acquirers | | | | |
| Interchange fee (%) | 1.7 | 0.8 | 1.7 | 0.7 |
| Number of installments | 5.9 | 4.1 | 6.4 | 4.4 |
| Credit-card transaction value (bn. BRL) | 0.315 | 4.982 | 0.270 | 2.726 |
| Number of transactions (thousands) | 36.6 | 916.0 | 29.5 | 553.3 |
| Number of observed acquirers | 16 | | 20 | |
| Panel C: Credit registry and Pix transactions | | | | |
| Monthly Pix value (thousand BRL) | 0.0 | 0.0 | 0.6 | 4.2 |
| Monthly Pix transactions | 0.0 | 0.0 | 1.6 | 5.9 |
| Credit limit (thousand BRL) | 1.8 | 8.0 | 2.2 | 8.3 |
| Credit balance (thousand BRL) | 1.3 | 3.6 | 1.1 | 3.8 |
| Interest rate (%) | 257.5 | 162.1 | 297.0 | 201.4 |
| Default indicator | 0.2 | 0.4 | 0.1 | 0.3 |
| Monthly income (thousand BRL) | 5.0 | 293.5 | 10.8 | 976.4 |
| Number of observed borrowers | 9,850 | | 11,615 | |
| Panel D: Merchant payment transactions | | | | |
| Pix payment value (thousand BRL) | 0.0 | 0.0 | 38.0 | 1,522.4 |
| Pix transactions | 0.0 | 0.0 | 71.3 | 4,188.2 |
| Credit-card payment value (mn. BRL) | 0.098 | 4.210 | 0.132 | 7.250 |
| Debit-card payment value (mn. BRL) | 0.041 | 1.388 | 0.058 | 1.943 |
| Number of observed merchants | 97,087 | | 101,575 | |

*Note:* This table provides descriptive statistics for the main non-survey datasets used in the analysis. Panel A reports issuer-quarter credit-card portfolio statistics. Panel B reports detailed acquirer credit-card transaction records. Panel C reports borrower-bank-month observations from the credit registry linked to individual-bank Pix transactions, restricting the registry sample to credit categories used in the main analysis. Panel D reports merchant-month payment transactions observed in both merchant files. Before Pix covers November 2018–October 2020; After Pix covers November 2020–October 2022. The number of observed entities reports distinct entities appearing at least once in the corresponding period. Monetary variables are expressed in nominal BRLs and use the units stated in the row labels. The table does not report survey or municipality-level statistics.

## *2.2 Empirical strategy*

Our empirical strategy combines event-study evidence around the introduction of Pix, within-borrower comparisons across banks in the credit registry, and an instrumental-variables design. These approaches address complementary identification concerns and distinguish an information channel from contemporaneous changes in card-market pricing or a broad change in consumers' demand for credit.

We first study the timing of changes in issuer and acquirer outcomes around the launch of Pix. For outcome $Y$ observed for issuer or acquirer $g$, payment-product cell $c$, and quarter $t$, we estimate

$$Y_{gct} = \sum_{k \neq -1} \beta_k \mathbb{1}\{t - t_0 = k\} + \alpha_g + \delta_c + X'_{gct}\Gamma + \varepsilon_{gct}, \tag{1}$$

where $t_0$ is the launch quarter (2020Q4), and the event dummy equals one when quarter $t$ is $k$ quarters from that launch. We omit the quarter immediately before launch ($k = -1$). The outcome is a measure of issuer activity or card profitability, or an acquirer measure of interchange, merchant discount rates, or installment use. The fixed effects absorb permanent differences across issuers or acquirers and payment-product cells, while $X_{gct}$ includes the available payment and merchant-segment controls. We cluster standard errors at the issuer or acquirer level. Because Pix was introduced nationally, these event studies document the timing and pre-launch evolution of the outcomes rather than provide a stand-alone causal estimate; their role is to show whether the changes in credit-card activity and prices coincide with the arrival of Pix.

We next use the linked SCR–Pix records to compare the terms offered by the bank that observes a consumer's Pix payments with those offered by banks that do not. At the extensive margin, we estimate

$$\mathbb{1}\{\text{has card}\}_{itb} = \alpha Pix_{itb} + \beta Pix_{i,t,-b} + \theta_t + \gamma_b + \eta_i + \omega_{it} + \varepsilon_{itb}, \tag{2}$$

where $Pix_{itb}$ is log Pix usage by individual $i$ at time $t$ through an account at bank $b$,

and $Pix_{i,t,-b}$ is log Pix usage through accounts at banks other than $b$. The dependent variable equals one if bank $b$ has issued a credit card to individual $i$ by month $t$. At the intensive margin, conditional on an observed borrower–card-bank relationship, we estimate

$$Y_{ibt} = \alpha Pix_{ibt} + \beta X_{ibt} + \gamma_b + \omega_{it} + \varepsilon_{ibt}, \tag{3}$$

where $Y_{ibt}$ is log credit limit, log credit balance, the interest rate, or the default indicator; $X_{ibt}$ includes income, credit limits, balances, interest rates, collateral and default indicators, and municipality-level demographic and economic controls. In each specification, the dependent variable is excluded from the corresponding list of controls. Default equals one when the borrower has an amount past due for at least 90 days. The own-bank sample consists of borrower–bank–months in which the card bank observes the borrower's Pix activity. For the other-bank comparison, we use a conservative no-overlap sample: the credit-card bank is never a Pix bank for that borrower in the entire sample. Thus, the other bank cannot observe that borrower's Pix transaction history through its own relationship.[4]

The results are robust to defining other-bank status using only past and current Pix relationships, without using future Pix activity. Under this definition, a credit-card bank qualifies as an other bank in month $t$ if the borrower has no recorded outgoing Pix activity at that bank through month $t$; banks used for Pix later in the sample contribute only before their first observed Pix month. Appendix Table A.14 reports the corresponding OLS and IV results.

Bank fixed effects absorb time-invariant differences in lender policies. In the own-bank sample, individual-by-month fixed effects compare the same person's credit terms across banks in a given month. Together with the rich borrower, credit, and municipality controls, this absorbs time-varying borrower-level shocks common to all of the consumer's lenders, including changes in income, credit demand, and general payment use. The

[4]The other-bank OLS specifications include individual and calendar-time fixed effects separately; the note to Table 4 explains why individual-by-month fixed effects cannot identify this comparison.

other-bank OLS specifications instead include separate individual and time fixed effects, so they do not absorb all borrower-month shocks. The own- and other-bank estimates therefore provide a comparison across relationships with different access to payment information, but under different fixed-effect specifications. Separating the extensive and intensive margins also prevents a change in card access from being conflated with a change in the terms of an existing relationship. Finally, we estimate the default outcome to test whether new or expanded card credit simply reflects looser risk taking. A rise in credit without higher subsequent default is instead consistent with improved screening from the payment information.

Residual endogeneity remains possible because credit supply or unobserved borrower demand may affect Pix use, even after the fixed effects. We therefore instrument for an individual's Pix usage with exposure to sender-bank Pix timeouts. Let $s$ index the banks through which individual $i$ sends Pix and let $w_{is\tau}$ denote the individual's Pix-value share at sender bank $s$ in month $\tau$. The instrument is

$$Z_{it} = \sum_{\ell=1}^{4} \sum_{s} w_{is,t-\ell} \, \text{Timeout}_{s,t-\ell}, \tag{4}$$

the cumulative Pix-value-share-weighted sender-bank timeout exposure over months $t-4$ through $t-1$. It varies across individuals and calendar months, but not across credit-card banks within an individual month. The first stage is

$$\log(1 + \text{Pix}_{it}) = \pi Z_{it} + X'_{ibt}\Gamma + \gamma_b + \eta_i + \tau_t + u_{ibt}, \tag{5}$$

where $X_{ibt}$ is the same set of controls used in the corresponding SCR specification, and $\gamma_b$, $\eta_i$, and $\tau_t$ are bank, individual, and calendar-time fixed effects. We report the Kleibergen–Paap first-stage statistic for each estimation sample. Individual-by-month fixed effects are not included in these specifications because they would mechanically absorb $Z_{it}$.

The second stage replaces observed Pix use in equation (3) with its fitted value from

equation (5). The relevance condition is that lagged exposure to sender-bank technical disruptions shifts an individual's intensive use of Pix; the reported first-stage statistics directly assess this condition. The exclusion restriction is that, conditional on the controls and fixed effects, these lagged disruptions affect current credit-card terms only through Pix usage and the payment information it produces, not through a direct effect on credit supply or credit demand. The lag structure and sender-side construction help limit concerns that contemporaneous credit conditions directly cause the measured disruption. This design addresses reverse causality and residual time-varying shocks jointly affecting Pix and credit, although the exclusion restriction would be threatened if past sender-bank outages directly changed the borrower's current credit demand or the card bank's supply decision independently of Pix use.

# 3 Empirical results

This section presents the main empirical results. First, we document several trends in payment markets around the launch of Pix. Next, we examine the information channel as an explanation for the rise in the use of credit cards.

## *3.1 Event Analysis*

We start by analyzing the responses to the two waves of surveys conducted by the Central Bank of Brazil. The surveys asked merchants and households about their acceptance and usage of each available payment method, respectively. Merchants were also asked about the discounts they offer when households pay in cash or Pix. Table 2 establishes the basic puzzle. Card acceptance was already widespread before Pix and rose further by 2023: 89.6% of merchants accepted credit cards and 92.3% accepted debit cards in 2019, compared with 96% and 96.7%, respectively, in 2023. Pix therefore entered a market in which payment acceptance was not an important constraint for cards.

Household preferences also shifted in a way that is difficult to explain with a simple

substitution story. While 83% of households use Pix and 97.3% of merchants accept it in 2023, the share using credit cards rose from 40.3% to 48%. In contrast, debit-card usage fell from 47% to 44%. Households cite credit-card rewards and discounts as reasons for their choices; debit cards offer neither rewards nor, typically, merchant discounts. The divergence between credit and debit cards suggests that the growth in credit-card use reflects functions that Pix does not replicate, rather than card acceptance alone.

Finally, Panel C shows that merchants have a clear private incentive to steer customers toward lower-cost payment methods. In 2019, 55.9% of merchants offered discounts for cash payments; the share rose to 80.6% in 2023. In 2023, 68.8% also offered Pix discounts, with 42% reporting discounts of 5–10%. These discounts make the simultaneous increase in Pix and credit-card use particularly informative: consumers continue to use cards even when merchants actively encourage alternatives.

#### *3.1.1 Issuer-level evidence*

We next turn to the quarterly reports of card issuers and estimate the event-study specification in equation (1) around the launch of Pix. Because issuers report the number of cards, transaction volumes, and volume per active card separately for debit and credit cards, the event studies allow us to compare whether the observed changes are broad across electronic payment methods or concentrated in a particular product. Figure 1 plots the estimated event-time coefficients for the log number of active cards (Panel A), the log value of card transactions (Panel B), and the log volume per active card (Panel C), separately for credit cards (orange lines) and debit cards (blue lines), normalized to the quarter before the Pix launch. In the two years before Pix, the credit-card and debit-card coefficients hover around zero and are statistically indistinguishable from one another.

After the introduction of Pix, the two lines diverge in Panels A and B. The number of active credit cards climbs steadily after the launch and is about 126% above its pre-launch level by the end of 2022, and credit-card transaction volume rises by roughly 171%.

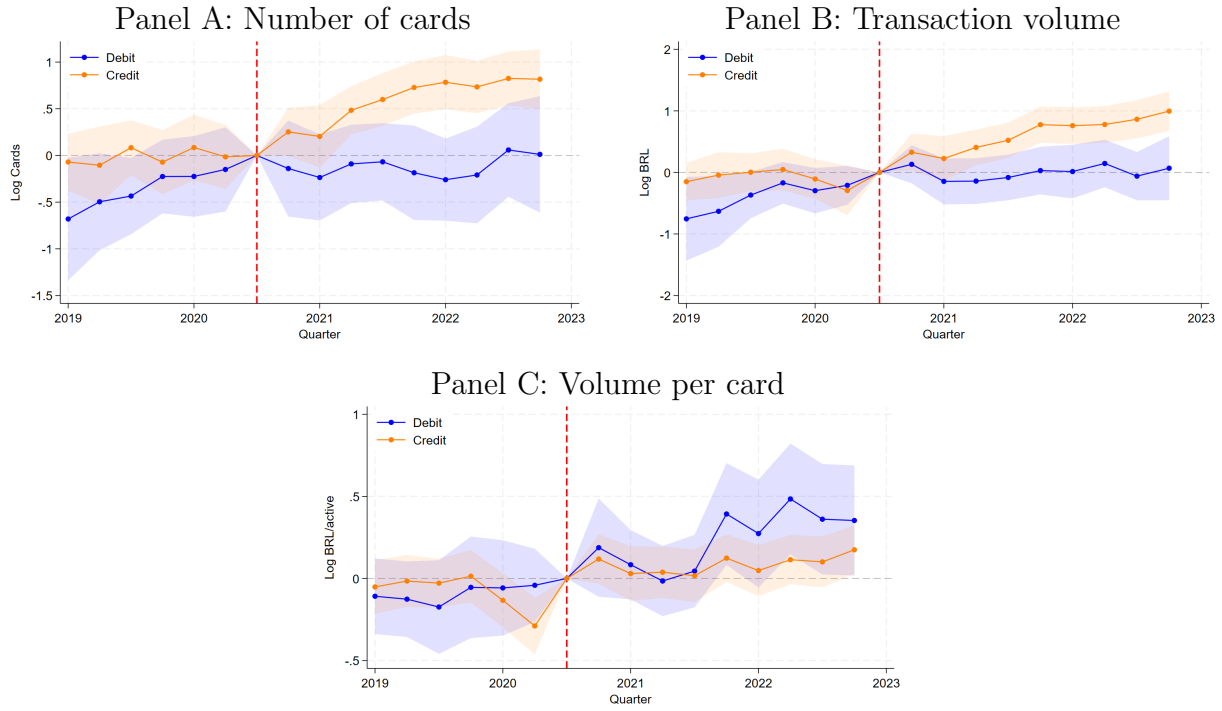
Table 2: Acceptance, Usage, and Discounts Across Payment Methods

| | 2019 | | 2023 | | |
|---|---|---|---|---|---|
| | Cash | Cards | Cash | Pix | Cards |
| Panel A: Merchant acceptance | | | | | |
| Share accepting cash | 96.2% | | 98.2% | | |
| Share accepting Pix | | | | 97.3% | |
| Share accepting debit cards | | 92.3% | | | 96.7% |
| Share accepting credit cards | | 89.6% | | | 96.0% |
| Panel B: Household usage | | | | | |
| Share using cash | 92.8% | | 92.0% | | |
| Share using Pix | | | | 83.0% | |
| Share using debit cards | | 47.0% | | | 44.0% |
| Share using credit cards | | 40.3% | | | 48.0% |
| Panel C: Merchant discounts | | | | | |
| Share offering discounts | 55.9% | | 80.6% | 68.8% | |
| Share offering 0–5% discounts | | | 57.0% | 55.0% | |
| Share offering 5–10% discounts | | | 40.0% | 42.0% | |

*Note:* This table shows statistics on merchant acceptance, household usage, and merchant discounts across payment methods in Brazil in 2019 and 2023. Panel A reports the share of merchants accepting each payment method. Panel B reports the share of households using each payment method. Panel C reports the share of merchants offering discounts by payment method. Pix was introduced in late 2020 and, therefore, is only reported for 2023.

The debit-card series, by contrast, remain near their pre-launch levels throughout the post period: the number of active debit cards and debit volume fluctuates around zero with no visible break at the launch quarter. The post-launch association is therefore concentrated in credit cards rather than debit cards. This contrast is less consistent with a broad secular or pandemic-related increase in electronic payments, although the national timing of Pix does not by itself rule out differential trends across card types. Panel C completes the decomposition: spending per active credit and debit card changes little after the launch, with point estimates that remain small and generally insignificant. The increase in credit-card volume is therefore associated primarily with more cards rather than deeper use of existing cards. This extensive-margin pattern points to changes

Figure 1: Credit and Debit Card Usage around the Launch of Pix



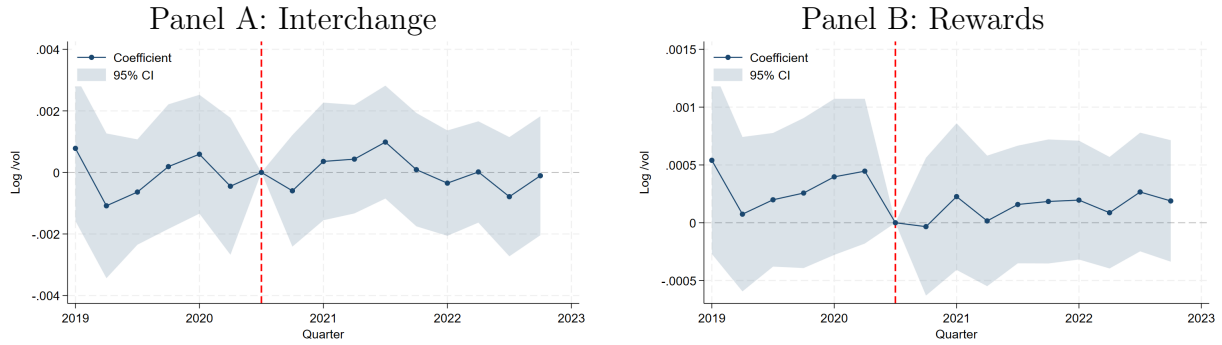

*Note:* This figure plots event-time coefficients from equation (1) for debit and credit card usage, normalized relative to 2020Q3. Orange lines correspond to credit cards, and blue lines correspond to debit cards. 95% confidence intervals are also plotted. A vertical dashed red line is the quarter of the launch of Pix. Panel A plots the number of active cards. Panel B plots transaction volume. Panel C plots the volume per active card.

in the availability of credit-card relationships, rather than merely more intensive use by the same cardholders, and motivates the borrower–bank analysis below.

A natural explanation for the expansion in credit cards is that banks responded to the competitive threat of Pix by making credit cards more attractive–raising rewards and financing them with higher interchange income. To evaluate this pricing channel, we track both margins. Reward expenses are reported by the credit card issuers along with the transaction volume, which allows us to calculate reward expenses per BRL of transaction. Issuers also report interchange income, allowing us to calculate interchange income per BRL of transaction. The interchange fee here only includes the fee charged by the issuer, i.e., it is net of the network and acquirer fees.

Figure 2 shows that neither margin moves. The event-time coefficients for interchange income (Panel A) and for rewards (Panel B) per BRL of transactions are never

Figure 2: Interchange Income and Rewards around the Launch of Pix



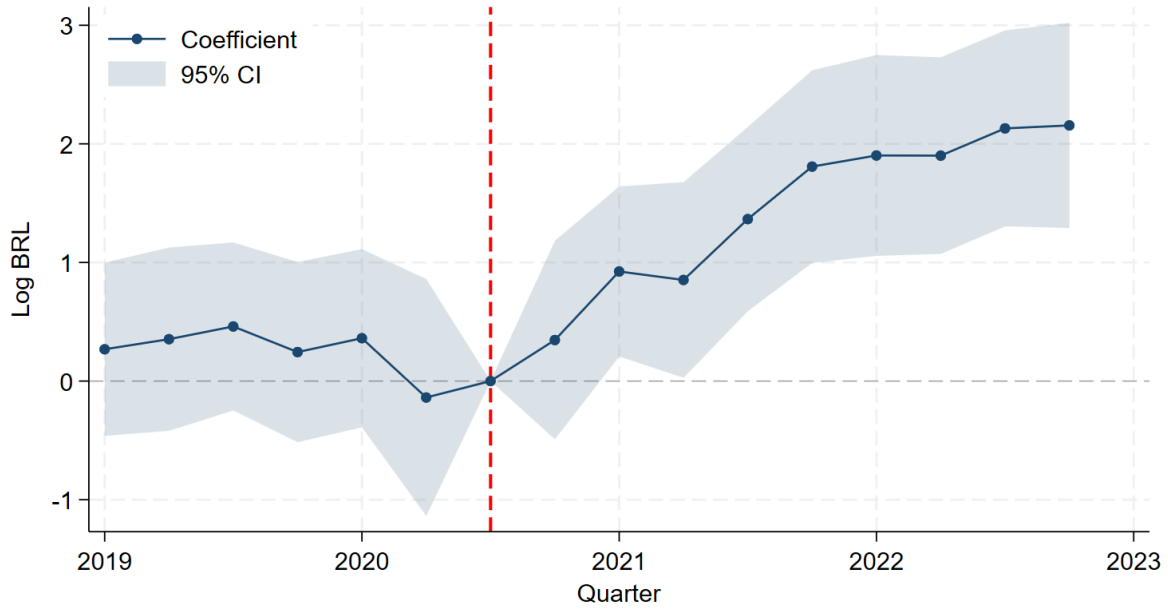

*Note:* This figure plots event-time coefficients from equation (1) for log interchange income per BRL of transaction (Panel A) and log reward expense per BRL of transaction (Panel B), normalized relative to 2020Q3. 95% confidence intervals are also plotted. A vertical dashed red line is the quarter of the launch of Pix.

statistically significant. There is no discernible break at the launch of Pix in either series, and the post-launch estimates are as close to zero as the pre-launch ones. A response in which issuers attempted to preserve card demand by making cards more attractive relative to Pix should raise reward spending and, plausibly, interchange income to fund it. The absence of either response weakens a pricing-based explanation for the rise in credit-card use and shifts attention toward the credit and information embedded in the card relationship.

Issuer data report interchange income, but acquirers provide a more detailed breakdown. They report interchange fees (in %) for different cards, forms of capture, industries, and numbers of installment payments. The corresponding heterogeneous event studies, reported in Appendix Figure A.1, show that the minor increase in interchange fees is not driven by card rewards, installments, forms of capture, or networks. Instead, it is concentrated in finance and regulatory sectors, rather than retail and services where Pix is most relevant. This composition makes it unlikely that a broad change in card pricing explains the patterns that follow; the increase may instead reflect COVID-19-related changes in retail payments.

We next investigate the association with borrowing, a distinctive feature of credit cards that Pix does not offer. Figure 3 plots event-time coefficients for the financed

Figure 3: Credit Card Borrowing Volume around the Launch of Pix

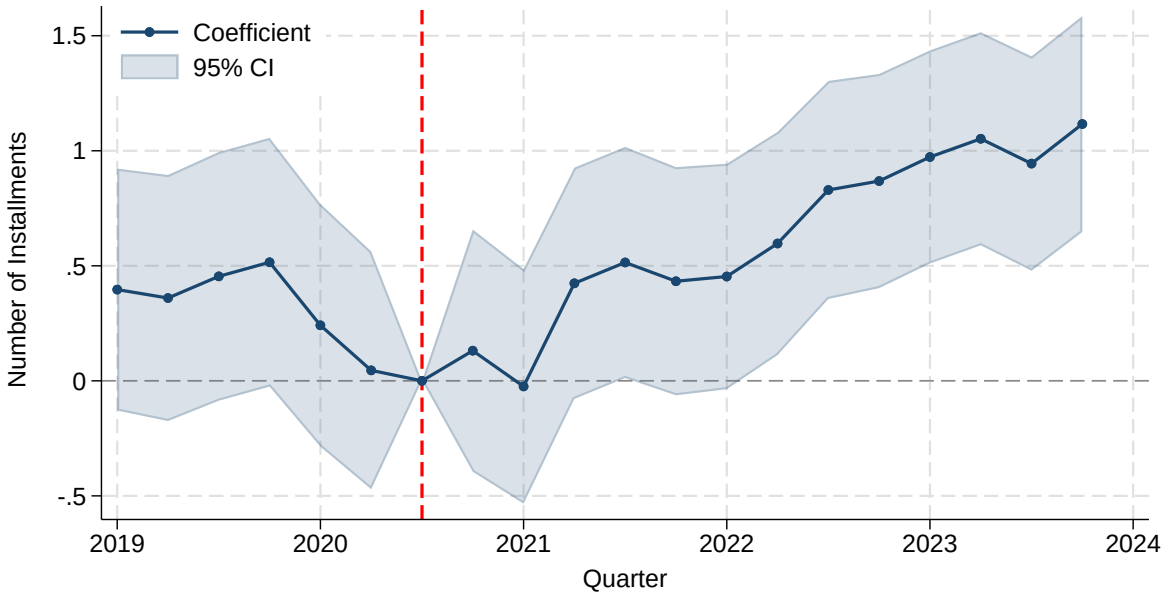



*Note:* This figure plots event-time coefficients from equation (1) for the borrowed amount on credit cards, i.e., the portion that is either rolled over or used in installments, normalized relative to 2020Q3. 95% confidence intervals are also plotted. A vertical dashed red line is the quarter of the launch of Pix.

portion of credit-card volume—the part that is rolled over or paid in installments. Before the launch of Pix, the coefficients are statistically insignificant. After the launch, financed volume rises sharply and continuously: it is roughly 152% above the pre-launch level within two quarters and about 569% higher by mid-2022. This pattern is consistent with credit cards retaining value as a financing product even as Pix becomes an attractive way to pay. It also raises the question of why the supply of card credit expands, which is the focus of the next section.

Borrowing on credit cards is not limited to revolving debt. Consumers can also finance purchases through monthly installments, a common feature of credit-card payments in Brazil. Figure 4 shows that the launch of Pix is associated with a substantial rise in installment use. Appendix Tables A.1 and A.2 further show that the average number of installments is almost one higher post-Pix, particularly for physical payments, and that credit-card volume relative to total payment volume is higher for transactions involving

Figure 4: Credit Card Installment Payments around the Launch of Pix




*Note:* This figure plots event-time coefficients from equation (1) for the number of installments on credit cards, normalized relative to 2020Q3. 95% confidence intervals are also plotted. A vertical dashed red line is the quarter of the launch of Pix.

installments. These patterns highlight an important dimension along which Pix and credit cards are complementary: Pix provides an alternative means of making immediate payments, while credit cards continue to provide access to delayed payment and financing.

## *3.2 Mechanism*

The evidence so far shows that the expansion of credit-card activity around the introduction of Pix is concentrated in the issuance of new cards and greater borrowing on those cards. At the same time, we find little change in rewards or interchange income, making it unlikely that this expansion is primarily a response to greater competition for payments, which would naturally lead issuers to make credit cards more attractive through higher rewards. We therefore consider an alternative mechanism: Pix may expand the information available to banks about their customers. Using Pix requires consumers to establish or use an account relationship with a financial institution, allowing the institution through which Pix transactions are conducted to observe customers'

Table 3: Impact of Pix on the Origination of New Credit Cards

| | $\mathbb{1}\{\text{has card}\}_{itb}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\log(1+\text{Pix}_{itb})$ | 0.0616*** | 0.0611*** | 0.0514*** | 0.0468*** |
| | (0.0004) | (0.0006) | (0.0067) | (0.0005) |
| $\log(1+\text{Pix}_{i,t,-b})$ | 0.0002*** | −0.0008*** | −0.0006** | −0.0131*** |
| | (0.0000) | (0.0000) | (0.0002) | (0.0008) |
| Month | Yes | Yes | Yes | |
| Individual | | Yes | Yes | |
| Bank | | | Yes | Yes |
| Individual × Month | | | | Yes |
| Obs. | 96,452,160 | 96,452,160 | 96,452,160 | 96,452,160 |
| $R^2$ (within) | 0.1398 | 0.1394 | 0.0981 | 0.0994 |

*Note:* This table reports estimates of the extensive-margin equation (2), where the dependent variable indicates whether borrower $i$ has a credit card from bank $b$ in month $t$. The regressors are log Pix use at bank $b$ and log Pix use at all other banks. The sample contains borrower–bank–month observations. Columns sequentially add month, individual, bank, and individual-by-month fixed effects as indicated. Standard errors are three-way clustered at the individual, bank and month levels and are displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

payment activity and learn about their financial condition. This information may improve the bank's assessment of credit risk and allow it to extend more credit or offer better terms (Berg et al., 2020; Ghosh et al., 2026). The mechanism delivers a sharp empirical prediction: if Pix expands credit through information acquisition, the effect should be concentrated at the bank that observes the customer's Pix transactions and should be substantially weaker at other banks.

We first report estimates of the extensive-margin specification in equation (2). Table 3 shows that using Pix is associated with a higher likelihood of receiving a credit card from the bank that observes the Pix activity. In contrast, the coefficient on Pix use at other banks is small in the preferred individual-by-month fixed-effects specification. This within-borrower comparison is difficult to reconcile with a common improvement in credit demand or borrower quality: those forces would also affect other lenders. Instead, the contrast points to the proprietary value of the digital footprint available to the own bank.

Table 4 reports estimates of the intensive-margin specification in equation (3), separately for the own-bank and strict other-bank samples. At the own bank, greater Pix use is associated with higher credit limits and balances, lower interest rates, and lower defaults. The OLS rate estimate is negative but not statistically significant; the IV estimate in Table 9 shows a decrease significant at the 10% level. These credit-term effects are substantially weaker in Panel B, where the card bank never observes the borrower's Pix activity. Taken together, the results suggest improved sorting rather than indiscriminate credit expansion: the own bank both extends more credit and does so without increasing subsequent default. This is the opposite of what one would expect if the results were driven primarily by a broad increase in competition for borrowers. The main own-bank pattern also persists when the estimation sample begins in September 2021 (Appendix Table A.4). Table 11 further addresses a cross-selling interpretation: among borrowers who use Pix at multiple banks, the bank observing a larger share of Pix activity provides higher limits and balances and lower interest rates, without increasing default. Table A.8 shows that the results are robust when Pix is measured in quantities rather than values. Table A.9 shows robustness to including individual and time FEs separately. Finally, Table A.10 shows that the increase in credit limits is not concentrated among high-income individuals, while the interest-rate decline is larger among high-income borrowers.

Appendix Table A.15 shows that the OLS results are robust to including bank-by-month fixed effects, which absorb time-varying lender policies and common bank-level shocks. The own-bank estimates continue to show significantly higher limits and balances and lower default, while the other-bank coefficients remain substantially smaller. Appendix Table A.6 reweights the other-bank sample so that each underlying credit record, or each borrower-month, receives equal total weight. All four OLS coefficients remain statistically insignificant under both weighting schemes, indicating that the other-bank results are not driven by the greater weight assigned to credit records paired with more Pix banks.

We also conduct a timing placebo in Appendix Table A.16. We replace each own-

Table 4: Impact of Pix on the Credit Card Terms

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.032** | 0.042*** | −4.413 | −0.001** |
| | (0.014) | (0.013) | (3.357) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 84,115 | 84,115 | 1,574 | 84,115 |
| $R^2$ | 0.812 | 0.587 | 0.735 | 0.637 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.001 | 0.001 | −0.113 | 0.000 |
| | (0.001) | (0.001) | (0.221) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,850 | 966,850 | 212,102 | 966,850 |
| $R^2$ | 0.892 | 0.843 | 0.759 | 0.358 |

*Note:* This table reports estimates of equation (3). Panel A covers the borrower's Pix bank; Panel B excludes credit banks ever used for Pix by that borrower. Bank FEs are included throughout; individual and month FEs are interacted in Panel A and separate in Panel B. Panel B pairs each credit record with every observed Pix bank in the borrower-month; observations count pairs. Individual-time FEs leave no covariance between Pix use and credit terms because each credit record faces the same Pix-usage distribution. Controls follow equation (3). Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

bank credit outcome with its value in the month immediately before the individual's first observed Pix use at any bank, retaining the original post-Pix regressors, controls, and fixed effects. Panel A shows that subsequent Pix usage does not significantly predict these pre-Pix limits, balances, interest rates, or defaults. The absence of a corresponding association with pre-Pix outcomes provides a timing-based check on the interpretation of the main results.

If banks use Pix to learn about borrowers and improve credit-card terms, they should also become better at predicting default. Tables 5–6 examine changes across periods and

Table 5: Future Default and Predictive Power

| | Future default | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Log Limit | 0.082 | −0.047 | −0.442* | −0.481* | −0.406*** | −0.435*** |
| | (0.183) | (0.201) | (0.245) | (0.249) | (0.139) | (0.140) |
| Sample | Pre-Pix | Pre-Pix | Post-Pix, High | Post-Pix, High | Post-Pix, Low | Post-Pix, Low |
| Bank FE | Yes | – | Yes | – | Yes | – |
| Time FE | Yes | – | Yes | – | Yes | – |
| Bank × Time FE | No | Yes | No | Yes | No | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 452,866 | 452,391 | 198,322 | 197,564 | 548,573 | 547,916 |
| $R^2$ | 0.084 | 0.090 | 0.125 | 0.136 | 0.096 | 0.103 |

*Note:* This table shows the results of regressing an indicator for default within the next 12 months on credit card terms. Bank, time, and bank-time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Pix-usage groups at the bank observing Pix; Table 7 additionally compares own and other banks. We measure future default twelve months ahead and examine whether the mapping from credit terms to subsequent default changes after Pix. The comparison of $R^2$s is especially informative because it asks whether observed terms and borrower characteristics explain more of future repayment once the bank can observe Pix activity. The default-prediction regressions include the same control variables as in equation (3); the controls for the residual-correlation regressions are listed below.

Table 5 shows the results. After the launch of Pix, banks offer higher limits to borrowers who do not default within the next 12 months, and this relationship is stronger for borrowers with above-average Pix usage. The pattern indicates that limits become more aligned with ex post repayment capacity when the bank observes more payment activity. We formally show that $R^2$s are higher after Pix and for individuals with above-average Pix usage. We follow Erickson and Whited (2002) and show the results in Table 6; a similar methodology has been used in Buchak et al. (2018) and Cherry (2026).

Finally, we follow Crawford et al. (2018) and Benetton and Buchak (2024) and an-

Table 6: Comparing Predictive Power with and without Pix

| Controls | Specification | | | $R^2$ | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Bank FE | Time FE | Bank × Time FE | Pre Pix | Post Pix, High | Post Pix, Low | Post − Pre (High) | Post − Pre (Low) | High − Low |
| All excl. interest | – | – | Yes | 0.073 | 0.117 | 0.091 | 0.043*** | 0.018*** | 0.026*** |
| All controls | – | – | Yes | 0.090 | 0.136 | 0.103 | 0.046*** | 0.014*** | 0.033*** |
| All excl. interest | Yes | Yes | – | 0.067 | 0.106 | 0.084 | 0.039*** | 0.017*** | 0.022*** |
| All controls | Yes | Yes | – | 0.084 | 0.125 | 0.096 | 0.041*** | 0.012*** | 0.029*** |

*Note:* This table reports the Erickson and Whited (2002) test based on changes in explanatory power across samples. "High" and "Low" refer to above- or below-average use of Pix by an individual, respectively. Bank, time, and bank-time FEs are included. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

alyze the correlation between residual credit limits and defaults to provide evidence of the potential presence of asymmetric information that Pix partly resolves. Specifically, we run the following seemingly unrelated regressions:

$$\log(Limit_{ibt}) = \beta X_{ibt} + \epsilon_{ibt} \tag{6}$$

$$Default_{i,t+12} = \beta X_{ibt} + \eta_{ibt} \tag{7}$$

where $\log(Limit_{ibt})$ is the log of the credit card limit and $Default_{i,t+12}$ is an indicator for individual $i$'s aggregate overdue credit balance across lenders exceeding R$60 twelve months later. Both panels use this borrower-wide default measure. $X_{ibt}$ contains a collateral indicator, log income, a state-capital indicator, and municipality-level urbanization, gender, age, literacy, household-composition, and per-capita-income measures. We include either bank-by-month fixed effects or separate bank and month fixed effects. We estimate both equations on the same observations within each column and compare the residuals $\epsilon_{ibt}$ and $\eta_{ibt}$ to evaluate whether variation in credit limits unexplained by observables predicts future default.

Table 7 shows the results for own banks in Panel A and other banks in Panel B. With bank-by-month fixed effects, the correlation is $-0.094$ for high-Pix borrowers at own banks, compared with $-0.001$ at other banks; the difference is statistically significant

($p < 0.001$). At own banks, the correlation is also more negative for high- than low-Pix borrowers ($-0.094$ versus $-0.043$, $p = 0.004$), while other banks show no significant high–low difference. The difference between the two panels' high–low contrasts is significant ($p = 0.006$). Separate bank and month fixed effects produce similar results. These patterns are consistent with information available to the bank observing Pix but not to other lenders.

### *3.3 Instrumental variables results*

We next use disruptions to the Pix payment network as a source of variation in consumers' Pix activity, described in Section 2.2. The baseline specifications absorb a rich set of borrower and lender characteristics, but Pix usage may still respond to unobserved changes in credit demand or supply. Our instrumental-variable strategy exploits exposure to sender-bank Pix timeouts, which disrupt consumers' ability to make Pix payments but are unrelated to their underlying creditworthiness. Conditional on the fixed effects and controls, the identifying assumption is that these technical disruptions affect subsequent credit-card outcomes only through their effect on Pix usage and the information generated by that activity. We use this variation in two complementary exercises. We first ask whether timeout-induced changes in the bank through which a consumer conducts Pix lead to the origination of a credit card at that bank. We then examine whether timeout-induced Pix usage affects the terms of existing credit-card relationships.

Appendix Table A.17 reports joint tests of whether bank size and credit characteristics predict timeout rates, controlling for bank and month fixed effects. Bank size is not jointly significant ($p = 0.477$). Credit characteristics are jointly significant ($p = 0.007$), as are bank size and credit characteristics together ($p = 0.002$).

We begin with the extensive margin: does a bank that starts to observe a consumer's Pix activity respond by issuing a credit card? The sample consists of individual–bank–month observations in which individual $i$ does not yet hold a credit card from bank $b$,

Table 7: Residual Correlation Tests

| | Panel A: Own bank | | | | | |
|---|---|---|---|---|---|---|
| | Pre Pix | | Post Pix, High | | Post Pix, Low | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Correlation between residuals | −0.064 | −0.056 | −0.094 | −0.089 | −0.043 | −0.040 |
| Breusch-Pagan $\chi^2$ | 367 | 278 | 634 | 571 | 163 | 145 |
| Difference with pre Pix | – | – | −0.030 | −0.033* | 0.021 | 0.016 |
| Difference with low Pix | – | – | −0.051*** | −0.049*** | – | – |
| Bank FE | – | Yes | – | Yes | – | Yes |
| Time FE | – | Yes | – | Yes | – | Yes |
| Bank× Time FE | Yes | – | Yes | – | Yes | – |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 89,675 | 89,824 | 71,794 | 71,966 | 89,875 | 90,026 |
| | Panel B: Other bank | | | | | |
| | Pre Pix | | Post Pix, High | | Post Pix, Low | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Correlation between residuals | 0.008 | 0.011 | −0.001 | 0.001 | −0.010 | −0.009 |
| Breusch-Pagan $\chi^2$ | 24 | 46 | 1 | 0 | 43 | 35 |
| Difference with pre Pix | – | – | −0.009 | −0.011 | −0.018* | −0.020** |
| Difference with low Pix | – | – | 0.008 | 0.009 | – | – |
| Bank FE | – | Yes | – | Yes | – | Yes |
| Time FE | – | Yes | – | Yes | – | Yes |
| Bank× Time FE | Yes | – | Yes | – | Yes | – |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs. | 362,511 | 363,041 | 363,576 | 363,890 | 460,778 | 461,283 |

*Note:* This table reports residual correlations from equation (6). Before Pix, Panel A identifies own banks by subsequent Pix relationships; afterward it requires current Pix activity at the lender. Panel B pairs credit records with observed Pix banks and excludes credit lenders ever used for Pix by the borrower; observations count records or pairs. Both panels use the same borrower-wide default measure and Pix classification. "High" and "Low" indicate above- and below-average borrower mean monthly total Pix value over observed active months, using an equal-borrower mean cutoff. Controls include collateral, log income, state-capital status, and municipality-level urbanization, gender, age, literacy, household composition, and per-capita income. Fixed effects are indicated in the table. Tests of differences use municipality-clustered residual-correlation influence functions, allowing for covariance across samples. Breusch-Pagan statistics equal $N\rho^2$ and are reported descriptively without a dependence adjustment. Reported significance levels are pointwise. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

so the analysis isolates the origination of entirely new card relationships. For each such pair, we measure the change over the subsequent three months in the share of the individual's Pix volume conducted through bank $b$, and we instrument this change with

Table 8: Impact of Pix on Credit Card Origination: Instrumental Variables

| | | | | |
|---|---|---|---|---|
| *Panel A: First stage* | | | | |
| | $\Delta$ Pix Share | | | |
| Cumulative Timeout Exposure | 1.5359*** | | | |
| | (0.0990) | | | |
| Individual × Bank FE | Yes | | | |
| Month FE | Yes | | | |
| Obs. | 672,979 | | | |
| $R^2$ | 0.181 | | | |
| *Panel B: Second stage* | | | | |
| | $t+3$ | $t+6$ | $t+9$ | $t+12$ |
| Fitted $\Delta$ Pix Share | 0.1733*** | 0.1987*** | 0.1416*** | 0.0851*** |
| | (0.0307) | (0.0334) | (0.0335) | (0.0298) |
| Individual × Bank FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| Obs. | 663,162 | 614,176 | 566,194 | 519,984 |
| Mean dep. var. | 0.0307 | 0.0448 | 0.0551 | 0.0630 |
| $R^2$ | 0.372 | 0.480 | 0.557 | 0.618 |

*Note:* This table reports 2SLS instrumental-variable estimates of the effect of Pix usage on the origination of new credit cards. The sample consists of individual–bank–month observations in which individual $i$ does not hold a credit card from bank $b$ in month $t$, so the dependent variables capture new card relationships at the banks toward which the individual reallocates Pix activity. Panel A reports the first-stage regression of the change in the share of individual $i$'s Pix volume conducted through bank $b$ over the subsequent three months on individual $i$'s cumulative exposure to sender-bank Pix timeouts at banks other than $b$ over the preceding four months. Panel B reports regressions of an indicator for whether individual $i$ holds a credit card from bank $b$ at horizons $t+3$, $t+6$, $t+9$, and $t+12$ on the fitted change in Pix share from Panel A. All specifications include month and individual × bank fixed effects. Standard errors are three-way clustered at the individual, bank, and month levels and are displayed in parentheses. *, **, and *** correspond to 10%, 5%, and 1% significance levels, respectively.

the individual's cumulative exposure to sender-bank Pix timeouts at banks other than $b$ over the preceding four months. The logic is one of switching: timeouts disrupt outgoing payments at the banks that currently route the individual's Pix transfers, pushing the individual to reallocate Pix activity toward unaffected banks—in particular, banks with which they have no card relationship—so the first stage should be positive. The specification includes month and individual-by-bank fixed effects, so identification comes from changes over time within the same individual–bank pair. In the second stage, we ask whether the predicted inflow of Pix activity into bank $b$ raises the probability that the individual obtains a credit card from that bank over the following three to twelve months.

Table 8 reports the results. The first stage confirms the switching behavior: individuals exposed to Pix timeouts reallocate a larger share of their Pix activity toward banks that did not experience them. The second stage shows that the banks receiving these payment flows respond by issuing new credit cards. A 10 percentage point increase in the predicted Pix share flowing to a bank raises the probability that the individual obtains a card from that bank by about 1.7 percentage points within three months and 2.0 percentage points within six months. The effect fades gradually but remains positive and significant one year out. These estimates reinforce the extensive-margin evidence in Table 3: when a consumer's Pix activity moves to a bank, which previously had neither a card relationship with the consumer nor data on their payments, becomes substantially more likely to issue them a credit card.

We next turn to the intensive margin and ask whether the same source of variation in Pix usage affects credit-card terms. Table 9 reports the two-stage least-squares estimates using the sender-timeout instrument, separately for the bank that observes the consumer's Pix activity and for other credit-card banks. The results confirm the central provider-specific result using variation in Pix use induced by payment disruptions. At the own bank, the IV estimates show higher limits and balances, lower defaults, and a decrease in interest rates significant at the 10% level. At other banks, the rate estimate is close to zero and not statistically significant, as are the estimates for limits, balances, and default. The difference between the own- and other-bank rate coefficients is not statistically significant. The first-stage statistics support instrument relevance in these baseline specifications. Thus, the IV results both reinforce the baseline estimates and make it less plausible that reverse causality from contemporaneous credit terms to Pix use accounts for the main findings. The own-bank credit-quantity and default results remain significant when the IV sample begins in September 2021 (Appendix Table A.5).

Appendix Table A.18 varies the timeout exposure window between two, three, four, and six months, holding the estimation sample fixed. Own-bank credit-limit estimates remain positive and interest-rate and default estimates remain negative across these

Table 9: Impact of Pix on the Credit Card Terms: Instrumental Variables

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.146** | 0.113** | −15.309* | −0.014*** |
| | (0.073) | (0.055) | (9.026) | (0.004) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 221,730 | 221,730 | 25,812 | 221,730 |
| $R^2$ | 0.176 | 0.182 | −0.022 | −0.012 |
| First-stage F-stat | 177 | 177 | 62 | 178 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.028 | 0.023 | 0.677 | −0.023 |
| | (0.061) | (0.055) | (8.581) | (0.017) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 965,673 | 965,673 | 211,789 | 965,673 |
| $R^2$ | 0.779 | 0.782 | 0.000 | 0.018 |
| First-stage F-stat | 212 | 212 | 139 | 212 |

*Note:* This table reports 2SLS estimates of equation (3). Panel A shows the results for cards issued by the bank through which the depositor uses Pix. Panel B includes only credit banks never used for Pix by the depositor. Each credit record is paired with every observed Pix bank in the borrower-month; observations count pairs. Log Pix Value is instrumented with cumulative sender-timeout exposure: the sum from $t-4$ through $t-1$ of the individual's Pix-value-share-weighted sender-bank timeout rate. Bank, individual, and time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. The first-stage F-statistic is the Kleibergen-Paap rk Wald statistic. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

windows, while the corresponding other-bank estimates are not statistically significant. Appendix Table A.19 excludes the three largest credit-card lenders, ranked separately by pre-Pix assets and early Pix transaction value. The positive own-bank limit estimate and the negative interest-rate and default estimates remain statistically significant under both rankings. Appendix Figure A.2 displays the distributions of own- and other-bank

estimates when each credit-card lender is excluded in turn, with dashed lines marking the corresponding full-sample IV estimates. The results are robust to leaving one bank out at a time.

Appendix Table A.7 applies equal credit-record and equal borrower-month weights to the other-bank IV sample. The estimates for limits, balances, and interest rates remain statistically insignificant under both schemes. With equal credit-record weights, the default estimate remains close to its baseline value but becomes significant at the 5% level as its standard error falls; with equal borrower-month weights, the default estimate is smaller and not statistically significant.

Panel B of Appendix Table A.16 applies the same timeout instrument, controls, and fixed effects to the pre-Pix credit outcomes. None of the four placebo coefficients is statistically significant at the 10% level, and the first-stage statistics range from 29 to 96. This complements the OLS timing placebo with the variation used in the main IV specification.

Appendix Tables A.12, A.13, and **??** report the corresponding IV estimates using Pix quantity, allowing for income heterogeneity, and excluding relationships in which Pix and card use begin together. The quantity specification also yields a decrease in own-bank interest rates significant at the 10% level; the income-interacted IV rate estimates are imprecise and have a weak first stage.

# 4 A structural model of payment-credit complementarities

Our empirical results reveal two forces. First, Pix is a payment substitute. Once available, households can move transactions away from cash, debit, or credit cards. Second, Pix is an information technology: the bank holding a household's Pix account can use the Pix-generated transaction history to screen that household more precisely than a potential lender without the information. The first force can reduce card use and does so

straightforwardly in the case of debit. The second force can change rates, issuer choice, and borrowing, ultimately resulting in greater credit card use. Equilibrium credit use reflects both forces, together with selection into instrument bundles and competition between card issuers.

That a government-created transaction technology like Pix is a net complement to privately issued credit cards is not necessarily a general result and depends on several institutional features. For example, in settings with smaller ex ante information frictions (highly informative public credit registries or better recovery technologies) the value of the produced information may be lower. Alternatively, seemingly subtle institutional design differences around information production can play a large role. For example, if the system limits private-sector access to the data (due to, e.g., privacy concerns or administering the system entirely through the government) the credit complementarity channel is shut off. Alternatively, if the system liberalizes data access (e.g., through mandated sharing to *all* financial institutions) the credit complementarity channel may be amplified.

To quantify the size of competing transaction substitution versus credit complementarity channels, as well as to consider the equilibrium impact of different institutional settings or designs, we build and estimate a structural model of consumer payments and credit. We develop this model below.

## *4.1 Model specification*

We study the equilibrium interaction of households that use various payment and credit technologies to complete transactions and card issuers that provide these products and set interest rates on the credit cards they provide. Households differ privately in liquidity demand, which impacts their desire to transact using credit versus paying in full, and default risk, which determines the cost of extending credit. Card providers receive imperfect signals over the type of the household, with the precision of the signal depending on the provider's relationship to the household, and set signal-contingent rates. Households

select their payment bundle, the signal is revealed, and then engage in transactions given the bundle they selected. We detail each part of the model below.

### *4.1.1 Environment and timing*

The model is static and takes place within a geographic market $m$ at time $t \in \{\text{pre}, \text{post}\}$, where the timing refers to pre- or post-Pix entry. A unit mass of households lives in each market. Each household is characterized by a latent type $k$, with default probability $p_k$ and liquidity demand $\ell_k$. In the baseline, we assume there are two types, $k \in \{1, 2\}$, with

$$p_1 < p_2, \qquad 0 = \ell_1 < \ell_2. \tag{8}$$

Roughly, the type $k = 1$ is the "good" household and type $k = 2$ is the "bad" household. Bad households have greater demand for liquidity and are more likely to default. This ordering generates adverse selection with borrowing demand being positively correlated with default likelihood.

Within market $mt$, unconditional population shares are

$$\pi_{2,mt} = \frac{\exp(X'_{mt}\zeta)}{1 + \exp(X'_{mt}\zeta)}, \qquad \pi_{1,mt} = 1 - \pi_{2,mt}, \tag{9}$$

where $X_{mt}$ contains observed market characteristics and $\zeta$ is a parameter to be estimated.

From each household's perspective, there are two credit card providers. Provider $H$ is the household's *own* bank, meaning that it administers its Pix account. Provider $O$ is a composite *other* bank, which does not administer the Pix account. If it has no Pix account (because it is the pre-Pix period or the household has opted to not include Pix in its transaction bundle), there is no $H$-type provider. We assume that the household has only one Pix account (its main Pix account) but can have a credit card from both types in its payment bundle. Events within a market-period $mt$ occur in the following order:

1. Card issuers commit to signal-contingent rate schedules.

2. Households choose payment bundles.

3. Signals realize and households receive the corresponding rate.

4. Transactions arrive and households select their payment technology for each.

We first describe the payment bundles. We then describe the model in reverse order: household transaction decisions conditional on their payment bundles and credit costs, signals over household type, household payment bundle choice, and finally issuers' pricing decisions.

### *4.1.2 Payment bundles*

Each household selects a payment technology bundle, to which it is committed for the entire period. Cash is available by default. A bundle is

$$b = (b_D, b_P, b_H, b_O) \in \{0,1\}^4, \tag{10}$$

where $D$, $P$, $H$, and $O$ denote debit, Pix, the own-bank card, and the other-bank card, respectively. Other-bank represents a composite option. Debit is required to use any payment technology besides cash,[5] and an "own" card is only meaningfully defined when Pix is in the bundle. This means that the feasible choice set is reduced:

$$\mathcal{B} = \{\mathsf{0}, \mathsf{d}, \mathsf{o}, \mathsf{p}, \mathsf{ph}, \mathsf{po}, \mathsf{pho}\} \tag{11}$$

Before Pix, the feasible set is

$$\mathcal{B}_{\text{pre}} = \{\mathsf{0}, \mathsf{d}, \mathsf{o}\}; \qquad \mathcal{B}_{\text{post}} = \mathcal{B}. \tag{12}$$

Table 10 summarizes the bundles.

[5]More precisely, a bank account is required to use Pix, and a debit card is required to use a credit card. Since most bank accounts come with a debit card, the assumption is plausible.

Table 10: Observed payment bundles and feasible methods

| Bundle | $b_D$ | $b_P$ | $b_H$ | $b_O$ | Feasible payment methods |
|---|---|---|---|---|---|
| 0 | 0 | 0 | 0 | 0 | cash |
| d | 1 | 0 | 0 | 0 | cash, debit |
| o | 1 | 0 | 0 | 1 | cash, debit, other card |
| p | 1 | 1 | 0 | 0 | cash, debit, Pix |
| ph | 1 | 1 | 1 | 0 | cash, debit, Pix, own card |
| po | 1 | 1 | 0 | 1 | cash, debit, Pix, other card |
| pho | 1 | 1 | 1 | 1 | cash, debit, Pix, own and other cards |

Finally, let

$$\mathcal{G}_b = \{H : b_H = 1\} \cup \{O : b_O = 1\} \tag{13}$$

denote the card providers available in bundle $b$.

### *4.1.3 Transaction and financing choices*

**Payment method.** Fix the bundle $b$. After the household's signal is realized and rates are assigned, transactions arrive. For each transaction, the household chooses among its available payment methods, denoted by

$$\mathcal{J}_b = \underbrace{\{0\}}_{\text{cash}} \cup \underbrace{\{D : b_D = 1\}}_{\text{debit}} \cup \underbrace{\{P : b_P = 1\}}_{\text{Pix}} \cup \underbrace{\mathcal{G}_b}_{\text{credit}}. \tag{14}$$

Let $\boldsymbol{s}_b$ collect the signals relevant to setting the interest rate for the household's bundle choice $b$.[6] Systematic indirect utility for each payment option is:

$$v_{0,kbmt}(\boldsymbol{s}_b) = 0, \tag{15}$$

$$v_{D,kbmt}(\boldsymbol{s}_b) = (Z^D_{mt})'\beta_D, \tag{16}$$

$$v_{P,kbmt}(\boldsymbol{s}_b) = (Z^P_{mt})'\beta_P, \tag{17}$$

$$v_{g,kbmt}(\boldsymbol{s}_b) = (Z^C_{mt})'\beta_C + \eta_H \mathbf{1}\{g = H\} + \mathcal{F}_{kgmt}(s_g), \qquad g \in \mathcal{G}_b. \tag{18}$$

[6] As we describe below, each credit card provider receives a signal. With no credit cards, the signal is degenerate. With one credit card, there is one signal. With two credit cards, there are two.

Super- and subscripts $\{0, D, P, g\}$ denote the payment technology. Subscripts $kbmt$ denote type $k$ with bundle $b$ in geography $m$ at time $t$. $\{\beta_D, \beta_P, \beta_C\}$ are technology-specific utility loadings on local shifters $\{Z^D_{mt}, Z^P_{mt}, Z^C_{mt}\}$ that include, beyond constants, market-specific characteristics such as merchants' technology acceptance rates, and local demographics that drive preferences over transaction technologies. $\eta_H$ is the household's preference for using its own-bank card rather than another card, conditional on availability and credit terms. $\mathcal{F}_{kgmt}(s_g)$ is the inclusive value of the pay-in-full versus financing option, which we discuss in the next section.

Beyond the systematic components of indirect utility, households receive preference shocks over the transaction technology drawn i.i.d. each time a transaction arrives. We assume these follow a nested-logit structure, with debit and Pix belonging to a common bank-account-funded nest,

$$\mathcal{A}_b = \{D : b_D = 1\} \cup \{P : b_P = 1\}, \tag{19}$$

the idea being that the preference shocks are in part driven by funding availability at the time of the transaction and that debit and Pix draw from identical sources. Other transaction technologies belong to singleton nests. The dissimilarity parameter for the debit-Pix nest is $\lambda_A \in (0, 1]$.[7] For the cash-only bundle, $\mathcal{A}_b = \varnothing$, and the account-funded nest is omitted from $\mathcal{N}_b$.

Under the logit assumption, the account-nest inclusive value (i.e., the ex ante expected utility of selecting this nest) is

$$I^A_{kbmt}(\boldsymbol{s}_b) = \lambda_A \log \left[ \sum_{j \in \mathcal{A}_b} \exp\left( \frac{v_{j,kbmt}(\boldsymbol{s}_b)}{\lambda_A} \right) \right]. \tag{20}$$

Cash has inclusive value zero, and each card provider $g$ has singleton inclusive value $v_{g,kbmt}(\boldsymbol{s}_b)$. Given the bundle, let $\mathcal{N}_b$ collect the nonempty nests. The transaction inclu-

[7] A smaller $\lambda_A$ makes debit and Pix closer substitutes. Setting $\lambda_A = 1$ reduces the transaction problem to multinomial logit.

sive value is

$$\mathcal{I}_{kbmt}(\boldsymbol{s}_b) = \log\left[\sum_{n\in\mathcal{N}_b} \exp\{I^n_{kbmt}(\boldsymbol{s}_b)\}\right]. \tag{21}$$

For method $j$ in nest $n(j)$, the transaction probability is

$$q_{j|kbmt}(\boldsymbol{s}_b) = \underbrace{\frac{\exp\{I^{n(j)}_{kbmt}(\boldsymbol{s}_b)\}}{\sum_{n\in\mathcal{N}_b}\exp\{I^n_{kbmt}(\boldsymbol{s}_b)\}}}_{\text{nest share}} \times \underbrace{\frac{\exp\{v_{j,kbmt}(\boldsymbol{s}_b)/\lambda_{n(j)}\}}{\sum_{a\in n(j)}\exp\{v_{a,kbmt}(\boldsymbol{s}_b)/\lambda_{n(j)}\}}}_{\text{within-nest share}}, \tag{22}$$

where $\lambda_n = 1$ for singleton nests.

**Financing conditional on card use.** Conditional on using provider $g \in \mathcal{G}_b$ for a card transaction, a type-$k$ household chooses between paying in full, $F$, and borrowing, $B$. Systematic indirect utilities for paying in full and borrowing are, respectively,

$$u^F_{kgmt}(s_g) = 0 \tag{23}$$

$$u^B_{kgmt}(s_g) = \alpha_B + \ell_k - \gamma r_{gmt}(s_g) \qquad \gamma > 0. \tag{24}$$

$u^F_{kgmt}(s_g) = 0$ is a normalization as the baseline level of credit card use is absorbed into the constant term in $\beta_C$, above. Borrowing provides a baseline utility $\alpha_B$, and an incremental liquidity value $\ell_k$ that depends on the borrower type.[8] $r_{gmt}(s_g)$ is the assigned annual borrowing rate, and $\gamma > 0$ is the household's disutility from rates.

As above, we assume a per-transaction i.i.d. logit shock to the systematic value of borrowing. Under this assumption, the conditional financing probability $\omega^B_{kgmt}$ and inclusive value $\mathcal{F}_{kgmt}$ are

$$\omega^B_{kgmt}(s_g) = \frac{\exp[u^B_{kgmt}(s_g)]}{1+\exp[u^B_{kgmt}(s_g)]}, \tag{25}$$

$$\mathcal{F}_{kgmt}(s_g) = \log\{1+\exp[u^B_{kgmt}(s_g)]\}. \tag{26}$$

[8]Recall that $\ell_1 = 0$ and $\ell_2 > 0$ means that this term captures the incremental value of borrowing for the bad type.

Thus, card transaction shares (for paid-in-full versus borrowing and own versus other) are

$$q^{F}_{g|kbmt}(\boldsymbol{s}_b) = q_{g|kbmt}(\boldsymbol{s}_b)[1 - \omega^{B}_{kgmt}(s_g)], \tag{27}$$

$$q^{B}_{g|kbmt}(\boldsymbol{s}_b) = q_{g|kbmt}(\boldsymbol{s}_b)\omega^{B}_{kgmt}(s_g), \qquad g \in \mathcal{G}_b. \tag{28}$$

### *4.1.4 Information environment*

Card issuers do not directly observe household type. Provider $O$ receives a binary baseline signal $s_O \in \{1, 2\}$. For a type-$k$ household, the signal distribution is

$$h_O(s \mid k) = \begin{cases} \rho_0, & s = k, \\ 1 - \rho_0, & s \neq k, \end{cases} \qquad \rho_0 \in [1/2, 1]. \tag{29}$$

The bank holding the household's Pix account, provider $H$, receives a more precise binary signal with distribution

$$h_H(s \mid k) = \begin{cases} \rho_H, & s = k, \\ 1 - \rho_H, & s \neq k, \end{cases} \qquad \rho_H = \rho_0 + \Delta\rho, \qquad \Delta\rho \in [0, 1 - \rho_0]. \tag{30}$$

For a dual-card household, the two signals are correlated. Conditional on household type $k$, first draw $s_H$ according to $h_H(\cdot \mid k)$. Provider $O$'s signal is then a garbled version of $s_H$:

$$\Pr(s_O \neq s_H \mid s_H, k) = \begin{cases} \dfrac{\rho_H - \rho_0}{2\rho_H - 1}, & \rho_H > 1/2, \\ 0, & \rho_H = \rho_0 = 1/2. \end{cases} \tag{31}$$

Let $h_b(\boldsymbol{s}_b \mid k)$ denote the joint distribution of signals relevant to bundle $b$, conditional on household type $k \in \{1, 2\}$,[9] and for any issuer $g \in \mathcal{G}_b$, let $h_g(s_g \mid k)$ denote the corresponding marginal distribution of issuer $g$'s signal. With $\sigma_{b|kmt}$ denoting bundle choice probability, defined in the next section, issuer $g$'s posterior probability that the household is type $k$ is

$$\mu_{g,mt}(k \mid s_g) = \frac{\pi_{k,mt}\left[\sum_{b:g\in\mathcal{G}_b} \sigma_{b|kmt}\right] h_g(s_g \mid k)}{\sum_{a=1}^{2} \pi_{a,mt}\left[\sum_{b:g\in\mathcal{G}_b} \sigma_{b|amt}\right] h_g(s_g \mid a)}. \tag{32}$$

Thus, the posterior depends on the population prior $\pi_{k,mt}$, endogenous selection into bundles containing issuer $g$, and the information contained in issuer $g$'s signal. Notice, the issuer does not observe the household's entire bundle. Rather, it observes only its own relationship with the household and integrates over the set of bundles containing its card. Because bundle probabilities depend on anticipated rates, information, issuer selection, and pricing are jointly determined in equilibrium.

#### *4.1.5 Bundle choice*

Households derive indirect utility from their bundle choice. This indirect utility has a standalone component, and a component that depends on the expected transaction value of the bundle. Let $\mathcal{J} = \{D, P, H, O\}$ denote the set of non-cash payment instruments. The systematic standalone value of bundle $b$ is

$$A_{bmt} = \sum_{j\in\mathcal{J}} b_j (W_{mt}^j)' \beta_j^A, \tag{33}$$

where $W_{mt}^j$ includes a constant and other market-time specific demand shifters. The transaction inclusive values are derived above as a function of rates. Because issuer signals realize after the bundle choice, the household takes an expectation over signal

[9] The distribution is degenerate for bundles without cards, equals the appropriate single-issuer signal distribution for single-card bundles, and follows equations (29)–(31) for the dual-card bundle.

realizations for each bundle to arrive at the expected inclusive value.

$$\overline{\mathcal{I}}_{kbmt} = \sum_{\boldsymbol{s}_b} h_b(\boldsymbol{s}_b \mid k)\mathcal{I}_{kbmt}(\boldsymbol{s}_b). \tag{34}$$

The deterministic bundle value is then

$$V_{kbmt} = A_{bmt} + \kappa\overline{\mathcal{I}}_{kbmt}, \tag{35}$$

where, as described above, parameter $\kappa$ combines discounted transaction frequency with the relative scale of bundle-choice shocks. Finally, we assume an i.i.d. logit shock over each bundle, which yields the conditional bundle choice probability:

$$\sigma_{b|kmt} = \frac{\exp(V_{kbmt})}{\sum_{a\in\mathcal{B}_t}\exp(V_{kamt})}. \tag{36}$$

#### *4.1.6 Credit pricing*

Issuers set signal-contingent rates in a simultaneous Bertrand-Nash equilibrium. Let $r_{Hmt}(s_H)$ and $r_{Omt}(s_O)$ denote the own and other issuer rate schedules, respectively. A paid-in-full transaction yields provider $g$ net revenue $\phi^F_{gmt}$, representing net interchange income. A borrowing transaction with a type-$k$ household yields expected margin

$$M^B_{gkmt}(s_g) = \underbrace{\phi^B_{gmt}}_{\text{interchange}} + \underbrace{(1-p_k)r_{gmt}(s_g)}_{\text{interest income}} - \underbrace{c^f_{gmt}}_{\text{funding cost}} - \underbrace{p_k\Lambda_g}_{\text{loss on default}}, \tag{37}$$

where $c^f_{gmt}$ is the funding and production cost and $\Lambda_g$ is loss given default.

At the bundle origination stage, the issuer earns a fixed net revenue per account (potentially negative), $\chi^C_{gmt}$. Let $\overline{n}_{mt}$ denote the number or dollar-equivalent scale of transaction opportunities per household in the profit calculation. Expected profit from provider $g$'s card for a type-$k$, bundle-$b$ household after signals $\boldsymbol{s}_b$ is

$$\pi^L_{gkbmt}(\boldsymbol{s}_b) = \chi^C_{gmt} + \overline{n}_{mt}q_{g|kbmt}(\boldsymbol{s}_b)\Big\{[1-\omega^B_{kgmt}(s_g)]\phi^F_{gmt} + \omega^B_{kgmt}(s_g)M^B_{gkmt}(s_g)\Big\}, \tag{38}$$

where $q_{g|kbmt}(\boldsymbol{s}_b)$ and $\omega^B_{kgmt}$ are the transaction and financing probabilities derived above, respectively. Finally, issuer $g$'s expected market profit is

$$\Pi_{gmt}(\boldsymbol{r}) = N_{mt} \sum_k \pi_{k,mt} \sum_{b:g\in\mathcal{G}_b} \sigma_{b|kmt} \sum_{\boldsymbol{s}_b} h_b(\boldsymbol{s}_b \mid k) \pi^L_{gkbmt}(\boldsymbol{s}_b), \tag{39}$$

where $N_{mt}$ is market size and $\boldsymbol{r}$ collects both issuers' schedules. Each issuer takes the rival schedule as given and maximizes expected profit:

$$\boldsymbol{r}^*_{gmt} \in \arg\max_{\boldsymbol{r}_{gmt}} \Pi_{gmt}(\boldsymbol{r}_{gmt}, \boldsymbol{r}^*_{-g,mt}). \tag{40}$$

#### *4.1.7 Equilibrium*

An equilibrium in market-time $mt$ consists of rate schedules $\boldsymbol{r}^*_{mt}$, bundle choice probabilities $\sigma_{b|kmt}$, payment choice probabilities $q_{j|kbmt}(\boldsymbol{s}_b)$, and financing choices $\omega^B_{kgmt}(s_g)$ such that

1. Financing choices satisfy equation (25);
2. Payment choices satisfy equation (22);
3. Bundle choices satisfy equation (36);
4. Rates satisfy the profit maximization problem in equation (40).

Given the population type shares and equilibrium bundle choices, lender beliefs are mechanically determined by Bayes' rule in equation (32). Observe that rates affect bundle adoption, card use, and financing, and these choices feed back into each issuer's customer pool. Thus, the equilibrium is a fixed point in rates, selection, and household demand.

Before taking the model to the data, we highlight the main mechanism. Setting $\Delta\rho = 0$ (and removing the own-card preferences $\eta_H$) shuts down the informational advantage associated with Pix. Pix then affects credit card use only through payment substitution. As Pix is a substitute for credit payment, the prediction is that Pix reduces card bundling

and transaction use. Allowing $\Delta\rho > 0$ adds screening, endogenous pricing, and selection into Pix use, and potentially increases credit use through a lender screening and pricing channel. The model does not have a data-free prediction on the net effect of Pix. Rather, the estimation quantifies which force dominates. Finally, counterfactuals that vary data access change which lenders (if any) observe the refined signal and therefore impact competition, rates, credit volume, and default.

## *4.2 Estimation*

We first describe the data we use to estimate the model and how the data maps to the model. We then describe the actual estimation procedure.

### *4.2.1 Data*

We estimate the model using a municipality-by-period panel, constructed from the merchant transaction data, credit registry, and individual Pix transactions. Due to sparse observations in some municipalities, we collapse the time dimension to two periods, *pre-Pix* and *post-Pix*. For each municipality-period row, we observe the bundle shares among the seven mutually exclusive bundles shown in Table 10, $S^{A,obs}_{b,mt}$, where $b$ indexes bundle types and distinguishes own- from other-bank cards. We also observe the unconditional transaction-value shares for cash, debit, Pix, and the four card outcomes, $S^{T,obs}_{j,mt}$, where $j$ indexes over transaction technologies. The unconditional shares distinguish own versus other, and paid-in-full versus financed transactions. We also observe average interest rates and default rates, $r^{obs}_{mt}$ and $d^{obs}_{mt}$. Note that these are not broken out by own versus other issuer.

Beyond bundle shares, transaction shares, and interest and default rates, we observe various demographic information at the $mt$ level, including population, income per capita, unemployment rate, share of workers in the informal sector, the rural versus urban share, the male versus female share, the age distribution, and the literacy rate. We observe economic information on the share of merchants accepting debit, Pix, and credit

cards, and supply-side rate shifters which we describe below. Collectively we denote these $mt$ variables as $Z_{mt}$, and use various subsets at different points in the estimation as instruments and demand shifters.

The data are aggregated at various levels. We do not see hidden type-specific shares, so all model-predicted outcomes must be appropriately integrated over the hidden type distribution implied by whatever parameters we are estimating. Additionally, as we do not observe issuer-specific interest or default rates, model-predicted rates must be aggregated by the observed or model-implied issuer shares. Finally, we observe unconditional transaction shares rather than transaction shares within a bundle, and so model-implied bundle-specific transaction shares must be integrated over observed or implied bundle shares. We define the corresponding model-implied aggregates below before specifying the moment conditions.

### *4.2.2 Estimation*

**Externally calibrated parameters.** We calibrate the parameters governing lender supply, $\chi^C_{gmt}$, $\phi^a_{gmt}$, $a \in \{F, B\}$, $c^f_{gmt}$, and $\Lambda_g$ externally to the model. This leaves the parameters governing demand, household types, and lender information, to be estimated from the data.

**Estimated parameters.** We partition the parameter space as

$$\begin{aligned}
\theta &= (\theta_F, \theta_T, \theta_A, \theta_\pi, \theta_I) \in \Theta, \\
\theta_F &= (\alpha_B, \gamma, \ell_2, p_1, p_2) && \text{(financing demand)}, \\
\theta_T &= (\beta_D, \beta_P, \beta_C, \eta_H, \lambda_A) && \text{(transaction demand)}, \\
\theta_A &= (\beta^A_D, \beta^A_P, \beta^A_H, \beta^A_O, \kappa) && \text{(bundle demand)}, \\
\theta_\pi &= \zeta && \text{(types)}, \\
\theta_I &= (\rho_0, \Delta\rho) && \text{(information)}.
\end{aligned} \tag{41}$$

**Model-implied aggregates.** For each candidate parameter vector, solving the model yields type-specific bundle probabilities, transaction probabilities, financing probabilities, and equilibrium rates described above. We aggregate these theoretical objects to match the municipality-period outcomes observed in the data. The model-implied share of households choosing bundle $b$ is

$$S^A_{bmt}(\theta) = \sum_{k=1}^{2} \pi_{k,mt}\sigma_{b|kmt}, \qquad b \in \mathcal{B}_t. \tag{42}$$

Next, let

$$\begin{aligned} \mathcal{J}^T_{\text{pre}} &= \{0, D, O^F, O^B\}, \\ \mathcal{J}^T_{\text{post}} &= \{0, D, P, H^F, H^B, O^F, O^B\} \end{aligned} \tag{43}$$

denote the transaction outcomes observed in the pre- and post-Pix periods. For cash, debit, and Pix, the model-implied unconditional transaction share is

$$S^T_{j,mt}(\theta) = \sum_{k=1}^{2} \pi_{k,mt} \sum_{\substack{b\in\mathcal{B}_t \\ j\in\mathcal{J}_b}} \sigma_{b|kmt} \sum_{\boldsymbol{s}_b} h_b(\boldsymbol{s}_b \mid k) q_{j|kbmt}(\boldsymbol{s}_b), \qquad j \in \{0, D, P\} \cap \mathcal{J}^T_t. \tag{44}$$

For issuer $g \in \{H, O\}$ and financing outcome $a \in \{F, B\}$, the corresponding card transaction share is

$$S^T_{g^a,mt}(\theta) = \sum_{k=1}^{2} \pi_{k,mt} \sum_{\substack{b\in\mathcal{B}_t \\ g\in\mathcal{G}_b}} \sigma_{b|kmt} \sum_{\boldsymbol{s}_b} h_b(\boldsymbol{s}_b \mid k) q^a_{g|kbmt}(\boldsymbol{s}_b). \tag{45}$$

Finally, because the observed interest and default rates are not issuer-specific, we aggregate them across types, bundles, issuers, and signals. Define the financed-transaction mass associated with issuer $g$, type $k$, bundle $b$, and signal vector $\boldsymbol{s}_b$ as

$$\xi^B_{gkbmt}(\boldsymbol{s}_b; \theta) = \pi_{k,mt}\sigma_{b|kmt} h_b(\boldsymbol{s}_b \mid k) q^B_{g|kbmt}(\boldsymbol{s}_b). \tag{46}$$

The model-implied aggregate interest and default rates are then

$$\begin{bmatrix} r_{mt}(\theta) \\ d_{mt}(\theta) \end{bmatrix} = \frac{\sum_{k,b,g,\boldsymbol{s}_b} \xi^B_{gkbmt}(\boldsymbol{s}_b;\theta) \begin{bmatrix} r_{gmt}(s_g) \\ p_k \end{bmatrix}}{\sum_{k,b,g,\boldsymbol{s}_b} \xi^B_{gkbmt}(\boldsymbol{s}_b;\theta)}. \tag{47}$$

(48)

**Moment conditions.** Due to the fixed-point nature of the equilibrium and the fact that our data is aggregated at various levels, we must estimate all parameters of the model simultaneously. Define

$$e^A_{bmt}(\theta) = S^{A,\text{obs}}_{bmt} - S^A_{bmt}(\theta), \qquad b \in \mathcal{B}_t \setminus \{0\}, \tag{49}$$

$$e^T_{jmt}(\theta) = S^{T,\text{obs}}_{jmt} - S^T_{jmt}(\theta), \qquad j \in \mathcal{J}^T_t \setminus \{0\}, \tag{50}$$

$$e^C_{mt}(\theta) = \begin{bmatrix} r^{\text{obs}}_{mt} - r_{mt}(\theta) \\ d^{\text{obs}}_{mt} - d_{mt}(\theta) \end{bmatrix}. \tag{51}$$

Here, $S^A_{bmt}(\theta)$ and $S^T_{jmt}(\theta)$ are the model-predicted bundle and transaction shares aggregated to the municipality-period level, and $r_{mt}(\theta)$ and $d_{mt}(\theta)$ are the corresponding model-predicted aggregate interest and default rates. Let $e^A_{mt}(\theta)$ and $e^T_{mt}(\theta)$ denote the vectors obtained by stacking the bundle-share and transaction-share residuals, respectively. Let $Q^A_{mt}$, $Q^T_{mt}$ and $Q^C_{mt}$ denote the instrument vectors for the bundle, transaction, and credit moments. These vectors contain constants and the relevant subsets of the observed market characteristics and supply-side rate shifters. The sample moment vector is

$$g_M(\theta) = \frac{1}{\sum_{m,t} w_{mt}} \sum_{m,t} w_{mt} \begin{bmatrix} Q^A_{mt} \otimes e^A_{mt}(\theta) \\ Q^T_{mt} \otimes e^T_{mt}(\theta) \\ Q^C_{mt} \otimes e^C_{mt}(\theta) \end{bmatrix}, \tag{52}$$

where $w_{mt}$ are municipality-time weights (e.g., population weights). The GMM estimator is

$$\widehat{\theta} = \arg\min_{\theta \in \Theta} g_M(\theta)' \Omega_M g_M(\theta), \tag{53}$$

where $\Omega_M$ is a feasible weighting matrix.

While the final estimation procedure solves the model for each candidate parameter vector $\tilde{\theta}$ and calculates the moment conditions, it is instructive to discuss identification as though the model were estimated sequentially. Additionally, in practice, we implement the estimation sequentially to produce a preliminary estimate and refine it at the end simultaneously, taking into account the full fixed point nature of the equilibrium. We describe the sequence below.

**1. Issuer-specific financing and the provisional type distribution.** We first posit a reduced-form composition among issuer-$g$ card transactions as,

$$\widetilde{\pi}^{C}_{2,gmt} = \frac{\exp(X'_{mt}\widetilde{\zeta}_g)}{1 + \exp(X'_{mt}\widetilde{\zeta}_g)}. \tag{54}$$

This is a reduced-form analog of (9). $\tilde{\zeta}_g$ is not the same as the structural parameter $\zeta$ as it does not yet account for selection into card choice. A candidate $\tilde{\theta}_F$ yields a share prediction $\tilde{\omega}^{B}_{g,mt}$, and an aggregate default prediction $\tilde{d}_{mt}$. The instruments are market-level economic variables that shift financing demand and default likelihood, together with supply-side rate shifters. This stage yields a provisional estimate of $\theta_F$ and the issuer-specific reduced-form composition parameters $\{\tilde{\zeta}_g\}_g$. It does not identify the structural parameter $\zeta$ as issuer-specific composition at this stage does not correct for endogenous selection into card ownership.

**2. Payment method and issuer use.** The provisional estimate in the previous step allows us to calculate the type-issuer specific inclusive value of financing, $\mathcal{F}_{kgmt}$. Additionally, using observed bundle shares, a candidate $\tilde{\theta}_T$ yields predicted unconditional transaction shares and consequently transaction share residuals. Instruments are as

above, yielding a provisional estimate of $\theta_T$.

**3. Bundle adoption.** The provisional estimates of $\theta_F$ and $\theta_T$, together with $\{\tilde{\zeta}_g\}_g$ allow us to calculate the inclusive value of a bundle for each type and bundle. Together with a candidate $\tilde{\theta}_A$, we are able to calculate predicted bundle shares and consequently bundle share residuals. Instruments are as above. This step yields a provisional estimate of $\theta_A$, and with it, a fully (though provisionally) estimated demand system.

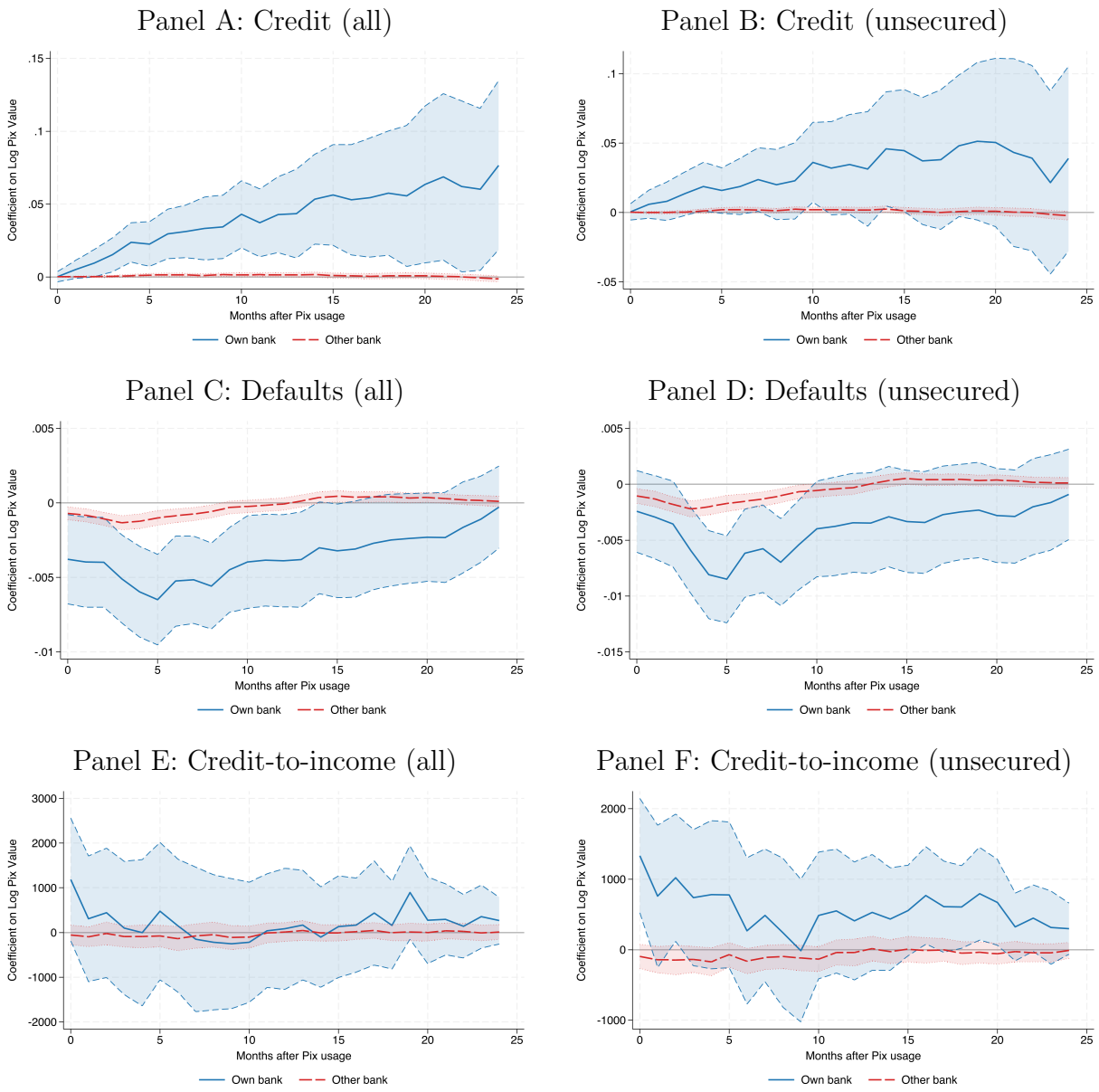
**4. Information and strategic pricing.** We are left with $\rho_0$ and $\Delta\rho$ to be estimated. In this stage, we use moments from the reduced-form results (e.g., changes in credit card takeup, rates, and default, pre- and post-Pix) to discipline the parameters. Broadly, the level of interest rates helps to discipline $\rho_0$, and the pre- versus post-Pix changes, together with "own" versus "other" differences, help to discipline $\Delta\rho$.

**5. Full equilibrium and joint refinement.** Finally, we reestimate the entire model, using the provisional estimates from Steps 1–4 as the initial guess. For each $\tilde{\theta}$, we recompute the full equilibrium and calculate all inputs into the moment conditions.

# 5 Additional results and robustness

In this section, we present additional results and robustness tests. Figure 5 asks whether the contemporaneous credit-card results represent a durable expansion of debt or simply a reshuffling of credit across lenders. We estimate local projections of future credit, defaults, and credit-to-income ratios on current Pix usage, separately for credit exposures at the Pix bank and at other banks. The increase in credit is concentrated at the Pix bank and is visible both for total credit and for unsecured credit, while the corresponding effects for other banks are small and generally centered around zero. Own-bank default estimates remain negative, while other-bank default effects are small and become positive at some intermediate horizons for total credit. Credit-to-income ratios do not show a comparable increase outside the Pix-bank relationship. This evidence is consistent with

Figure 5: Long-Term Impact on Credit and Defaults




*Note:* This figure shows local projection estimates of the effect of Pix usage on future credit and default outcomes. Panels A and B plot the effect on total credit, separately for all credit and unsecured credit. Panels C and D plot the effect on defaults. Panels E and F plot the effect on credit-to-income ratios. Blue solid lines correspond to credit at the bank through which the individual uses Pix, and red dashed lines correspond to credit at other banks. Own-bank projections retain bank and individual-time FEs; other-bank projections include bank, individual, and time FEs separately. Shaded areas and dashed confidence intervals report pointwise 95% confidence intervals clustered at the municipality level.

a persistent relaxation of credit at the informed lender, rather than a broad deterioration in household indebtedness across all lenders.

Table 11 provides a direct test of whether the main results simply reflect cross-

Table 11: Pix Allocation across Multiple Banks and Credit Terms

| | Credit limit (1) | Credit balance (2) | Interest rate (3) | Default (4) |
|---|---|---|---|---|
| Pix Share at Card Bank | 0.016** | 0.015** | −4.822** | −0.000 |
| | (0.008) | (0.007) | (2.039) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 67,254 | 67,254 | 1,574 | 67,254 |
| $R^2$ | 0.828 | 0.595 | 0.737 | 0.612 |

*Note:* This table reports OLS estimates relating the share of borrower $i$'s Pix value routed through card bank $b$ in month $t$ to credit terms at that bank. The sample is restricted to borrower-months with Pix transactions at two or more banks, so each bank in the comparison observes Pix activity and can cross-sell credit products. Bank and individual-time FEs are included. The dependent variables, controls, and outcome-specific sample restrictions are as in Table 4. The instrumental variable is absorbed by the individual-time FEs, so the table reports OLS estimates only. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

selling by banks that offer Pix. We restrict the sample to borrower-months in which the borrower uses Pix at two or more banks and calculate each bank's share of the borrower's Pix value. Individual-time FEs compare credit terms across banks for the same borrower in the same month. A 10 percentage point larger Pix share predicts 1.6% higher limits, 1.5% higher balances, and lower interest rates, with no increase in default. Because every bank in the comparison already observes some Pix activity and can market credit products to the borrower, the results cannot be explained solely by the existence of a Pix account or a general propensity to cross-sell. Instead, credit terms vary with the intensity of transaction activity observed by the specific bank, as the information channel predicts.

One concern with interpreting the credit card results as an information channel is that banks may bundle Pix and credit cards, opening both products for the same customer at the same time. In that case, the estimated effect of Pix could partly reflect a joint product-offering decision rather than the use of Pix transaction information to update credit terms. To address this concern, Table 12 excludes observations where Pix and a credit card at the same bank are first used in the same month. The results are robust:

Table 12: Impact of Pix on the Credit Card Terms: Excluding Bundled Payment Methods

| | Credit limit | Credit balance | Interest rate | Default |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.042** | 0.043*** | −5.160 | −0.001 |
| | (0.017) | (0.015) | (3.937) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual-Time FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 75,232 | 75,232 | 1,361 | 75,232 |
| $R^2$ | 0.809 | 0.585 | 0.741 | 0.649 |

*Note:* This table shows the results from estimating equation (3). We exclude observations where Pix and a credit card at the same bank are first used in the same month. The sample contains borrower–card-bank–months in which the card bank observes the borrower's Pix activity. Bank and individual-time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Pix usage remains associated with higher credit limits and higher credit card balances, while there is no evidence of higher defaults. Appendix Table A.11 shows that the results are similar when we apply a more conservative lagged exclusion around the start of Pix usage.

Next, we ask whether the effect of Pix depends on how much prior information the bank already has about the borrower. Table 13 interacts Pix usage with the length of the pre-existing borrower-bank relationship. The results are consistent with a signaling interpretation: Pix has a stronger positive effect on credit limits when the bank has a shorter prior relationship with the borrower. The interest-rate coefficients, including the relationship-length interaction, are not statistically significant but they point in the right direction. In other words, Pix is most valuable precisely when the bank has less pre-existing soft information, which reinforces the interpretation that Pix helps lenders learn about borrower quality.

Finally, Appendix Table A.3 aggregates the data to the quarterly level to assess whether the main estimates are driven by monthly timing or transitory high-frequency variation in Pix use and credit terms. The results remain similar: higher Pix usage is

Table 13: Impact of Pix on the Credit Card Terms: Relationship Length

| | Credit limit (1) | Credit balance (2) | Interest rate (3) | Default (4) |
|---|---|---|---|---|
| Log Pix Value | 0.142*** | −0.052 | −18.187 | −0.002 |
| | (0.053) | (0.048) | (14.415) | (0.003) |
| Log Relationship Length | 0.200 | −0.056 | −45.202 | 0.000 |
| | (0.123) | (0.097) | (30.061) | (0.005) |
| Log Pix Value × Log Relationship Length | −0.029* | 0.024* | 3.513 | 0.000 |
| | (0.015) | (0.014) | (3.530) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual-Time FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 84,112 | 84,112 | 1,572 | 84,112 |
| $R^2$ | 0.812 | 0.587 | 0.737 | 0.637 |

*Note:* This table shows the results from estimating equation (3), where Pix usage is interacted with the log length of the borrower-bank relationship. Relationship length is measured as the log of one plus the median number of months since the beginning of the relationship between the borrower and the credit card bank. The sample is restricted to the bank through which the depositor uses Pix. Bank and individual-time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

associated with larger credit limits and balances at the own bank and lower defaults, while the corresponding effects at other banks are much smaller for credit quantities and defaults. This pattern supports an information effect within the Pix-bank relationship rather than mechanical month-to-month fluctuations in credit reporting.

# 6 Conclusion

This paper studies how a low-cost instant payment system affects an incumbent payment instrument that also provides credit. The launch of Pix in Brazil is followed by substantial increases in credit-card issuance, transaction volume, financed spending, and installment use, while debit-card activity does not exhibit a comparable expansion. Rewards and interchange income change little, and merchants frequently offer discounts for Pix and cash. These associations do not appear to reflect issuers making card payments more

attractive. Instead, Pix and credit cards become complements because cards provide unsecured credit while Pix generates information that supports its provision.

The linked credit registry and transaction data allow us to identify where this information is used. Comparing the same consumer across banks in the same month, we find that greater Pix activity predicts card issuance, higher limits and balances, and better terms at the bank that observes the transactions, but not at other banks. The additional credit is not accompanied by higher default, and the observing bank becomes better able to predict repayment. The results survive exclusions for jointly opened Pix and card relationships, alternative timing and aggregation choices, and an instrumental-variables design based on sender-bank Pix timeouts. They are stronger when the lender has a shorter prior relationship with the borrower, where new information should be most valuable. Longer-horizon estimates show persistent credit expansion at the informed bank without a broad increase in indebtedness or defaults at other lenders.

The results of the paper have important implications for the policy debate surrounding competition between fast payment systems and credit cards. We show that credit-card use need not be displaced and can instead be fueled by the launch of an instant payment system. The broader implication of the results is that payment systems are also information infrastructures. Even when a payment technology is universally available, the data it generates may remain specific to the intermediary observing the transactions. Payment innovation can therefore improve screening and expand credit while strengthening the informational advantage of a customer's payment provider. This channel explains why credit-card use grows despite an alternative that has lower fees for consumers and merchants. Interesting questions for future research are how data portability and open banking allocate these gains; whether payment-based credit expansion improves financial inclusion without increasing long-run debt or default; and how data ownership and privacy shape competition.

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# Internet Appendix

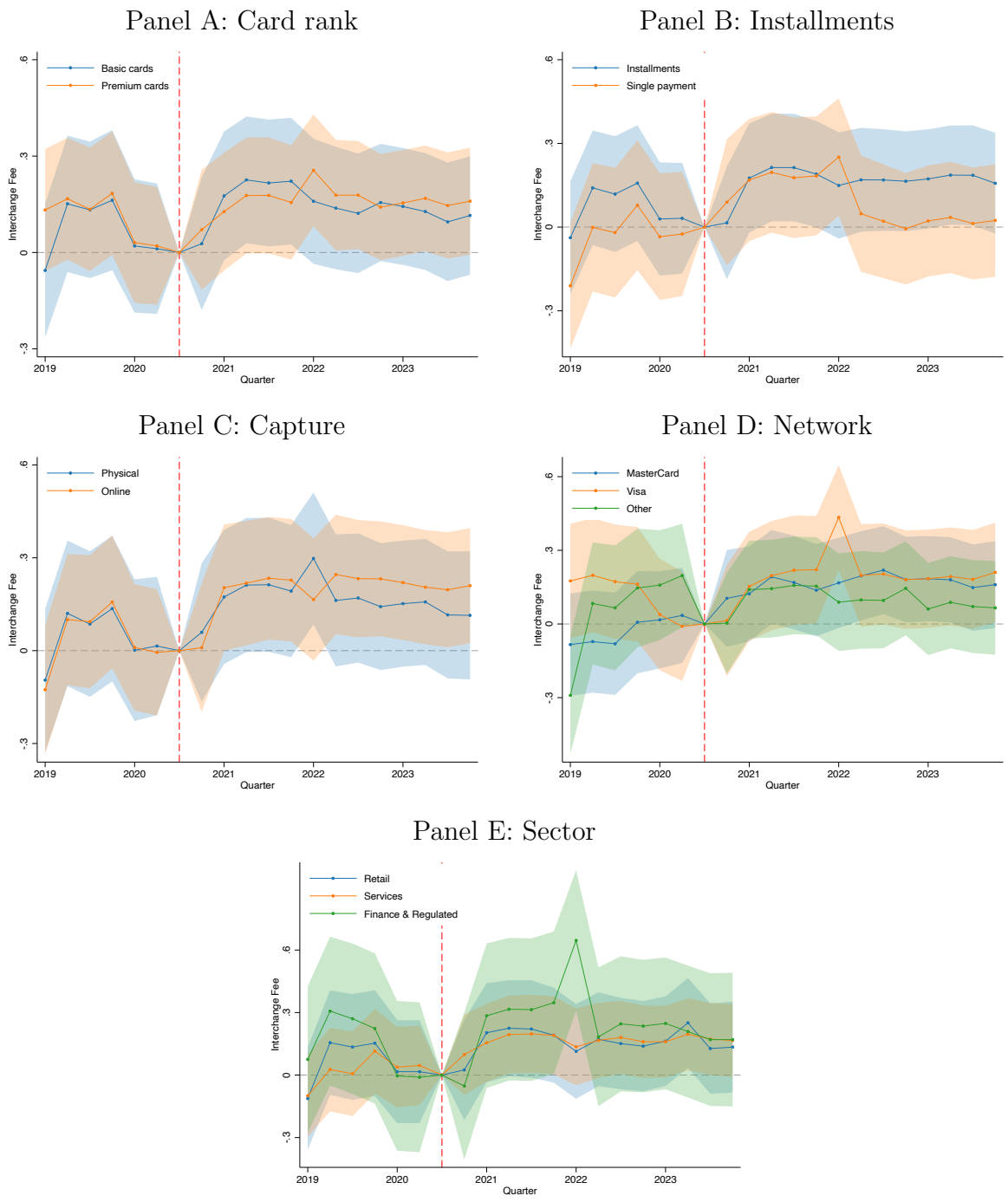
# A Additional tables and figures

Table A.1: Credit Card Installment Payments around the Launch of Pix

| | Number of installments | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Pix | 0.835*** | | 0.815*** | |
| | (0.151) | | (0.110) | |
| Physical | −0.117 | −0.113 | −0.036 | −0.049 |
| | (0.101) | (0.088) | (0.114) | (0.109) |
| Physical × Pix | 0.391** | 0.370** | 0.312** | 0.308** |
| | (0.152) | (0.144) | (0.143) | (0.134) |
| Sample | Full | Full | From 2018 | From 2018 |
| Bank FE | Yes | Yes | Yes | Yes |
| Network FE | Yes | Yes | Yes | Yes |
| Class FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | – | Yes | – |
| Industry × Quarter FE | No | Yes | No | Yes |
| Obs. | 3,446,777 | 3,446,774 | 3,135,669 | 3,135,667 |
| $R^2$ | 0.065 | 0.079 | 0.068 | 0.077 |

*Note:* This table shows the results from regressing the number of installments reported by acquirers on a time dummy for the Pix launch, a physical-payment dummy, and the interaction term. Columns (1) and (2) cover the full sample from 1999, while columns (3) and (4) limit the sample to start in 2018. Bank, network, class, industry, and industry-time FEs are included. Standard errors are clustered at the bank level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Figure A.1: Interchange Fees for Different Cards around the Launch of Pix




*Note:* This figure plots event-time coefficients from equation (1) for interchange fees reported by acquirers, normalized relative to 2020Q3 and split by different levels of heterogeneity. Panel A shows the fees of basic cards (blue line) and premium cards (orange line). Panel B shows the fees for cards used for installment payments (blue line) and cards used for upfront payments (orange line). Panel C shows the fees for physical payments (blue line) and online payments (orange line). Panel D shows the fees for MasterCard cards (blue line), Visa cards (orange line), and all other cards (green line). Panel E shows the fees for the retail sector (blue line), services (orange line), and the financial sector (green line). 95% confidence intervals are also plotted. A vertical dashed red line is the quarter of the launch of Pix.

Table A.2: Credit Card Transaction Volume and Installments around the Launch of Pix

| | Scaled volume | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Installments | −3.353*** | −3.457*** | −1.813*** | −1.887*** |
| | (1.059) | (1.137) | (0.455) | (0.020) |
| Installments × Pix | 3.025*** | 3.138*** | 1.562*** | 1.640*** |
| | (0.977) | (1.058) | (0.405) | (0.021) |
| Physical | 5.568 | 5.271 | 2.502 | 2.340*** |
| | (3.844) | (3.598) | (1.912) | (0.183) |
| Physical × Pix | −4.993 | −4.625 | −2.036 | −1.842*** |
| | (3.752) | (3.460) | (1.766) | (0.198) |
| Sample | Full | Full | From 2018 | From 2018 |
| Bank FE | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | – | Yes | – |
| Network FE | Yes | Yes | Yes | Yes |
| Class FE | Yes | Yes | Yes | Yes |
| Industry FE | Yes | – | Yes | – |
| Industry × Quarter FE | No | Yes | No | Yes |
| Obs. | 3,057,913 | 3,057,910 | 2,951,520 | 2,951,518 |
| $R^2$ | 0.021 | 0.025 | 0.013 | 0.017 |

*Note:* This table shows the results from regressing credit card transaction volume scaled by total payment volume reported by acquirers on a time dummy for the Pix launch, the number of installments, a physical-payment dummy, and the interaction terms. Columns (1) and (2) cover the full sample from 1999, while columns (3) and (4) limit the sample to start in 2018. Bank, quarter, network, class, industry, and industry-time FEs are included. Standard errors are clustered at the bank level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.3: Impact of Pix on the Credit Card Terms: Quarterly Aggregation

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.052*** | 0.042*** | −5.919*** | −0.002** |
| | (0.012) | (0.011) | (1.808) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual-Quarter FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 51,901 | 51,901 | 2,295 | 32,417 |
| $R^2$ | 0.829 | 0.569 | 0.781 | 0.670 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.000 | 0.000 | −0.144 | −0.001** |
| | (0.001) | (0.001) | (0.189) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Quarter FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 519,040 | 519,040 | 147,006 | 364,041 |
| $R^2$ | 0.888 | 0.836 | 0.754 | 0.404 |

*Note:* This table shows the results from estimating equation (3), where the data are aggregated at the quarterly level. Panel A shows the results for the sample where the credit card is issued by the bank through which the depositor uses Pix. Panel B restricts the sample to credit-card banks that are never a Pix bank for the borrower. Each eligible credit record in Panel B is paired with every observed Pix bank for the same borrower and quarter; observations count these pairs. Panel A includes bank and individual-quarter FEs; Panel B includes bank, individual, and quarter FEs separately. Standard errors are clustered at the municipality level and displayed in parentheses. $^{*}$,$^{**}$, and $^{***}$ correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.4: Impact of Pix on the Credit Card Terms: Later Sample

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.032** | 0.048*** | −5.143 | −0.001** |
| | (0.015) | (0.013) | (3.580) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 78,051 | 78,051 | 1,496 | 78,051 |
| $R^2$ | 0.810 | 0.586 | 0.727 | 0.640 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.000 | 0.001 | −0.110 | 0.000 |
| | (0.001) | (0.001) | (0.226) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 914,872 | 914,872 | 197,714 | 914,872 |
| $R^2$ | 0.895 | 0.846 | 0.762 | 0.370 |

*Note:* This table reproduces Table 4 after restricting the estimation sample to September 2021 onward. Panel A covers the borrower's Pix bank; Panel B excludes credit banks ever used for Pix by that borrower, where the lifetime classification is constructed using the full observed history. Each eligible credit record in Panel B is paired with every observed Pix bank for the same borrower and month. Panel A includes bank and individual-time FEs; Panel B includes bank, individual, and month FEs separately. The outcomes, controls, and other sample restrictions follow Table 4. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.5: Impact of Pix on the Credit Card Terms: Instrumental Variables, Later Sample

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.219** | 0.180** | −2.957 | −0.031*** |
| | (0.105) | (0.077) | (15.452) | (0.007) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 206,525 | 206,525 | 23,593 | 206,525 |
| $R^2$ | 0.165 | 0.169 | −0.000 | −0.139 |
| First-stage F-stat | 72 | 73 | 26 | 73 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.076 | −0.034 | 5.154 | −0.032 |
| | (0.079) | (0.070) | (14.155) | (0.026) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 914,872 | 914,872 | 197,714 | 914,872 |
| $R^2$ | 0.778 | 0.784 | −0.004 | −0.002 |
| First-stage F-stat | 95 | 95 | 63 | 94 |

*Note:* This table reproduces Table 9 after restricting the estimation sample to September 2021 onward. Panel A covers the borrower's Pix bank; Panel B includes only credit banks never used for Pix by the borrower, where the lifetime classification is constructed using the full observed history. Log Pix Value is instrumented with cumulative sender-timeout exposure, measured as the sum from $t-4$ through $t-1$ of the individual's Pix-value-share-weighted sender-bank timeout rate. Observations before September 2021 remain available when constructing this lagged instrument. The outcomes, controls, fixed effects, and other sample restrictions follow Table 9. Standard errors are clustered at the municipality level and displayed in parentheses. The first-stage F-statistic is the Kleibergen–Paap rk Wald statistic. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.6: Impact of Pix on the Credit Card Terms: Other-Bank Weighting

| | Panel A: Equal weight per credit record | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.001 | 0.001 | 0.077 | 0.001 |
| | (0.001) | (0.001) | (0.260) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,850 | 966,850 | 212,102 | 966,850 |
| $R^2$ | 0.891 | 0.840 | 0.764 | 0.379 |
| | Panel B: Equal weight per borrower-month | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.000 | 0.001 | 0.145 | 0.000 |
| | (0.001) | (0.001) | (0.264) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,850 | 966,850 | 212,102 | 966,850 |
| $R^2$ | 0.909 | 0.869 | 0.777 | 0.422 |

*Note:* This table reports weighted OLS estimates of equation (3) for the other-bank sample in Panel B of Table 4. Credit banks ever used for Pix by the borrower are excluded. Each eligible credit record is paired with every observed Pix bank in the borrower-month. Panel A assigns each pair weight $1/K$, where $K$ is the number of copies of its underlying credit record; Panel B assigns weight $1/(KR)$, where $R$ is the number of eligible credit records in the borrower-month. Counts are outcome-specific and computed within the baseline estimation sample. For interest rates, the underlying record is a borrower-credit-bank-month; for the other outcomes, it is an original SCR record. The observations, outcomes, and controls are unchanged; reported observations count pairs. Bank, individual, and month FEs are included separately. Standard errors are clustered at the municipality level and displayed in parentheses. $^{*}$,$^{**}$, and $^{***}$ correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.7: Impact of Pix on the Credit Card Terms: Instrumental Variables and Other-Bank Weighting

| | Panel A: Equal weight per credit record | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.018 | 0.014 | 2.010 | −0.022** |
| | (0.034) | (0.030) | (6.064) | (0.011) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 965,673 | 965,673 | 211,789 | 965,673 |
| $R^2$ | 0.771 | 0.774 | 0.000 | 0.019 |
| First-stage F-stat | 308 | 308 | 176 | 306 |
| | Panel B: Equal weight per borrower-month | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.001 | −0.000 | 2.149 | −0.006 |
| | (0.035) | (0.031) | (6.155) | (0.009) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 965,673 | 965,673 | 211,789 | 965,673 |
| $R^2$ | 0.781 | 0.783 | −0.001 | 0.032 |
| First-stage F-stat | 450 | 450 | 190 | 449 |

*Note:* This table reports weighted 2SLS estimates of equation (3) for the other-bank sample in Panel B of Table 9. Panel A assigns each pair weight $1/K$ and Panel B assigns weight $1/(KR)$, where $K$ counts Pix-bank copies of a credit record and $R$ counts credit records in its borrower-month. Counts are computed within each outcome's baseline estimation sample. Record definitions follow Appendix Table A.6. The observations, outcomes, controls, and instrument are unchanged; reported observations count pairs. Log Pix Value is instrumented with cumulative sender-timeout exposure, the sum from $t-4$ through $t-1$ of the individual's Pix-value-share-weighted sender-bank timeout rate. Bank, individual, and month FEs are included separately. Standard errors are clustered at the municipality level and displayed in parentheses. The first-stage F-statistic is the Kleibergen–Paap rk Wald statistic. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.8: Impact of Pix Quantity on the Credit Card Terms

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Quantity | 0.075*** | 0.108*** | −4.623 | −0.004*** |
| | (0.026) | (0.024) | (6.199) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 84,115 | 84,115 | 1,574 | 84,115 |
| $R^2$ | 0.812 | 0.587 | 0.734 | 0.637 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Quantity | −0.001 | 0.000 | −0.202 | 0.006*** |
| | (0.001) | (0.001) | (0.408) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,850 | 966,850 | 212,102 | 966,850 |
| $R^2$ | 0.892 | 0.843 | 0.759 | 0.359 |

*Note:* This table shows the results from estimating equation (3). The Pix variable is a quantity, not a value. Panel A shows the results for the sample where the credit card is issued by the bank through which the depositor uses Pix. Panel B restricts the sample to credit-card banks that are never a Pix bank for the borrower. Each eligible credit record in Panel B is paired with every observed Pix bank for the same borrower and month; observations count these pairs. Panel A includes bank and individual-time FEs; Panel B includes bank, individual, and month FEs separately. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.9: Impact of Pix on the Credit Card Terms: No Individual-Time FEs

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.020*** | 0.018*** | −1.821** | −0.002*** |
| | (0.005) | (0.004) | (0.921) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 222,022 | 222,022 | 25,843 | 222,022 |
| $R^2$ | 0.863 | 0.552 | 0.704 | 0.363 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.001 | 0.001 | −0.113 | 0.000 |
| | (0.001) | (0.001) | (0.221) | (0.000) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,850 | 966,850 | 212,102 | 966,850 |
| $R^2$ | 0.892 | 0.843 | 0.759 | 0.358 |

*Note:* This table shows the results from estimating equation (3). Panel A shows the results for the sample where the credit card is issued by the bank through which the depositor uses Pix. Panel B restricts the sample to credit-card banks that are never a Pix bank for the borrower. Each eligible credit record in Panel B is paired with every observed Pix bank for the same borrower and month; observations count these pairs. Bank, individual, and time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.10: Impact of Pix on the Credit Card Terms: Interactions with Income

| | Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.014 | 0.036*** | −0.008 | −0.002*** |
| | (0.013) | (0.008) | (1.800) | (0.001) |
| High Income | 0.137 | 0.115 | 11.517 | −0.011** |
| | (0.100) | (0.074) | (20.210) | (0.005) |
| Log Pix Value × High Inc | 0.005 | 0.052*** | −5.418** | 0.000 |
| | (0.015) | (0.011) | (2.714) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 187,843 | 187,843 | 23,542 | 187,843 |
| $R^2$ | 0.800 | 0.303 | 0.367 | 0.104 |

*Note:* This table shows the results from estimating equation (3), where we also include interactions with a high-income (above-median) dummy. Income is self-reported in the credit registry. The results are for the sample where the credit card is issued by the bank through which the depositor uses Pix. Bank, individual, and time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.11: Impact of Pix on the Credit Card Terms: Excluding Bundled Payment Methods with a Lag

| | Credit limit | Credit balance | Interest rate | Default |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.042** | 0.042** | −5.027 | −0.001 |
| | (0.020) | (0.019) | (4.863) | (0.001) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual-Time FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 66,202 | 66,202 | 1,185 | 66,202 |
| $R^2$ | 0.805 | 0.584 | 0.741 | 0.656 |

*Note:* This table shows the results from estimating equation (3). We exclude observations where Pix and a credit card at the same bank are first used in the same month or one month apart. The sample contains borrower–card-bank–months in which the card bank observes the borrower's Pix activity. Bank and individual-time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.12: Impact of Pix Quantity on the Credit Card Terms: Instrumental Variables

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Quantity | 0.206** | 0.159** | −24.270* | −0.019*** |
| | (0.102) | (0.077) | (13.808) | (0.006) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 221,730 | 221,730 | 25,812 | 221,730 |
| $R^2$ | 0.179 | 0.186 | −0.022 | −0.005 |
| First-stage F-stat | 190 | 190 | 44 | 190 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Quantity | −0.040 | 0.033 | 0.913 | −0.033 |
| | (0.088) | (0.079) | (11.565) | (0.025) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 965,673 | 965,673 | 211,789 | 965,673 |
| $R^2$ | 0.779 | 0.782 | 0.000 | 0.013 |
| First-stage F-stat | 146 | 146 | 126 | 143 |

*Note:* This table reports 2SLS estimates of equation (3), using Log Pix Quantity as the endogenous variable. It is instrumented with cumulative sender-timeout exposure: the sum from $t-4$ through $t-1$ of the individual's Pix-value-share-weighted sender-bank timeout rate. Panel A shows the own-bank sample and Panel B the sample of credit-card banks that are never a Pix bank for the depositor. Each eligible credit record in Panel B is paired with every observed Pix bank in the borrower-month; observations count pairs. Bank, individual, and time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. The first-stage F-statistic is the Kleibergen-Paap rk Wald statistic. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.13: Impact of Pix on the Credit Card Terms: Instrumental Variables and Income

| | Credit limit (1) | Credit balance (2) | Interest rate (3) | Default (4) |
|---|---|---|---|---|
| Log Pix Value | −0.044 | 0.221** | 78.751 | −0.013* |
| | (0.137) | (0.097) | (79.097) | (0.007) |
| Log Pix Value × High Income | 0.227 | −0.208 | −151.862 | −0.001 |
| | (0.205) | (0.129) | (116.372) | (0.008) |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 187,192 | 187,192 | 22,907 | 187,192 |
| $R^2$ | 0.158 | 0.162 | −0.640 | −0.013 |
| First-stage F-stat | 74 | 75 | 1 | 75 |

*Note:* This table reports 2SLS estimates of equation (3) in the own-bank sample, allowing the effect of Pix to differ by whether self-reported income is above the October 2020 sample median. Log Pix Value and its interaction with High Income are instrumented with cumulative sender-timeout exposure and its interaction with High Income, respectively. Cumulative sender-timeout exposure is the sum from $t-4$ through $t-1$ of the individual's Pix-value-share-weighted sender-bank timeout rate. The High Income level effect is absorbed by individual FEs. Bank, individual, and time FEs are included. Standard errors are clustered at the municipality level and displayed in parentheses. The first-stage F-statistic is the Kleibergen-Paap rk Wald statistic for the two endogenous variables. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.14: Credit Terms at Banks without Prior or Current Pix Activity

| | Panel A: Other-bank OLS | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.0006 | 0.0008 | −0.096 | 0.0003 |
| | (0.0007) | (0.0006) | (0.219) | (0.0003) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 1,011,388 | 1,011,388 | 223,287 | 1,011,388 |
| $R^2$ | 0.887 | 0.828 | 0.758 | 0.351 |
| | Panel B: Other-bank IV | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.048 | 0.024 | −8.933 | −0.021 |
| | (0.056) | (0.049) | (8.778) | (0.015) |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 1,009,864 | 1,009,864 | 222,921 | 1,009,864 |
| $R^2$ | 0.761 | 0.764 | −0.012 | 0.022 |
| First-stage F-stat | 235 | 235 | 154 | 235 |

*Note:* This table reports the other-bank OLS and 2SLS specifications from Tables 4 and 9, changing only the relationship eligibility rule. At month $t$, a credit lender is eligible if the borrower has no recorded outgoing Pix activity at that lender in any month up to and including $t$. Lenders used for Pix later in the sample contribute only before their first observed Pix month. Each eligible credit record is paired with every observed Pix bank in the borrower-month; observations count pairs. All outcomes, controls, and other sample restrictions are unchanged. The interest-rate outcome is defined in Section 2.1. Both panels include separate bank, individual, and month fixed effects. Panel B instruments log Pix value with the same cumulative, Pix-value-share-weighted sender-bank timeout exposure over months $t-4$ through $t-1$. Standard errors in parentheses are clustered by municipality. The first-stage statistic is the Kleibergen–Paap rk Wald F statistic. *, **, and *** denote pointwise significance at 10, 5, and 1%, respectively.

Table A.15: Impact of Pix on Credit Card Terms: OLS with Bank-by-Month Fixed Effects

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | 0.035** | 0.045*** | −6.452 | −0.0014** |
| | (0.015) | (0.014) | (4.242) | (0.0006) |
| Bank × Month FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 83,879 | 83,879 | 1,117 | 83,879 |
| $R^2$ | 0.814 | 0.591 | 0.873 | 0.654 |

| | Panel B: Other bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.0009 | 0.0011* | −0.026 | 0.0004 |
| | (0.0007) | (0.0006) | (0.140) | (0.0003) |
| Bank × Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 966,424 | 966,424 | 211,349 | 966,424 |
| $R^2$ | 0.897 | 0.848 | 0.893 | 0.384 |

*Note:* This table reports the OLS specifications from Table 4 with bank-by-month fixed effects. Panel A uses the own-bank sample; Panel B retains the main other-bank definition: the credit lender is never used for Pix by the borrower during the observed sample. Each eligible other-bank credit record is paired with every observed Pix bank in the borrower-month; Panel B observations count pairs. Sample eligibility, outcomes, controls, and municipality clustering are unchanged; additional singleton observations are dropped under the stronger fixed effects. The interest-rate outcome is defined in Section 2.1. Panel A retains individual-by-month fixed effects; Panel B retains individual fixed effects. Standard errors clustered by municipality are in parentheses. *, **, and *** denote significance at 10, 5, and 1%, respectively.

Table A.16: Placebo Timing: Credit Terms before First Pix Use

| | Panel A: Own-bank OLS | | | |
|---|---|---|---|---|
| Pre-Pix outcome | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.071 | −0.060 | −6.650 | 0.0001 |
| | (0.057) | (0.071) | (23.938) | (0.0001) |
| $p$-value | 0.213 | 0.398 | 0.787 | 0.453 |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual × Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 15,487 | 15,755 | 130 | 16,135 |
| Borrowers | 511 | 537 | 11 | 560 |
| Distinct pre-Pix outcomes | 1,191 | 1,245 | 23 | 1,302 |
| Municipality clusters | 288 | 297 | 10 | 309 |
| $R^2$ | 0.801 | 0.630 | 0.840 | 0.728 |

| | Panel B: Own-bank IV | | | |
|---|---|---|---|---|
| Pre-Pix outcome | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Log Pix Value | −0.054 | 0.037 | 0.753 | 0.0000 |
| | (0.040) | (0.023) | (0.670) | (0.0009) |
| $p$-value | 0.179 | 0.110 | 0.262 | 0.990 |
| Bank FE | Yes | Yes | Yes | Yes |
| Individual, Month FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |
| Obs. | 86,129 | 87,433 | 5,500 | 87,986 |
| Borrowers | 2,439 | 2,558 | 397 | 2,581 |
| Distinct pre-Pix outcomes | 3,153 | 3,315 | 414 | 3,377 |
| Municipality clusters | 925 | 954 | 236 | 960 |
| $R^2$ | 0.010 | 0.042 | −0.006 | 0.002 |
| First-stage F-stat | 95.9 | 90.4 | 29.1 | 91.9 |

*Note:* This table replaces the dependent variables in the own-bank specifications of Tables 4 and 9 with outcomes measured in the month immediately before the individual's first observed outgoing Pix use at any bank. Limits and balances are in logs; the interest rate is defined in Section 2.1 (SCR category 0204, positive exposure); default indicates an amount past due for at least 90 days. The original post-Pix regressors and contemporaneous controls are retained. Each column uses the original estimation sample with an observed pre-Pix outcome, dropping additional fixed-effect singletons; missing baseline records are not set to zero. Non-rate outcomes are matched by borrower, bank, and credit product; rates by borrower and bank. The same pre-Pix outcome is repeated across eligible post-Pix months; the distinct-outcome row counts baseline records. Panel A includes bank and individual-by-month FEs; Panel B includes bank, individual, and month FEs separately and uses the original cumulative sender-timeout instrument over $t-4$ through $t-1$. The first-stage statistic is the Kleibergen–Paap rk Wald F statistic. Standard errors clustered by municipality are in parentheses. *, **, and *** denote significance at 10, 5, and 1%, respectively.

Table A.17: Bank Characteristics and Pix Timeout Rates

| | Size | Credit characteristics | Size and credit |
|---|---|---|---|
| | (1) | (2) | (3) |
| Log bank assets | −0.271 | | −0.217 |
| | (0.268) | | (0.212) |
| Log Pix value | −0.027 | | −0.060 |
| | (0.078) | | (0.074) |
| Log credit limit | | −0.071** | −0.071** |
| | | (0.032) | (0.029) |
| Log credit balance | | 0.023 | 0.023 |
| | | (0.028) | (0.027) |
| Default rate | | −0.062 | −0.069 |
| | | (0.056) | (0.057) |
| Interest rate | | −0.042 | −0.039 |
| | | (0.040) | (0.036) |
| Bank FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |
| Obs. | 3,037 | 3,037 | 3,037 |
| Banks | 121 | 121 | 121 |
| Within $R^2$ | 0.0010 | 0.0056 | 0.0068 |
| Joint test ($p$-value) | 0.477 | 0.007 | 0.002 |

*Note:* This table reports regressions of monthly sender-bank Pix timeout rates, measured in percentage points, on bank size and credit characteristics. Bank size is measured by log total assets and log settled Pix value. Total assets come from the most recent quarterly balance sheet dated three to five months before the timeout month. Credit characteristics are bank-level means of log(1+credit limit), log(1+credit balance), an indicator for positive overdue balances, and revolving interest rates in the linked credit registry. Pix value enters as log(1 + value); Pix value and credit characteristics are lagged one month. Regressors are standardized on the common estimation sample. Bank and time FEs are included. Within $R^2$ measures the share of variation explained after removing these FEs; the joint test evaluates all reported regressors. Standard errors are clustered at the bank level and displayed in parentheses. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.18: Impact of Pix on Credit Card Terms: Alternative Timeout Windows

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| 2-month window | 0.193* | 0.130* | −11.031 | −0.029*** |
| | (0.102) | (0.077) | (11.989) | (0.007) |
| First-stage F-stat | 64.6 | 64.7 | 33.9 | 65.0 |
| 3-month window | 0.181* | 0.098 | −13.949 | −0.023*** |
| | (0.094) | (0.068) | (13.695) | (0.006) |
| First-stage F-stat | 95.8 | 95.8 | 40.3 | 96.4 |
| 4-month window | 0.197** | 0.087 | −12.650 | −0.019*** |
| | (0.086) | (0.062) | (10.610) | (0.005) |
| First-stage F-stat | 122.3 | 122.3 | 48.2 | 123.3 |
| 6-month window | 0.191** | 0.110* | −19.886* | −0.012** |
| | (0.086) | (0.059) | (11.410) | (0.005) |
| First-stage F-stat | 152.0 | 152.0 | 50.7 | 153.4 |
| Obs. | 215,437 | 215,437 | 24,879 | 215,437 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| 2-month window | 0.015 | 0.005 | 17.358 | −0.012 |
| | (0.059) | (0.055) | (13.841) | (0.022) |
| First-stage F-stat | 79.4 | 79.4 | 45.1 | 79.1 |
| 3-month window | 0.013 | 0.004 | 8.354 | −0.019 |
| | (0.063) | (0.058) | (11.503) | (0.021) |
| First-stage F-stat | 118.8 | 118.8 | 75.3 | 118.2 |
| 4-month window | 0.003 | 0.006 | 5.230 | −0.023 |
| | (0.064) | (0.057) | (10.027) | (0.020) |
| First-stage F-stat | 146.6 | 146.6 | 99.2 | 146.0 |
| 6-month window | 0.008 | −0.001 | 5.103 | −0.026 |
| | (0.066) | (0.058) | (9.957) | (0.019) |
| First-stage F-stat | 199.4 | 199.4 | 121.3 | 198.8 |
| Obs. | 944,705 | 944,705 | 205,991 | 944,705 |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |

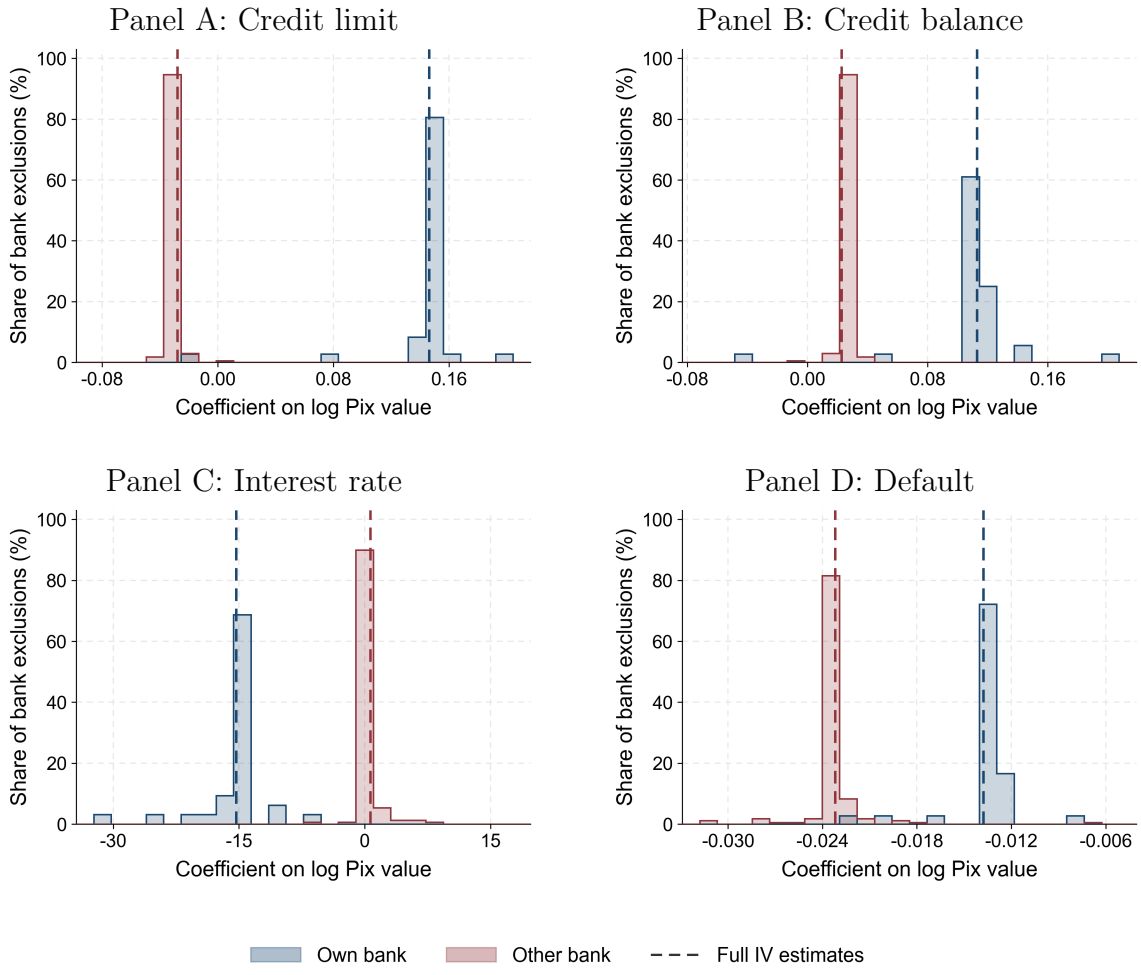
*Note:* This table reports 2SLS estimates of equation (3). Each row reports the coefficient on Log Pix Value, instrumented with the sum of the individual's Pix-value-share-weighted sender-bank timeout rates over the preceding two, three, four, or six months. Panel A shows own banks; Panel B includes credit banks never used for Pix by the depositor, paired with every observed Pix bank in the borrower-month. All windows use the same outcome-specific sample, beginning in May 2021 so that six preceding calendar months are available. The four-month specification is reestimated on this common sample. Outcomes, controls, and other sample restrictions follow Table 9. All specifications include bank, individual, and time FEs. Standard errors are clustered at the municipality level and displayed in parentheses. First-stage F-statistics are the Kleibergen–Paap rk Wald statistics. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Table A.19: Impact of Pix on Credit Card Terms: Excluding the Three Largest Banks

| | Panel A: Own bank | | | |
|---|---|---|---|---|
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Ranked by assets | 0.207** | −0.032 | −35.301** | −0.011*** |
| | (0.101) | (0.071) | (16.837) | (0.004) |
| Obs. | 159,520 | 159,520 | 12,886 | 159,520 |
| First-stage F-stat | 91.9 | 90.1 | 26.4 | 90.5 |
| Ranked by Pix value | 0.208** | −0.032 | −41.322** | −0.012*** |
| | (0.102) | (0.072) | (16.934) | (0.005) |
| Obs. | 163,631 | 163,631 | 15,271 | 163,631 |
| First-stage F-stat | 91.4 | 89.4 | 30.0 | 89.5 |
| | Panel B: Other bank | | | |
| | Credit limit | Credit balance | Interest rate | Default |
| | (1) | (2) | (3) | (4) |
| Ranked by assets | −0.001 | −0.004 | −5.389 | −0.020 |
| | (0.062) | (0.057) | (7.715) | (0.017) |
| Obs. | 937,946 | 937,946 | 203,735 | 937,946 |
| First-stage F-stat | 199.7 | 199.7 | 130.7 | 199.3 |
| Ranked by Pix value | −0.001 | −0.004 | −5.070 | −0.020 |
| | (0.062) | (0.057) | (7.699) | (0.017) |
| Obs. | 938,855 | 938,855 | 204,408 | 938,855 |
| First-stage F-stat | 200.1 | 200.1 | 133.5 | 199.6 |
| Bank FE | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes |

*Note:* This table reports 2SLS estimates of equation (3), excluding credit-card lenders among the three largest banks. Each specification reports the coefficient on Log Pix Value. The asset ranking uses standalone total assets in September 2020 and excludes Banco do Brasil, Caixa, and Bradesco. The Pix ranking uses national settled Pix value in January 2021 and excludes Itaú, Bradesco, and Banco do Brasil. Exclusions apply to credit-card lenders; the four-month timeout instrument is unchanged. Panel A shows own banks; Panel B includes credit banks never used for Pix by the depositor, paired with every observed Pix bank in the borrower-month. Outcomes, controls, and other sample restrictions follow Table 9. All specifications include bank, individual, and time FEs. Standard errors are clustered at the municipality level and displayed in parentheses. First-stage F-statistics are the Kleibergen–Paap rk Wald statistics. *,** , and *** correspond to 10-, 5-, and 1% significance levels, respectively.

Figure A.2: Impact of Pix on Credit Card Terms: Leave-One-Bank-Out Estimates




*Note:* This figure shows the distributions of 2SLS coefficients on Log Pix Value after excluding each credit-card lender in turn from the corresponding outcome-specific sample. Panels A–D show credit limits, credit balances, interest rates, and default. Blue distributions correspond to own banks and red distributions to other banks. Histograms give equal weight to each omitted lender and are normalized separately to sum to 100%. Color-matched dashed lines mark the full IV estimates. Exclusions remove the lender's credit observations; the four-month timeout instrument is unchanged. Outcomes, controls, fixed effects, and other sample restrictions are unchanged from the full-sample IV specifications. All bank-exclusion estimates are shown.