Invoice Digitization and Credit Availability: Hard-Information Effects in Small Business Lending

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Abstract

This paper studies the impact of digitized invoices on small business lending through China's Golden Tax Phase III Project, which established a unified tax-reporting system and digitized VAT invoices. Using the project's staggered provincial rollout as a natural experiment and a difference-in-differences (DID) design, I find that small businesses in treated provinces obtain substantially larger credit lines, pay lower interest rates, and enjoy longer maturities. Banks also report higher measured incomes and assign higher internal credit scores to these firms. I trace these improvements to the elimination of fraudulent invoices, which endows banks with more reliable hard information for income assessment and risk-based pricing. As a result, digital invoices mitigate information asymmetries in lending, allowing banks to extend more and cheaper credit to firms lacking hard information. Consistent with this mechanism, the largest credit gains occur in invoice-intensive industries; the most opaque borrowers (e.g., individually owned businesses) and small banks experience the greatest benefits. Finally, macro-level credit data from the People's Bank of China corroborate my micro-level findings. Overall, this study highlights the critical role of digital tax infrastructure in alleviating financing frictions for small businesses and underscores its broader importance for financial inclusion and economic growth.

Keywords: Banking, Credit, Financial Technology, Household Finance, Lending

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1 Introduction

Information asymmetry is first order in credit markets and especially acute for small businesses (Leland and Pyle, 1977; Sufi, 2007; Petersen and Rajan, 2002, 1994), which typically lack tax statements (Artavanis et al., 2016), audited statements (Lisowsky, 2010; Allee and Yohn, 2009; Lisowsky and Minnis, 2020), and pledgeable collateral (Beck, 2008; Beck et al., 2005). In these environments lenders lean toward soft information that is locally produced and costly to transmit across distance and organizational layers rather than toward hard information that is verifiable and portable (Petersen and Rajan, 1995; Berger and Udell, 2006; Stein, 2002; Degryse and Ongena, 2005; Liberti and Petersen, 2019; Boot and Thakor, 2000). China provides a quasi natural experiment, the nationwide shift to authenticated electronic VAT invoices expands bank verifiable hard information for small firms, allowing me to test whether hard information substitutes for soft information in pricing, with lessons for paper invoice systems such as the United States. I study whether a national digital tax infrastructure, by increasing the supply of hard information through rendering firms' invoices verifiable to banks, can alleviate information asymmetries, thereby reallocating credit and ultimately improving credit availability. This paper examines whether hard information can substitute for soft information in loan pricing and, if so, who gains more from this substitution. It also examines whether technological progress generates a Matthew effect that amplifies the advantages of already favored borrowers and lenders, or instead reduces inequality by lowering informational and transactional barriers and thereby improving outcomes for disadvantaged borrowers and lenders.

Electronic invoicing is now embedded in many VAT regimes: Italy mandates nationwide B2B and B2C e-invoices through a single exchange (since 2019), Spain requires near–real-time reporting for large taxpayers, Brazil's Nota Fiscal Eletrônica is the default for goods and services, Mexico mandates CFDI nationwide, and India phased in GST e-invoicing by turnover thresholds. By contrast, the United States lacks a federal VAT and relies on decentralized state and local sales taxes; B2B transactions are often outside the tax base, there

is no nationwide B2B e-invoice mandate or single exchange, and bank-facing interfaces are not standardized. Existing programs (e.g., the Treasury's Invoice Processing Platform) primarily cover public procurement rather than economy-wide B2B flows, and private adoption is fragmented and selective. This fragmentation deprives researchers and policymakers of an economy-wide, plausibly exogenous surge in bank-verifiable transaction records, making the U.S. an important audience but a poor identification setting for cleanly testing how "hardening" borrower income affects limits, pricing, maturities, and the distance gradient.

China's Golden Tax Phase III provides precisely the missing experiment: it unified tax databases, converted VAT invoices into security-coded digital records verifiable within the tax system, and created standardized bank interfaces, with a staggered provincial rollout that yields a clean policy shock. Leveraging this rollout and a multi-bank, loan-level dataset with internal income measures, credit scores, and borrower–branch geolocation, I show that authenticated invoice information raises credit limits, lowers rates and spreads, lengthens maturities, and flattens distance penalties—effects that concentrate in invoice-intensive sectors, among more opaque borrowers, and where competition enables pass-through. The universal lesson is that when public systems generate verifiable, standardized, and interoperable transaction data, lenders substitute hard for soft information and expand efficient credit supply. These magnitudes and design principles travel to non-VAT settings: for the U.S., they speak directly to the potential gains from building standardized, bank-interoperable B2B invoicing rails (even absent a VAT) and from policies that raise the verifiability and portability of transaction data across a fragmented tax landscape.

I bring bank-side evidence to this question using a private loan-level dataset of small business lending that covers multiple banks nationwide from an institution. The data span 2012 to 2022 and consist entirely of inclusive-finance loans under the regulatory definition that caps credit lines below RMB 10 million. Underwriting is decided at city branches, which allows me to proxy the cost of soft-information production by the geographic separation between the approving city branch and the borrower's district; I complement straight-line

distance with driving distance and driving time. These features let me connect administrative infrastructure to how banks measure income, score risk, set prices, and size credit for opaque borrowers.

My setting is China's Golden Tax Phase III, a staggered reform that unified tax databases and established a verification system for digitized VAT invoices, enabling automated verification within the tax system and bank interfaces. I implement staggered difference-in-differences and event-study designs that exploit province-level rollout to estimate policy effects on credit lines, interest rates and spreads, maturities, measured income, and internal credit scores of banks. The core results are that credit limits rise, rates and spreads fall, maturities lengthen, measured income increases, and internal scores improve after invoice digitization, consistent with banks incorporating hardened tax records into risk-based pricing.

Next, I use heterogeneity to test for information asymmetry and to show how hard information reallocates credit when soft information is scarce. Specifically, for the method I use difference in differences with a continuous moderator. First, I treat distance between the approving city branch and the borrower's district as a canonical proxy for soft-information frictions. Before the reform, distant borrowers face smaller limits and lower bank-recorded income, consistent with soft information decaying in space; after invoices become verifiable, the distance penalty in quantities flattens, the lending distance expands, and the same attenuation appears for income. The reforms also widened the lending distance of banks. This pattern matches theories in which soft information is local and costly to communicate while hard information is verifiable and portable across hierarchy, and it echoes evidence on distance, relationships, and organizational design in small-business credit (Petersen and Rajan, 2002, 1994; Stein, 2002; Degryse and Ongena, 2005; Agarwal et al., 2020; Liberti and Mian, 2009).

Second, I test how market structure, sectoral invoicing intensity, and information quality shape pass-through. I construct a city level Herfindahl–Hirschman Index using the distribution of bank branches across banks within each city, which serves as a proxy for local banking

competition. Rate reductions are larger in more competitive cities, consistent with the mapping from competition to loan pricing and with evidence that competitive markets transmit information improvements to borrowers (Petersen and Rajan, 1995; Degryse and Ongena, 2005; Rice and Strahan, 2010; Granja et al., 2022; Yannelis and Zhang, 2023). Following industry classification of Artavanis et al. (2016) and Pomeranz (2015), I partition industries into business-to-business sectors with high invoicing intensity and business-to-consumer sectors with low invoicing intensity. Quantity effects concentrate in invoice-intensive sectors, tying the response to the VAT reporting chain and the third-party-verification channel. Finally, I use provincial level auditing data from the China Taxation Inspection Bureau on the rate of problematic invoices. The results indicate that when invoices are noisier, price improvements are weaker, that consistent with hard-information precision driving results. Together these tests show both the presence of information asymmetry and its mitigation by invoice digitization, with effects that are strongest where soft information was initially most fragile.

This paper contributes to the literature on loan pricing and on the role of tax based information in financing. I provide the first evidence that hard information improves small firms' access to credit, mitigates inequities in the allocation of credit, and thereby expands overall credit availability. In the small-business lending literature, prior work emphasizes soft information, geographic proximity, and organizational design, documenting that relationship inputs decay with distance and are hard to transmit within hierarchies (Petersen and Rajan, 2002, 1994; Stein, 2002; Degryse and Ongena, 2005; Liberti and Mian, 2009; Agarwal and Hauswald, 2010; Skrastins and Vig, 2019). I shift the margin from soft to hard information by exploiting a national infrastructure shock and bank-side data. I show that an exogenous increase in verifiable, tax-based hard information substitutes for soft information in both credit sizing and pricing, with the largest gains at greater distances and among borrowers and banks previously most reliant on soft information. The evidence is enabled by a new multi-bank, loan-level dataset that lets me trace the chain from invoice verification

to measured income, internal scoring, and contract terms, complementing work on IT and hard-information processing in lending (Core and De Marco, 2024).

On the tax side, most finance and accounting research studies avoidance, enforcement, and disclosure, linking tax opacity to higher debt costs, often for large firms or from the firm side (Hanlon and Heitzman, 2010; Hasan et al., 2014; Gallemore and Jacob, 2020; Shevlin et al., 2020; Beck et al., 2014). I reframe taxes from liabilities to information. The reform I study did not cut statutory rates; it digitized and verified invoices, improving the precision of banks' income assessments and reducing borrowing costs. Because my dataset concentrates on inclusive-finance loans, the results speak directly to financial inclusion: individually owned businesses and clients of smaller regional banks benefit disproportionately, consistent with digital public infrastructure acting as a levelling technology that reduces information-driven inequality in access and prices. This perspective complements work on information sharing and credit registries and places inclusion in a market-structure framework where competitive discipline transmits information gains to borrowers (Rice and Strahan, 2010; Hertzberg et al., 2011; Balakrishnan and Ertan, 2021).

In sum, digitizing and verifying invoices hardens the income signals at the heart of small-business underwriting. As those signals become portable across distance and hierarchy, banks expand their lending distance, price more sharply, and extend more credit, with gains concentrated where soft information was initially scarce. The evidence links public digital tax infrastructure to private loan pricing and credit allocation, and shows how hard information can mitigate information asymmetry and reallocate credit toward disadvantaged borrowers and banks in ways that enhance financial inclusion(Gallemore and Jacob, 2020; Hertzberg et al., 2011; Balakrishnan and Ertan, 2021; Hou et al., 2025; Jiangli et al., 2008; Stiglitz and Weiss, 1981).

The remainder of the paper is organized as follows, Section I motivates the question and contributions, Section II sets out the institutional background, Section III describes the data and empirical design, Section IV reports the main results, Section V analyzes mechanisms

and heterogeneity, Section VI corroborates the findings with macro evidence, Section VII concludes with implications.

2 Background of Golden Tax Project

Since 1994, China has operated a turnover tax system based on value-added tax (VAT) and has introduced a special VAT invoice mechanism that enables purchasers to deduct input taxes using these invoices. These invoices serve both as proof of transaction and as records of the seller's tax obligations and the purchaser's input-tax credits. However, in the system's early stages, inadequate controls over special VAT invoices led to tax-evasion practices such as resale, forgery, and false invoicing.

To plug these loopholes, in February 1994 the State Council launched the Golden Tax Project, equipping special VAT invoices issued by pilot enterprises with anti-counterfeiting fiscal devices and deploying a computer-assisted VAT cross-checking system across fifty cities. The system relied on manual collection of invoice data for comparison to detect false or forged invoices. Phase I, beginning in 1996, initially targeted enterprises issuing special VAT invoices exceeding one million RMB and was expanded in 2000 to cover all special VAT invoices over one hundred thousand RMB. Nonetheless, the heavy reliance on manual data collection resulted in a high error rate.

On August 31, 2000, the China Administration of Taxation approved the Golden Tax Phase II Project, formally initiating a system upgrade. Phase II comprised four subsystems: anti-counterfeiting VAT invoicing, tax-device authentication, computer-based cross-checking, and invoice assistance inquiries, and it was rolled out nationwide as of July 1, 2001. This upgrade achieved end-to-end monitoring from invoice issuance and authentication to tax filing, audit, and inquiry.

Phases I and II effectively sealed major gaps in special VAT-invoice management and significantly enhanced the tax authorities' ability to detect and combat forgery. Nevertheless,

incidents of false or omitted special VAT invoices continued because the Golden Tax system's coverage remained limited to special VAT invoices. However, other critical elements of VAT administration, including general-taxpayer registration, invoice issuance and sale, and VAT declaration and payment, had not yet been integrated into the networked system.

To address these shortcomings, beginning in 2013 the State Administration of Taxation rolled out the Golden Tax Phase III Project in a staggered, province-by-province manner. This Phase III Project established a national, three-tiered ("central–provincial–county") taxation system, providing standardized, self-service, online and offline integration for taxpayers and the public. The principal innovations of the system are: 1. Information Sharing: By leveraging the internet, the system broke down legacy data silos and interfaced with relevant government agencies (e.g., the Administration of Industry and Commerce) and third parties (e.g., banks), enabling data sharing and cross-validation. It also unified the tax system of national and local tax bureaus into a single, consolidated tax system. 2. Digital Invoices: An electronic and networked invoice system was created; all digital invoices contain a security code that can be verified in the tax system.

The Phase III Project's most transformative feature is its highly integrated tax information verification platform. It enabled "one-time data collection, network-wide sharing" within tax authorities and across government departments, while, for the first time, seamlessly connecting taxpayer data with external institutions such as banks and customs. Real-time invoice issuance and authentication data are pushed directly to banking systems, allowing credit institutions to rapidly and accurately verify firms' transaction records and true income, thereby mitigating credit risk arising from information asymmetry. Furthermore, national standardization of tax metadata means that data obtained by any location or department adhere to uniform definitions, eliminating delays and errors caused by inconsistent formats.

In contrast to Phases I and II—which relied primarily on physical anti-counterfeiting devices and manual or semi-automated invoice data collection—Phase III fully digitized the invoicing process. From issuance to authentication, every step is conducted online without

requiring physical stamps or paper. Instantaneous, cloud-based storage of electronic invoices empowers tax authorities to capture critical data at the transaction moment and swiftly detect anomalous invoicing behavior. For firms, electronic invoicing reduces the costs of tax filing, reconciliation, and document management, and provides on-demand access to their own and counterparties' invoice records via online query. This comprehensive, end-to-end digital transformation not only boosts tax-administration efficiency but also delivers reliable, high-frequency financial transparency to the credit market.

3 Data and Empirical Model

3.1 Data

My analysis is based on a confidential dataset of small-business loans from a Chinese financial institution. The data comprise detailed credit records obtained via a stratified random sample of loans across multiple banks nationwide, which helps ensure the sample is representative and that my findings are not driven by sampling bias. The sample covers loan originations from 2012 through 2022. To preserve observations with missing fields, I follow Kabir and Ruan (2023) and convert each continuous control variable into quintile-based categorical bins, with an additional indicator for missing values (coded as -999). This approach retains all loans in the estimation sample without introducing distortions from imputation.

Table 1 presents summary statistics for the key variables. I measure the credit line (my primary outcome) in thousands of RMB and take its natural logarithm (ln(credit line)), so that changes can be interpreted as percentage changes. For instance, a loan's interest spread is defined as the loan's interest rate minus the benchmark Loan Prime Rate (LPR) set by the People's Bank of China. Each loan's internal credit rating (as provided by the originating bank) is converted into an internal credit score on a 1–100 scale by mapping the rating to its percentile rank within that bank's distribution of ratings. These transformations and definitions ensure that all variables are comparable and interpretable in economic terms.

3.2 Empirical Strategy

I exploit the staggered, province-by-province rollout of the Golden Tax Phase III Project as a natural experiment. This national tax reform was introduced in different provinces at different times, enabling a difference-in-differences (DiD) design. The treatment group consists of provinces that implemented the Golden Tax Phase III platform in the early (pilot) phase of the rollout, while the control group comprises provinces that had not yet implemented the system during those same time periods. The identifying assumption is that, absent the reform, credit outcomes in treated and control provinces would have followed parallel trends. In other words, any divergence in outcomes after the rollout can be attributed to the reform, provided there were no differential pre-existing trends. I later assess this parallel-trends assumption with an event-study analysis.

Formally, I estimate the following baseline regression via OLS on the loan-level panel data:

$$y_{ijtcp} = \alpha + \beta\big(Treat_{jp} \times Post_{tp}\big) + \gamma_t + \delta_c + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}. \tag{1}$$

where the outcome y_{ijtcp} is the natural log of the credit line (in thousands of RMB) for loan i made to firm j by bank b, in city c, at time t. The term $Treat_{jp}$ is an indicator for whether the observation is in a treated province p (i.e., one of the pilot provinces for the Phase III Project). $Post_{tp}$ is an indicator for the post-reform period for province p, equal to 1 if time t is on or after the implementation date in that province and 0 otherwise. The interaction $Treat_{jp} \times Post_{tp}$ thus equals 1 for loans to firms in treated provinces after the reform, and zero otherwise. Its coefficient β captures the average treatment effect of the Golden Tax Phase III reform on the outcome of interest. I include a full set of fixed effects: γ_t are time fixed effects (year–month dummies) controlling for shocks common to all provinces in each period, and δ_c are city fixed effects controlling for time-invariant differences across locations. In addition, I saturate the model with bank fixed effects ϕ_b to absorb any heterogeneity in

lending practices across banks, and industry fixed effects $\iota_{\text{sector}(j)}$ to absorb level differences across the borrowers' sectors. The vector X_{ijtcp} represents other control variables at the loan or borrower level (e.g., borrower characteristics and loan terms), which enter the regression as categorical dummies (as described above) to flexibly account for observable heterogeneity. Standard errors are clustered at the Bank*Year-Month level (bank $\times$ month) throughout the analysis, allowing for arbitrary correlation among loans originated by the same bank in the same month. This high-frequency clustering is conservative and aligns with recent recommendations for difference-in-differences inference in panels Kabir and Ruan (2023).

To verify the parallel-trends assumption and to explore the dynamic effects of the reform, I also estimate an event-study version of the model. This specification replaces the single treatment indicator with a series of lead and lag indicators for each period relative to the reform's implementation in each treated province. In particular, let D_k be an indicator variable equal to 1 if period t is the k-th period (in quarters) relative to province p's implementation date, and 0 otherwise. I set $k = 0$ for the quarter of implementation and omit the last pre-reform period ($k = -1$) as the baseline. I then estimate:

$$y_{ijtcp} = \alpha + \sum_{k \neq -1} \beta_k \left(Treat_{jp} \times D_k \right) + \gamma_t + \delta_c + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}, \qquad (2)$$

where all fixed effects and controls are the same as in Equation (1). The coefficients β_k trace out the dynamic treatment effects by event time. By construction, β_{-1} is normalized to zero, so all β_k for $k \geq 0$ measure the impact k quarters after the reform relative to the quarter immediately preceding the reform. Likewise, coefficients for $k < 0$ indicate any pre-trends in outcomes prior to treatment. I plot and inspect these estimates to check that there are no significant differences between treated and control groups in the periods before the reform (i.e., $\beta_k \approx 0$ for $k < 0$), consistent with the parallel-trends assumption. I also expect to see β_k turn positive and significant for $k > 0$ if the reform has a sustained beneficial effect

on credit outcomes. This event-study approach thus provides a robust visual and statistical test of my identification strategy, confirming that treated provinces and control provinces were on similar trajectories pre-reform and illustrating how the effect of the Golden Tax Phase III Project on small-business credit unfolds over time.

4 Result

4.1 Baseline results

Table 2 reports the baseline difference-in-differences estimates of the effect of the Golden Tax Phase III Project on small business borrowers' credit line. Credit lines are measured in units of one thousand RMB and log-transformed to capture percentage changes. In column 1, which includes only time and city fixed effects, the estimated coefficient implies a 63.3 percent increase in credit lines post-treatment. As borrower- and loan-level controls are introduced sequentially, the treatment effect remains positive and statistically significant. In column 4, where fixed effects for all potential control variables are absorbed, the estimate indicates an 18.4 percent increase in credit lines. These findings robustly confirm my hypothesis that the Golden Tax Phase III Project materially enhances credit availability for small business borrowers.

Table 3 reports the robustness checks of my baseline findings by expressing changes in credit line in absolute units. In column 1, which includes only fixed effects on time and city, the estimated post-treatment increase in the borrowers' credit line is RMB 1,517.99 (thousand). As I sequentially add industry and borrower level controls fixed effects, the effect persists in both magnitude and statistical significance. In column 4, where fixed effects for all potential control variables are absorbed, the point estimate implies a RMB 351.59 (thousand) increase in credit lines. The result presented in Table 3 corroborates my hypothesis that the Golden Tax Phase III Project materially enhances small business borrowers' credit availability, both in absolute terms and as a proportionate change.

4.2 Robustness check

4.2.1 Interest Rate

Table 4 reports robustness checks that measure credit availability via lending rates. In the most stringent specification—with fixed effects for time, city, industry and all other control variables—the Golden Tax Phase III Project is estimated to reduce lending rates by 0.413 percentage points. This result is consistent with my baseline evidence on credit line expansions and implies that enhanced tax information under the Golden Tax Phase III Project lowers perceived borrower risk, thereby broadening small business access to credit.

An alternative concern is that the observed decline in lending rates may simply reflect a drop in the benchmark rate. Following Chen et al. (2020), I define the interest spread as the interest rate of the loan minus the Loan Prime Rate.[1] Table 5 reports additional robustness checks that assess credit availability via interest spreads. In the fully saturated specification including fixed effects for time, city, industry, and all other control variables, the Golden Tax Phase III Project is estimated to compress the borrower interest spreads by 0.451 percentage points. This finding confirms that the policy not only lowers the headline lending rates but also narrows the spread over benchmark rates, thereby further enhancing credit access for small-enterprise borrowers.

4.2.2 Parallel Trend Check

I employ an event study framework to assess the assumption of parallel trends for ln(Credit Line). Specifically, rather than using calendar years as the temporal unit of analysis, I exploit quarterly intervals to better utilize the advantages of my loan level dataset, and more precisely trace the evolution of credit lines around the policy shock. To maintain temporal symmetry, my event study estimates span eight quarters before and eight quarters after the treatment

[1]The Loan Prime Rate (LPR) is a market-based reference rate, published monthly by the People's Bank of China based on the interest rates that leading commercial banks charge their clients, and because it reflects banks' marginal funding costs and lending intentions, it serves as an appropriate benchmark for loan pricing.

event, with all coefficients normalized to zero in the quarter immediately preceding implementation (Quarter -1).

Figure 1 implements an event study specification to validate the assumption of parallel trends for ln(Credit Line). By exploiting quarterly intervals, I precisely trace credit line dynamics around the policy shock, coefficients for the eight pre-treatment quarters are jointly indistinguishable from zero, offering visual and statistical confirmation of flat pre-trends. In contrast, post-treatment estimates exhibit a clear upward trajectory beginning one quarter after implementation, reflecting a treatment effect that emerges and intensifies over time. my symmetric event window (spanning eight quarters on either side of the shock and normalized at quarter –1) guards against arbitrary window choices and mirrors best practices in staggered DID designs.

4.2.3 Placebo Test

Figure 2 is a placebo test with 1000 replications as a robustness check for the Difference-in-Differences estimation, it designed to mimic the null of no treatment effect by randomly reassigning both the timing and location of the policy intervention. This figure plots the empirical distribution of the placebo DID estimates, which clusters tightly around zero, as anticipated under the null. By contrast, my actual DID coefficient of –0.064 lies in the extreme left tail of this distribution. Fewer than 1 in 1,000 placebo draws produce an estimate as negative as –0.064, corresponding to an empirical p-value effectively equal to zero. Moreover, over 95 percent of the placebo coefficients exceed conventional significance thresholds, confirming that the majority of randomized estimates are statistically indistinguishable from zero. This permutation-based inference, demonstrating that an effect of the observed magnitude would almost never arise by chance, provides compelling evidence that my DID results are not driven by sampling variation or unobserved confounders, thereby reinforcing the validity of my causal interpretation.

4.3 Heterogeneity Test

4.3.1 Industry

In the realm of individually-owned businesses, nearly half of all economic activity remains unreported, with professional services accounting for the largest share of tax evasion (Artavanis et al., 2016). Because approximately 80 percent of my sample consists of loans to these individual operators, I further examine heterogeneity by industry. Firms are categorized using "China National Economic Industry Classification Standard" (GB/T 4754–2017) and aggregated into twenty primary sectors. To preserve statistical power and ensure reliable inference, I restrict my analysis to industries with a minimum of 3,000 observations.

Table 6 shows that the Golden Tax Phase III Project significantly expands credit lines in manufacturing; wholesale and retail trade; and information transmission, software, and information technology services. In contrast, I do not observe statistically significant effects in agriculture, forestry, animal husbandry, and fishery; construction; transportation, warehousing, and postal services; accommodation and catering; leasing and business services; or residential services, repairs, and other personal services. This industry-level heterogeneity reflects the greater prevalence of tax non-compliance and sparse invoicing in consumer-facing sectors, which limits their ability to benefit from the electronic-invoicing mandate. Consistent with this interpretation, Pomeranz (2015) documents that retail, dining, and consumer service industries(where invoices are issued infrequently), exhibit higher evasion rates, whereas manufacturing inherently produces extensive invoice trails, fostering transparency. Similarly, Bergolo et al. (2023) demonstrate that under a VAT regime, invoices generate third-party–verified paper records shared by both buyer and seller, resulting in fewer evasion in B2B sectors.

In this context, industries that routinely issue receipts and invoices derive the greatest benefit from the Golden Tax Phase III Project digitization mandate, securing larger credit lines as a result. In contrast, consumer-facing operators (where invoices are generated less

frequently) experience more modest gains. Moreover, the construction and transportation, warehousing, and postal services sectors in my sample are overwhelmingly comprised of individually-owned businesses that serve end consumers, such as local renovation contractors and private courier or truck operators, rather than corporate clients. Because these consumer-facing operators issue formal invoices only infrequently, they derive limited benefit from the Golden Tax Phase III Project's digital invoices requirement, which explains the absence of statistically significant credit line gains in these industries.

4.3.2 Individually-owned Business vs. Small Enterprise

To assess heterogeneous effects of the Golden Tax Phase III Project across borrower types, I divide my sample into loans to corporate small enterprises and loans to individually-owned business. Although all observations qualify as small-enterprise credit, individually-owned business represent a distinct legal form in which a natural person applies in their own name for business financing. Compared with corporate entities, these firms are generally smaller and family-run. Holding observable variables constant, both loan types follow identical application and underwriting procedures; however, individually-owned business typically provide far fewer verifiable financial records ex ante.In contrast, corporate small enterprises routinely maintain audited financial statements and formal tax filings; for lenders, this relative paucity of hard data renders individually-owned businesses more opaque than small enterprises.

Table 7 shows that the Golden Tax Phase III Project raises credit lines by 16.5 percent for loans to individually-owned businesses, compared with an 8.9 percent increase for corporate small enterprises. This gap reflects that, as verifiable tax data increase, the most opaque borrowers reap the greatest benefits. In other words, by augmenting reliable hard information, banks can more precisely gauge creditworthiness and mitigate informational uncertainty. Consequently, credit supply expands most markedly for individually-owned businesses(those most deficient in reliable hard information), while the improvement for small enterprises(already endowed with comparatively richer hard information) remains more

modest.

4.3.3 Bank

To examine the role of hard information, I partition banks into two categories: nationally listed "large" banks and regionally focused "small" banks,which is city commercial banks with branches in multiple provinces but without a nationwide network. Berger et al. (2005) document that small local banks hold a comparative advantage in relationship lending grounded in soft information, whereas large national banks rely more heavily on hard information when making credit decisions. Consistent with this, Stein (2002) theorizes that decentralized lenders perform best when information remains "soft" and cannot be credibly transmitted, while hierarchical institutions excel when information can be readily "hardened" and disseminated within the organization.

Table 7 shows that, after the policy is published, the credit extended by small banks rises by 16.7%, whereas large banks experience only a 9.1% increase. This disparity suggests that an influx of hard information disproportionately benefits small banks, whose pre-reform lending strategies relied more heavily on soft information and therefore gain more from verifiable data. In contrast, large banks gain smaller incremental improvements because they already relied on audited financial statements and internal credit scores. The markedly stronger effect for small banks thus fits the notion that these institutions, previously at an informational disadvantage, are better able to refine their lending decisions when provided with additional hard data.

4.4 Mechanism Test

What drives the effect of invoice digitization on credit availability? Consistent with the proposed mechanism, digitized invoices and a unified digital tax infrastructure make tax information verifiable, providing banks with a reliable channel of hard information. Banks can then use this information to assess the true income of firms. Consequently, with this

additional hard information, banks can first revise their income estimates upward. This improvement enables banks to extend larger credit lines and ultimately leads to narrower lending spreads. In this section, I measure income as the natural logarithm of reported revenue (ln(income)) and construct credit scores by converting each bank's categorical borrower ratings into percentile ranks within its own borrower rating distribution.

Table 8 reports estimates of the impact of the Golden Tax Phase III Project on small business borrowers' income. In column 1 (which controls only for time and city fixed effects), borrowers' income rises by 31.4% following the policy shock. As additional controls are introduced, the effect remains both positive and statistically significant. Notably, in column 4 (with all baseline fixed effects and controls included), borrowers' income increases by 19.3%. These results affirm the hypothesis that the Golden Tax Phase III Project materially raises measured income for small business borrowers.

Table 9 presents estimates of the impact of the Golden Tax Phase III Project on banks' internal credit score. In column 3 (controlling for fixed effects of time, city, bank, and industry), borrower percentile ranks increase by 3.319% following the policy intervention. As additional controls are introduced, the effect remains robust; in column 4 (with all baseline fixed effects and controls included), the percentile rank increases by 6.752%. These results support the conjecture that the Golden Tax Phase III Project enhances internal credit scores by increasing the underlying measure of borrower income.

4.5 Alternative Analysis

4.5.1 Loan Term

Platikanova (2017) posits that creditors tend to extend shorter term debt to tax avoiding firms so as to more frequently reassess tax related risk. Motivated by this insight, I examine loan maturities (measured in days) to test whether reduced tax risk leads to longer tenors. Table 10 presents estimates of impact of Golden Tax Phase III Project on loan maturities of small business. In the specification that controls for fixed effects of time, city, and bank

(column 2), the Project increases maturities by 62.286 days. Moreover, in the fully saturated model, including fixed effects for all control variables, the effect remains robust at an increase of 48.530 days. Given that the average loan maturity in my sample is approximately 500 days, this represents a meaningful rise of about 10%. These findings indicate that the Golden Tax Phase III Project materially lengthens loan term, thereby providing indirect evidence that borrower risk has diminished and credit availability has improved.

5 Hard Information and Lending Frictions

This section studies whether the digitization of VAT invoices supplied verifiable hard information that substitutes for locally produced soft information and, in doing so, relaxes geographic and informational frictions in bank lending. I implement difference-in-differences and triple-difference designs that interact the policy with borrower–bank distance, local market concentration, sectoral invoicing intensity, and province–year measures of invoice quality. High-dimensional fixed effects absorb location–time and borrower–bank heterogeneity, and an event-study verifies parallel trends. The framework speaks directly to theories in which transferable, auditable records enable arm's-length lending and travel more effectively within organizations than soft information (Stein, 2002; Liberti and Petersen, 2019; Berger et al., 2005; Berger and Udell, 2002).

The evidence indicates an outward expansion of banks' spatial reach and a flattening of the distance gradient in quantities, robust to alternative measures of distance and to dynamic specifications with pre-trend checks. These patterns are consistent with information technologies weakening the dependence of contracting on proximity and making distant lending feasible (Petersen and Rajan, 2002; Degryse and Ongena, 2005; Agarwal and Hauswald, 2010). In this interpretation, digitized invoices provide hard, verifiable signals that reach places where soft information is scarce, thereby substituting for relationship-based inputs that decay with distance.

Pricing and cross-sectional results reinforce the same mechanism. Loan rates decline on average following the reform, with larger reductions in more competitive markets, and competition also amplifies the outward extension of lending beyond local informational advantages (Petersen and Rajan, 1995; Degryse and Ongena, 2005; Rice and Strahan, 2010; Granja et al., 2022; Yannelis and Zhang, 2023). A triple-difference by invoicing intensity shows that quantity gains concentrate in sectors where invoice visibility is intrinsically higher, linking the response to the VAT reporting chain (Artavanis et al., 2016; Pomeranz, 2015). Moreover, where audit data indicate noisier invoices, the price effects are attenuated, consistent with improvements in the precision of hard information driving the results. Taken together, the findings support a unified view: technology-enabled hard information substitutes for soft information precisely where the latter is scarce, improves risk-based pricing, and expands the set of bankable borrowers.

5.1 Distance and Spatial Reach

5.1.1 Expansion of the Lending Distance

Following Petersen and Rajan (2002), I examine whether the Golden Tax Phase III shifts the geography of lending by estimating specifications with the natural logarithm of lending distance as the dependent variable. Following the baseline specification in Equation (1), I estimate the effect of the reform on banks' lending distance by replacing the dependent variable with the natural logarithm of the *lending distance* and by including City*Year-Month fixed effects. Here and below, YM denotes Year-Month.

$$\begin{aligned}\ln(\mathit{Distance})_{ijtcp} = \alpha + \beta\left(\mathit{Treat}_{jp} \times \mathit{Post}_{tp}\right) + \gamma_t + \text{City} \times \text{YM}_{ct} + \phi_b + \iota_{\text{sector}(j)} \\ + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}\end{aligned} \tag{3}$$

Here, $\ln(\mathit{Distance})_{ijtcp}$ is the natural logarithm of the *lending distance* (in kilometers) between the lending bank b and borrowing firm j for loan i in city c, province p, at time t.

The interaction term $\left(Treat_{jp} \times Post_{tp}\right)$ is defined as before: it equals one for loans to firms in treated provinces p after the reform implementation date and zero otherwise. The coefficient β therefore captures the reform's effect on the lending distance. In this specification I replace the city fixed effects from the baseline with City*Year-Month fixed effects City $\times$ YM$_{ct}$, which control for all time-varying factors at the city level. Therefore, I compare the impact of the reform within the same city over time. All other covariates and fixed effects remain as in Equation (1): I include time fixed effects γ_t, bank fixed effects ϕ_b, industry fixed effects $\iota_{\text{sector}(j)}$, and the same loan- and borrower-level controls X_{ijtcp}. Standard errors are clustered at the Bank*Year-Month level throughout.

In this specification we replace the city fixed effects from the baseline with the City*Year-Month fixed effect City $\times$ YM$_{ct}$, which controls for all time-varying factors at the city level. Therefore, we can compare the impact of reforms within the same city. All other covariates and fixed effects remain as in equation (1): we include time fixed effects γ_t, bank fixed effects ϕ_b, Industry sector fixed effects ιsector(j), and the same loan- and borrower-level controls X_{ijtcp}. Standard errors are clustered at the Bank*Year-Month level throughout.

Table 11 reports the results. β is 0.135 with a clustered standard error of 0.049 and a p-value of 0.0059. The point estimate implies that average lending distance increases by approximately $e^{0.135} - 1 \approx 14.4\%$ in treated cities after the policy, relative to control cities within the same month. The evidence indicates that banks expanded their lending distance following the reform. This finding is economically meaningful and consistent with the view that additional verifiable information relaxes informational frictions that limit remote lending. Prior work documents that technological and informational improvements are associated with longer borrower and lender distance and with a greater feasibility of arm's length lending (Petersen and Rajan, 2002; Agarwal and Hauswald, 2010). Spatial competition and pricing also depend on geographic reach and the information environment (Degryse and Ongena, 2005). My design contributes by isolating a plausibly exogenous shock to hard information at the city and month level and by showing a within period expansion of lend-

ing distance, which provides causal support for the mechanism that hard information makes distant lending feasible.

5.1.2 Distance Heterogeneity in Credit Lines

This subsection examines whether the policy's effect on loan sizing varies with borrower to bank distance and quantifies how the reform attenuates the baseline distance penalty. I measure distance as the logarithm of the straight-line distance separation between the borrower and the lending branch.

To study heterogeneity by distance, I estimate

$$\begin{aligned}\ln(\textit{Credit Line})_{ijtcp} = \alpha &+ \beta_1 \left(\textit{Treat}_{jp} \times \textit{Post}_{tp}\right) + \beta_2 \ln(\textit{Distance})_{ijtcp} \\ &+ \beta_3 \left[\left(\textit{Treat}_{jp} \times \textit{Post}_{tp}\right) \times \ln(\textit{Distance})_{ijtcp}\right] \\ &+ \text{City} \times \text{YM}_{ct} + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}\end{aligned} \tag{4}$$

Here, $\ln(\textit{Distance})_{ijtcp}$ denotes the natural logarithm of the *lending distance* between the borrower and the lending bank for loan i (with subscripts j, t, c, and p as defined above). Aside from these new distance terms, the fixed effects and controls mirror the baseline specification: as in Equation (1), I include bank fixed effects ϕ_b, industry fixed effects $\iota_{\text{sector}(j)}$, and the same borrower- and loan-level controls X_{ijtcp}. I replace the city fixed effects δ_c with City*Year-Month fixed effects City $\times$ YM$_{ct}$, which absorb any shocks or policy changes common to all loans in the same city and month. The coefficient β_3 on $\left(\textit{Treat}_{jp} \times \textit{Post}_{tp}\right) \times \ln(\textit{Distance})_{ijtcp}$ is the key parameter of interest: it measures how the impact of the reform on log credit lines changes with distance. Because the City*Year-Month fixed effects absorb all city-level changes over time, the two-way treatment indicator ($\textit{Treat}_{jp} \times \textit{Post}_{tp}$) has no remaining within–city–month variation, so β_1 is not separately identified in this specification. In essence, β_2 captures the baseline distance "penalty" on credit line size, while β_3 shows how that penalty is attenuated or amplified by the policy. Standard errors are clustered at the Bank*Year-Month level.

The coefficient β_3 on $(Treat_{jp} \times Post_{tp}) \times Distance_i$ is therefore the key parameter of interest: it measures how the impact of the reform on log credit lines changes per unit of (log) distance. In essence, β_1 captures the baseline distance "penalty" on credit line (holding other factors constant), while β_3 captures how that penalty is attenuated (or amplified) by the policy. All other elements of the model remain as in the baseline. We again cluster standard errors at the bank×Year-Month level, allowing for arbitrary correlation of loans issued by the same bank in a given month.

Table 11 reports a side-by-side comparison of the prereform and postreform slopes. In the prereform sample the distance gradient is -0.123. Doubling distance reduces the credit line by about $0.123 \times \ln 2 \approx 0.086$ log points, which corresponds to $e^{-0.086} - 1 \approx -8.3\%$. In the postreform sample the slope is -0.070, which implies an attenuation of the distance penalty of about 43% relative to the prereform benchmark $\big[(0.123 - 0.070)/0.123\big]$. In the unified specification with the triple interaction the coefficient on (Treat × Post) × Dist equals 0.052, so when distance doubles the policy raises credit lines for farther borrowers by $0.052 \times \ln 2 \approx 0.036$ log points relative to otherwise similar nearer borrowers in the same city and month. To assess robustness, I construct driving distance using Baidu Maps' routing API, China's largest mapping platform, and re-estimate the baseline specification with this metric. Column (2) of Table **??** reports closely comparable results: the slope on ln(Driving Distance) is -0.119 ($t = -21.55$), the interaction (Treat × Post) × ln(Driving Distance) equals 0.057 ($t = 5.22$), and Treat × Post is -0.343 ($t = -1.83$). The signs, magnitudes, and significance mirror the distance estimates, indicating that the postreform attenuation of the distance penalty does not depend on the distance metric.

Figure 3 plots how the reform's effect varies with borrower distance. The estimation sample is partitioned into 99 global percentiles of ln(Distance); for each percentile I estimate the marginal effect of Treat*Post on ln(Credit Line) from a DID with percentile indicators interacted with Treat*Post, absorbing all fixed effects mentioned before. The values are semi-elasticities $d\ln(\text{Credit Line})/d\,\text{Treat} * \text{Post}$, with 95% confidence intervals based on standard

errors clustered at the Bank*Year-Month level.

These findings are consistent with the distance literature that documents the weakening of geographic frictions with the diffusion of information technologies (Petersen and Rajan, 2002). They also align with evidence that soft information decays with distance, which makes hard, verifiable information more valuable at greater separations (Agarwal and Hauswald, 2010). In my context the invoice digitization reform supplies hard information that substitutes for soft information and delivers disproportionately larger gains for distant borrowers.

5.1.3 Parallel trend with distance heterogeneity

To assess parallel trends when treatment effects vary by distance, I extend the event-study specification to include interactions with logged *lending distance*. In particular, I replace the single post-treatment indicator with a series of relative-time indicators D_k for each period k before or after a province's reform implementation, and interact these indicators with the centered log-distance term. *Event-time frequency:* D_k is defined at the *quarter* frequency, and the quarter immediately preceding implementation ($k = -1$) is the reference period. Let $\ln(Distance)^c_{ijtcp} \equiv \ln(Distance)_{ijtcp} - \overline{\ln(Distance)}$, where the overbar denotes the sample mean of $\ln(Distance)$ computed over the estimation sample. The estimating equation is:

$$\begin{aligned} \ln(Credit\ Line)_{ijtcp} = \alpha &+ \sum_{k\neq -1} \beta_k \left(Treat_{jp} \times D_k \right) + \lambda \ln(Distance)^c_{ijtcp} \\ &+ \sum_{k\neq -1} \delta_k \left(Treat_{jp} \times D_k \times \ln(Distance)^c_{ijtcp} \right) \\ &+ \gamma_t + \delta_c + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp} \end{aligned} \tag{5}$$

This specification mirrors the baseline event-study model (Equation (2)) but allows the treatment effect to vary with distance. In Equation (5), the coefficients $\{\beta_k\}$ trace out the dynamic policy effect on $\ln(Credit\ Line)$ at the average distance, and the coefficients $\{\delta_k\}$ capture how this effect varies with distance over event time. Thus, under the parallel-trends assumption, all pre-reform coefficients should be statistically indistinguishable from zero

(i.e., $\beta_k \approx 0$ and $\delta_k \approx 0$ for all $k < 0$). I demean log *lending* distance to make the event-time coefficients $\{\beta_k\}$ interpretable at the sample-mean distance rather than at ln(Distance) = 0 (distance = 1), and to improve numerical stability when triple interactions are included; this reparameterization leaves identification and the $\{\delta_k\}$ path unchanged. *Notation clarification:* δ_k indexes the event-time-by-distance interaction path, whereas δ_c denotes city fixed effects; the two symbols have distinct meanings and do not conflict. Note also that the symbol λ is used both for the coefficient on centered log-distance and for the vector of control coefficients; context distinguishes these uses. In addition, $Treat_{jp}$ varies at the province level and is indexed by j via the borrower's province of registration. I retain city fixed effects (δ_c) rather than City*Year-Month fixed effects because the event-time indicators for treated observations ($Treat_{jp} \times D_k$) are constant within a City*Year-Month; with City $\times$ YM fixed effects they are perfectly collinear with those fixed effects and are therefore not identified. Using δ_c and γ_t preserves the cross-city, within-time variation needed to estimate $\{\beta_k\}$, while all other controls remain as in the baseline. Figure 4 visualizes the sequence $\{\beta_k\}$ (with 95% confidence intervals, based on standard errors clustered at the Bank*Year-Month level) and reports the corresponding $\{\delta_k\}$ estimates.

5.1.4 Distance gradient and post-reform attenuation

I first document the baseline cross-sectional gradient with respect to distance. In the pre-reform sample, the slope on ln(Distance) is -0.123, implying that a one-unit increase in ln(Distance) is associated with a reduction in ln(Credit Line) of about $e^{-0.123} - 1 \approx -11.6\%$. Over the interquartile range of ln(Distance) in the prereform data, the implied change is -0.144 log points, or $e^{-0.144} - 1 \approx -13.4\%$.

I then estimate the slope after the reform within treated cities. The postreform gradient equals -0.070, which corresponds to $e^{-0.070} - 1 \approx -6.8\%$ per one-unit increase in ln(Distance). Over the postreform interquartile range, the implied change is -0.114 log points, or $e^{-0.114} - 1 \approx -10.8\%$.

Table 11 reports a side-by-side comparison of the prereform and postreform estimates, including robust t-statistics, fixed-effect structure, sample sizes, and R^2. Comparing the two slopes indicates that the distance penalty is attenuated by about 43% relative to the baseline, $\big[(0.123 - 0.070)/0.123\big]$, consistent with the reform relaxing distance-related information frictions in quantities. These results are consistent with hard information substituting for soft information in small-business lending: by relaxing distance-related informational frictions, the reform disproportionately raises credit limits for more distant borrowers and compresses the preexisting distance gradient.

5.1.5 Distance, Hard Information, and Income Measurement

To reinforce the analysis in Section 5.1.2, I replicate the distance-based identification using reported income as the outcome. Measured income serves as a "thermometer" for hard information: if the Golden Tax Phase III reform expands banks' access to verifiable tax records, one would expect reported incomes to become more accurate (and generally higher) after the reform, especially for loans where soft information is scarce (i.e., at longer distances). By design, I omit any fixed effect or control for income in this regression (since income is now the dependent variable) so that the reform's impact on reported income can manifest. Specifically, I estimate the following staggered difference-in-differences specification:

$$\begin{aligned} \ln(\mathit{Income})_{ijtcp} = \alpha &+ \beta_1 \left(\mathit{Treat}_{jp} \times \mathit{Post}_{tp}\right) + \beta_2 \ln(\mathit{Distance})_{ijtcp} \\ &+ \beta_3 \left[\left(\mathit{Treat}_{jp} \times \mathit{Post}_{tp}\right) \times \ln(\mathit{Distance})_{ijtcp}\right] \\ &+ \text{City} \times \text{YM}_{ct} + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp} \end{aligned} \tag{6}$$

This specification mirrors the baseline interactive item model in Equation (4), with the sole change that I replace the city fixed effects (δ_c) with a full set of City*Year-Month fixed effects City $\times$ YM$_{ct}$. In other words, identification now comes from comparing treated and control loans within the same city and month, which flexibly absorbs any time-varying shocks common to all loans in a given city. All other fixed effects remain the same as in

the baseline, except that a separate time fixed effect is redundant given the inclusion of City*Year-Month dummies: I include bank fixed effects ϕ_b, industry fixed effects $\iota_{\text{sector}(j)}$, and the same loan and borrower controls X_{ijtcp} as before. Standard errors are clustered at the Bank*Year-Month level throughout, a high-frequency clustering that is conservative and aligns with recent recommendations for difference-in-differences inference in panels (Kabir and Ruan, 2023). In Equation (6), the coefficient on $\ln(\mathit{Distance})_{ijtcp}$ (denoted β_2) captures the general relationship between distance and reported income independent of the reform. Most importantly, the interaction term $\left(\mathit{Treat}_{jp} \times \mathit{Post}_{tp}\right) \times \ln(\mathit{Distance})_{ijtcp}$ enters with coefficient β_3, which measures how the treatment effect varies with distance. Because the City*Year-Month fixed effects absorb all within–city–month changes over time, the two-way treatment indicator $\left(\mathit{Treat}_{jp} \times \mathit{Post}_{tp}\right)$ has no separate variation at that level, so β_1 is not separately identified in this specification. The coefficient β_2 on $\ln(\mathit{Distance})_{ijtcp}$ captures the baseline distance gradient in the outcome, holding fixed effects and controls constant. Standard errors are clustered at the Bank*Year-Month level. Standard errors are clustered at the Bank*Year-Month level.

The result estimates are consistent with hard information substituting for soft information. Table 11 reports pre-policy distance slope is negative and precisely estimated, $\hat{\beta}_2 = -0.212$ ($t = -12.49$), implying lower measured income at greater distances. The interaction is positive and precise, $\hat{\beta}_3 = 0.155$ ($t = 7.95$), so the post-policy slope equals $\hat{\beta}_2 + \hat{\beta}_3 = -0.057$. The level effect at zero distance is small and imprecise, $\hat{\beta}_1 = 0.172$ ($t = 0.47$), because I control the City-Year-Month FE. In terms of magnitudes, doubling distance increases log distance by $\ln 2$, so the income penalty declines from about $0.212 \times \ln 2 \approx 0.147$ before the policy to $0.057 \times \ln 2 \approx 0.040$ after the policy. The incremental policy effect grows with distance, $\partial \mathit{log_Combined_Income}/\partial \mathit{Did} = \hat{\beta}_1 + \hat{\beta}_3 \times \mathit{Log_Distance}$, which is increasing in *Log_Distance*.

Overall, the policy compresses the soft-information gap between distant and nearby borrowers. Hard information improvements substitute for soft information, with stronger cor-

rective effects where soft information is limited. Income, a core pricing input, displays the same distance gradient as my pricing results, supporting the view that the policy mitigates distance-induced information frictions and strengthens the foundations of loan pricing.

5.2 Market Structure and Pricing

I measure local banking competition at the City*Year-Month level using a Herfindahl-Hirschman Index (HHI) based on branch presence. Using China's branch-license registry, I harmonize head-office names, map outlets to prefecture-level cities, reconcile openings and closures to construct monthly activity spells, and count active outlets by bank; the HHI is the sum of squared branch shares in each city–month, as in Degryse and Ongena (2005) and complementary to deposit-share HHIs in Rice and Strahan (2010). Consistent with the literature, I focus on *interest rates* as the canonical outcome linking competition to pricing (Petersen and Rajan, 1995; Degryse and Ongena, 2005; Rice and Strahan, 2010; Granja et al., 2022; Yannelis and Zhang, 2023).

$$HHI_{ct} = \sum_{b} \left(\frac{n_{bct}}{\sum_{b'} n_{b'ct}} \right)^2 \tag{7}$$

where n_{bct} denotes the number of active branches of bank b in city c during month t; shares are computed over banks active in city c at month t. To study heterogeneity by competition, I estimate the following staggered difference-in-differences specification. Relative to the baseline, I replace bank fixed effects ϕ_b with Bank*Year-Month fixed effects $\phi_{b\times t}$ so that identification comes from within Bank*Year-Month comparisons; since $\phi_{b\times t}$ absorbs the month effects, a separate γ_t is omitted in this specification:

$$\begin{aligned} InterestRate_{ijtcp} = \alpha + \beta_1 \left(Treat_{jp} \times Post_{tp} \right) + \beta_2 \, HHI_{ct} + \beta_3 \left(Treat_{jp} \times Post_{tp} \times HHI_{ct} \right) \\ + \delta_c + \phi_{b\times t} + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp} \end{aligned} \tag{8}$$

Here, $InterestRate_{ijtcp}$ denotes the interest rate on loan i to firm j (in sector sector(j)) by

bank b, in city c, at time t (province p). The policy indicator $\left(Treat_{jp} \times Post_{tp}\right)$ equals 1 for loans to firms in treated provinces after the reform in that province. Its coefficient β_1 captures the average policy effect. The triple interaction $Treat_{jp} \times Post_{tp} \times HHI_{ct}$ allows the policy effect to vary with local concentration; β_3 measures how the treatment impact differs in more concentrated versus more competitive markets. Using $\phi_{b\times t}$ (Bank*Year-Month fixed effects) ensures comparisons within the same bank and month, purging bank-specific monthly pricing shocks and any differential pass-through at the bank and month level. City fixed effects δ_c absorb time-invariant city heterogeneity, and $\iota_{\text{sector}(j)}$ plus X_{ijtcp} mirror the baseline

Table 12 reports the estimates. The policy indicator is negative and precisely estimated ($\hat{\beta}_1 = -0.833$, $p < 0.01$), baseline pricing increases with concentration ($\hat{\beta}_2 = 5.134$, $p < 0.01$), and the interaction is positive and significant ($\hat{\beta}_3 = 2.834$, $p < 0.01$). The marginal policy effect equals $\beta_1 + \beta_3 \cdot \text{HHI}$, which crosses zero at $|\beta_1|/\beta_3 \approx 0.294$. Thus, the policy's rate reduction is largest in more competitive cities and attenuates as concentration rises. These results align with the standard mapping from competition to pricing in loan markets documented in the literature (Petersen and Rajan, 1995; Degryse and Ongena, 2005; Rice and Strahan, 2010; Granja et al., 2022; Yannelis and Zhang, 2023).

Invoice digitization supplies verifiable hard information that mitigates borrower opacity; the interaction with local market structure shows that the price effect is strongest where soft information is relatively scarce and competitive discipline is higher. This findinglinks classic theories of relationship lending under competition (Petersen and Rajan, 1995; Boot and Thakor, 2000) with evidence on spatial competition and contractual terms (Degryse and Ongena, 2005; Granja et al., 2022) and recent work on competitive selection and learning in credit markets (Yannelis and Zhang, 2023).

I use the insight of (e.g., Granja et al., 2022) that average borrower distance is an informative proxy for how far lenders operate beyond local informational advantages. They document that the average distance between small corporate borrowers and their lending branches increased before the Global Financial Crisis, and that the increase was stronger in

more competitive markets.

Guided by the same mechanism, I re-estimate the model using the natural logarithm of *driving distance* as the outcome. This robustness specification is otherwise identical but reverts to bank fixed effects ϕ_b and keeps time fixed effects explicit, alongside province fixed effects (since HHI is city level):

$$\ln(Distance)_{ijtcp} = \alpha + \beta_1 \left(Treat_{jp} \times Post_{tp}\right) + \beta_2 HHI_{ct} + \beta_3 \left(Treat_{jp} \times Post_{tp} \times HHI_{ct}\right) + \gamma_t + \pi_p + \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp} \tag{9}$$

In Equation (9), i indexes loans, b banks, c cities, p provinces, and t time. Province fixed effects π_p replace city fixed effects, while γ_t, ϕ_b, $\iota_{\text{sector}(j)}$, and X_{ijtcp} are maintained as above. The coefficient β_3 again captures how the reform's effect varies with concentration—now for distance. This specification mirrors the baseline model but with the modified fixed-effect structure, enabling comparisons within provinces (across cities) over time. Standard errors are again clustered at the Bank*Year-Month level.

I divide the overall HHI into two groups using the median value computed from the pre-policy sample to avoid contamination from post-policy changes in market structure. Table 12 reports that in low HHI cities, $\beta_3 = -9.124$ ($t = -6.10$), while in high HHI cities, $\beta_3 = -1.302$ ($t = -4.44$). The negative sign in both cases indicates that the policy-induced increase in lending distance weakens as markets become more concentrated, and the much larger magnitude in the low HHI group implies that competition amplifies this response. In other words, conditional on the same level of market concentration, the policy is associated with banks lending to more distant borrowers, and this outward expansion is considerably stronger in more competitive environments. Taken together, the results suggest that the increase in available hard information enables banks to extend credit over greater distances, and that banks operating in more competitive markets are more willing and able to exploit such information advantages.

5.3 Invoicing Intensity and Credit Limits: DDD Evidence

I classify industries at the GB/T 4754 section level using the industry grouping method in Artavanis et al. (2016). Their approach organizes sectors by the visibility of invoice based and bank verified records, which I implement as a split between *high invoicing* and *low invoicing*. Pomeranz (2015) shows that the VAT invoice chain with third party reporting strengthens compliance along supplier and buyer links, which motivates a focus on invoicing intensity as the channel. High invoicing sectors include agriculture, mining, manufacturing, utilities, construction, wholesale and retail trade, transport and warehousing, information services, finance, real estate, water and environmental management, public administration and social organizations, and international organizations. Low invoicing sectors include accommodation and food services, leasing and business services, resident and repair services, education, health and social work, culture and entertainment, and scientific research and technical services.

I estimate a difference in differences in differences specification that interacts the policy indicator with the two invoicing groups:

$$\begin{aligned}\ln(\textit{Credit Line})_{ijtcp} = \alpha &+ \beta_{\text{High}} \Big(\textit{Treat}_{jp} \times \textit{Post}_{tp} \times \mathbf{1}\{\text{high invoicing}\}_j \Big) \\ &+ \beta_{\text{Low}} \Big(\textit{Treat}_{jp} \times \textit{Post}_{tp} \times \mathbf{1}\{\text{low invoicing}\}_j \Big) + \text{City} \times \text{YM}_{ct} \\ &+ \text{City} \times \text{Invoicing Group}_{cG(j)} + \text{YM} \times \text{Invoicing Group}_{tG(j)} \\ &+ \phi_b + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}\end{aligned} \tag{10}$$

I estimate a staggered difference-in-difference-in-differences (DDD) specification that interacts the province-level treatment indicator $\textit{Treat}_{jp}$ with the province-specific post dummy $\textit{Post}_{tp}$ and ex-ante invoicing groups. The coefficients β_{High} and β_{Low} capture the group-specific treatment effects for high- and low-invoicing borrowers, respectively; their difference $\beta_{\text{High}} - \beta_{\text{Low}}$ is the DDD contrast. Because I include saturated City*Year-Month fixed ef-

fects (City $\times$ YM_{ct}), the two-way treatment indicator ($Treat_{jp} \times Post_{tp}$) has no separate within–city–month variation and is therefore not separately identified in this specification; identification comes from within–city–month differences across invoicing groups as provinces enter the reform at different times. Fixed effects mirror the baseline specification except that I replace city fixed effects with City*Year-Month fixed effects to enable within-city comparisons over time, and I add City $\times$ $\text{Invoicing Group}_{cG(j)}$ and YM $\times$ $\text{Invoicing Group}_{tG(j)}$ to absorb lower-order components involving invoicing groups. Bank fixed effects ϕ_b, industry fixed effects $\iota_{\text{sector}(j)}$, and the control vector X_{ijtcp} are included exactly as in the baseline. Standard errors are clustered at the Bank*Year-Month level.

Table 13reports the group specific effects on the log credit limit. In high invoicing industries the policy increases credit limits by 0.189 log points ($t = 3.32$), about 20.8 percent. In low invoicing industries the coefficient is 0.152 log points ($t = 1.50$). This pattern indicates that the increase in credit limits is primarily driven by high invoicing sectors, which confirms that the improvement in limits originates from invoices.

5.4 Information Quality and Pricing

To examine the role of information quality, I link each province–year observation to the share of problematic invoices (*Problematic Invoice Ratio*) drawn from the annual audit records of the National Tax Inspection Bureau of China.[2] This variable measures the proportion of invoices flagged as abnormal, unverifiable, or fictitious relative to total invoices reported in each province and year. A higher value indicates weaker invoice quality and a noisier information environment.

Following Partha Sengupta (1998), Minnis (2011), and Sufi (2007), I estimate the following specification to examine how the reform's impact varies with information quality:

[2]The audit dataset is published in the *China Tax Inspection Annual Report*, which is available only up to the 2018 edition. The 2018 volume reports inspections conducted in 2017, so the effective coverage of the problematic invoice data ends in 2017. The variable is annual and aggregated at the provincial level.

$$
\begin{aligned}
InterestRate_{ijtcp} = \alpha &+ \beta_1 \left(Treat_{jp} \times Post_{tp} \right) + \beta_2 \, ProblematicInvoiceRatio_{pt} \\
&+ \beta_3 \left(Treat_{jp} \times Post_{tp} \times ProblematicInvoiceRatio_{pt} \right) + \gamma_t + \delta_c \\
&+ \phi_{b \times t} + \iota_{\text{sector}(j)} + X'_{ijtcp}\lambda + \varepsilon_{ijtcp}
\end{aligned} \tag{11}
$$

This specification mirrors the baseline nteractive item model in Equation (4). I include time fixed effects γ_t, city fixed effects δ_c, and Bank*Year-Month fixed effects $\phi_{b\times t}$. The Bank*Year-Month fixed effects $\phi_{b\times t}$ compare loans issued by the *same bank* in the *same month* across different cities and borrowers, purging bank specific pricing shocks at that bank in that month. By including γ_t, I absorb common shocks in a given month that affect all banks and all cities; by including δ_c, I absorb time invariant city heterogeneity; and by including $\phi_{b\times t}$, I absorb any additional within bank month variation in pricing policy. In Equation (11), $Treat_{jp}$ and $Post_{tp}$ are defined exactly as in the baseline specification, so $(Treat_{jp} \times Post_{tp}) = 1$ for loans in pilot provinces after the Phase III implementation date and 0 otherwise. The term $ProblematicInvoiceRatio_{pt}$ is a continuous province level measure of invoice irregularities. Interacting this measure with the treatment indicator allows the reform effect to vary with information quality. The coefficient β_3 on $(Treat_{jp} \times Post_{tp} \times ProblematicInvoiceRatio_{pt})$ captures this heterogeneity, while β_1 represents the average treatment effect when $ProblematicInvoiceRatio_{pt} = 0$, and β_2 captures the direct association between invoice quality and interest rates in the absence of the reform. Standard errors are clustered at the Bank*Year-Month level throughout.

Table 13 presents the results. The policy reduces loan interest rates on average, $\hat{\beta}_1 = -0.651$ ($t = -2.84$). The coefficient on the Problematic Invoice Ratio is negative and significant, $\hat{\beta}_2 = -0.0065$ ($t = -3.62$), while the interaction term is positive and significant, $\hat{\beta}_3 = 0.0071$ ($t = 2.44$). These findings suggest that the implementation of the Golden Tax Phase III system reduces distortions in credit pricing caused by problematic invoices and, from another perspective, highlight the role of invoice digitization in improving the accuracy

and efficiency of credit pricing.

6 Macro evidence from PBOC

To further assess the external validity of my findings, I obtain monthly provincial bank credit balance sheets from Wind and implement a macro-level Difference-in-Differences analysis.[3] In my empirical setup, the policy shock is defined as the month in which each province implements the Golden Tax Phase III Project. I thus construct a Treated×Post indicator, where Treated equals one for pilot provinces and Post equals one for months following rollout. my dependent variables are the monthly outstanding credit balance and its natural logarithm.

$$\ln(\mathit{Credit\ Line})_{ptkl} = \alpha + \beta\big(\mathit{Treat}_p \times \mathit{Post}_{tp}\big) + \gamma_t + \theta_k + \eta_l + \varepsilon_{ptkl} \tag{12}$$

We adapt the baseline difference in differences framework to provincial panel data with staggered rollout of the Golden Tax Phase III Project. The dependent variable $\ln(\mathit{Credit\ Line})_{ptkl}$ is the natural log of the monthly outstanding credit balance for province p in month t within borrower type k and loan term l. The indicator $\mathit{Treat}_p \times \mathit{Post}_{tp}$ equals one for treated provinces after their province specific implementation month and zero otherwise. The coefficient β captures the average treatment effect on log credit lines for provinces that implement the reform relative to those that have not yet or never implement it. The term γ_t denotes year–month fixed effects that absorb shocks common to all provinces in a given month. The terms θ_k and η_l are borrower type and loan term fixed effects that control for time invariant differences across credit categories. Province fixed effects are not included due to the staggered adoption design. Standard errors are clustered at the Province*Year-Month level.

Since the PBOC discloses credit data on a provincial basis, the level of granularity varies: some provinces report only aggregate credit balances for confidentiality reasons, whereas oth-

[3]Wind's data are sourced directly from the People's Bank of China and the respective provincial statistical bureaus.However, due to variation in official disclosure standards, I are unable to secure credit data for all provinces.

ers provide more detailed breakdowns—such as operating loans within household short-term credit.To minimize endogeneity concerns, I select the most detailed series that remains available across the largest set of provinces—that is, the "loan borrower type – loan maturity" category. The PBOC credit balance dataset reports amounts in 0.1 billions of RMB and covers 21 provinces (see Appendix A).

Table 14 reports the summary statistics of credit line from People's Bank of China.

Table 15 reports the impact of the Golden Tax Phase III Project on bank credit line in each province. The result shows that after the implementation of Golden Tax Phase III Project, the credit line in the pilot provinces increased by about RMB 537.9854 billion, and the loan scale increased by 8.7%, both of which were significant at 1% level.

This direction is highly consistent with the conclusion that credit line rose at the micro level, indicating that the exogenous impact of tax information not only affects a single loan, but also has the same impact at the macro credit aggregation level, thus further enhancing the robustness and extrapolation of the research conclusions.[4]

7 Conclusion

This paper documents that when public systems convert tax records into verifiable, bank-usable signals, information frictions that constrain small-business credit recede. The result is a broader and cheaper supply of credit, concentrated where soft information was initially most fragile and where market structure permits pass-through. When hard information emerges, borrowers get larger loans, lower interest rates, and longer terms, banks expand their lending distance. Competitive markets transmit informational improvements more fully into prices, and quantity effects concentrate in invoice-intensive sectors that are naturally embedded in the VAT reporting chain. For the province where audit data indicate noisier invoices, price improvements are weaker.

[4]I tried to collect macro-tax data from China to conduct a similar analysis. Unfortunately, China only reports annual tax information, which makes it impossible for me to conduct an analysis.

The findings speak to classic themes in corporate finance and banking, the role of hard versus soft information in screening and pricing, the organizational limits of information transmission, and the mapping from competition to pass-through. They also document a tight link between public information architecture and private capital allocation. At the micro level, authenticated invoices alter how banks assess and price risk, at the macro level, provincial credit aggregates move in the same direction, reinforcing external validity. By focusing on inclusive-finance loans and on borrower and bank types that are most reliant on soft information, the analysis highlights distributional consequences, digital tax infrastructure can level the playing field for opaque entrepreneurs and for intermediaries historically disadvantaged in hard-information production.

These results affect policymakers, the financial sector, and society. The mechanism, digitized verifiable invoices that harden cash-flow information and lower the reliance on locally produced soft information, operates through design principles that matter in practice, verifiability (security-coded invoices), standardization (common tax metadata), and interoperability (bank-facing interfaces). For the financial sector, authenticated tax data complement relationship inputs and, where soft information is costly to acquire or transmit, effectively substitute for them, reducing the variance of income assessment and enabling arm's-length lending. For policymakers and society, when such infrastructure is paired with competitive local banking markets and credible audit regimes, informational gains pass through to borrowers as lower prices and broader access, and investments in digital public infrastructure can advance financial inclusion by easing information-driven barriers that disproportionately bind smaller firms and first-time borrowers, provided that data governance, privacy, and accountability are treated as first-order constraints. On the instrument side, authenticated invoices raise the pledgeability of receivables, implying testable effects in trade credit, factoring and reverse-factoring programs, supply-chain finance, and receivables securitization, with tighter pricing, longer maturities, and broader geographic reach where invoice trails are dense, mirroring the loan-market results documented here.

On the industry side, sectors with high B2B invoicing intensity (manufacturing, wholesale, logistics, information services) should experience stronger reallocation toward arm's-length finance and smaller firms, while low-invoicing consumer-facing services may see muted effects unless e-receipts or platform-mediated records become similarly bank-verifiable. On the design side, the paper's heterogeneity by competition and information quality points to three agendas, quantifying general-equilibrium pass-through (investment, employment, input choice, and entry or exit along supply chains), mapping optimal architecture (how verifiability, standardization, and bank interoperability trade off against privacy and audit intensity), and studying market structure (how digital public infrastructure shifts the margin between relationship lenders and arm's-length intermediaries, including fintech originators). Because the core lesson is environment agnostic, when transaction data are made verifiable and portable, the cost of credit falls where soft information was binding, the estimates provide externally valid benchmarks for economies contemplating e-invoice or e-receipt rails and motivate new identification from staggered rollouts of digital procurement, payroll or withholding, or platform-transaction reporting that replicate the clean, bank-connected policy shock exploited here.

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Tables

In my loan-level dataset, Unpaid Balance denotes the outstanding principal at the observation date (RMB). The Interest Rate is the contractual nominal annual rate (percent). Credit Line refers to the maximum approved borrowing capacity (RMB). Age records the borrower's age in years. Income captures the bank-recorded annual income of the borrower (RMB). Loan Term measures the contractual maturity length from origination to due date, expressed in days. Interest Spread is defined as the loan's interest rate minus the relevant benchmark rate (e.g., LPR), reported in percentage points; negative values indicate pricing below the benchmark. Credit Score is the institution's internal risk metric, originally reported on bank-specific scales but transformed into an internal percentile rank (0–100) for comparability, where higher values indicate better credit quality (100 = best, 0 = worst).

Table 1: Summary Statistics: Main Dataset

VarName	Obs	Mean	SD	Min	Median	Max
Unpaid Balance	154,233	868,692	1,344,702	0	490,000	38,970,000
Interest Rate	154,233	7.3	2.1	0.0	7.0	17.4
Credit Line	154,182	1,521,540	2,050,424	6	800,000	100,000,000
Age	122,723	44	9	18	44	75
Income	126,465	14,585,246	299,040,364	0	4,300,000	99,999,997,952
Loan Term	154,182	507	599	0	364	7,305
Interest Spread	154,233	2.7	2.0	-4.3	2.2	12.6
Credit Score	101,242	46.0	28.7	0.1	49.1	100.0

Notes: The raw dataset contains 20 observations with the nominal loan interest rate recorded as exactly 0. To preserve data integrity and auditability, I retain these records without ad-hoc exclusion; consequently, the distribution of Interest Spread (loan rate minus the benchmark rate) includes negative values. Differences in observation counts across variables reflect heterogeneous field coverage across banks—some institutions did not report specific items—resulting in variable-specific missingness. To preserve the estimating sample, all control variables are modeled as categorical, with missing values assigned to an explicit category. This approach prevents casewise exclusion due to incomplete covariates and allows fixed effects to absorb level differences associated with missingness.

Table 2: DID Results for Credit Line Analysis

Dep. Var.:	**ln(Credit Line)**			
	(1)	(2)	(3)	(4)
Treat × Post	0.633***	0.353***	0.336***	0.184***
	(8.43)	(7.16)	(5.59)	(3.26)
Constant	6.298***	6.469***	6.480***	6.572***
	(141.74)	(224.34)	(186.25)	(189.93)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,166	154,166	154,166	154,164
R-squared	0.310	0.426	0.429	0.543

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on ln(credit line). The specification includes the interaction $Treat \times Post$. The dependent variable is ln(Credit Line). Columns 1 to 4 progressively add fixed effects and controls as indicated in the table. All models include Time FE and City FE, with additional Bank FE, Industry FE, and control fixed effects added as shown. Standard errors are clustered by Bank × Year-Month.

Table 3: DID Results for Credit Line Analysis

Dep. Var.:	**Credit Line**			
	(1)	(2)	(3)	(4)
Treat × Post	1,517.988***	544.310***	608.842***	351.592***
	(10.17)	(6.11)	(5.13)	(3.50)
Constant	592.903***	1,188.557***	1,149.079**	1,306.428***
	(6.44)	(22.64)	(16.58)	(21.31)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,166	154,166	154,166	154,164
R-squared	0.208	0.294	0.296	0.436

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on credit lines. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is Credit Line. Columns 1 to 4 progressively add fixed effects and controls as indicated in the table. All models include Time FE and City FE, with additional Bank FE, Industry FE, and other control fixed effects added as shown. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 4: DID Results for Interest Rate Analysis

Dep. Var.:	**Interest Rate**			
	(1)	(2)	(3)	(4)
Treat × Post	0.098	-0.381***	-0.469***	-0.413***
	(0.54)	(-4.19)	(-4.42)	(-3.62)
Constant	7.230***	7.523***	7.576***	7.542***
	(62.83)	(142.49)	(122.91)	(107.65)
Year-Month FE	No	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,217	154,217	154,217	154,215
R-squared	0.492	0.617	0.619	0.670

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on interest rates. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is Interest Rate. Columns 1 to 4 progressively add fixed effects and controls as indicated in the table. All models include Time FE and City FE, with additional Bank FE, Industry FE, and other control fixed effects added as shown. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 5: DID Results for Interest Spread Analysis

Dep. Var.:	**Interest Spread**			
	(1)	(2)	(3)	(4)
Treat × Post	0.086	-0.403***	-0.498***	-0.451***
	(0.47)	(-4.21)	(-4.40)	(-3.80)
Constant	2.602***	2.902***	2.960***	2.931***
	(22.49)	(52.06)	(44.93)	(40.18)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,217	154,217	154,217	154,215
R-squared	0.423	0.566	0.568	0.628

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on interest spreads. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is Interest Spread. Columns 1 to 4 progressively add fixed effects and controls as indicated in the table. All models include City FE and Time FE, with additional Bank FE, Industry FE, and other control fixed effects added as shown. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 6: DID Results by Industry

Dep. Var.:	**ln(Credit Line)**				
	A	C	E	F	G
Treat × Post	0.120	0.125***	0.067	0.240***	0.094
	(0.73)	(3.54)	(0.95)	(2.99)	(1.15)
Constant	5.898***	6.685***	6.557***	6.560***	6.535***
	(66.68)	(297.24)	(190.89)	(132.10)	(148.78)
Year-Month FE	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes
Control FE	Yes	Yes	Yes	Yes	Yes
Observations	3,484	34,370	11,452	63,381	3,682
R-squared	0.603	0.647	0.537	0.525	0.702

Dep. Var.:	**ln(Credit Line)**			
	H	I	L	O
Treat × Post	0.059	0.801***	0.171	0.168
	(0.48)	(6.62)	(1.40)	(1.24)
Constant	6.428***	6.489***	6.743***	6.373***
	(90.66)	(60.40)	(86.58)	(87.91)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Control FE	Yes	Yes	Yes	Yes
Observations	4,760	7,077	4,514	3,573
R-squared	0.580	0.475	0.636	0.541

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table examines how invoice digitization influence log(credit line) by industry in the Gold Tax III Project. All fixed effects are consistent with the previous model, except for the industry fixed effect, which is removed because we split the industry.

Industry Code Definitions

Code	**Industry**
A	Agriculture, forestry, animal husbandry and fishery industry
C	Manufacturing industry
E	Construction industry
F	Wholesale and retail industry
G	Transportation, warehousing and postal industry
H	Accommodation and catering industry
I	Information transmission, software and information technology service industry
L	Leasing business services industry
O	Residential services, repair and other service industry

Table 7: DID Results for ln(Credit Line): Borrower Type and Bank Size

Dep. Var.:	**ln(Income)**			
	Borrower Type		**Bank Size**	
	Individually-Owned Business	Small Enterprise	Small	Large
Treat × Post	0.165**	0.089***	0.167**	0.091***
	(2.25)	(3.19)	(2.45)	(3.40)
Constant	6.388***	7.858***	6.497***	7.414***
	(150.86)	(341.85)	(150.48)	(668.56)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Control FE	Yes	Yes	Yes	Yes
Observations	132,635	21,482	139,255	14,870
R-squared	0.457	0.347	0.518	0.660

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on credit lines. The dependent variable is ln(Credit Line). The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post period. Columns report separate regressions for four subsamples, individually owned businesses, small enterprises, small banks, large banks, with subsamples defined by borrower type or ex ante bank size. All models include City FE, Time FE, Bank FE, Industry FE, and other control fixed effects as indicated. Borrower type fixed effects are omitted by construction because the sample is split by borrower type. Bank fixed effects are retained even though the sample is split by bank size. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 8: DID Results for Income Analysis

Dep. Var.:	**ln(Income)**			
	(1)	(2)	(3)	(4)
Treat × Post	0.314***	0.211***	0.212***	0.193***
	(3.47)	(3.32)	(3.17)	(3.45)
Constant	14.756***	14.820***	14.820***	14.831***
	(264.00)	(375.69)	(355.83)	(428.26)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	126,263	126,263	126,263	126,262
R-squared	0.292	0.376	0.385	0.480

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on income. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is ln(Income). Columns 1 to 4 progressively add fixed effects and controls as indicated in the table, models include City FE and Time FE in all columns, with Bank FE, Industry FE, and other control fixed effects added as shown. Income fixed effects are omitted by construction since income is the dependent variable. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 9: DID Results for Credit Score Analysis

Dep. Var.:	**Credit Score**			
	(1)	(2)	(3)	(4)
Treat × Post	-3.222	1.931	3.319**	6.752***
	(-1.64)	(1.21)	(2.00)	(4.70)
Constant	48.733***	44.357***	43.177***	40.262***
	(31.19)	(32.80)	(30.83)	(33.11)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	101,224	101,224	101,223	101,223
R-squared	0.081	0.118	0.120	0.151

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on credit scores. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is Credit Score. Columns 1 to 4 progressively add fixed effects and controls as indicated in the table, models include City FE and Time FE in all columns, with Bank FE, Industry FE, and other control fixed effects added as shown. Credit score fixed effects are omitted by construction since credit score is the dependent variable. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 10: DID Results for Loan Term Analysis

Dep. Var.:	**Loan Term**			
	(1)	(2)	(3)	(4)
Treat × Post	4.688	62.286***	74.902***	48.530**
	(0.15)	(2.61)	(3.30)	(2.27)
Constant	503.969***	468.733***	461.015***	477.151***
	(19.86)	(24.85)	(26.80)	(34.03)
Year-Month FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,166	154,166	154,166	154,166
R-squared	0.313	0.452	0.452	0.610

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: This table reports difference in differences estimates of the reform effect on loan terms. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. The dependent variable is Loan Term (measured in days, units follow the source dataset). Columns 1 to 4 progressively add fixed effects and controls as indicated in the table. All models include City FE and Time FE, with additional Bank FE, Industry FE, and other control fixed effects added as shown. Loan term fixed effects are omitted by construction since loan term is the dependent variable. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 11: Distance Mechanism: Distance, Credit Lines, and Income

Dep. Var.	**ln(Distance)**	**ln(Credit Line)**				**ln(Income)**
	(1) Lending Distance	(2) Straight line	(3) Driving distance	(4) Pre (gradient)	(5) Post (gradient)	(6) Income
Treat × Post	0.135***	0.083	−0.343			0.172
	(2.78)	(0.55)	(−1.83)			(0.47)
ln(Distance)		−0.124***		−0.123***	−0.070***	−0.212***
		(−20.75)		(−21.97)	(−7.83)	(−12.49)
ln(Driving Distance)			−0.119***			
			(−21.55)			
Treat × Post × ln(Distance)		0.052***				0.155***
		(4.43)				(7.95)
Treat × Post × ln(Driving Distance)			0.057***			
			(5.22)			
Constant	2.855***	6.715***	7.551***	6.583***	6.930***	15.273***
	(121.10)	(91.91)	(81.85)	(386.46)	(280.06)	(79.86)
City × YM FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE						Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Control FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	93,993	93,993	93,993	48,626	45,337	86,026
R-squared	0.395	0.533	0.533	0.458	0.544	0.430

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Columns report difference in differences estimates using four dependent variables, ln(Distance), ln(Credit Line) with straight-line distance, ln(Credit Line) with driving distance, and ln(Income), along with distance gradients estimated separately in the pre- and post-reform samples (Columns 4–5). Distance measures are in kilometers. Models include City × YM FE (City × Year-Month), Bank FE, Industry FE, and other control fixed effects as indicated in the table; Column 6 additionally lists Year-Month FE as in the original specification. Standard errors are clustered by Bank × Year-Month (BankYM). Throughout, ln denotes the natural logarithm.

Table 12: Policy Effect by Competition: Interest Rate and Distance

Dep. Var.	**Interest Rate**	**ln(Distance)**	
	(1)	(2) Low HHI	(3) High HHI
Treat × Post	−0.833***	0.637***	0.312***
	(−4.12)	(5.38)	(5.14)
HHI	5.134***	24.993***	4.855***
	(7.09)	(20.10)	(25.31)
Treat × Post × HHI	2.834***	−9.124***	−1.302***
	(4.27)	(−6.10)	(−4.44)
Constant	7.124***	0.532***	2.256***
	(56.10)	(5.03)	(61.26)
City FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Bank FE		Yes	Yes
BankYM FE	Yes		
Industry FE	Yes	Yes	Yes
Control FE	Yes	Yes	Yes
Observations	140,252	56,943	39,829
R-squared	0.663	0.195	0.241

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Column (1) reproduces Table 16 (Interest Rate) and Columns (2)–(3) reproduce Table 17 (ln(Distance)) without any change to coefficients, t-statistics, or fixed effects. Low/High HHI are defined by the pre-reform city-level HHI median. Column (1) includes City FE, Year-Month FE, BankYM FE (Bank × Year-Month), Industry FE, and other control fixed effects. Columns (2)–(3) include City FE, Year-Month FE, Bank FE, Industry FE, and other control fixed effects. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 13: Policy Effects by Invoicing Intensity and Information Quality

Dep. Var.	**ln(Credit Line)**	**Interest Rate**
	(1)	(2)
Treat × Post × High invoicing	0.189***	
	(3.32)	
Treat × Post × Low invoicing	0.152	
	(1.50)	
Treat × Post		−0.651***
		(−2.84)
Problematic Invoice Ratio		−0.0065***
		(−3.62)
Treat × Post ×		0.0071**
Problematic Invoice Ratio		(2.44)
Constant	6.601***	8.735***
	(175.78)	(77.59)
City FE	Yes	Yes
Year-Month FE	Yes	Yes
Bank FE	Yes	
BankYM FE		Yes
Industry FE	Yes	Yes
Control FE	Yes	Yes
City × Invoicing FE	Yes	
Year-Month × Invoicing FE	Yes	
Observations	141,922	75,457
R-squared	0.557	0.535

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Column (1) reproduces the DDD specification by invoicing intensity with interactions $Treat \times Post \times$ High invoicing and $Treat \times Post \times$ Low invoicing; the dependent variable is ln(Credit Line). Column (2) reproduces the DID specification using information quality with $Treat \times Post \times$ Problematic Invoice Ratio; the dependent variable is Interest Rate. Low/High invoicing denote ex ante invoicing-intensity groups; the Problematic Invoice Ratio is the share of invoices flagged in tax audits (higher values indicate lower information quality). Column (1) includes City FE, Year-Month FE, Bank FE, Industry FE, Control FE, and group-specific FE City × Invoicing and Year-Month × Invoicing. Column (2) includes City FE, Year-Month FE, BankYM FE (Bank × Year-Month), Industry FE, and Control FE. Standard errors are clustered by BankYM (Bank × Year-Month).

Table 14: Summary Statistics: macro Credit Line from PBOC

VarName	Obs	Mean	SD	Min	Median	Max
Credit Line	9586	13700.221	16713.155	13.560	7618.515	119450.216

Notes: This table reports summary statistics for the macro Credit Line series compiled from the People's Bank of China. Statistics are computed on non-missing observations, values are in levels and nominal terms. Currency and measurement units follow the source dataset. Observations refer to the number of region by period entries in the sample, if applicable. Mean, SD, Min, Median, Max are calculated over the full sample.

Table 15: DID Results for macro Credit Line from PBOC

Dep. Var.:	Credit Line	ln(Credit Line)
Treat × Post	5,379.854***	0.087***
	(15.68)	(3.49)
Constant	8,594.243***	8.701***
	(43.24)	(1,718.70)
Year-Month FE	Yes	Yes
Borrower Type FE	Yes	Yes
Loan Term FE	Yes	Yes
Observations	9,586	9,586
R-squared	0.256	0.296

Robust t-statistics in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Note: This table reports difference in differences estimates of the reform effect on credit supply using the macro Credit Line series from the People's Bank of China. The specification includes the interaction $Treat \times Post$, where Treat indicates treated units and Post indicates the post reform period. Columns report level and log specifications with dependent variables Credit Line and ln(Credit Line). All models include Time FE, Borrower Type FE, Loan Term FE. Standard errors are clustered by ProvinceYM (Province × Year-Month). Currency and measurement units follow the source dataset. Throughout, ln denotes the natural logarithm.

Figures

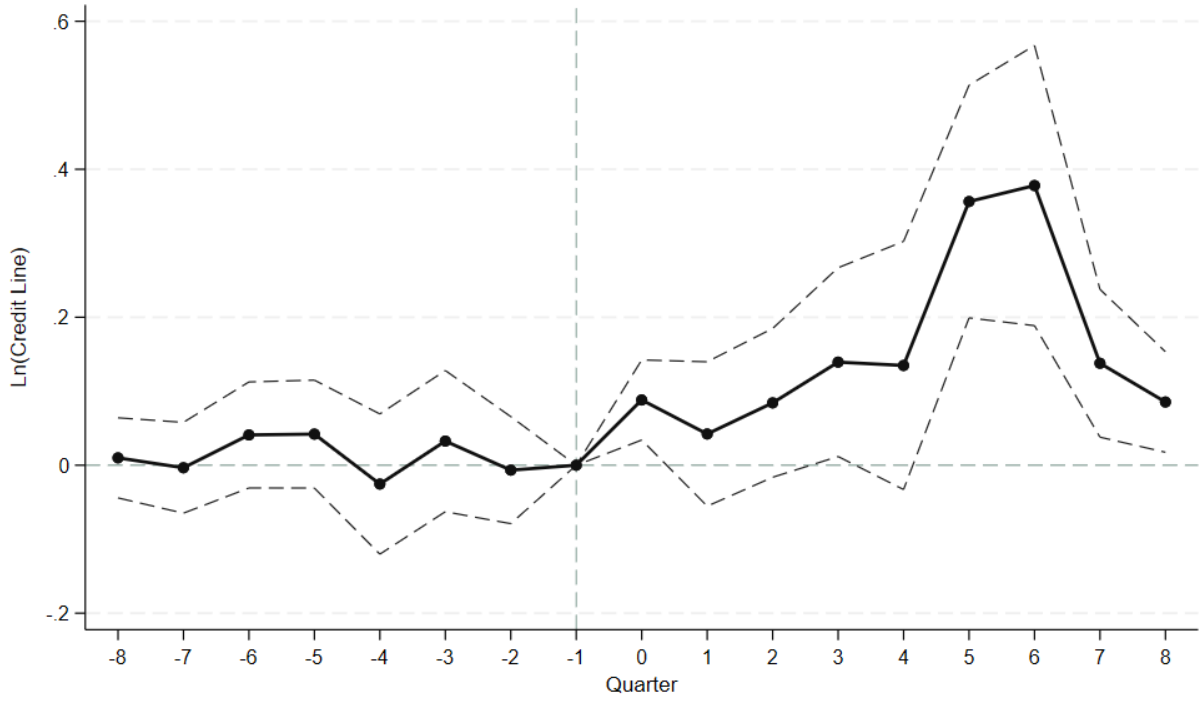


Figure 1: Parallel Trend: Treat*Post

Note: The figure presents estimated coefficients and 95% confidence intervals from an event study based on Equation (2), controlling for all fixed effects mentioned in the model. The reference period is -1(one quarter before the policy start). Standard errors are clustered at the Bank*Year-Month level.

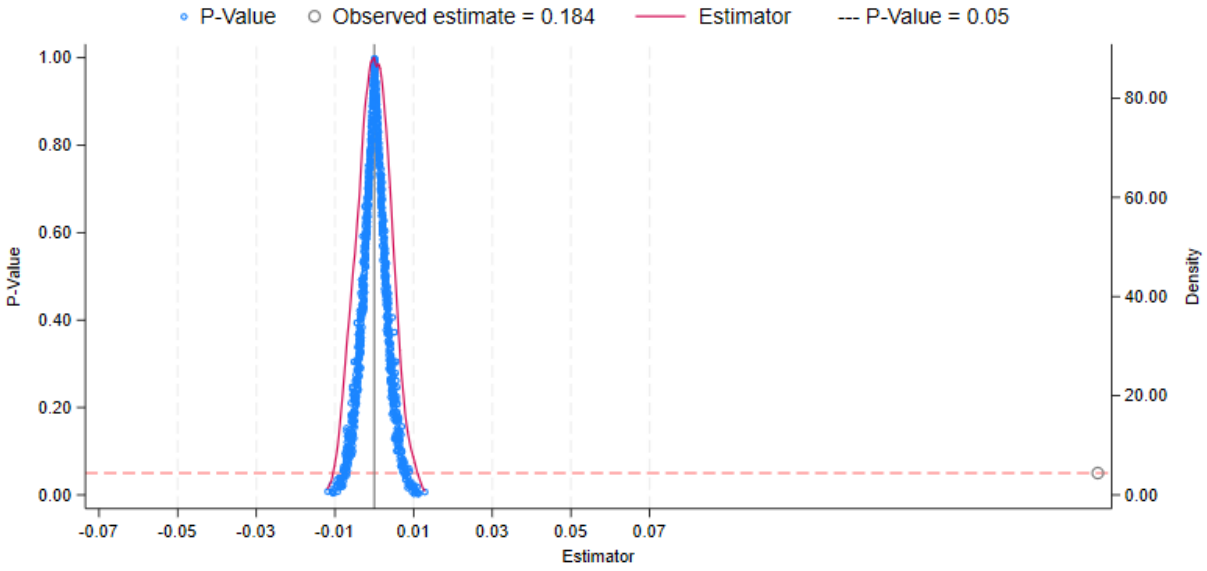


Figure 2: Placebo Test

Note: The figure presents a placebo test with 1000 replications as a robustness check for the Difference-in-Differences estimation; which is designed to mimic the null of no treatment effect by randomly reassigning both the timing and location of the policy intervention.

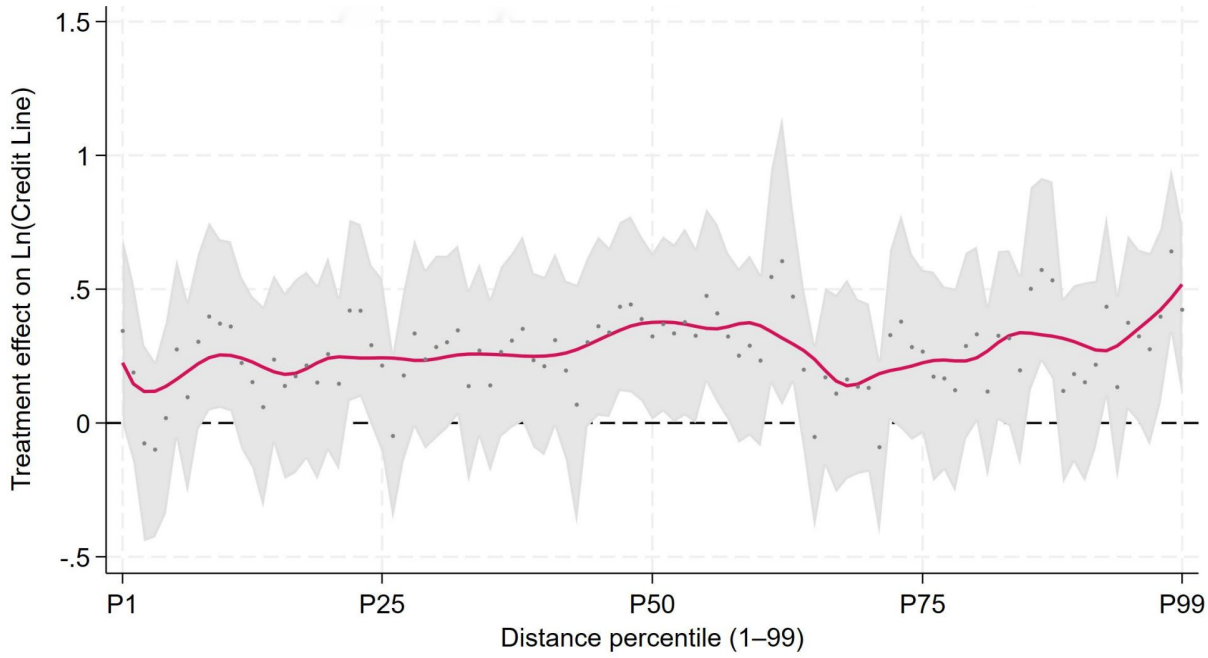


Figure 3: Policy Effect by Geographic Distance

Note: The figure presents estimated coefficients for each percentile in distance and 95% confidence intervals from the result of Equation (4),controlling for all fixed effects mentioned in the model. The reference period is -1(one quarter before the policy start). Standard errors are clustered at the Bank*Year-Month level.

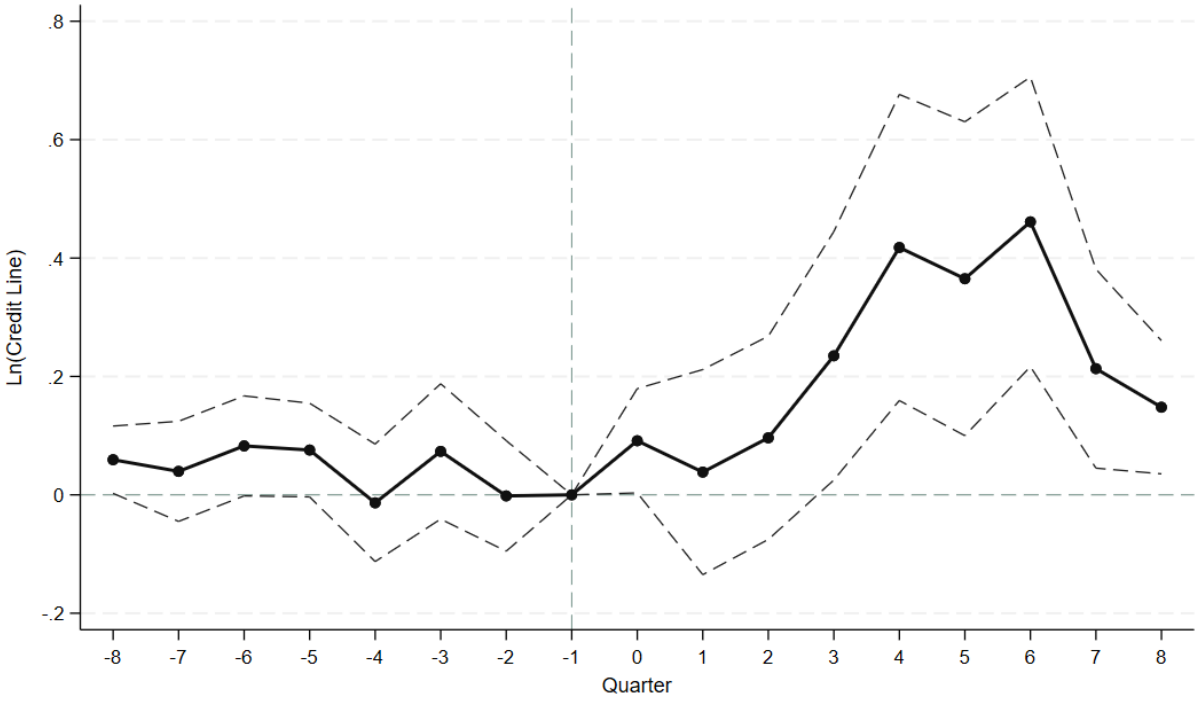


Figure 4: Parallel Trend: Treat*Post*Distance

Note: The figure presents estimated coefficients and 95% confidence intervals from an event study based on Equation (5), controlling for firm and time fixed effects. The reference period is -1(one quarter before the policy start). Standard errors are clustered at the Bank*Year-Month level.

Appendix A. Fixed Effects and Control Structure

This appendix lists all fixed effects (FEs) used across specifications, explains each FE, and documents (i) the column-by-column progression of FEs in the main tables and (ii) outcome-specific FE inclusions/exclusions to avoid definitional collinearity. Standard errors are clustered at the Bank*Year-Month level. For all FEs, I set the missing values to -999 to accommodate as many observations as possible.

A.1 Fixed effects used in the paper

- `time FE` (monthly): absorbs economy-wide shocks common to all units in month t.
- `geography FE` (city-level baseline): absorbs time-invariant regional heterogeneity.
- `bank FE`: absorbs bank-specific, time-invariant differences (risk appetite, underwriting standards, funding model).
- `industry FE` (menlei, first-level classification): captures time-invariant sectoral differences (e.g., invoice intensity, tax structure). I adopt first-level categories to maximize sample coverage under the Chinese industrial classification.
- `borrower-type FE`: distinguishes whether the borrower takes the loan in a personal name or in a corporate name.
- `age FE` (persons only): defined only for personal-name loans and discretized into five groups (quintiles) based on the within-personal-loan distribution; no artificial "missing-age" category is created for corporate loans.
- `income FE`: borrower income discretized into five groups (quintiles) based on inner group distribution(quintile) to capture non-linear level differences across income strata .
- `leverage FE` (corporate only): asset–liability ratio and discretized into five groups (quintiles) based on inner group distribution.
- `credit-score FE`: some banks report categorical grades, others a numeric score. I use bank + category as the new credit score category FE. For the numeric credit score, I divide it into 10 and then add the bank to it as the new credit score category FE. Also for comparability, I map each borrower's raw grade/score to a within-bank percentile rank on a 1–100 scale (1 = lowest credit quality; 100 = highest). Separately, I construct

a continuous standardized score `Credit_Score` (1–100) only for outcome analyses(as y, not as FE); when this is the dependent variable, the internal credit-score FE is omitted.

- `loan term FE`: maturity buckets $\{< 1\text{y}, 1\text{–}3\text{y}, 3\text{–}5\text{y}, > 5\text{y}\}$ capturing constant pricing differences tied to term structure.
- `repayment-method FE`: e.g., amortizing vs. interest-only; captures systematic pricing/underwriting differences.

A.2 Column-by-column FE progression in main tables

The main tables build FEs cumulatively across columns as follows:

Column (1) `absorb(geography FE, time FE)`.

Column (2) Adds banks: `absorb(geography FE, time FE, bank FE)`.

Column (3) Adds industries: `absorb(geography FE, time FE, bank FE, industry FE)`.

Column (4) Full controls on top of Column (3):
`absorb(geography FE, time FE, bank FE, industry FE, repayment-method FE, age FE, borrower-type FE, income FE, leverage FE, credit-score FE, term FE)`.
Scope restrictions: age FE applies to personal-name loans only; leverage FE applies to corporate-name loans only.

A.3 Outcome-specific FE inclusion/exclusion (collinearity rule)

By default, all outcomes follow the column progression in Section A.2 and include the full set of control fixed effects listed in Section A.1. To avoid definitional (near-)collinearity, I remove exactly one control FE when it is mechanically tied to the dependent variable, as detailed below.

(i) `log_Income` I omit the *income FE*. This FE is a coarsened discretization of the same underlying income measure as the dependent variable; including it would mechanically soak up the very cross-sectional variation the regression is intended to explain and may induce (near-)collinearity. All other control FEs are retained.

(ii) `Loan_Term` I omit the *loan term FE*. The term FE is directly constructed from the dependent variable (maturity in days); including it would deterministically partition the outcome and eliminate the variation of interest across term buckets. All other control FEs are retained.

(iii) `Credit_Score` (1–100) I omit the *internal credit-score FE*. This FE is derived from the same internal scoring system that underlies the dependent variable (via the within-bank percentile mapping); including it would partial out the outcome itself. All other control FEs are retained.

(iv) Other outcomes: `log_Credit_Line`, `Credit_Line`, `Interest_Rate`, `Rate_Difference` No control FE is definitionally collinear with these outcomes, so I keep the full control set following the column progression in Section A.2.

Appendix B. Provinces in the PBOC Macro Sample

Table 16: PBOC Macro Sample: Provinces

Province / Region
Shanghai Municipality
Inner Mongolia Autonomous Region
Beijing Municipality
Jilin Province
Sichuan Province
Tianjin Municipality
Anhui Province
Shandong Province
Guangdong Province
Guangxi Zhuang Autonomous Region
Xinjiang Uygur Autonomous Region
Jiangsu Province
Hebei Province
Zhejiang Province
Hainan Province
Gansu Province
Fujian Province
Tibet Autonomous Region
Guizhou Province
Shaanxi Province
Qinghai Province

Appendix C. Golden Tax Phase III Rollout by Province

This appendix lists the go live dates for Golden Tax Phase III by province level unit. Dates use YYYY.MM.DD.

Table 17: Golden Tax Phase III launch dates

Province or Municipality	Launch date	Province or Municipality	Launch date
Chongqing Municipality	2013.02.22	Shanxi Province	2013.10.08
Shandong Province	2013.10.08	Guangdong Province	2015.01.08
Henan Province	2015.01.08	Inner Mongolia Autonomous Region	2015.01.08
Ningxia Hui Autonomous Region	2015.07.01	Tibet Autonomous Region	2015.09.01
Hebei Province	2015.09.08	Guizhou Province	2015.09.08
Yunnan Province	2015.09.08	Guangxi Zhuang Autonomous Region	2015.09.08
Hunan Province	2015.10.08	Qinghai Province	2015.10.08
Hainan Province	2015.10.08	Gansu Province	2015.10.08
Anhui Province	2016.01.08	Xinjiang Uygur Autonomous Region	2016.01.08
Sichuan Province	2016.01.08	Jilin Province	2016.01.08
Shanghai Municipality	2016.07.08	Fujian Province	2016.07.08
Jiangxi Province	2016.07.08	Liaoning Province	2016.07.08
Beijing Municipality	2016.08.08	Tianjin Municipality	2016.08.08
Heilongjiang Province	2016.08.08	Hubei Province	2016.08.08
Shaanxi Province	2016.08.08	Jiangsu Province	2016.10.08
Zhejiang Province	2016.10.08		