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

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Abstract

This paper asks whether making firms' value added tax (VAT) invoices digitally verifiable relaxes information frictions in small business lending. I exploit the staggered provincial rollout of China's Golden Tax Phase III (GT3), which unified tax reporting and introduced security coded electronic VAT invoices that lenders can authenticate within the tax system. Using loan originations from multiple banks and an event study difference in differences design around province go live dates, I find that GT3 expands credit availability and improves contract terms for small firms. Banks record higher borrower income and assign higher internal credit scores, consistent with improved underwriting inputs. Lending becomes less local, and the pre reform distance penalty in credit limits attenuates, consistent with portable hard information substituting for relationship based soft information. Effects are larger in industries with weaker ex ante invoice paper trails and in more competitive local banking markets, weaker when audit based measures indicate noisier invoice environments, and no longer detectable once subway openings reduce travel frictions and facilitate soft information production. Bank financial statements show higher net interest income without faster loan growth or worse asset quality, consistent with improved pricing rather than relaxed standards.

Keywords: Banking, Credit, Financial Technology, Information Asymmetry.

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1 Introduction

Imperfect information about borrower cash flows and risk is a first order friction in credit markets, distorting screening, pricing, and credit allocation when lenders cannot credibly verify borrower quality (Stiglitz and Weiss, 1981; Leland and Pyle, 1977). The problem is especially acute for small businesses, which often lack audited financial statements and standardized income documentation that lenders can verify at low cost (Petersen and Rajan, 1994; Minnis, 2011). In these settings, banks rely on relationship based soft information, but soft information is hard to verify and transmit within organizations, and it decays with geographic distance (Stein, 2002). By contrast, hard information that is standardized and auditable can travel across hierarchy and space, enabling more arms length lending and weakening the link between proximity and contracting (Stein, 2002). This paper asks whether an exogenous increase in verifiable transaction records causes hard information to substitute for soft information in small business lending, and who benefits from that shift.

A hard information shock delivers sharp, testable predictions. If public infrastructure converts transaction records into standardized, verified signals that banks can verify at low cost, credit supply should expand and pricing should become more risk sensitive. Because soft information is local, the shock should also weaken distance related frictions, flattening the pre reform distance gradient in loan size and allowing lending to extend farther from branches. Finally, if the mechanism is substitution, the marginal effect should be smaller where soft information becomes cheaper to produce, or where banks already have stronger capacity to process hard data (Stein, 2002; Berger et al., 2005).

China's Golden Tax Phase III (GT3)[1] provides a rare policy shock to the verifiability of firm transactions. The reform unified tax reporting and introduced security code digital VAT invoice that can be verified within the tax system, and it was rolled out across provinces on an administrative schedule that generates staggered variation in when invoice information becomes bank verifiable. This institutional setting offers plausibly exogenous variation in the

[1]Hereafter, I refer to Golden Tax Phase III as GT3.

supply of verifiable cash flow records, separate from banks' voluntary technology adoption and from borrowers' selection into data generating platforms (Kleven et al., 2011; Pomeranz, 2015). This design also speaks to a broad audience, including the United States, where small business credit remains relationship intensive and where the absence of a federal VAT and a centralized invoice verification rail makes it hard to observe an economy wide and plausibly exogenous hardening of business transaction records.

I study these predictions using a private loan level dataset from multiple banks nationwide covering 2012 to 2022, with a focus on small business inclusive finance lending. Because lending decisions are made at city branches, I can link each loan to borrower branch distance and measure banks' spatial reach. I estimate staggered difference-in-differences and event study specifications around province adoption, and I complement average effects with targeted heterogeneity tests that map directly into the hard information mechanism, including distance, local market concentration, invoicing intensity, and invoice quality.

The results show that invoice digitization expands credit availability. After implementation, small firms receive larger credit limits, lower loan rates and spreads, and longer maturities. Banks also revise recorded borrower income upward and assign higher internal credit scores, consistent with improved underwriting inputs when invoices become verifiable. Event study estimates show no evidence of differential pre trends and indicate that effects emerge after implementation and persist.

Mechanism and boundary condition evidence supports hard information substituting for soft information. Lending becomes less local: average borrower branch distance rises and the pre reform distance penalty in loan size attenuates markedly, consistent with verifiable signals traveling better across space and hierarchy than relationship based inputs. Effects are stronger in industries with weaker invoice based paper trails ex ante (more consumer facing and information poor) and in more competitive local markets, consistent with the marginal value of verifiable hard information being higher where baseline verifiability is limited and with competition governing pass through (Pomeranz, 2015; Artavanis et al., 2016). The

policy effects are also weakened in cities where the opening of subway lines increases the availability of soft information, or in banks with greater financial technology capabilities.

Using bank financial statements, I find higher net interest income without faster loan growth or worse asset quality, pointing to improved pricing rather than relaxed lending standards.

To implement these tests and keep the evidence tightly linked to the mechanism, I combine the loan data with several targeted auxiliary datasets. I compute borrower branch distance from geocoded borrower and branch locations, constructing both straight line and driving distance measures using digital map based calculations, so the core policy by distance specification directly quantifies how invoice verifiability changes the distance gradient in credit supply. To proxy time variation in the cost of producing relationship based soft information, I hand collect city subway opening dates and estimate specifications that compare treated and untreated province borrowers within the same city and month before and after subway operations begin. To measure banks' hard information processing capacity, I merge the Peking University Commercial Bank Digital Transformation Index and classify banks by digitalization when estimating heterogeneous treatment effects. To study incidence and rule out relaxed standards, I use commercial bank income statement outcomes from CSMAR in a bank year panel, focusing on banks whose branch networks are confined to a single province so that exposure maps cleanly to the local Phase III rollout. To assess scope, I also assemble listed firms' borrowing events from publicly available borrowing announcements and show that loan amounts for large, transparent borrowers do not respond, consistent with the reform operating through the marginal value of verifiable hard information.

This paper makes three contributions. First, it bridges two literatures that often operate in parallel. In corporate finance and accounting, evidence on hard information in debt contracting largely comes from larger and public firms, where audited financial statements and disclosure quality are observable and verification affects borrowing costs and contract terms (Partha Sengupta, 1998; Bharath et al., 2008; Costello and Wittenberg-Moerman,

2011; Minnis, 2011; Hanlon and Heitzman, 2010). In contrast, the small business lending literature emphasizes soft information and relationship lending, documenting distance gradients and organizational limits to transmitting soft information (Petersen and Rajan, 1994, 2002; Stein, 2002; Degryse and Ongena, 2005; Agarwal and Hauswald, 2010). Much less is known about whether supplying verifiable hard information for small private firms can causally relax these frictions, in part because many small firms do not produce audited statements and their transaction records are not standardized or bank verifiable (Allee and Yohn, 2009; Minnis, 2011). By exploiting the GT3 rollout, I provide causal evidence that hardening transaction records expands credit supply and reshapes the spatial and cross sectional allocation of credit toward borrowers and intermediaries that were previously more reliant on soft information.

Second, the paper complements the growing fintech and alternative data literature by delivering clean identification of a hard information supply shock. Causal evidence on hard information substituting for soft information is hard to obtain because information environments typically change endogenously, banks choose whether to adopt information technologies, borrowers select into digital platforms, and the resulting data are proprietary and bundled with changes in underwriting and market structure (Buchak et al., 2018; Fuster et al., 2018). A growing literature shows that digital footprints and automated scoring can predict credit risk and expand access, but these settings combine data generation with selection and platform specific contracting (Berg et al., 2020). In contrast, the GT3 hardens transaction records through public infrastructure for incumbent banks and borrowers regardless of platform choice, which allows me to isolate how verifiability changes lending distance, pricing, and who benefits.

Third, the paper contributes to the literature on tax based information and information sharing by showing how public digital tax infrastructure can shape private credit markets. Where the VAT chain and third party reporting strengthen verifiability, invoice digitization improves the precision of bank side income measurement and risk based pricing, and mar-

ket structure governs how much of that informational gain is passed through to borrowers (Kleven et al., 2011; Pomeranz, 2015; Hertzberg et al., 2011; Yannelis and Zhang, 2023). By connecting bank internal underwriting inputs to contract terms and to competitive pass through, the results provide design relevant evidence on when and how verifiable public records relax financing frictions for small businesses.

The remainder of the paper is organized as follows. Section 2 describes the GT3 and the invoice verification infrastructure. Section 3 presents the data and identification strategy. Section 4 reports baseline effects and event study and placebo evidence. Section 5 studies bank underwriting inputs and evidence based on invoicing intensity and information quality. Section 6 tests substitution between hard and soft information using distance, competition, bank organization, and counterfactual comparisons. Section 7 addresses alternative interpretations and incidence, and Section 8 concludes.

2 Institutional Setting: Golden Tax Phase III and Invoice Digitization

China operates a value added tax (VAT) system in which business transactions are documented with VAT invoices. Purchasers use these invoices to claim input tax credits, so the invoice trail is a natural third party record of firm sales. In principle, invoices can discipline reporting and support verification. In practice, prior to GT3, invoicing and verification were largely paper based and fragmented across local tax bureaus, which made authentication slow and costly for lenders and limited the use of invoices as scalable hard information in underwriting.

Before GT3, firms typically issued VAT invoices on pre printed paper forms using tax control devices and physical stamps, and the resulting invoices circulated and were archived as paper documents. Banks therefore observed invoices mainly through borrower submitted copies and manual document checks. Even when authenticity could be confirmed through ad

hoc inquiries, the process was not standardized across locations and was rarely cost effective to apply at scale for small borrowers. After a province goes live under GT3, invoices can be issued in electronic form and are recorded digitally at issuance. Electronic VAT invoices carry a security code, so a presented invoice can be authenticated by matching that code to the tax system record. Borrowers may still print an invoice for bookkeeping, but the relevant object for verification is the electronic record, which makes routine authentication faster and more uniform.

GT3 operationalized this shift by deploying a unified national tax platform with standardized invoice definitions and an electronic invoicing module. For banks, GT3 lowers the marginal cost of turning invoices into verifiable hard information about transactions and cash flows, relative to reliance on relationship based soft information. The platform was rolled out province by province on an administrative schedule (Appendix Table 18), generating staggered variation in when invoice information becomes digitally verifiable. In the loan level analysis, I treat a province as exposed from its GT3 go live date onward, and in annual panels exposure starts in the rollout year.

3 Data and Research Design

3.1 Data

I assemble four primary datasets that correspond to the four empirical components of the paper (loan level outcomes, tax mechanism measures, bank profitability and risk, and listed firm borrowing events). Additional shifters used for mechanism and heterogeneity tests (distance, market structure, subway access, invoice quality, and bank digitalization) are merged into the main loan data and are summarized briefly here, with construction details reported in an online appendix.

(1) Loan origination data, main sample. The main analysis uses a confidential loan origination dataset that aggregates small business credit records from multiple banks nationwide. The unit of observation is a loan origination. The sample spans 2012 to 2022 and includes contract terms (credit line, interest rate, maturity), borrower identifiers and characteristics, the originating bank identifier, and the city of the originating branch. My primary outcome is the approved credit line. I express credit line in thousands of RMB and use ln(Credit Line). I also study loan pricing (interest rate and interest spread) and non price terms (maturity in days). Interest spread is defined as the contractual loan rate minus the benchmark Loan Prime Rate at origination. To capture underwriting inputs, I use bank recorded annual borrower income and analyze ln(Income). For readability, Table 1 reports Income in millions of RMB. To make internal ratings comparable across banks, I map each bank's categorical ratings into a within bank percentile based credit score on a 1 to 100 scale.

Policy exposure in this dataset is assigned using the province specific GT3 go live calendar date $\widetilde{T}_p$ (Appendix Table 18). For each loan with origination date $\widetilde{t}$ and borrower registration province $p(j)$, exposure equals $\mathbf{1}\{\widetilde{t} \geq \widetilde{T}_{p(j)}\}$. For fixed effects, clustering, and merges to city month covariates, I index time at the year month level using the origination date. Table 1 reports summary statistics for the main loan sample.

To maximize sample size, I discretize continuous controls into coarse bins and include an explicit missing category, so that regressions do not drop observations due to incomplete covariates. When an outcome is mechanically tied to a control block (income, maturity, or the percentile based internal credit score), I omit that specific control block to avoid definitional collinearity (Agarwal et al., 2024; Kabir and Ruan, 2025).

Merged shifters for mechanism and heterogeneity (loan sample). Several analyses require variables that are not native to the loan file but can be merged at the branch city by year month level, the borrower province by year level, or the bank by year level. First, I measure borrower to branch distance using geocoded borrower and branch locations and

compute straight line distance in kilometers (with a driving distance robustness measure). Second, I proxy local banking competition with a city by month Herfindahl Hirschman Index based on branch presence. Third, I code subway access using the first operating month of city subway systems and construct a city month indicator. Fourth, I measure invoice quality using a province year problematic invoice ratio from tax audit publications, mapped to loan months within the same calendar year. Fifth, I merge Peking University China Commercial Bank Digital Transformation Index and classify banks into high versus low fintech groups within each year. These variables enter either as shifters in interaction designs or as group indicators in triple difference style specifications, and their construction and merge keys are documented in the appendix.

(2) Tax and fiscal measures from CNRDS. To study the tax mechanism, I use province level tax and fiscal series from the CNRDS database. These measures provide an external view of the local tax environment that complements the loan level credit outcomes. The tax variables are observed at the province level, primarily at annual frequency, and are merged by borrower registration province and calendar year.

Summary statistics for the tax measures used in the mechanism section are reported in the appendix.

(3) Bank annual report panel. To assess whether GT3 operates through improved pricing rather than relaxed lending standards, I compile a bank year panel from publicly available annual reports (via CSMAR and related sources). The unit of observation is a bank fiscal year. I restrict the baseline panel to banks whose domestic branch network is confined to a single province, so that exposure maps cleanly to a unique provincial GT3 rollout year. Outcomes include net interest income scaled by assets, interest income and interest expense scaled by assets, quantity outcomes (loan growth and balance sheet loan measures), and risk outcomes (for example, the nonperforming loan ratio and provision coverage). The panel includes standard lagged balance sheet controls (size, capitalization, asset quality, liquidity,

operating costs, and funding structure). Bank panel summary statistics are reported in the appendix.

(4) Listed firm loan announcements. To test whether GT3 affects borrowing amounts for large and transparent borrowers, I use listed firm borrowing announcements from CSMAR. The unit of observation is a borrowing announcement event. I observe the announcement calendar date, the upper bound of the announced loan amount, the lending bank, and contract attributes such as loan type, purpose, currency, collateralization, and syndication status. A key requirement for assigning GT3 exposure in this sample is that the disclosure text reports the originating lending branch location. Accordingly, I restrict the sample to announcements for which the lending branch can be identified, and I exclude events without a clearly reported lending branch, events booked at head office, and foreign banks. Exposure is assigned at the day level using the announcement date and the GT3 go live date of the lending branch province, $\mathbf{1}\{\text{AnnDate} \geq \widetilde{T}_{p(\text{Branch})}\}$. Summary statistics for the announcements sample are reported in the appendix.

3.2 Empirical Strategy

3.2.1 Policy timing, exposure, and notation

I exploit the staggered province rollout of GT3, which makes VAT invoices digitally verifiable. Let p index provinces and let $\widetilde{T}_p$ denote the GT3 go live *calendar date* in province p.

Because the empirical exercises use datasets with different time frequencies, I construct GT3 exposure at the native frequency of each dataset. For any calendar date d, define the date level exposure indicator

$$GT3^{d}_{p,d} \equiv \mathbf{1}\{d \geq \widetilde{T}_p\}. \tag{1}$$

For outcomes observed at the month frequency, define the corresponding go live year month

as $T_p \equiv m(\widetilde{T}_p)$, where $m(\cdot)$ maps a calendar date into its year month, and define

$$GT3^m_{p,t} \equiv \mathbf{1}\{t \geq T_p\}, \tag{2}$$

where t indexes year month. For annual bank panel outcomes, define the go live calendar year as $\mathsf{RollYear}_p \equiv \text{year}(\widetilde{T}_p)$ and define

$$GT3^y_{p,y} \equiv \mathbf{1}\{y \geq \mathsf{RollYear}_p\}, \tag{3}$$

where y indexes fiscal years.

In the main loan level dataset, let i index loans, j borrowers, b banks, c the city of the originating branch, and let $\widetilde{t}_i$ denote the loan origination *calendar date*. Let $t \equiv m(\widetilde{t}_i)$ denote the corresponding origination year month. Let $p(j)$ denote the borrower registration province. Because branches can lend across provincial borders, $p(j)$ need not coincide with the province that contains branch city c. This feature allows specifications with city by year month fixed effects while retaining variation in province level exposure at the loan level.

For each loan, exposure is $GT3^d_{p(j),\widetilde{t}_i}$. The identifying assumption is a parallel trends restriction, absent GT3, outcomes in provinces that go live earlier and later would have evolved similarly, conditional on fixed effects and controls. I assess this assumption using event study specifications.

3.2.2 Baseline loan level difference-in-differences

For a generic loan outcome y_{ijbct}, I estimate the baseline specification

$$y_{ijbct} = \alpha + \beta\, GT3^d_{p(j),\widetilde{t}_i} + \mathcal{F}^{(0)}_{ijbct} + X'_{ijbct}\lambda + \varepsilon_{ijbct}, \tag{4}$$

where β captures the average effect of invoice digitization after GT3 goes live in the borrower province.

Baseline fixed effects and inference. Unless otherwise stated, the baseline fixed effect block $\mathcal{F}^{(0)}_{ijbct}$ includes year month fixed effects, city fixed effects, bank fixed effects, and industry fixed effects. Standard errors are clustered at the bank by year month level throughout the loan level analysis, allowing arbitrary correlation among loans originated by the same bank in the same month, following recent guidance for DID inference in panels (Kabir and Ruan, 2025).

3.2.3 Dynamic effects, event study

To study dynamics and verify parallel trends, I estimate an event study version of Equation (4). Let $q(\cdot)$ map a calendar date into its calendar quarter, and define event time in quarters as

$$\tau_{p,d} \equiv q(d) - q(\widetilde{T}_p).$$

Let $D_{k,pd} \equiv \mathbf{1}\{\tau_{p,d} = k\}$ indicate that province p is k quarters away from its go live quarter at calendar date d. I omit $k = -1$ (the quarter immediately prior to go live) as the reference period and estimate

$$y_{ijbct} = \alpha + \sum_{k \neq -1} \beta_k \, D_{k,p(j)\widetilde{t}_i} + \mathcal{F}^{(0)}_{ijbct} + X'_{ijbct}\lambda + \varepsilon_{ijbct}. \tag{5}$$

The coefficients $\{\beta_k\}$ trace the path of the policy effect relative to quarter -1. Parallel trends implies $\beta_k \approx 0$ for $k < 0$.

3.2.4 Continuous heterogeneity designs

Many mechanism tests interact exposure with a continuous shifter Z (distance, competition, invoice quality). I use the unified interaction specification

$$y_{ijbct} = \alpha + \beta_1 \, GT3^d_{p(j),\widetilde{t}_i} + \beta_2 \, Z_{ijbct} + \beta_3 \left(GT3^d_{p(j),\widetilde{t}_i} \times Z_{ijbct}\right) + \mathcal{F}^{(Z)}_{ijbct} + X'_{ijbct}\lambda + \varepsilon_{ijbct}. \tag{6}$$

Interpretation. The coefficient β_3 measures how the GT3 effect varies with Z. In the distance application, $Z_{ijbct} = \ln(\text{Distance}_{ijbct})$ where distance is measured between the borrower and the originating branch. In the market structure application, $Z_{ijbct} = HHI_{ct}$ where HHI is constructed from branch shares in city c and month t. In the information quality application, $Z_{ijbct} = \text{PIR}_{p(j),t}$ where PIR is the province year problematic invoice ratio mapped to loan months.

Fixed effects. The fixed effect block $\mathcal{F}^{(Z)}_{ijbct}$ is chosen to match the identifying comparison in each application. Across specifications, it may include a subset of year month fixed effects, city fixed effects or city by year month fixed effects, bank fixed effects or bank by year month fixed effects, province fixed effects, and industry fixed effects. I report the exact fixed effect structure in each table, and the notation in Equation (6) keeps the estimating equation compact while allowing the fixed effect set to vary across designs. If a particular fixed effect structure absorbs a lower order term, that coefficient is not separately identified and the corresponding table focuses on estimable parameters (typically interaction terms).

3.2.5 Event study with heterogeneous effects

To examine whether heterogeneous effects emerge only after go live, I interact event time indicators with Z. Let $\widetilde{Z}_{ijbct} \equiv Z_{ijbct} - \overline{Z}$ denote the demeaned shifter (demeaned within the estimation sample) so that event time coefficients are interpretable at the sample mean of Z. I estimate

$$y_{ijbct} = \alpha + \sum_{k \neq -1} \beta_k \, D_{k,p(j)\widetilde{t}_i} + \theta \, \widetilde{Z}_{ijbct} + \sum_{k \neq -1} \delta_k \left(D_{k,p(j)\widetilde{t}_i} \times \widetilde{Z}_{ijbct} \right) + \mathcal{F}^{(ES,Z)}_{ijbct} + X'_{ijbct}\lambda + \varepsilon_{ijbct}. \quad (7)$$

Here $\{\beta_k\}$ describes dynamics at the mean of Z, while $\{\delta_k\}$ describes how the event time path varies with Z.

3.2.6 Triple difference style designs

For discrete heterogeneity, I estimate triple interaction specifications that compare differential changes across groups. Let G_{ijbct} denote a group indicator (for example, high invoicing industries, subway availability, or high fintech banks). I estimate

$$y_{ijbct} = \alpha + \beta \left(GT3^{d}_{p(j),\widetilde{t}_i} \times G_{ijbct} \right) + \mathcal{F}^{(DDD)}_{ijbct} + X'_{ijbct}\lambda + \varepsilon_{ijbct}. \tag{8}$$

The coefficient β is the triple difference contrast, it captures whether the GT3 effect is different in group $G = 1$ relative to group $G = 0$, conditional on the fixed effects and controls in $\mathcal{F}^{(DDD)}_{ijbct}$ and X_{ijbct}.

3.2.7 Controls, missing values, and outcome specific omissions

To maximize sample size and avoid dropping observations due to incomplete covariates, I discretize continuous controls into coarse bins and include an explicit missing category. I then include these controls as indicator variables, following Kabir and Ruan (2025).

In some outcomes, one control block is constructed from the same raw variable as the dependent variable. Including that control block would mechanically absorb the outcome variation and can create definitional collinearity. Accordingly, when the dependent variable is ln(Income), I omit the income group fixed effects. When the dependent variable is maturity in days, I omit the maturity bucket indicators. When the dependent variable is the percentile based internal credit score, I omit the internal credit score category fixed effects. All other control blocks are retained.

3.2.8 Bank annual report panel, profitability and risk

To test whether GT3 operates through improved pricing rather than relaxed standards, I also estimate a bank year panel on annual report data. Let b index banks and let y index fiscal years. Let $p(b)$ denote bank b's province (restricting to banks whose branch network

is confined to a single province). I estimate

$$Y_{by} = \alpha_b + \eta_y + \beta\, GT3^{y}_{p(b),y} + \Gamma' W_{b,y-1} + u_{by}, \tag{9}$$

where α_b and η_y are bank and year fixed effects. The vector $W_{b,y-1}$ includes lagged balance sheet controls (size, capitalization, asset quality, liquidity, operating costs, and funding structure). Standard errors are clustered at the province level, consistent with the level of treatment variation (Bertrand et al., 2004).

3.2.9 Listed firm loan announcements

Finally, I test whether GT3 affects borrowing amounts for large, transparent firms using borrowing announcements by listed firms from CSMAR. Let e index announcements, f firms, and b banks. Let AnnDate_e denote the announcement calendar date.

A key requirement in this sample is that the disclosure text reports the originating *lending branch* location. Accordingly, I restrict the sample to announcements for which the lending branch can be identified. I exclude events without a clearly reported lending branch, as well as events booked at the head office or otherwise not tied to a specific branch. I also exclude foreign banks.

Exposure is assigned at the day level using the announcement date and the GT3 go live date of the lending branch province,

$$GT3^{\text{ann}}_{p(\text{Branch}_e),e} \equiv \mathbf{1}\{\text{AnnDate}_e \geq \widetilde{T}_{p(\text{Branch}_e)}\},$$

where $p(\text{Branch}_e)$ denotes the province of the lending branch identified from the disclosure.

Let CreditLine_{efb} denote the upper bound of the announced loan amount (in the units of the source announcements). I estimate

$$\ln\big(\text{CreditLine}_{efb}\big) = \alpha_f + \gamma_{m(\text{AnnDate}_e)} + \phi_b + \beta\, GT3^{\text{ann}}_{p(\text{Branch}_e),e} + \mathcal{F}^{(\text{contract})}_{efb} + \varepsilon_{efb}, \tag{10}$$

where α_f are firm fixed effects, $\gamma_{m(\text{AnnDate}_e)}$ are year month fixed effects, and ϕ_b are bank fixed effects. The term $\mathcal{F}_{efb}^{(\text{contract})}$ denotes contract attribute fixed effects included in richer specifications (loan type, purpose, currency, and additionally collateral and syndication status). Standard errors are clustered at the lending branch province by year month level.

4 Baseline Effects on Loan Contract Terms

This section reports baseline difference-in-differences estimates of the effect of GT3 implementation on loan contract terms. Unless otherwise noted, Tables 2–5 report estimates of β in Equation (4). Figure 1 implements the event-study specification in Equation (5), and Figure 2 reports a permutation-based placebo that repeatedly re-estimates Equation (4) under randomized rollout timing and location.

4.1 Credit Line

Table 2 reports baseline difference-in-differences estimates of the effect of GT3 implementation on small-business borrowers' credit lines. The dependent variable is ln(Credit Line), where credit line is expressed in thousands of RMB as described in Section 3.1. In column (1), which includes year-month fixed effects and bank fixed effects, the estimated coefficient implies an increase of approximately $e^{0.360} - 1 \approx 43.3\%$ in credit lines after implementation. The estimate remains similar when city fixed effects (column (2)) and industry fixed effects (column (3)) are added. In column (4), which includes the full set of control fixed effects, the GT3 effect remains positive and statistically significant, with an estimate of 0.179 log points (about $e^{0.179} - 1 \approx 19.6\%$).

4.2 Loan Pricing: Interest Rate and Spread

Table 3 reports difference-in-differences estimates of the reform effect on interest rates. In the most saturated specification (column (4)), which includes year-month, bank, city, industry,

and control fixed effects, GT3 implementation reduces lending rates by 0.404 percentage points.

Table 4 reports additional robustness checks that assess credit availability via interest spreads. In the most saturated specification (column (4)), which includes year-month, bank, city, industry, and control fixed effects, GT3 implementation reduces interest spreads by 0.442 percentage points.

4.3 Maturity

Table 5 presents estimates of the impact of GT3 implementation on loan maturities (measured in days). In column (2), which controls for year-month, bank, and city fixed effects, GT3 implementation increases maturities by 63.272 days. In the most saturated specification (column (4)), which adds the full set of control fixed effects, the estimate is 49.014 days. Given that the average loan maturity in the sample is about 507 days (Table 1), this corresponds to roughly 12.5% and 9.7%, respectively.

4.4 Parallel Trend Check

I employ an event-study framework to assess parallel trends for ln(Credit Line) (where Credit Line is measured in thousands of RMB) by estimating Equation (5). Event time is measured in quarters. The event window spans eight quarters before and eight quarters after GT3 implementation, with coefficients normalized to zero in quarter -1 (the quarter immediately prior to implementation).

Figure 1 plots the event-time coefficients. The eight pre-implementation coefficients are jointly indistinguishable from zero. Post-implementation coefficients turn positive starting one quarter after implementation and remain elevated thereafter.

4.5 Placebo Test

Figure 2 reports a permutation-based placebo test with 1,000 replications. In each replication, I randomly reassign both the treated provinces and implementation dates, re-estimate Equation (4), and record the placebo coefficient. The placebo estimates are centered near zero. By contrast, the baseline estimate in Table 2 (column (4), 0.179) lies far in the right tail: at most 1 out of 1,000 placebo draws produces an estimate at least as large as 0.179, implying an empirical p-value of about 0.001 or smaller.

5 Mechanism: Verifiable Tax Records and Bank Underwriting Inputs

This section provides evidence on the mechanism. GT3 makes VAT invoices digitally verifiable, increasing the supply of hard information that can substitute for relationship based soft information in small business lending (Stein, 2002; Berger et al., 2005; Liberti and Petersen, 2019). I combine three sets of results. First, reported tax collections rise after GT3 goes live, consistent with a tighter reporting environment (Kleven et al., 2011; Pomeranz, 2015). Second, banks revise underwriting inputs, including recorded borrower income and internal credit scores. Third, the cross sectional patterns match the hard information channel: effects are stronger in industries with weaker ex ante invoice paper trails, pricing improvements attenuate when invoices are noisier, and quantity and distance responses are larger in more competitive local banking markets (Artavanis et al., 2016; Yannelis and Zhang, 2023). Unless otherwise noted, loan level specifications follow Section 3.2, and standard errors are clustered at the bank by year month level.

5.1 Tax collections and the hard information shock

A necessary condition for the mechanism is that GT3 strengthens tax reporting and the verifiability of transaction records. To provide external evidence, I examine whether reported tax collections increase when a province transitions to GT3, consistent with enforcement and third party reporting in VAT settings (Kleven et al., 2011; Pomeranz, 2015).

Table 6 reports estimates from an annual version of Equation (4), using the year level exposure indicator defined in Equation (3). The dependent variable is ln(Tax Amount), measured at the province tax category taxpayer category year level in the CNRDS provincial tax statistics. The specification absorbs province fixed effects and category by year fixed effects that net out nationwide changes specific to each tax item and taxpayer category. Following the tax enforcement and information literature (Kleven et al., 2011; Pomeranz, 2015; Gallemore and Jacob, 2020), I control for lagged province fundamentals that proxy for the local tax base.

The estimates show that reported tax collections rise after GT3 goes live. The coefficient on GT3 is 0.076 ($t = 2.29$), implying an increase of approximately $e^{0.076} - 1 \approx 7.9\%$ in tax collections. This pattern provides external validation that the GT3 rollout is associated with a measurable strengthening of the tax information environment, supporting its interpretation as an exogenous hard information shock in the lending analyses that follow.

5.2 Internal bank mechanisms

Digitized and verifiable invoices provide a more reliable signal of borrower cash flows. If banks use these records in underwriting, GT3 should lead banks to revise income assessments and internal risk classifications, which can translate into larger credit lines and lower borrowing costs.

Table 7 reports estimates of Equation (4) with ln(Income) as the dependent variable, where Income is bank recorded annual borrower income. Recorded income increases after GT3. In the most saturated specification (column (4)), the estimate is 0.193 log points,

corresponding to approximately 21.3 percent.

Table 8 reports estimates of Equation (4) for internal credit scores, measured as within bank percentile ranks on a 1 to 100 scale. The reform increases internal scores by 6.642 percentile points in column (4). Together, the income and score results indicate that GT3 improves underwriting inputs, providing a direct channel consistent with the larger credit lines and lower rates documented in Section 4.

5.3 Invoicing paper trails and credit lines: grouped evidence

A key mechanism behind VAT enforcement is the invoice paper trail, which is naturally stronger in B2B transactions than in B2C final sales because firms have incentives to request invoices to claim input credits (Pomeranz, 2015). Consistent with this logic, Almunia and Lopez-Rodriguez (2018) emphasize that enforcement is comparatively weaker in consumer facing activities where third party verifiability is limited. Motivated by these insights, and by the visibility based classification of information opacity in Artavanis et al. (2016), I construct three stepwise (nested) industry group definitions at the GB/T 4754 one digit section level that progressively expand the set of industries with weaker invoice based paper trails (more B2C oriented and information poor). Version 1 defines a narrow B2C set (F, H, O, R). Version 2 adds education and health services (P, Q). Version 3 further adds business and professional services (L, M). In each case, the comparison group is the complement set of industries, which are relatively more B2B oriented and thus more likely to generate verifiable invoices.

I then re-estimate the baseline loan level DID in Equation (4) separately within each subsample using the baseline fixed effects block $\mathcal{F}^{(0)}_{ijbct}$, and I cluster standard errors at the bank by year month level. Table 9 shows that the credit line response is consistently larger in the weaker paper trail group: the GT3 effect on ln(Credit Line) is 0.220 log points versus 0.135 in Version 1, 0.215 versus 0.141 in Version 2, and 0.212 versus 0.128 in Version 3. The fact that the effect is largest precisely in industries where verifiable hard information is

scarcest ex ante supports the hard information interpretation of GT3, namely that invoice digitization improves lenders' ability to rely on scalable, verifiable records and relaxes credit constraints most strongly in information poor sectors. Appendix Table D.1 further reports separate DID estimates for each one-digit industry, providing a granular complement to the grouped results.

5.4 Invoice quality and pricing

To examine information quality, I use a province year Problematic Invoice Ratio constructed from tax audit publications (higher values indicate noisier invoices). This design follows the continuous heterogeneity framework in Equation (6), with Z equal to the Problematic Invoice Ratio and the interest rate as the outcome.

Table 10 shows that GT3 lowers interest rates on average, but the effect attenuates when invoice quality is worse (the interaction GT3 $\times$ Problematic Invoice Ratio is positive). This pattern supports an information mechanism in which pricing improvements are driven by more precise hard information. When invoices are noisier, verifiability gains translate less into lower rates.

5.5 Market structure and competition

I measure local banking concentration at the city $\times$ year month level using a Herfindahl Hirschman Index (HHI) based on branch presence:

$$HHI_{ct} = \sum_{b} \left(\frac{n_{bct}}{\sum_{b'} n_{b'ct}} \right)^2, \tag{11}$$

where n_{bct} is the number of active branches of bank b in city c during month t.

I then estimate Equation (6) with $Z = HHI_{ct}$ and ln(Credit Line) as the outcome, using bank $\times$ year month fixed effects so that identification comes from within bank and month comparisons across locations. Table 11 column (1) shows that the credit line expansion is

larger in more competitive markets (lower HHI), the interaction GT3 × HHI is negative and significant.

Finally, I study spatial reach using ln(Distance) as the outcome. I estimate Equation (6) with $Z = HHI_{ct}$, so the implied GT3 effect varies with local market structure and equals $\beta_1 + \beta_3 HHI_{ct}$. As a robustness check, I re-estimate this interaction specification separately in low and high HHI subsamples defined by the pre reform median. Table 11 columns (2) and (3) show that the interaction term remains negative and statistically significant, implying that the GT3 induced increase in lending distance is larger in more competitive markets. This pattern is consistent with competitive pass through of informational improvements into broader spatial reach.

5.6 Bank size heterogeneity

Bank organization may shape how effectively new hard information is translated into credit supply. Following the lending technology view in Stein (2002) and Berger et al. (2005), I split banks into nationally listed large banks and more regionally focused small banks, and re estimate Equation (4) within each subsample.

Table 12 shows that credit lines increase more for small banks (0.167 log points) than for large banks (0.072 log points). The larger response among small banks is consistent with GT3 relaxing a more binding informational constraint for banks that rely relatively more on local soft information ex ante.

6 Hard information substitutes for soft information

This section tests whether verified invoice information substitutes for soft information in small business lending. The core counterfactual implication is that the marginal effect of invoice digitization should be weaker when soft information becomes cheaper to produce (a proximity channel) or when banks already have stronger capacity to process hard information

(a processing channel). These predictions follow the classic view that soft information is costly to transmit across space and hierarchy, whereas hard information is verifiable and portable (Stein, 2002; Petersen and Rajan, 2002; Agarwal and Hauswald, 2010).

Throughout this section, *GT3* refers to the date level exposure indicator defined in Equation (1) and used in the baseline loan level design in Equation (4). Unless otherwise noted, standard errors are clustered at the bank by year month level.

6.1 Distance and Spatial Reach

6.1.1 Expansion of the lending distance

Following Petersen and Rajan (2002), I examine whether GT3 changes banks' spatial reach by estimating the baseline DID in Equation (4) with ln(Distance) as the dependent variable. Distance is the borrower to originating branch separation (kilometers), using the geocoded borrower and branch locations described in Section 3.1. To absorb any local shocks common to a city in a given month, I replace the baseline geography and time fixed effects with city by year-month fixed effects, while retaining bank fixed effects, industry fixed effects, and the full set of controls.

Table 13 column (1) shows that GT3 increases ln(Distance) by 0.135 (clustered $t = 2.78$), implying that average lending distance rises by about 14.4 percent. This within city-month expansion is consistent with verifiable hard information relaxing the informational frictions that constrain remote lending.

6.1.2 Distance heterogeneity in credit lines

To quantify how GT3 reshapes the distance gradient in credit supply, I estimate the continuous heterogeneity specification in Equation (6) with $y = \ln(\text{Credit Line})$ and $Z = \ln(\text{Distance})$. I include city by year-month fixed effects, bank fixed effects, industry fixed effects, and the same control structure as in the baseline, so identification compares borrowers within the same city and month. In this setup, the coefficient on ln(Distance) captures

the pre GT3 distance gradient, and the interaction term (GT3 $\times$ ln(Distance)) captures how that gradient changes when invoices become verifiable.

Table 13 column (2) shows a strong pre GT3 distance penalty, the coefficient on ln(Distance) is -0.124. The interaction is positive and precisely estimated, GT3 $\times$ ln(Distance) equals 0.052, indicating that GT3 attenuates the distance penalty in credit limits. A convenient interpretation uses doubling distance: a distance doubling increases ln(Distance) by $\ln 2$, so the interaction implies that the GT3 effect on credit limits is about 0.036 log points larger for borrowers at twice the distance, holding city-month conditions fixed.

Columns (4) and (5) of Table 13 report the distance gradients estimated separately in the pre and post periods. The slope attenuates from -0.123 before GT3 to -0.070 after GT3, implying that the distance penalty is compressed by roughly 43 percent relative to its pre benchmark. Figure 3 visualizes the same pattern by plotting estimated GT3 effects along the distribution of distance, borrowers farther from branches experience larger credit line gains.

As a robustness check, I construct driving distance using Baidu Maps routing and re estimate the same interaction design using ln(Driving Distance). Table 13 column (3) shows very similar results, the driving distance slope is -0.119 and the interaction is 0.057, indicating that the post GT3 attenuation of the distance gradient is not sensitive to the distance metric.

6.1.3 Dynamic patterns with distance heterogeneity

To assess parallel trends when effects vary by distance, I estimate the heterogeneous event study specification in Equation (7). Event time is defined in quarters as in Equation (5), and the shifter is demeaned ln(Distance) so that the main event time coefficients are interpretable at the sample mean distance. Figure 4 plots the resulting event time patterns. The pre implementation coefficients are close to zero, and post implementation coefficients indicate that the GT3 effect becomes larger for more distant borrowers, consistent with a causal attenuation of the distance penalty rather than differential pre trends.

6.1.4 Distance, hard information, and income measurement

Finally, I replicate the distance based identification using bank recorded income as the outcome, treating income as an underwriting input that should become more accurately measured when invoices become verifiable. I estimate Equation (6) with $y = \ln(\text{Income})$ and $Z = \ln(\text{Distance})$, using the same city by year-month fixed effects design as above. Consistent with Appendix A, I omit the income group fixed effects because income is the dependent variable.

Table 13 column (6) shows that recorded income has a strong negative distance gradient before GT3, the coefficient on ln(Distance) is -0.212. The interaction GT3 $\times$ ln(Distance) is 0.155, so the distance gradient in recorded income becomes much flatter after GT3 (about -0.057). This compression of the distance related gap in an underwriting input is consistent with verifiable invoices substituting for locally produced soft information.

6.2 Subway openings as a counterfactual for soft information costs

A key substitution prediction is that the marginal benefit of invoice digitization should be smaller when relationship based soft information becomes easier to produce. I operationalize this prediction using city subway openings, which plausibly reduce within city travel frictions and facilitate face to face interactions between borrowers and bank staff.

Empirically, I estimate a triple difference specification that interacts GT3 exposure with $Subway_{ct}$, an indicator equal to one in city c and month t once the subway system is operating. The specification absorbs City $\times$ Year Month fixed effects, Bank fixed effects, Industry fixed effects, and the full set of contract and borrower control fixed effects used in the baseline. Standard errors are clustered at the bank by year month level.

Table 14 column (1) shows a clear pattern consistent with substitution between hard and soft information. In city months without subway service, the GT3 coefficient is positive and statistically significant, indicating that invoice verifiability expands credit limits when relationship based information is costly to produce. In contrast, once the subway is

operating, the incremental GT3 effect becomes economically small and is not statistically distinguishable from zero. This disappearance of the GT3 effect when travel frictions fall supports the interpretation that GT3 operates by supplying portable hard information that matters most when soft information is difficult to obtain.

6.3 Bank fintech capability and the processing of hard information

A complementary counterfactual concerns banks' fintech capability, which proxies for how effectively a bank can ingest, verify, and operationalize hard information in underwriting. Even if GT3 makes VAT invoices digitally verifiable, banks still need internal systems to integrate invoice records into screening, pricing, and contract design. If a bank already has stronger fintech capability, the marginal effect of GT3 should be smaller because information processing constraints are less binding.

To test this prediction, I construct a pre-policy measure of bank fintech capability using the 2010–2012 average value of the fintech index at the bank level and standardize it to mean zero and unit standard deviation across banks. I then estimate a continuous heterogeneity specification following Equation (6), interacting GT3 exposure with this standardized fintech measure while absorbing the rich fixed effects used in the baseline loan-level analysis.

Column (2) of Table 14 shows a negative and statistically significant interaction between GT3 and fintech capability (-0.583, clustered $t = -3.36$). Because fintech capability is standardized, this estimate implies that a one standard deviation increase in fintech capability reduces the GT3 effect on credit lines by about 0.583 log points. Consistent with the processing channel, banks with weaker fintech capability benefit more from GT3, while banks with stronger fintech capability experience smaller incremental gains, suggesting that when hard information processing is already less constrained, additional invoice verifiability generates a smaller marginal improvement in credit availability.

7 Ruling out alternative stories and incidence

7.1 Credit availability: pricing accuracy or relaxed lending standards?

An improvement in verifiable information can increase observed credit availability, but this pattern is not uniquely informative about the underlying mechanism. In particular, a richer hard information environment may enhance banks' ability to price risk and allocate credit more efficiently, but it may also relax screening, collateral, or documentation requirements, thereby lowering the effective lending threshold and expanding credit to previously rationed borrowers. Classic models of adverse selection emphasize that limited information can generate credit rationing and binding lending standards (Stiglitz and Weiss, 1981). Consistent with this view, a large literature shows that better information production or information sharing alters both credit access and contract terms (Petersen and Rajan, 1994; Pagano and Jappelli, 1993; Hertzberg et al., 2011). In the context of compliance and third party reporting, stronger enforcement regimes generate additional hard information that can spill over into financial contracting (Kleven et al., 2011; Pomeranz, 2015; Gallemore and Jacob, 2020). Accordingly, even if we observe higher bank interest profitability after GT3, a natural concern is that the policy operates through relaxed lending standards and mechanical balance sheet expansion, rather than through more accurate pricing.

To distinguish these channels, I exploit the staggered province level rollout of GT3 and estimate the bank year panel specification in Equation (9) using bank annual report data. I collect the universe of publicly available bank annual report financial statements, and I manually identify banks whose domestic branch network is confined to a single province. This restriction yields a clean mapping from each bank to a unique province level adoption year, and avoids attenuating exposure for banks operating in multiple provinces. In this panel, $GT3^{y}_{p(b),y}$ equals one in fiscal years at or after the rollout year in bank b's province, and zero otherwise (Section 3.2). Following standard practice, I lag the controls by one year

to mitigate simultaneity. The baseline control set includes log total assets, the equity to assets ratio, the nonperforming loan ratio, the share of cash and interbank claims in total assets, operating expenses scaled by total assets, and deposits scaled by total assets (Ho and Saunders, 1981; Kashyap and Stein, 2000; Khwaja and Mian, 2008; Drechsler et al., 2017; Berger and Bouwman, 2013; Gatev et al., 2009). Standard errors are clustered at the province level, consistent with the level of treatment variation (Bertrand et al., 2004).

Table 15 reports the results. I proxy bank pricing capacity using net interest income scaled by total assets, *NII_to_Assets*, and also consider its log transformation when positive. In column (1), the estimate implies that GT3 increases *NII_to_Assets* by 0.221 percentage points, which corresponds to 9.67 percent of the sample mean in the main estimation sample. The log specification in column (2) yields a similar magnitude. Importantly, decomposing net interest income into the asset and liability sides shows that the effect is driven by higher interest income to assets (column (3)), while interest expense to assets does not rise (column (4)). This pattern is consistent with improved asset side pricing, rather than a funding cost channel.

The relaxed standards hypothesis generates two additional testable implications. First, if the policy mainly lowers lending thresholds, we should observe expansion in loan quantities. Second, if expansion is indiscriminate, ex post risk should deteriorate, or banks should weaken loss absorbing buffers. I therefore examine loan growth, the ratio of total loans to total assets, and the log level of total loans (columns (5) to (7)), as well as the nonperforming loan ratio and the provision coverage ratio (columns (8) and (9)), which are widely used indicators of credit risk and provisioning capacity (Fahlenbrach et al., 2018; Beatty and Liao, 2011; Bushman and Williams, 2012; Kashyap and Stein, 2000; Khwaja and Mian, 2008; Jiménez et al., 2014). Across these outcomes, I find no evidence of a post policy increase in loan quantities, and I do not detect statistically significant deterioration in asset quality or provisioning. Taken together, the evidence is more consistent with the interpretation that the additional hard information generated by GT3 improves banks' pricing and allocation

of credit, rather than simply lowering lending standards to expand the loan book.

7.2 Large borrowers: no evidence that Golden Tax Phase III affects loan amounts

Standard theories of financial intermediation emphasize that lenders screen and monitor borrowers using information, and that improvements in the information environment should matter most when baseline information frictions are severe (Diamond, 1984; Rajan, 1992; Petersen and Rajan, 1994). For large, publicly listed firms, however, the marginal informational benefit of tax digitization is plausibly limited. These firms operate in relatively transparent environments due to mandatory disclosure, external audits, and repeated interactions with sophisticated intermediaries. Consistent with this view, the accounting and debt contracting literature shows that the quality of publicly available financial information shapes debt contract design and syndicate structure, suggesting that lenders already incorporate rich non tax information when contracting with transparent borrowers (Healy and Palepu, 2001; Bharath et al., 2008; Ball et al., 2008; Sufi, 2007; Ivashina, 2009).

I test this implication using the listed firm borrowing announcements design in Equation (10). The dependent variable is the natural log of the upper bound of the announced loan amount. The key regressor is $GT3^{\text{ann}}_{p(\text{Branch}_e),e}$, which equals one for events announced on or after the GT3 go live date in the lending branch province, and zero otherwise. All specifications absorb firm fixed effects, year month fixed effects, and bank fixed effects. I then progressively absorb fixed effects for key contract design dimensions (loan type, loan purpose, currency, and additionally collateral and syndication status). Standard errors are clustered at the lending branch province by year month level.

Table 16 reports the results. Across all specifications, the estimated effect of GT3 on loan amounts for large borrowers is economically small and statistically indistinguishable from zero. In column (1), the point estimate is -0.006 (t statistic -0.25). Columns (2) and (3) add increasingly rich sets of contract attribute fixed effects, and the estimates remain close to

zero and far from conventional significance levels. Taken together, these findings are consistent with an information mechanism in which GT3 primarily matters when it meaningfully increases verifiability of borrower information. For large, publicly listed firms whose tax and financial information is already comparatively well documented and monitored, digitization does not appear to materially affect the scale of bank borrowing.

8 Conclusion

This paper shows that when public tax infrastructure converts VAT invoices into digitally verifiable, bank usable records, information frictions that constrain small business lending recede. Exploiting the staggered rollout of Golden Tax Phase III (GT3) across Chinese provinces and loan level data from multiple nationwide banks from 2012 to 2022, I find that firms obtain larger credit limits, lower borrowing costs, and longer maturities after GT3 goes live in the borrower registration province. Banks also record higher borrower income and assign higher internal credit scores, consistent with improved underwriting inputs once invoice information becomes verifiable. The lending technology shifts in ways that match hard information substituting for relationship based soft information, lending becomes less local, average borrower branch distance increases, and the pre-reform distance penalty in credit limits attenuates. Cross sectional patterns further align with the information mechanism, effects are larger in industries with weaker ex ante invoice paper trails and in more competitive local banking markets, and they weaken when audit based measures indicate noisier invoice environments. A counterfactual using subway openings points in the same direction, GT3 effects on credit limits are present when cities have not yet opened subway systems, but once subway service begins and soft information becomes easier to produce, the GT3 effect is no longer detectable. Complementary bank annual report evidence indicates higher net interest income driven by asset side interest income, without faster loan growth or worse asset quality, consistent with improved pricing rather than relaxed standards. Finally,

borrowing announcements for listed firms show no detectable response in loan amounts, consistent with GT3 operating through the marginal value of verifiable hard information for opaque borrowers rather than through broad credit expansion for already transparent firms.

These findings speak to classic themes in corporate finance and banking, including the role of verifiable information in screening and pricing, organizational limits to producing and transmitting soft information, and the role of competition in pass through. They also document a tight link between public information architecture and private capital allocation. At the micro level, verifiable invoices change how banks measure income, update internal risk assessments, and set contract terms, and they reshape the spatial reach of lending by weakening distance related informational frictions. The counterfactual evidence from subway openings reinforces the substitution interpretation, when proximity frictions fall and relationship based information becomes cheaper to obtain, the incremental value of invoice verifiability declines. For policymakers and practitioners, the mechanism highlights implementable design principles, verifiability (security coded invoices), standardization (common tax metadata), and interoperability (bank facing access that supports routine authentication). At the same time, because the channel relies on the operational use of sensitive transaction records, governance, privacy, and accountability remain first order constraints.

The results also point to several research agendas. First, future work can quantify pass through into real outcomes, including investment, employment, and firm dynamics, and evaluate general equilibrium responses. Second, additional evidence on design choices would be valuable, clarifying how verifiability, standardization, interoperability, and audit intensity jointly shape information quality and the distribution of gains, and how these trade off against privacy safeguards. Third, a broader intermediation perspective can examine how digital public infrastructure shifts the margin between relationship lending and arm's length lending, how it interacts with banks' internal data processing capacity, and how market structure governs who captures informational rents. More generally, as transaction records become verifiable and portable, the marginal role of soft information should fall, motivating

studies that leverage staggered rollouts of related public reporting systems, such as digital procurement, payroll and withholding, or platform transaction reporting, to identify comparable bank connected hard information shocks.

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Tables

Table 1: Summary Statistics

	N	Mean	Std. Dev.	P25	Median	P75
Panel A. Small Business Loan Dataset						
Credit Line (RMB)	154,233	1,521,540	2,050,424	330,000	800,000	1,890,000
Interest Rate	154,233	7.290	2.133	5.655	6.960	8.528
Interest Spread	154,233	2.655	2.015	1.250	2.213	3.700
Maturity	154,233	506.850	598.722	356	364	366
Credit Score	101,191	46.005	28.678	21.000	49.130	64.385
Income	126,465	14.585	299.040	0.800	4.300	12.000
Distance	96,883	27.220	25.035	8.073	22.351	38.772
Panel B. Local Banking Market Structure Dataset (City × Year-Month)						
Total Branches	294,063	171.741	311.768	7	56	216
Number Of Banks	294,063	6.628	6.942	2	5	8
HHI	294,063	0.470	0.302	0.235	0.343	0.662
Panel C. Invoice Audit Dataset (Province × Year)						
Problematic Invoice Ratio	216	63.664	30.105	36.560	69.810	92.800

Notes: Panel A reports summary statistics for the main loan level dataset used in the baseline analyses. Credit Line (RMB) is the approved credit limit in RMB. ln(Credit Line) is the natural logarithm of credit line measured in thousands of RMB. Interest Rate is the contractual nominal annual interest rate (percent). Interest Spread equals Interest Rate minus the relevant benchmark rate (LPR), expressed in percentage points. Maturity is contractual maturity in days. Income is bank recorded annual borrower income, reported in millions of RMB in this table for readability. Distance is the straight line distance (kilometers) between the borrower location and the originating lending branch, and is available only when both borrower and branch locations can be geocoded. When used in regressions, I take natural logarithms as indicated by the dependent variable label in each table, including ln(Credit Line) (Credit Line measured in thousands of RMB), ln(Income), and ln(Distance). Regression sample sizes may differ slightly across tables due to dependent variable availability and the inclusion of fixed effects that require non missing identifiers.

Panel B reports summary statistics for the local banking market structure dataset constructed on a City × Year-Month panel (one observation per city-month). Total Branches is the number of active bank branches in a city-month, Number Of Banks is the number of distinct banks with at least one active branch, and HHI is the Herfindahl–Hirschman index based on branch shares and ranges from 0 to 1.

Panel C reports summary statistics for the invoice audit dataset from CNRDS constructed on a Province × Year panel (one observation per province-year). Problematic Invoice Ratio is expressed in percent (0–100). Statistics are computed on non-missing observations for each variable.

Table 2: Baseline DID Estimates: Credit Line

Dep. Var.:	ln(**Credit Line**)			
	(1)	(2)	(3)	(4)
GT3	0.360***	0.349***	0.331***	0.179***
	(17.58)	(7.10)	(5.53)	(3.23)
Constant	6.464***	6.471***	6.482***	6.575***
	(447.63)	(225.04)	(186.92)	(192.43)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,182	154,172	154,172	154,170
R^2	0.384	0.426	0.429	0.543

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

Notes: This table reports estimates of Equation (4) with dependent variable ln(Credit Line). GT3 equals one if loan i is originated on or after the GT3 go live date in the borrower registration province $p(j)$. Columns progressively add fixed effects as indicated; the full set of control fixed effects is defined in Appendix A. Standard errors are clustered at the bank by year month level.

Table 3: Baseline DID Estimates: Interest Rate

Dep. Var.:	**Interest Rate**			
	(1)	(2)	(3)	(4)
GT3	-0.315***	-0.378***	-0.464***	-0.404***
	(-3.96)	(-4.16)	(-4.38)	(-3.57)
Constant	7.482***	7.521***	7.574***	7.537***
	(144.29)	(142.51)	(122.99)	(108.31)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,233	154,223	154,223	154,221
R^2	0.579	0.617	0.619	0.670

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

Notes: This table reports estimates of Equation (4) with dependent variable Interest Rate (in percentage points). GT3 is defined as in Table 2. Fixed effects are added as indicated; the full set of control fixed effects is defined in Appendix A. Standard errors are clustered at the bank by year month level.

Table 4: Baseline DID Estimates: Interest Spread

Dep. Var.:	**Interest Spread**			
	(1)	(2)	(3)	(4)
GT3	-0.338***	-0.400***	-0.493***	-0.442***
	(-4.08)	(-4.18)	(-4.36)	(-3.75)
Constant	2.862***	2.900***	2.957***	2.926***
	(52.91)	(52.05)	(44.94)	(40.38)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,233	154,223	154,223	154,221
R^2	0.523	0.566	0.568	0.628

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

Notes: This table reports estimates of Equation (4) with dependent variable Interest Spread (in percentage points), defined as the contractual interest rate minus the benchmark Loan Prime Rate (LPR) at origination. GT3 is defined as in Table 2. Fixed effects are added as indicated; the full set of control fixed effects is defined in Appendix A. Standard errors are clustered at the bank by year month level.

Table 5: Baseline DID Estimates: Maturity

Dep. Var.:	**Maturity**			
	(1)	(2)	(3)	(4)
GT3	110.939***	63.272***	75.501***	49.014**
	(5.71)	(2.66)	(3.35)	(2.30)
Constant	438.933***	468.122***	460.635***	476.854***
	(29.33)	(24.89)	(26.91)	(34.13)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	154,182	154,172	154,172	154,170
R^2	0.419	0.452	0.452	0.610

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

Notes: This table reports estimates of Equation (4) with dependent variable Maturity (in days). GT3 is defined as in Table 2. Fixed effects are added as indicated; the full set of control fixed effects is defined in Appendix A. Following Appendix A, Loan term FE are omitted because maturity is the dependent variable. Standard errors are clustered at the bank by year month level.

Table 6: GT3 and provincial tax collections

Dep. Var.:	ln(**Tax Amount**)
GT3	0.076**
	(2.29)
Lagged ln(GDP per capita)	1.142***
	(7.08)
Lagged ln(Population)	1.328***
	(3.92)
Lagged ln(Road infrastructure)	-0.196**
	(-2.60)
Lagged secondary sector share	0.532
	(0.50)
Lagged tertiary sector share	0.212
	(0.15)
Lagged ln(Trade)	-0.100
	(-1.69)
Constant	-10.081***
	(-2.78)
Province FE	Yes
Tax category × Year FE	Yes
Taxpayer category × Year FE	Yes
Observations	44,239
R^2	0.759

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

Notes: The unit of observation is a province tax category taxpayer category year cell from the CNRDS provincial tax statistics. The specification is an annual version of Equation (4), using the year level exposure indicator defined in Equation (3). All control variables are lagged one year. Standard errors are clustered at the province level (Bertrand et al., 2004).

Table 7: Baseline DID estimates: bank recorded income

Dep. Var.:	ln(**Income**)			
	(1)	(2)	(3)	(4)
GT3	0.132***	0.210***	0.211***	0.193***
	(3.04)	(3.28)	(3.14)	(3.45)
Constant	14.869***	14.821***	14.820***	14.831***
	(522.86)	(372.70)	(352.92)	(428.47)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	126,282	126,268	126,268	126,268
R^2	0.337	0.376	0.385	0.480

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

Notes: This table reports estimates of Equation (4) with dependent variable ln(Income), where Income is bank recorded annual borrower income (RMB). GT3 equals one if a loan is originated on or after the GT3 go live date in the borrower registration province. Columns progressively add fixed effects as indicated, the full set of control fixed effects is defined in Appendix A. Following Appendix A, the income bin fixed effects are omitted because income is the dependent variable. Standard errors are clustered at the bank by year month level.

Table 8: Baseline DID estimates: internal credit score

Dep. Var.:	**Credit Score**			
	(1)	(2)	(3)	(4)
GT3	2.987***	1.880	3.233*	6.642***
	(2.62)	(1.17)	(1.92)	(4.62)
Constant	43.459***	44.400***	43.250***	40.354***
	(44.64)	(32.65)	(30.54)	(33.19)
Year Month FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
City FE	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Control FE	No	No	No	Yes
Observations	101,240	101,229	101,228	101,228
R^2	0.090	0.118	0.120	0.151

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

Notes: This table reports estimates of Equation (4) with the internal credit score as the dependent variable. Credit Score is constructed by mapping each bank's internal rating categories into within bank percentile ranks and scaling to a 1 to 100 range. Columns progressively add fixed effects as indicated, the full set of control fixed effects is defined in Appendix A. Following Appendix A, the internal credit score category fixed effects are omitted because the dependent variable is derived from the same internal rating system. Standard errors are clustered at the bank by year month level.

Table 9: Policy effects by stepwise paper trail industry groups

Dep. Var.	ln(**Credit Line**)					
	Version 1		**Version 2**		**Version 3**	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
GT3	0.135***	0.220***	0.141***	0.215***	0.128***	0.212***
	(3.44)	(3.06)	(3.58)	(3.00)	(3.99)	(2.92)
Constant	6.666***	6.500***	6.660***	6.506***	6.649***	6.526***
	(270.42)	(149.60)	(270.30)	(150.08)	(328.60)	(147.33)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes	Yes	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	68,767	85,351	67,658	86,459	61,934	92,188
R^2	0.603	0.511	0.605	0.510	0.612	0.514

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

Notes: This table reports grouped difference-in-differences estimates of the Golden Tax Phase III (GT3) on ln(Credit Line). Each column is estimated on a separate subsample defined by industry group membership (Low versus High). GT3 is defined as in Table 2. All specifications include City FE, Year Month FE, Bank FE, Industry FE (GB/T 4754 (2017), one digit), and the full set of control fixed effects defined in Appendix A (Contract file FE, Payment method FE, Borrower type FE, Age group FE, Income group FE, Leverage quintile FE, Internal credit score FE, and Loan term FE). Standard errors are clustered at the bank by year month level.

Stepwise industry group definitions (GB/T 4754 (2017), one digit): Version 1 High = {F, H, O, R}. Version 2 High = Version 1 High ∪ {P, Q}. Version 3 High = Version 2 High ∪ {L, M}. The Low group contains all remaining one digit industries.

Table 10: Invoice quality and pricing

Dep. Var.	**Interest Rate**
GT3	-0.644^{***}
	(-2.81)
Problematic Invoice Ratio	-0.0066^{***}
	(-3.63)
GT3 × Problematic Invoice Ratio	0.0071^{**}
	(2.42)
Constant	8.737^{***}
	(77.26)
City FE	Yes
Bank × Year Month FE	Yes
Industry FE	Yes
Control FE	Yes
Observations	75,457
R^2	0.535

Notes: This table reports continuous heterogeneity estimates based on Equation (6) using a province year Problematic Invoice Ratio (percent, 0 to 100; higher values indicate lower invoice quality) matched to loans by borrower province and calendar year. Standard errors are clustered at the bank by year month level.

Table 11: Policy effect by competition: credit line and distance

Dep. Var.	ln(**Credit Line**)	ln(**Distance**)	
	(1)	(2) Low HHI	(3) High HHI
GT3	0.335***	0.638***	0.301***
	(2.80)	(5.39)	(5.04)
HHI	−0.484	25.020***	4.855***
	(−1.40)	(20.15)	(25.36)
GT3 × HHI	−1.302***	−9.159***	−1.298***
	(−2.93)	(−6.14)	(−4.41)
Constant	6.543***	0.531***	2.259***
	(107.01)	(5.03)	(60.72)
City FE	Yes		
Province FE		Yes	Yes
Bank FE		Yes	Yes
Year Month FE		Yes	Yes
Bank × Year Month FE	Yes		
Industry FE	Yes	Yes	Yes
Control FE	Yes	Yes	Yes
Observations	140,230	56,943	39,829
R^2	0.524	0.195	0.241

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

Notes: This table reports estimates based on Equation (6). HHI is constructed at the city × year month level from branch shares (Equation (11)). Low and High HHI subsamples are defined using the median HHI in the pre reform period. Column (1) uses bank × year month fixed effects so identification comes from within bank and month comparisons across cities. Columns (2) and (3) report the distance specification separately by HHI subsample. Standard errors are clustered at the bank by year month level. HHI is scaled to 10,000.

Table 12: Baseline DID estimates for ln(Credit Line): bank size

Dep. Var.:	ln(**Credit Line**)	
	Small	Large
GT3	0.167**	0.072***
	(2.49)	(2.73)
Constant	6.497***	7.422***
	(152.66)	(683.57)
Year Month FE	Yes	Yes
City FE	Yes	Yes
Bank FE	Yes	Yes
Industry FE	Yes	Yes
Control FE	Yes	Yes
Observations	139,260	14,870
R^2	0.518	0.660

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

Notes: This table reports subsample estimates of Equation (4) for small and large banks. All specifications include Year Month FE, City FE, Bank FE, Industry FE, and the full set of control fixed effects (Appendix A). Standard errors are clustered at the bank by year month level.

Table 13: 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
GT3	0.135***	0.083	−0.343*			0.172
	(2.78)	(0.55)	(−1.82)			(0.47)
ln(Distance)		−0.124***		−0.123***	−0.070***	−0.212***
		(−20.75)		(−21.98)	(−7.83)	(−12.49)
ln(Driving Distance)			−0.119***			
			(−21.54)			
GT3 × ln(Distance)		0.052***				0.155***
		(4.43)				(7.94)
GT3 × ln(Driving Distance)			0.057***			
			(5.21)			
Constant	2.855***	6.715***	7.551***	6.582***	6.930***	15.273***
	(121.06)	(91.87)	(81.82)	(386.55)	(280.05)	(79.83)
City × YM FE	Yes	Yes	Yes	Yes	Yes	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	48,609	45,354	86,026
R^2	0.395	0.533	0.533	0.458	0.544	0.430

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

Notes: Columns report DID estimates using ln(Distance), ln(Credit Line) (straight line distance), ln(Credit Line) (driving distance), and ln(Income), as well as distance gradients estimated separately in the pre and post periods (columns (4) and (5)). Distance measures are in kilometers. Models include City × Year-Month fixed effects, Bank fixed effects, Industry fixed effects, and the control fixed effects defined in Appendix A. Standard errors are clustered at the bank by year month level. Throughout, ln denotes the natural logarithm.

Table 14: Heterogeneity in GT3 effects: subway access and bank fintech capability

Dep. Var.:	ln(**Credit Line**)	
	(1) Subway access	(2) Fintech capability
GT3	0.086**	1.001***
	(2.47)	(3.80)
GT3 × Subway available	-0.039	
	(-0.46)	
GT3 × Fintech capability		-0.583***
		(-3.36)
Constant	6.648***	6.519***
	(281.19)	(93.53)
City × Year Month FE	Yes	Yes
Bank FE	Yes	Yes
Industry FE	Yes	Yes
Control FE	Yes	Yes
City × Subway Status FE	Yes	No
Year Month × Subway Status FE	Yes	No
Observations	150,630	132,956
R^2	0.589	0.567

Notes: The dependent variable is the natural logarithm of the approved credit line (measured in thousands of RMB). GT3 is an indicator equal to one if a loan is originated on or after the GT3 go live date in the borrower registration province. Column (1) interacts GT3 with an indicator for whether the city has an operating subway system in the loan month (Subway available). The coefficient on GT3 captures the GT3 effect when the subway is not available, and the interaction term captures the incremental change in the GT3 effect when the subway is available. Column (2) interacts GT3 with bank fintech capability, measured as the pre policy (2010–2012) average fintech index at the bank level and standardized to mean zero and unit standard deviation across banks. All specifications absorb the fixed effects listed in the table. Standard errors are clustered at the bank × year month level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 15: GT3, pricing outcomes, and tests of relaxed lending standards

	Pricing outcomes				Quantity outcomes			Risk outcomes	
	(1) Net interest income / assets	(2) Log(Net interest income / assets)	(3) Interest income / assets	(4) Interest expense / assets	(5) Loan growth	(6) Loans / assets	(7) Log(Total loans)	(8) Nonperforming loan ratio	(9) Provision coverage ratio
GT3	0.0022129***	0.0960960**	0.0030544***	0.0008860	0.0169795	-0.0071246	0.0029911	0.2093076	32.89271
	(3.39)	(2.18)	(3.27)	(1.22)	(0.28)	(-0.53)	(0.17)	(1.49)	(0.96)
Log(Total assets)	0.0025470	0.1137690	0.0085467***	0.0059421***	-0.8245553	0.0036095	0.7858320***	0.4755218***	-282.5671**
	(1.59)	(1.27)	(5.28)	(5.49)	(-1.11)	(0.17)	(21.70)	(3.49)	(-2.39)
Equity / assets	0.0007575	-0.4598462	-0.0072422	-0.0092258	-0.7798754	0.1366645	0.8396332*	2.268237**	-1322.005***
	(0.04)	(-0.36)	(-0.39)	(-0.87)	(-0.35)	(1.23)	(1.79)	(2.29)	(-2.78)
Nonperforming loan ratio	-0.0002832	-0.0300607	-0.0000019	0.0002639*	0.0603360	0.0084422***	0.0098739**	0.5343000***	-45.34288***
	(-1.29)	(-1.67)	(-0.01)	(1.77)	(0.81)	(3.07)	(2.48)	(12.74)	(-3.38)
Liquid assets / assets	-0.0008587	0.0580227	-0.0011836	-0.0001138	-0.6957222	-0.3041330***	-0.7547681***	-0.4411841	372.7760*
	(-0.16)	(0.23)	(-0.19)	(-0.03)	(-0.70)	(-3.18)	(-5.59)	(-0.93)	(2.00)
Operating expenses / assets	0.1450769	5.4820160	0.3082458	0.1468713	-1.2991110	2.4864970**	7.5742770***	-17.75863	-16406.28*
	(1.19)	(1.34)	(1.40)	(0.66)	(-0.18)	(2.32)	(3.38)	(-1.48)	(-2.03)
Deposits / assets	0.0013075	0.0821969	0.0002479	-0.0010120	-1.1736020	0.0228293*	0.0725598	-0.1048996	14.18660
	(1.09)	(1.04)	(0.47)	(-0.73)	(-0.79)	(1.89)	(1.32)	(-1.18)	(0.54)
Constant	-0.0437552	-6.813149***	-0.1737740***	-0.1283473***	21.83500	0.4248597	4.756332***	-11.02351***	7588.710**
	(-1.08)	(-2.97)	(-4.14)	(-4.44)	(1.11)	(0.82)	(5.29)	(-3.32)	(2.53)
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,500	1,497	1,500	1,492	1,494	1,494	1,494	1,477	1,328
Mean dep. var.	0.022885	-3.875262	0.043624	0.020836	0.249305	0.502381	24.38901	1.729501	296.9807

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

Notes: The sample includes banks whose domestic branch network is confined to a single province. "GT3" equals one in fiscal years at or after the GT3 rollout year in the bank's home province. All regressors other than GT3 are control variables and are lagged one year. All specifications include bank and year fixed effects. Standard errors are clustered at the province level.

Table 16: DID Results for Listed Firms' Announced Credit Line

Dep. Var.:	ln(**Credit Line**)		
	(1)	(2)	(3)
GT3	-0.006	0.011	0.010
	(-0.25)	(0.52)	(0.50)
Constant	9.235***	9.221***	9.221***
	(493.76)	(512.34)	(519.93)
Firm FE	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Loan Type FE	No	Yes	Yes
Loan Purpose FE	No	Yes	Yes
Currency FE	No	Yes	Yes
Collateral FE	No	No	Yes
Syndicate FE	No	No	Yes
Observations	151,074	151,066	151,065
R^2	0.613	0.656	0.657

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

Notes: The dependent variable is ln(Credit Line), where Credit Line is the upper bound of the loan amount disclosed in the announcement. GT3 equals one if the announcement date is on or after the GT3 go live date in the lending branch province, and zero otherwise. All specifications absorb firm fixed effects, year month fixed effects, and bank fixed effects. Column (2) additionally absorbs contract attribute fixed effects for loan type, loan purpose, and currency. Column (3) further absorbs collateral and syndication status fixed effects. Standard errors are clustered at the province × year month level.

Figures

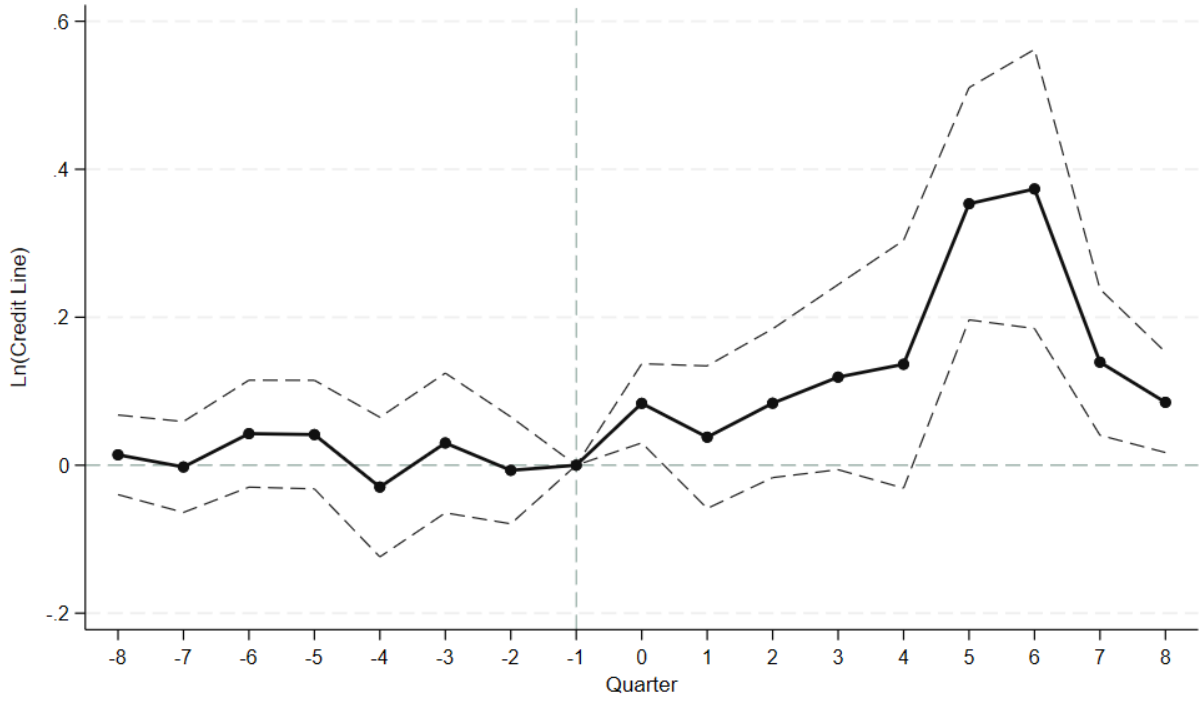


Figure 1: Event-study estimates around GT3 go live

Notes: The figure plots coefficients from Equation (5) for ln(Credit Line). Event time is measured in quarters relative to the GT3 go live quarter and normalized to zero at $k = -1$. Standard errors are clustered at the bank by year month level.

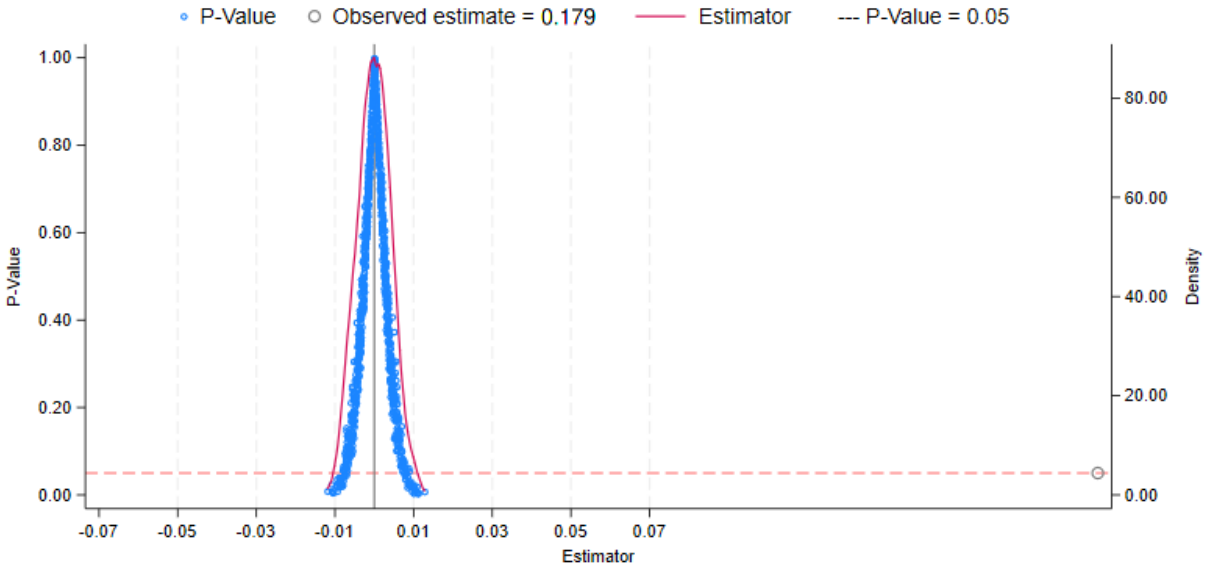


Figure 2: Placebo Test

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

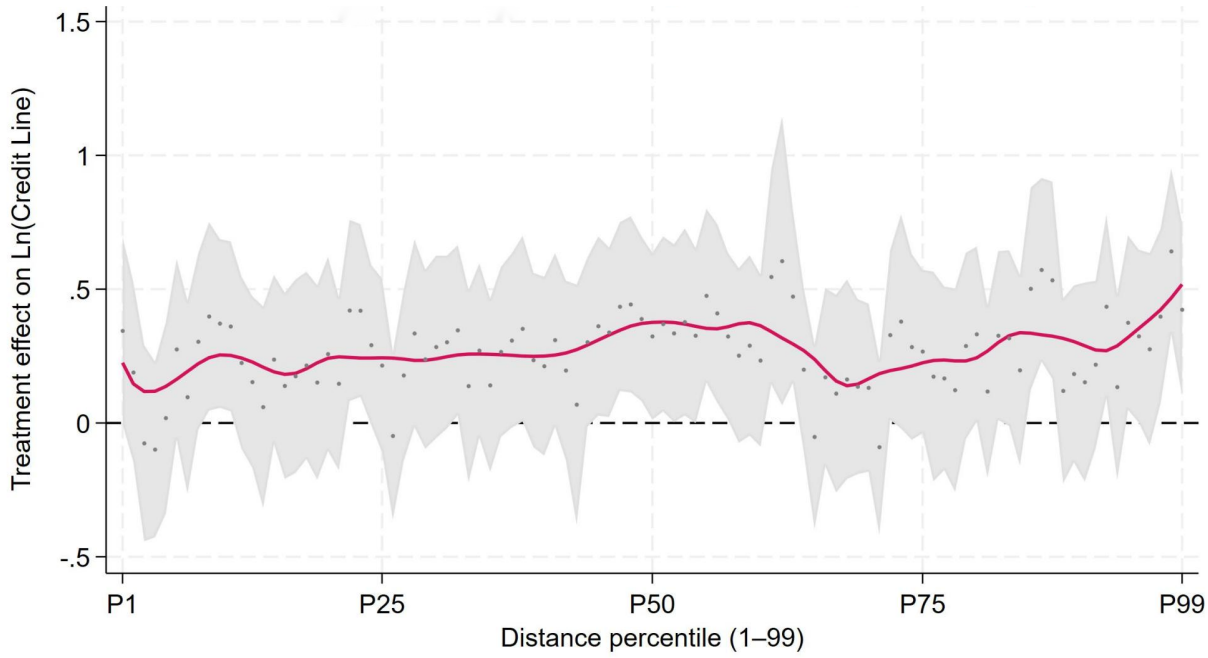


Figure 3: Policy Effect by Geographic Distance

Notes: The figure plots estimated GT3 effects by percentiles of ln(Distance). The estimation sample is partitioned into 99 global percentiles of ln(Distance). For each percentile, I estimate the marginal effect of GT3 on ln(Credit Line) from a DID specification that interacts percentile indicators with GT3, absorbing the fixed effects and controls described in Section 3.2. Error bars are 95% confidence intervals based on standard errors clustered at the bank by year month level.

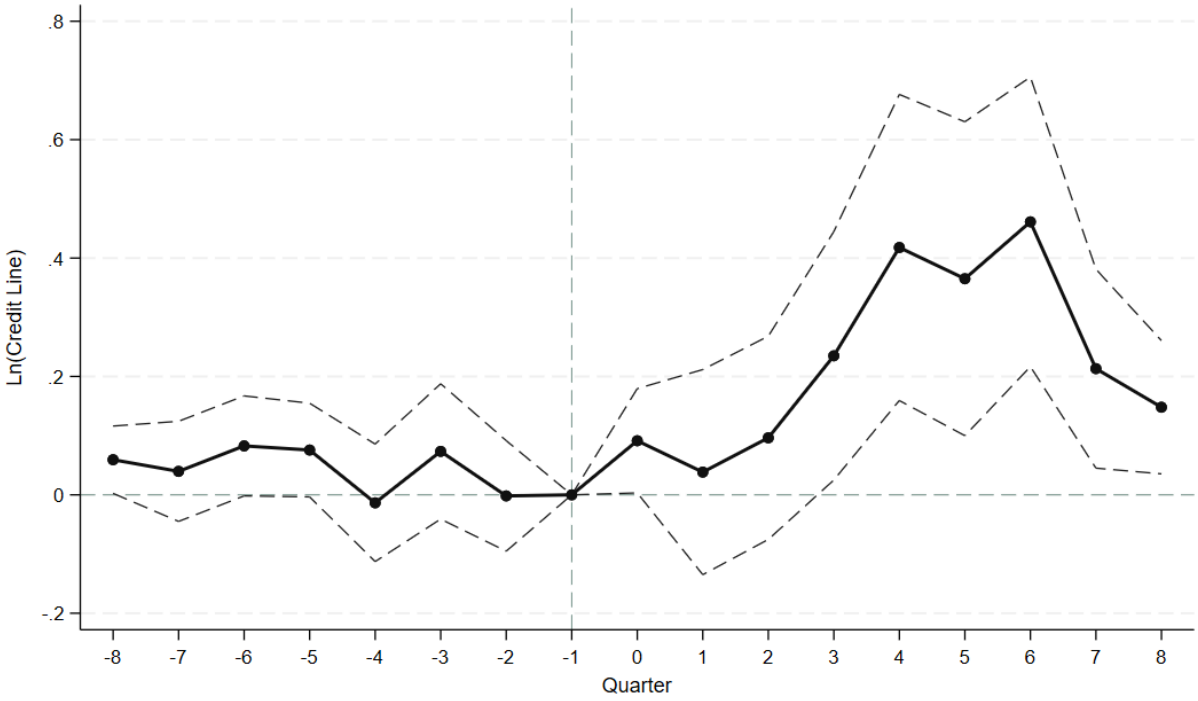


Figure 4: Parallel trends with distance heterogeneity

Notes: The figure visualizes the heterogeneous event study in Equation (7) with $Z = \ln(\text{Distance})$ (demeaned) and outcome ln(Credit Line). Event time is in quarters and the reference period is $k = -1$ (one quarter prior to GT3 go live). Standard errors are clustered at the bank by year month level.

A Fixed Effects and Control Structure

This appendix defines the fixed effects and coarsened control indicators used in the loan level specifications. The goal is to make the absorbed components transparent and to document the outcome specific omission rule used to avoid mechanical collinearity. Unless otherwise stated in a table note, standard errors are clustered at the bank by year month level.

A.1 Fixed effects in the loan level specifications

The baseline loan level specifications use four core fixed effects.

- **Year Month FE:** absorbs aggregate shocks common to all loans originated in calendar month t.
- **City FE:** absorbs time invariant differences across branch cities.
- **Bank FE:** absorbs time invariant differences across banks, including business model, risk appetite, and underwriting policies.
- **Industry FE:** defined at the one digit level of GB/T 4754 (2017), absorbing time invariant sectoral heterogeneity.

Some mechanism designs replace City FE and Year Month FE with City by Year Month FE, or replace Bank FE with Bank by Year Month FE, so that identification comes from within city month or within bank month variation. Each table header lists the exact fixed effects included in that specification.

A.2 Control fixed effects

To maximize sample size, I discretize borrower and contract covariates into coarse groups and include them as sets of indicator variables. Each set includes an explicit Missing group, so observations are not dropped when a covariate is unavailable.

The full set of control fixed effects used in the main loan level tables is:

- **Contract file FE:** absorbs differences across loan product files in the raw data.
- **Payment method FE:** absorbs systematic differences across repayment schedules.
- **Borrower type FE:** indicates whether the loan is booked under a personal name or a corporate name.

- **Age group FE:** applies only to personal name loans. I form five age groups using the within personal loan age distribution.
- **Income group FE:** formed from five groups (within sample quintiles) of bank recorded annual borrower income.
- **Leverage quintile FE:** applies only to corporate name loans and is formed from the asset to liability ratio distribution.
- **Internal credit score FE:** absorbs bank specific rating categories. For banks reporting letter grades, I interact the bank identifier with the grade. For banks reporting numeric scores, I interact the bank identifier with score bins (deciles).
- **Loan term FE:** captures non linear level differences tied to maturity. I use four maturity buckets, less than 1 year, 1 to 3 years, 3 to 5 years, and above 5 years.

A.3 Column progression in the main tables

The baseline tables with columns (1) to (4) build fixed effects cumulatively:

Column (1) Year Month FE and Bank FE.

Column (2) Column (1) plus City FE.

Column (3) Column (2) plus Industry FE.

Column (4) Column (3) plus the full set of control fixed effects in Section A.2.

A.4 Outcome specific omissions

When a control block is constructed from the same underlying variable as the dependent variable, including that control block would mechanically partial out the outcome. I therefore apply the following omission rule:

- When the dependent variable is ln(Income), I omit Income group FE.
- When the dependent variable is Maturity measured in days, I omit Loan term FE.
- When the dependent variable is the within bank percentile credit score scaled from 1 to 100, I omit Internal credit score FE.

All other fixed effects and control blocks are retained.

B Provinces in the PBOC Macro Sample

Table 17: 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

C Golden Tax Phase III Rollout by Province

This appendix lists the implementation date for GT3 by province level unit. Dates use YYYY.MM.DD.

Table 18: Golden Tax Phase III implementation date

Province or Municipality	Implementation date	Province or Municipality	Implementation 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		

D Industry Heterogeneity

This appendix reports industry level heterogeneity in the effect of the Golden Tax Phase III rollout on credit availability. A large share of the sample consists of individually owned businesses, a borrower group for which verifiable financial records are often limited, so industry composition can be informative about where invoice verification should matter most. Industries are defined using the one digit sections of the China National Economic Industry Classification Standard (GB/T 4754 (2017)). To preserve statistical power and avoid noisy industry specific inference, I report estimates for industries with at least 3,000 loan observations.

Appendix Table D.1 presents separate difference-in-differences regressions of ln(Credit Line) by industry. The estimates show meaningful heterogeneity across one digit sectors, with several industries exhibiting sizable and precisely estimated credit expansions while others display smaller or less precisely estimated effects in this sample. These granular patterns complement the grouped evidence in Section 5.3, which directly tests heterogeneity by ex ante invoice paper trail strength.

Table D.1: DID Results by Industry

Panel A. **Dep. Var.:**	**ln(Credit Line)**				
	A	C	E	F	G
GT3	0.120	0.116***	0.066	0.238***	0.096
	(0.73)	(3.38)	(0.94)	(3.04)	(1.19)
Constant	5.898***	6.690***	6.557***	6.562***	6.534***
	(66.68)	(305.21)	(190.20)	(135.35)	(150.23)
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	11,453	63,386	3,681
R^2	0.603	0.647	0.537	0.525	0.702

Panel B. **Dep. Var.:**	**ln(Credit Line)**			
	H	I	L	O
GT3	0.064	0.821***	0.160	0.164
	(0.52)	(6.69)	(1.31)	(1.23)
Constant	6.425***	6.471***	6.750***	6.376***
	(91.25)	(59.38)	(86.74)	(89.84)
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	4,761	7,077	4,514	3,574
R^2	0.580	0.475	0.636	0.541

Notes: This table reports difference-in-differences estimates of the GT3 on ln(credit line) by one-digit industry. Each column is estimated on a separate industry subsample. All specifications include Year Month FE, City FE, Bank FE, and the full set of control fixed effects. Industry FE is omitted by construction because the sample is split by industry. Standard errors are clustered at the bank by year month level. Clustered t statistics are reported in parentheses.

Industry code definitions (GB/T 4754 (2017)): A Agriculture, forestry, animal husbandry and fishery; C Manufacturing; E Construction; F Wholesale and retail; G Transportation, warehousing and postal; H Accommodation and catering; I Information transmission, software and information technology services; L Leasing and business services; O Residential services, repair and other services.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

E Summary statistics for auxiliary datasets

Table E.1: Summary statistics, listed firm loan announcements

	N	Mean	Std. Dev.	P25	Median	P75
Credit Line (million RMB)	151,074	342.522	13,544.210	50	100	250

Notes: The unit of observation is a listed firm borrowing announcement. Announced credit line is the disclosed upper bound, converted to million RMB for readability. Invoice digitization reform (GT3) equals one if the announcement date is on or after the GT3 go live *date* in the lending branch province (daily timing). Statistics are computed on the estimation sample used in Table 16.

Table E.2: Summary statistics, provincial tax collections panel

	N	Mean	Std. Dev.	P25	Median	P75
Tax Amount	21,732	570,077.4	1,964,154.0	4,809.5	49,377.5	300,181.5
Log(GDP per capita)	21,732	10.033	0.830	9.383	10.155	10.655
Log(population)	21,732	8.111	0.835	7.797	8.247	8.718
Log(road infrastructure)	21,732	5.109	0.902	4.700	5.278	5.707
Secondary sector share	21,732	0.428	0.082	0.383	0.437	0.483
Tertiary sector share	21,732	0.450	0.089	0.397	0.432	0.485
Log(trade)	21,732	16.767	1.880	15.433	16.757	18.037

Notes: The unit of observation is a province by tax category by taxpayer category by year cell. Invoice digitization reform (GT3) equals one in years at or after the GT3 rollout *year* in the province (annual timing). All macro controls are measured in the previous year. Tax collections (level) are reported in the original units of the underlying CNRDS provincial tax statistics. Statistics are computed on the estimation sample used in Table 6.

Table E.3: Summary statistics, bank annual report panel

	N	Mean	Std. Dev.	P25	Median	P75
Net Interest Income/Assets	1,500	0.0229	0.0099	0.0165	0.0215	0.0281
Log(Net Interest Income/Assets)	1,497	-3.8753	0.4881	-4.1012	-3.8371	-3.5708
Interest Income/Assets	1,500	0.0436	0.0118	0.0366	0.0420	0.0490
Interest Expense/Assets	1,493	0.0208	0.0085	0.0171	0.0200	0.0233
Loan Growth	1,494	0.2493	2.0537	0.1141	0.1626	0.2227
Loans/Assets	1,494	0.5024	0.1606	0.4363	0.5092	0.5712
Log(Total Loans)	1,494	24.3890	1.1443	23.5962	24.3206	25.2078
Nonperforming Loan ratio	1,479	1.7295	1.3987	0.9900	1.5000	1.9800
Provision Coverage Ratio	1,331	296.9807	333.2781	174.8200	223.8500	324.4100
Log(Total Assets)	1,500	25.1083	1.1744	24.2798	25.0917	25.9433
Equity/Assets	1,500	0.0783	0.0203	0.0653	0.0771	0.0880
Liquid Assets/Assets	1,245	0.2374	0.1086	0.1549	0.2155	0.3026
Operating Expenses/Assets	1,488	0.0095	0.0038	0.0071	0.0090	0.0113
Deposits/Assets	1,493	0.7541	0.2589	0.6676	0.7570	0.8333

Notes: The unit of observation is a bank fiscal year. The sample includes banks whose domestic branch network is confined to a single province. Invoice digitization reform (GT3) equals one in fiscal years at or after the GT3 rollout *year* in the bank's home province (annual timing). All balance sheet controls enter regressions in the previous year. Statistics are computed on the main estimation sample used in Table 15.