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Alejandro Casado, Marco Giometti, José E. Gutiérrez,
David Martínez-Miera, Alexandra Matyunina
and Tammaro Terracciano

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Alejandro Casado (**)

BANCO DE ESPAÑA

Marco Giometti

UNIVERSIDAD CARLOS III DE MADRID

José E. Gutiérrez (***)

BANCO DE ESPAÑA

David Martínez-Miera

UNIVERSIDAD CARLOS III DE MADRID AND CEPR

Alexandra Matyunina (****)

BANCO DE ESPAÑA

Tammaro Terracciano

IESE BUSINESS SCHOOL

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(**) alejandro.casadom@bde.es
(***) josee.gutierrez@bde.es
(****) alexandra.matyunina@bde.es

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Abstract

We document the effects of electronic invoicing (eInvoicing) on credit markets. By making invoices more standardized, verifiable and harder to falsify, eInvoicing changes lenders' information sets, facilitating invoice-based financing and credit risk assessment. We exploit a regional eInvoicing mandate and administrative credit data using a difference-in-differences design to provide three main insights. First, credit reallocates toward firms already relying on invoice-based credit ("invoice firms") and away from non-invoice firms. Second, the cost of (non-)invoice credit falls (rises) for (non-)invoice firms, consistent with a supply-driven mechanism. Third, banks' information production changes: eInvoicing widens (reduces) the dispersion of rates and banks' risk assessments and improves (worsens) their predictive accuracy for (non-)invoice firms. Overall, eInvoicing reshapes credit market outcomes, with uneven effects across borrowers.

Keywords: digitalization, e-Invoicing, credit markets, banks' information production.

JEL classification: G21, G38.

Resumen

En este documento se analizan los efectos de la facturación electrónica *(e-Invoicing)* sobre los mercados de crédito. Al estandarizar la información, hacerla verificable y dificultar su falsificación, la *e-Invoicing* modifica el conjunto de información disponible para los prestamistas, lo que facilita la financiación basada en facturas (por ejemplo, el *factoring)* y la evaluación del riesgo crediticio. La identificación empírica se basa en la implantación de la *e-Invoicing* en el País Vasco y no en las provincias limítrofes, así como en el uso de datos administrativos de crédito, combinados con una estrategia de diferencias en diferencias. Los resultados muestran, en primer lugar, una reasignación del crédito hacia las empresas que ya recurrían a la financiación basada en facturas y una contracción del crédito para aquellas que no la utilizan. En segundo lugar, el coste del crédito disminuye (aumenta) para las primeras (segundas), en línea con la existencia de un canal de oferta. En tercer lugar, se observan cambios en la producción de información por parte de las entidades bancarias: la *e-Invoicing* incrementa (reduce) la dispersión de los tipos de interés y de las evaluaciones de riesgo, y mejora (empeora) la capacidad predictiva de dichas evaluaciones para las empresas que utilizan (no utilizan) la financiación basada en facturas. En conjunto, los resultados sugieren que la *e-Invoicing* reconfigura los resultados del mercado de crédito, con efectos heterogéneos entre los prestatarios.

Palabras clave: digitalización, facturación electrónica, mercados de crédito, producción de información.

Códigos JEL: G21, G38.

1 Introduction

Digitalization is reshaping how firms operate, make payments, and obtain financing (Berg et al., 2022; Goldfarb and Tucker, 2019; Adrian and Mancini-Griffoli, 2021). While prior work on digital payments emphasizes their implications for financial inclusion, efficiency, and competition, it has focused on payment execution, without analyzing the invoicing infrastructure that underpins economic transactions.[1] This distinction matters because invoices record the counterparties, timing, and economic content of such transactions. Electronic invoicing, or eInvoicing, transforms these records into standardized, traceable, and verifiable digital documents, thereby changing the quality and usability of the recorded information. Despite its growing adoption worldwide and policy relevance, as reflected, for example, in the European Commission's view of eInvoicing as a building block for a fully digital Single Market, there is little evidence on whether eInvoicing affects firms' access to finance or credit markets more broadly.[2]

The premise of this study is that eInvoicing can affect credit markets by changing the information firms can credibly provide to lenders. Although eInvoicing is often introduced to reduce tax evasion and misreporting to tax authorities, its relevance for lenders need not stem from the revelation of previously hidden sales or revenues.[3] Rather, eInvoicing may matter because invoices become more standardized, easier to verify, and harder to falsify or duplicate. These features can facilitate financing arrangements directly tied to invoices, such as factoring or confirming, and may also improve credit risk assessment more broadly. By reshaping the transactional information available to lenders, eInvoicing may affect both the availability and the composition of credit to firms.

Our contribution is to document the effects of eInvoicing on credit markets by exploit-

1. See, among others, Parlour et al. (2022); Ghosh et al. (2026); Vives (2019); Jack and Suri (2014); Dalton et al. (2024); Ouyang (2026).
2. For more information on countries mandating eInvoicing adoption for business transactions, see the statistics reported on ESKER's webpage. For the European Commission's stance, see the dedicated eInvoicing webpage.
3. Firms may have incentives to conceal activity from tax authorities, and eInvoicing can increase tax compliance (Bellon et al., 2022). Nevertheless, banks need not rely on firms' legally reported income as they can infer borrowers' true income for their credit decisions (Artavanis et al., 2016).

ing a unique policy change that mandated eInvoicing adoption at the regional level and combining it with exhaustive administrative credit data. We document substantial heterogeneity across firms, depending on their reliance on invoice-based financing arrangements prior to the eInvoicing mandate. Firms that already relied on invoice-based credit ("invoice" firms) benefit from an increase in both invoice-based and overall credit, while firms that did not rely on invoice-based credit ("non-invoice" firms) experience a reduction in overall credit. We provide evidence consistent with a credit-supply mechanism, driven by changes in lenders' ability to produce and use information, explaining these results. Specifically, we document how lenders have relatively more (less) information to differentiate between invoice (non-invoice) borrowers. Overall, our results suggest that eInvoicing primarily benefits firms for which better transactional records of commercial activity are more relevant to credit risk assessment, while generating negative spillovers for firms for which such records are less informative.

To conduct our analysis, we exploit a unique policy change that mandated eInvoicing adoption at the regional level, combined with administrative credit data from the Bank of Spain. Our setting is the introduction of TicketBAI, a mandatory eInvoicing framework rolled out in Spain's Basque Country region from 2021 onward, while the rest of Spain was not subject to a similar mandate.[4] A key advantage of this setting is that we can observe loan-level information, banks' internal assessments of borrower risk, loan defaults, and firms' balance sheets for both the Basque Country and the neighboring, unaffected regions. This allows us to implement a difference-in-differences design to identify the effects of eInvoicing on firms' financing, credit allocation, and lenders' information production. Specifically, we compare the evolution of credit market outcomes for firms subject to the Basque eInvoicing mandate with those of firms in neighboring regions.

TicketBAI was introduced by the provincial tax authorities of Spain's Basque Country to strengthen tax compliance and digitize firms' invoicing practices. The reform requires firms to issue digital invoices using certified software that generates a unique identification

4. At the time of the TicketBAI reform, no eInvoicing mandate was in place in the rest of Spain. A national framework, VeriFactu, was announced in 2023 and is scheduled to be implemented in January 2027.

code and transmits the invoice to the provincial tax authority upon issuance. While this system digitalizes, standardizes, and authenticates the information, it does not grant lenders automatic access to eInvoices; firms must still voluntarily share them. The rollout of TicketBAI varied across the three Basque provinces of Álava, Biscay, and Gipuzkoa and included two phases: a voluntary phase, during which firms were encouraged to adopt the system early through fiscal incentives, and a mandatory phase, during which non-compliance became subject to monetary penalties. The voluntary phase began in Gipuzkoa in January 2021 and in Álava and Biscay in January 2022, while mandatory adoption followed later and at different dates across provinces.[5] For our main analysis, we make two assumptions. First, we conservatively define the beginning of the voluntary phase as the start of treatment, given the numerous and generous fiscal incentives for early compliance.[6] Second, given the absence of firm-level compliance information, we assign treatment based on the province where the firm's headquarters is located. Firms headquartered in the neighboring provinces of Burgos, La Rioja, Cantabria, and Navarre, where no comparable eInvoicing mandate was introduced during our sample period, serve as the control group.

We combine this institutional variation with administrative data from the Bank of Spain. The CIR (Central Credit Register) provides monthly loan-level information on nearly the universe of credit exposures in Spain, including origination, type, maturity, amount, interest rate, and default status. A distinctive feature of these data is the inclusion of banks' internal estimates of borrowers' probability of default (PD).[7] We link these data to annual balance sheet and income statement information from the CBI (Central Balance Sheet Data Office), which allows us to analyze firms' credit conditions and lenders' information production. Our sample covers January 2021 to December 2024. We exclude earlier years because the COVID-19 crisis and the associated government loan guarantee program may have distorted credit market dynamics and outcomes. We also restrict the

5. Mandatory adoption began in 2022 for some categories of firms in Álava and Gipuzkoa, and later became universal in December 2022 and June 2023, respectively; in Biscay, it began in January 2024 for large firms and became universal in 2026.

6. When we relax this assumption and allow for differential treatment in the voluntary and mandatory phases, we find that results are generally stronger for the mandatory phase.

7. This information is available at the bank-firm-quarter level only for banks using the Internal Ratings-Based (IRB) approach under Basel rules.

sample to micro, small, and medium enterprises, which are less likely to borrow or do business extensively outside their home province, and exclude Gipuzkoa because it entered the reform in early 2021, leaving no reliable pre-treatment-period data. The final sample includes 566,679 loans and 24,710 firms across the treated provinces of Álava and Biscay and the control provinces of Burgos, La Rioja, Cantabria, and Navarre. Firm-, loan-, and province-level characteristics are broadly balanced across the treated and control group.

Our empirical strategy to identify the effects of eInvoicing adoption on firms' credit availability and composition uses a standard difference-in-differences design with granular fixed effects, comparing the evolution of credit for similar firms and lending relationships between treated and control provinces. Specifically, we estimate a specification at the bank-firm-month level that includes industry×size-tercile×month fixed effects to account for credit demand factors (à la Degryse et al., 2019), bank×month fixed effects to absorb any other relevant lender-specific variation, bank×firm fixed effects to account for non-random matching, as well as standard firm-level controls. Because the beginning of our sample period coincides with the COVID-19 crisis and related public support programs, whose impacts might have differed across treated and control groups and potentially correlated with credit market outcomes, we control for a wide range of COVID-related factors.[8] We also estimate specifications at the firm-month level to see if results aggregate at the firm level.

Our analysis yields three sets of results. The first is on credit quantities. We show that, at the aggregate level, we do not observe a relevant effect of eInvoicing on firms' overall credit.[9] This aggregate result, however, masks substantial heterogeneity across firms. On the one hand, invoice firms in treated provinces experience an increase in invoice credit

8. Our main specification include: i) at the province-month level, a Google Mobility index to capture potential restrictions to economic activity and the severity of the crisis, specifically, we use the change in visitors to public transport hubs relative to a baseline; ii) at the province-industry-month level, the number of temporary furloughs that benefited from public support under the *Expediente de Regulación Temporal de Empleo* (ERTE) program to capture the potentially heterogeneous decrease in firms' operating expenses; iii) at the bank-firm-month level, the stock of loans subject to government guarantees provided by the Spanish *Instituto de Crédito Oficial* (ICO), to account for potential changes in the composition of borrowing, credit terms, and default dynamics.

9. While we do observe an increase in invoice-based credit by 2.43% at the firm level, consistent with eInvoicing improving the properties of invoices relevant to lenders, this effect is quantitatively modest, as invoice-based credit accounts for around 7% of total credit for the average firm in our sample.

by 4.02% and in total credit by 0.59% relative to invoice firms in the control group. These average relationship-level estimates also aggregate at the firm level: invoice credit increases by 2.11% and total credit rises by 1.11%. These effects operate through both an intensive-margin effect, through larger credit amounts from pre-existing invoice relationships, and an extensive-margin effect, through a higher likelihood of originating new invoice-based relationships. On the other hand, non-invoice firms in treated provinces experience a decrease in total credit by 0.66% relative to non-invoice firms in the control group at the bank-firm level, and by 1.37% at the firm level. For non-invoice firms, the decline in credit operates through the intensive margin, i.e., through lower credit amounts from pre-existing relationships, rather than through a significant reduction in new relationship formation. Overall, these findings indicate that eInvoicing leads to a reallocation of credit toward firms for which invoice records already played a relevant role in their financing mix.

To reinforce the interpretation that eInvoicing is driving these effects, we also conduct a battery of robustness tests. First, we distinguish between the voluntary and mandatory phases of the reform and find that effects, while already present during the voluntary phase, intensify once adoption becomes mandatory. Second, we estimate a more demanding within-province triple-difference specification that accounts for province-specific time-varying shocks and obtain similar results. Third, we address the concern that our estimates may reflect improved tax compliance rather than changes in the informational properties of invoices, and find no evidence of a differential increase in sales in treated provinces after the reform.

Our second set of results is on interest rates and links the observed credit-quantity patterns to a plausible credit-supply mechanism. After the introduction of eInvoicing, invoice firms in treated regions face a relative decline in the cost of invoice credit, while non-invoice credit becomes relatively more expensive; non-invoice firms, in turn, face higher borrowing costs. For invoice firms, the cost of invoice credit in our bank-firm (firm) specifications falls by about 1.98 (2.63%), whereas the cost of non-invoice credit rises by about 3.28% (9.61%). For non-invoice firms, total borrowing costs increase by about 3.23% (8.30%). Taken together with the quantity results, these patterns are harder to reconcile with a pure demand story and instead point to a supply-driven reallocation of credit toward invoice-based lending for invoice firms.

Our third and final set of results suggests that the above findings are driven by changes in banks' information production. Specifically, we explore two complementary dimensions. First, we focus on whether eInvoicing increases the cross-sectional dispersion of loan rates and banks' internal risk assessments, with the idea that more standardized and verifiable information content of digital invoices helps banks to better discriminate among borrowers. Empirically, we estimate whether the reform impacted the standard deviation and the interquartile range of loan rates and banks' internal PDs, using a province-quarter-level specification with province and quarter fixed effects and a comprehensive set of time-varying province controls. We find that eInvoicing increases (decreases) dispersion in the cost of credit by roughly 34% (−17%) and internal risk assessments by 15% (−13%) for invoice (non-invoice) firms.

Second, we assess whether eInvoicing improves the quality of banks' internal risk assessments. Specifically, we analyze changes in the predictive accuracy of internal PDs using the Area Under the Receiver Operating Characteristic curve (AUROC), which measures how well PDs rank borrowers with respect to ex-post default. We adopt a graphical difference-in-difference approach (as in Howes and Weitzner, forthcoming), and find a relative improvement (decrease) in predictive accuracy for invoice (non-invoice) firms.[10] For invoice firms, the treated group exhibits a pre-reform AUROC approximately 0.037 smaller than the control group, a difference that is statistically significant at conventional levels. However, this gap shrinks after the reform and becomes statistically indistinguishable from zero, suggesting a substantial relative improvement in predictive accuracy. For non-invoice firms, treated and control groups display statistically similar AUROCs in the pre-reform period, whereas after eInvoicing, the AUROC for the control group is roughly 0.052 larger than for the

10. We conduct extensive robustness tests to address two concerns: first, since the design does not allow us to include covariates or fixed effects as in a regression framework, AUROC comparisons may be affected by compositional differences; and second, the post-reform period overlaps with public policies introduced during the COVID-19 pandemic. These tests include matching on observable characteristics, restricting the sample to stayers, splitting by relationship cohorts, and excluding observations in which all outstanding loans are either ICO-guaranteed or under payment moratoria.

treated group, a difference that is statistically significant at the 1% level.[11] Overall, these results are consistent with eInvoicing affecting banks' information production in both pricing and risk assessment, particularly for firms whose access to financing is more tightly linked to invoices. This suggests that banks reallocate information production and screening efforts toward those firms, and away from borrowers whose invoice records are less informative.

In conclusion, our findings show that eInvoicing reshapes credit allocation by changing the information set available to lenders, with uneven consequences across borrowers. When new digital records are more informative for some borrowers than for others, lenders' improved ability to discriminate creates winners and losers, tightening credit precisely for those borrowers whose activity is least represented digitally. From a policy perspective, this suggests that the design of invoice digitalization initiatives should be evaluated not only by their average effects but also by their distributional consequences to mitigate unintended consequences.

1.1 Related Literature

We contribute to four streams of literature. The first one studies digitalization in finance, with a particular focus on digital payments and the broader transformation of financial intermediation. A large body of work shows that digital payments can expand financial inclusion and improve welfare (Ouyang, 2026; Jack and Suri, 2014; Suri and Jack, 2016), affect economic growth (Dubey and Purnanandam, 2023; Chodorow-Reich et al., 2020), change monetary policy transmission (Huang, 2024; Liang et al., 2024) and alter market structure and competition in financial services (Vives, 2019; Sarkisyan, 2023; Parlour et al., 2022). Dalton et al. (2024) show that the adoption of electronic payments in Kenya expands financing opportunities for SMEs. Our contribution is to shift attention from the digital execution of payments to an equally fundamental input into corporate finance: the invoicing infrastructure that underlies business-to-business transactions. We show that digitizing invoices matters for firms' financing, highlighting

11. These differences are also economically meaningful, as even a 0.01 improvement in AUROCs *"is considered a noteworthy gain in the credit scoring industry"* (Iyer et al., 2016, p. 1565).

the potential spillovers of digitalization policies to credit markets.

Closer to our paper, a related literature studies how financial technology can shape credit access and market structure by changing the information set available to lenders (Babina et al., 2025; Berg et al., 2022; He et al., 2023; Hau et al., 2024; Erel and Liebersohn, 2022; Buchak et al., 2018; Fuster et al., 2019; Russel et al., 2024). A narrower set of studies highlights the important role of digital footprints and transaction data for credit risk assessment and lending decisions (Berg et al., 2020; Ghosh et al., 2026; Alok et al., 2024; Frost et al., 2019; Yin, 2024; Huang et al., 2023; Di Maggio and Ratnadiwakara, 2025). Our contribution is to study a distinct source of digital information: standardized, verifiable invoice records generated by eInvoicing. By documenting heterogeneous effects across firms, our evidence highlights that the gains from digitalization in credit markets are not uniform and speaks to the broader question of who benefits from digitalization.

The third literature stream concerns the role of information in credit markets. A central question in this literature is whether greater availability of public information substitutes for or complements banks' production of private information. On the one hand, better public signals may reduce banks' incentives to invest in costly screening and monitoring by compressing informational rents (Hauswald and Marquez, 2003; Dell'Ariccia and Marquez, 2004; Vives and Ye, 2025a,b). On the other hand, they may complement banks' monitoring and screening efforts by making private information more valuable or easier to use (Pagano and Jappelli, 1993; Karapetyan and Stacescu, 2014; Padilla and Pagano, 1997; Bennardo et al., 2015). Empirical work, mostly focused on information-sharing arrangements among lenders, offers mixed evidence (Brown et al., 2009; Djankov et al., 2007; Doblas-Madrid and Minetti, 2013; Jappelli and Pagano, 2002; Brown and Zehnder, 2007; Sutherland, 2018; Hertzberg et al., 2011; Cheng and Degryse, 2010). D'Andrea et al. (forthcoming) show that improvements in information technology increase banks' information acquisition by studying the introduction of broadband internet in Italy. Our contribution is to bring this question to a new setting in which firms generate authenticated, verifiable, and standardized data on their transactions through eInvoicing. We show that eInvoicing affects banks' information production and that the consequences of improved hard information are not uniform, but depend on how relevant the new records are to lenders' screening and monitoring of different borrowers.

Finally, our paper contributes to the nascent literature on eInvoicing. Virtually all the existing studies focus on the implications of digitising invoicing for tax compliance. This is particularly relevant in developing economies, and evidence from countries such as Peru, Rwanda, and Ethiopia consistently points to the positive role of eInvoicing adoption in value-added tax (VAT) compliance and collection (Bellon et al., 2022; Kotsogiannis et al., 2025; Mascagni et al., 2021; Ali et al., 2021; Hakizimana and Santoro, 2023). Tao and Li (2026) and Fan et al. (2018) reach similar conclusions studying the 2001 rollout of mandatory computerization of VAT invoices in China. Heinemann and Stiller (2025) show that the introduction of eInvoicing in Italy in 2019 reduced cross-border tax fraud. Bellon et al. (2023) analyze the determinants of eInvoicing adoption and find significant positive adoption spillovers along the supply chain. Relative to these studies, we are the first to document that the effects of eInvoicing adoption extend beyond tax compliance and can spill over into firms' financing and credit markets.

2 The Basque eInvoicing Mandate

TicketBAI is a regulatory initiative introduced by the provincial tax authorities of Spain's Basque Country, a region composed of the three provinces Álava, Biscay, and Gipuzkoa, to digitalize the documentation of business activity (see Figure 1).[12, 13] The reform mandates that firms issue invoices using certified invoicing software that automati-

12. TicketBAI is part of a broader process of invoice digitalization in Spain. The Spanish tax authority is launching a national eInvoicing mandate, VeriFactu, which is scheduled to take mandatory effect on 1 January 2027, i.e., after the end of our sample period. Link: `https://sede.agenciatributaria.gob.es/Sede/ayuda/consultas-informaticas/presentacion-declaraciones-ayuda-tecnica/aplicacion-gratuita-verifactu-aeat.html`.
13. It is worth noting that, while it is not a digitalization initiative per se, some firms in Spain are subject to the *Suministro Inmediato de Información* (SII), a VAT compliance and tax-control system for large VAT taxpayers that was introduced in 2017. SII mandates that specific firms – e.g., those with annual turnover above €6 million – *self-report* VAT-book billing records to the tax authority within four days. Link: `https://sede.agenciatributaria.gob.es/Sede/en_gb/iva/suministro-inmediato-informacion/informacion-general.html`. Notably, our findings are unlikely to be driven by the SII. See Appendix B.5 for a detailed discussion.

cally transmits structured, tamper-proof invoice data to tax authorities in real time. Each invoice contains a cryptographic hash and QR code, ensuring authenticity and traceability. Therefore, the reform improves the credibility and verifiability of firms' commercial activity by standardizing transaction-level information, as all firms have to use such software when invoicing. Firms can then provide this standardized, verifiable documentation of accounts receivable and contractual relationships to banks, thereby changing lenders' information set. Because firms must still voluntarily share electronic invoices and authorize lenders to access them, our estimates likely represent a lower bound on the effects of reforms or technologies that would give lenders automatic access to these data, provided that such access does not generate significant general equilibrium effects.

(a) Spain. **(b)** Basque Country and surroundings.

Figure 1. Sample of provinces

Notes: Panel 1a shows the Spanish area we study, while, in panel 1b, we zoom in on this green area. In this latter, the bold line delimits the Basque Country, and its provinces (Álava, Biscay, and Gipuzkoa). Neighboring provinces (in grey) are used as a control group in the empirical analysis.

The possible relevance of such a mandate for credit stems from anecdotal evidence suggesting that banks actively rely on invoice-related information when assessing firms' credit risk. Industry and policy discussions also point in the same direction, highlighting that the standardized format and near-real-time generation of eInvoicing data can facilitate the assessment of ceded invoices and reduce frictions in receivables financing.[14] This view is also consistent with recent market initiatives aimed at improving the reliability of invoice-based financing. For example, the interbank platform *InBlock*, launched after our sample period (October 2025), was designed to reduce the risk of financing non-existent or duplicated invoices, indicating that lenders place value on invoice-level information that is easier to trust and process.[15]

Several features of TicketBAI's introduction are particularly relevant for our empirical design. First, adoption was limited to the Basque Country – it did not occur in the other regions of Spain – and unfolded gradually across provinces. It took place in two phases: a voluntary phase supported by substantial fiscal incentives and a mandatory phase enforced through binding legal penalties. Voluntary adoption began in Gipuzkoa in January 2021, and in Álava and Biscay in January 2022. The start of mandatory adoption varied across provinces and firm types. In Álava, eInvoicing was compulsory for tax professionals starting in April 2022, followed by a sectoral rollout that culminated in December 2022, when every firm had to comply with the eInvoicing mandate. In Gipuzkoa, mandatory adoption started in July 2022 and rolled out across sectors through June 2023. In Biscay, mandatory adoption began in January 2024 for large firms and was phased in for SMEs through January 2026. Figure 2 summarizes the different phases of the rollout across the three provinces.

Second, we do not directly observe firm-level compliance. Our design identifies the effect of province-time-level exposure to TicketBAI rather than the effect of actual firm compliance. In the absence of firm-level adoption data, we rely on the timing of province-wide implementation in our identification strategy.

Third, the institutional design included both strong incentives for early compliance and stringent penalties for non-compliance once the mandatory phase began. On the one hand, Álava and Gipuzkoa allowed firms to deduct 30–60% of eligible implementation

14. See, for instance, the European Commission's stance on eInvoicing. Link: `https://ec.europa.eu/digital-building-blocks/sites/spaces/DIGITAL/pages/467108637/eInvoicing`.
15. See *Asociación Española de Factoring*'s newsletters about *InBlock* and *Factura Eletronica* for additional information. Link: `https://factoringasociacion.com/publicaciones-y-newsletter/`.

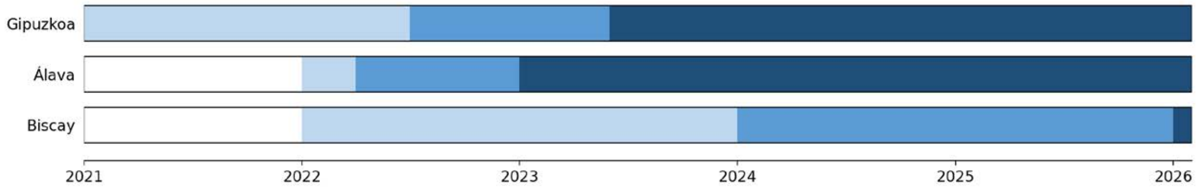


Figure 2. Gantt chart of TicketBAI staggered introduction

Notes: White areas indicate no implementation; light blue areas indicate periods of voluntary adoption; blue areas indicate periods in which some industries face mandatory adoption while others remain voluntary; and dark blue areas indicate mandatory adoption for all industries.

costs from taxable income, while Biscay offered a 30% deduction, capped at €5,000.[16] These incentives likely induced meaningful adoption even before legal mandates took effect. On the other hand, firms could be fined €2,000 per non-compliant invoice, with systematic violations penalized up to 20–30% of prior-year turnover. Severe infractions involving data manipulation carried a minimum fine of €40,000.[17] Taken together, these incentives and penalties make it plausible that province-time-level exposure is related to actual adoption already during the voluntary period, even though we do not observe firm-level compliance directly. For this reason, we conservatively assume the treatment begins with the voluntary phase and continues through the mandatory phase.

Fourth, although the legal rollout of mandatory adoption also differed across industries, we cannot exploit this variation in our identification strategy for several reasons. On the one hand, TicketBAI uses the *Impuesto sobre Actividades Económicas* (Economic Activity Tax – IAE) classification to determine when firms become subject to mandatory adoption, whereas the industry information in our data is based on the standard NACE classification. Because there is no clean one-to-one mapping between the two systems, industry-level treatment assignment within a province would be measured with substantial error.[18] On the other hand, industry-specific mandatory deadlines are an imperfect proxy

16. See Biscay's Norma Foral 8/2023 and accompanying Batuz decrees; Álava's Normative Decree of Fiscal Urgency DN 9/2022; Gipuzkoa's Foral Decree DF 32/2020.
17. These figures refer to Biscay; see `https://www.bizkaia.eus/es/web/comunicacion/noticias/-/news/detailView/20665`. Álava and Gipuzkoa have similar rules.
18. For example, NACE codes 6201, 6202, and 6209 map into both IAE sectors 2-76 and 1-845, with the former subject to mandatory implementation in Álava starting in July 2022 and the latter in

for treatment timing, since firms could and had incentives to adopt earlier during the voluntary phase. Finally, the timing differences across industry blocks are often short, which limits the amount of relevant variation available for identification. For these reasons, we do not exploit the industry-specific rollout and instead rely on treatment timing defined at the province-time level, starting with the voluntary period.

3 Data

In this section, we describe the data sources used in the analysis, the sample construction, and the main sample characteristics. We also present covariate balance tests between treated and control provinces and descriptive evidence on the use of invoice-based credit.

3.1 Data Sources and Sample Construction

Our analysis combines two administrative data sources provided by the Bank of Spain. The Central Credit Register (CIR) provides monthly loan-level information on nearly the universe of credit exposures in Spain, as its reporting threshold is set at €3,000. It includes detailed information on loan terms and outcomes, such as origination, type, maturity, amount, interest rate, and default status. For banks using the Internal Ratings-Based (IRB) approach under Basel regulation, the CIR also reports internal estimates of borrowers' one-year probability of default (PD), which we use in the analysis of lenders' information production. We complement these data with annual firm-level balance sheet and income statement information from the Central Balance Sheet Data Office (CBI), including total assets, sales, leverage, liquidity, tangibility, profitability, age, and the firm's location and industry.[19] Merging CIR and CBI allows us to construct a

December 2022. This type of mismatch arises because the IAE classification distinguishes among business, professional, and artistic activities for tax purposes, whereas NACE classifies firms by their principal economic activity.

19. Firm size categories follow the European Commission Recommendation 2003/361/EC. Micro enterprises are firms with fewer than 10 employees and annual turnover or balance sheet total not exceeding €2 million. Small enterprises have fewer than 50 employees and turnover or balance sheet total not exceeding €10 million. Medium-sized enterprises employ fewer than 250 people and have either an annual turnover not exceeding €50 million or a balance sheet total not exceeding €43 million. Firms with more

high-frequency loan-firm panel with matched balance-sheet, credit, and ex-post default information. To account for the COVID-19 crisis and related policy responses, we also collect province-industry-time information on temporary furloughs under the *Expediente de Regulación Temporal de Empleo* (ERTE) program and community mobility indices at the province-time level from Google COVID-19 Mobility Reports.

We obtain all loans issued to firms headquartered in the three provinces of the Basque Country – Álava, Biscay, and Gipuzkoa – and the four neighboring provinces – Burgos, Cantabria, La Rioja, and Navarre. Because treatment is assigned at the province level based on the location of a firm's headquarters, we restrict the sample to micro enterprises and small and medium-sized enterprises (SMEs), which are less likely to borrow or operate extensively outside their headquarters' province. We further restrict the sample to the period from January 2021 to December 2024, and to firms that were active in 2021.[20] We exclude earlier years because the COVID-19 crisis and the associated loan-guarantee and support programs may have distorted credit conditions and outcomes. This choice also implies excluding Gipuzkoa, which entered the voluntary phase of the reform in early 2021 and therefore lacks a reliable pre-treatment period.[21] Our treated provinces are thus Álava and Biscay, while Burgos, Cantabria, La Rioja, and Navarre serve as controls. Throughout the analysis, we aggregate loan-level information to the bank-firm-month level unless otherwise specified. All variables are winsorized at the 1st and 99th percentiles.

Our final dataset includes 566,679 loans and 24,710 firms that account for almost 1.9 million bank-firm-time observations, among treated and control provinces, from January 2021 to December 2024. In the next subsections, we report the balanced tables between treated and control provinces (see Table 1), and discuss the differences between invoice and non-invoice firms (see Table IA.III). Additional summary statistics are in Appendix A.

than 250 employees are classified as large enterprises.

20. In addition, we drop all observations for firms that exhibit abnormally large magnitudes of within-firm credit growth at any point in our sample.

21. Nevertheless, the inclusion of Gipuzkoa does not change the results. See Appendix B.3.

3.2 Sample Characteristics

Table 1 reports summary statistics and a covariate balance analysis between treated and control provinces. Panel A describes bank-firm-level characteristics for the full sample of SME bank-firm relationships as of the end of 2021. Treated and control bank-firm relationships look similar along most dimensions. The average bank-firm relationship involves about €240,000 in total credit in treated provinces and €251,000 in control provinces, with comparable risk profiles: the average probability of default is 4.63% in treated versus 5.06% in control provinces, and the realized default rate is 2.38% versus 2.77%. Pricing and maturity are also closely aligned, with average interest rates of 3.05% in treated provinces and 2.86% in control provinces, and an average maturity of about 80 months in both groups. Invoice-based credit accounts for a similar share of total credit in the two groups, around 6–7%. None of these differences is statistically significant. The only meaningful gap is in the share of ICO-guaranteed credit, which is 28% in treated versus 33% in control provinces.

Panel B reports firm-level characteristics and again indicates that the two groups are broadly comparable. Firms in treated and control provinces have a similar number of loans and bank relationships. The share of firms classified as invoice firms (see next subsection) is almost identical across the two groups, at 23% in treated versus 24% in control provinces. Treated and control firms are also of comparable size, holding about €1.6 million in assets and generating about €1.4 million in annual sales, and they look alike in terms of leverage (0.30 vs. 0.33), liquidity (0.66 vs. 0.62), profitability (0.02 in both groups), age (about 17 years), and exposure to ERTE. The only statistically significant difference is in tangibility, which is somewhat lower in treated than in control provinces (0.23 vs. 0.29).

Panel C reports province-level aggregates and confirms the same pattern of broad similarity. Treated and control provinces have comparable IRB banks' market shares (0.39 vs. 0.45), and industry composition, with shares of credit to agriculture, mining, manufacturing, construction, retail/wholesale, transport, and hospitality that are virtually indistinguishable across the two groups. Mobility patterns during the sample period are also similar (Google Mobility Index of -16.68 vs -1.99). The main differences are that treated provinces have a slightly lower share of SMEs (0.93 vs. 0.96) and somewhat lower concentration in corporate credit markets (HHI of 0.10 vs. 0.14). Overall, Table 1 suggests that the Basque's neighboring provinces are a plausible control group, since treated and control areas are broadly comparable on observables relevant to credit demand, credit supply, and lenders' risk assessments.

3.3 Stylized Facts about Invoice-Based Credit Use

The TicketBAI reform affects the properties of invoices, and therefore, its most likely effect is to change the information environment surrounding invoice-based credit, that is, credit granted on the basis of the purchase of invoices, bills, or other collection rights arising from the deferral of payments associated with the sale of goods or the provision of services.[22] Because the reform standardizes, authenticates, and makes transaction-level information underlying these instruments more verifiable, its effects are likely to be strongest for firms for which invoice-based financing is already an important part of their financing mix.

The data reveal a high degree of persistence in the use of invoice-based credit. Firms that use invoice-based credit in a given quarter continue to do so in the next quarter with probability 90.5%, whereas firms that do not use it remain non-users with probability 98.1%. At the yearly horizon, the corresponding transition probabilities are 87.0% and 97.1%, respectively (see Table 2). Transitions between the two states are therefore rare, suggesting that the use of invoice-based credit is a stable feature of firms' financing structure rather than a transitory borrowing choice.

As a consequence, we classify firms according to their pre-reform use of invoice-based credit. Specifically, we define an "invoice firm" as a firm with at least one outstanding invoice-based loan in the pre-reform period in our sample—that is, in 2021—and a "non-invoice firm" otherwise. Invoice and non-invoice firms differ markedly along several dimensions (see Table IA.III). Invoice firms account for 23.3% of firms in the sample, but they are substantially larger, older, and more connected to the banking system than non-invoice firms. They also differ systematically along other margins: invoice

22. See Appendix A.2 for the precise set of Bank of Spain categories included in our definition.

Table 1. Descriptive Statistics: Treated vs. Control Groups

This table presents descriptive statistics for the treated and control groups. All variables are measured as of the end of 2021, the pre-reform period, and are defined in Table IA.I. Columns (1) and (2) report the mean for the treated and control groups, respectively. Column (3) reports the difference between the treated and control groups, and Column (4) reports the p-value of a two-sided test for the equality of means across the treated and control groups. Standard deviations appear in parentheses, except for N, for which the relative share appears in parentheses. Panel A presents bank-firm-level variables for the full sample of bank-firm relationships as of the end of 2021. Panel B describes firm-level variables. Panel C reports province-level aggregates. For the tests of equality of means, standard errors are clustered at the province×industry level in Panels A and B, and heteroskedasticity-robust in Panel C. p-values are reported to two decimal digits, with values below 0.01 reported as < 0.01. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Treated	Control	Diff.	P-value
	(1)	(2)	(3)	(4)
Panel A: Bank-Firm Characteristics				
N	20,283 (43.89%)	25,929 (56.11%)		
Total credit, thousand €	239.53 (794.31)	251.04 (668.81)	-11.51	0.67
Probability of default, %	4.63 (15.71)	5.06 (17.05)	-0.43	0.33
Default rate, %	2.38 (15.19)	2.77 (16.33)	-0.39	0.20
Interest rate, %	3.05 (3.31)	2.86 (2.65)	0.19	0.13
Maturity, months	80.29 (51.20)	79.57 (53.55)	0.72	0.89
Share of invoice-based credit	0.06 (0.20)	0.07 (0.20)	-0.01	0.88
Share of ICO guaranteed credit	0.28 (0.39)	0.33 (0.41)	-0.05	$< 0.01^{***}$
Panel B: Firm Characteristics				
N	9,774 (44.03%)	12,424 (55.97%)		
N. loans	6.59 (12.68)	6.83 (11.21)	-0.24	0.75
N. bank relationships	2.21 (1.79)	2.29 (1.83)	-0.08	0.58
N. IRB bank relationships	1.45 (0.75)	1.43 (0.74)	0.02	0.77
Invoice firm	0.23 (0.42)	0.24 (0.42)	-0.01	0.90
Total assets, million €	1.63 (3.82)	1.64 (3.67)	-0.01	0.97
Sales, million €	1.42 (3.64)	1.46 (3.70)	-0.04	0.88
Leverage	0.30 (0.30)	0.33 (0.29)	-0.03	0.14
Liquidity	0.66 (0.31)	0.62 (0.29)	0.04	0.35
Tangibility	0.23 (0.27)	0.29 (0.27)	-0.06	0.04^{**}
Profitability	0.02 (0.18)	0.02 (0.15)	0.00	0.26
Age	17.02 (10.94)	17.64 (11.54)	-0.62	0.47
ERTE	81.91 (152.22)	32.19 (61.30)	49.72	0.18
Panel C: Province Characteristics				
N	2 (33.33%)	4 (66.67%)		
Share of SMEs	0.93 (0.00)	0.96 (0.01)	-0.03	0.03^{**}
IRB banks' market share	0.39 (0.07)	0.45 (0.04)	-0.06	0.24
Corporate credit market HHI	0.10 (0)	0.14 (0.02)	-0.04	0.10
Share of credit to :				
Agriculture	0.01 (0.00)	0.01 (0.00)	0.00	0.22
Mining	0.00 (0.00)	0.00 (0.00)	0.00	0.17
Manufacturing	0.15 (0.02)	0.17 (0.03)	-0.02	0.44
Construction	0.06 (0)	0.06 (0.02)	0.00	0.73
Retail/Wholesale	0.10 (0.04)	0.12 (0.02)	-0.02	0.42
Transport	0.02 (0.01)	0.04 (0.02)	-0.02	0.29
Hospitality	0.02 (0.01)	0.03 (0.01)	-0.01	0.18
Google Mobility Index	-16.68 (4.84)	-1.99 (19.32)	-14.69	0.37

Table 2. Persistence in Invoice-Based Credit Use.

This table reports transition probabilities in firms' use of invoice-based credit. Rows indicate whether firm f has positive invoice-based credit in period t, while columns indicate whether the same firm has positive invoice-based credit in period $t+1$. Firms classified as having no invoice-based credit may still use other forms of bank credit. Panel A reports transitions at the firm-quarter level; Panel B reports transitions at the firm-year level.

Panel A: Firm-quarter level

	No Invoice-based Credit$_{f,t+1}$	Invoice-based Credit$_{f,t+1}$
No Invoice-based Credit$_{f,t}$	98.1%	1.9%
Invoice-based Credit$_{f,t}$	9.5%	90.5%

Panel B: Firm-year level

	No Invoice-based Credit$_{f,t+1}$	Invoice-based Credit$_{f,t+1}$
No Invoice-based Credit$_{f,t}$	97.1%	2.9%
Invoice-based Credit$_{f,t}$	13.0%	87.0%

firms are more leveraged, hold a larger share of liquid assets, and rely more heavily on ICO-guaranteed credit, while exhibiting lower tangibility and slightly lower return on assets. These patterns are consistent with business models in which trade receivables are a recurrent and important component of working capital, and in which physical collateral plays a relatively smaller role. Taken together, the persistence in invoice-based credit use and the sharp differences in firm characteristics suggest that invoice and non-invoice firms rely on structurally different financing strategies rather than representing temporary users and non-users of the same credit product.

This motivates an analysis of the effects of eInvoicing separately for invoice and non-invoice firms. A natural question is whether treated and control firms are comparable within each of these two groups. Table IA.IV reports a covariate balance test for invoice firms and non-invoice firms across treated and control provinces. Overall, within each group, treated and control firms are balanced on most observable characteristics. Among invoice firms, treated provinces exhibit higher liquidity, lower tangibility, lower profitability, and a lower share of ICO-guaranteed loans than control provinces. Among non-invoice firms, treated and control provinces are even more similar, with only differences in tangibility and share of ICO-guaranteed loans.

4 eInvoicing and Credit Market Dynamics

After describing basic facts about firms, credit markets, and the use of invoice-based credit in our sample, we analyze the effects of eInvoicing on firms' credit. First, we examine whether eInvoicing adoption increases the use of invoice-based credit (hereafter, invoice credit). By requiring firms to issue standardized, digitally signed invoices, the reform alters the informational properties of the records that underlie business transactions, as discussed in Section 2. This should be particularly relevant for credit instruments backed by invoices, where lenders rely on information about the existence, authenticity, and quality of the underlying commercial claims. In this sense, eInvoicing lowers lenders' information-processing costs: it reduces both the cost of verifying the assets underlying invoice-based lending and the cost of standardizing invoice information before it can be incorporated into screening, monitoring, and credit-allocation decisions. This mechanism is closely related to theories in which more verifiable cash flows and pledgeable claims relax financing constraints and support credit provision (Hart and Moore, 1994; Holmström and Tirole, 1997). We therefore expect eInvoicing to increase firms' use of invoice credit.

We next analyze whether this increase in invoice credit reflects substitution across financing instruments or broader credit growth. If eInvoicing mainly improves the relative attractiveness of invoice-backed lending, firms may shift borrowing toward invoice-based credit without increasing total credit, leading to substitution. By contrast, if standardized and reliable invoice records also improve lenders' assessment of borrower quality or reduce monitoring costs more broadly, total credit may increase as well.[23] This view is consistent with the broader idea that more reliable hard information can improve credit decisions (Liberti and Petersen, 2019), and with more recent evidence that digital records and alternative data can facilitate borrower screening (Berg et al., 2020; Di Maggio and

23. The effect on total credit is, in principle, ambiguous. Better information may increase or reduce overall credit depending on whether, on average, the information revealed by eInvoicing leads lenders to reassess firms more positively or more negatively.

Ratnadiwakara, 2025). Hence, eInvoicing may also lead to substitution across instruments and/or to an expansion in total credit.

Relatedly, we analyze whether the effect of eInvoicing is heterogeneous across firms. We argue that the reform may have larger effects for firms whose financing is already closely linked to invoices and receivables, both because invoice records are likely to be more informative for lenders' credit decisions and because the reform may lower the cost of expanding an existing financing technology. For firms with no prior use of invoice-based credit, the effect might not be so clear-cut. While they may also benefit if the reform reduces adoption frictions and facilitates entry into this form of financing, the effect could also be negative if lenders face capacity constraints in processing additional information (Campbell et al., 2019; Mariathasan and Zhuk, 2022) or if they reallocate credit from one type of firm to the other due to better information about invoice firms. In such a case, the reform may reallocate screening effort and credit supply toward firms for which invoice data are most informative and away from firms for which such data are less useful. More generally, to the extent that invoice data improve lenders' ability to assess firms' commercial activity, the effects on total credit should be stronger among firms for which invoices are more informative about credit risk. Which force dominates, and for which firms, is ultimately an empirical question.

4.1 Identification Strategy

To test these hypotheses, we employ a standard difference-in-differences (DiD) design that exploits the introduction of the TicketBAI eInvoicing reform in the Basque Country as a source of plausibly exogenous variation in firms' eInvoicing use. Our design is driven by three features. First, Álava and Biscay share the same start of the voluntary adoption phase, in January 2022, even though the timing of mandatory adoption later differs across the two provinces. Second, we define treatment as starting with the voluntary phase rather than the mandatory one. As discussed in Section 2, both provinces offered substantial incentives for early compliance, making it likely that some firms adjusted their invoicing systems before adoption became legally mandatory. We therefore use January 2022 as the common treatment start date for Álava and Biscay, and regard this choice as conservative, since any resulting misclassification would attenuate our estimates toward zero.[24] Third, because our sample begins in 2021 for the reasons discussed in Section 3, this treatment definition leaves no reliable pre-reform period for Gipuzkoa, so we exclude that province from the analysis.[25]

Formally, we estimate the following specification from January 2021 to December 2024 at the bank-firm-month level:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}. \quad (1)$$

where f, b, p, i, s, and t index firm, bank, province, industry, size tercile, and month, respectively. The dependent variable $Y_{f,b,t}$ denotes either the amount of total credit, invoice credit, or non-invoice credit extended by bank b to firm f in month t. Because invoice-based credit is inherently short-term, the data contain many bank-firm-month observations in which this outcome equals zero. We therefore estimate Equation (1) using Poisson Pseudo-Maximum Likelihood (PPML), which allows us to retain zero observations and to interpret the coefficients as semi-elasticities (Chen and Roth, 2023).[26] Standard errors are clustered at the province$\times$month level.

The coefficient of interest is β, which captures how credit outcomes evolve after January 2022 for firms in treated provinces relative to firms in control provinces. The identifying variation comes from comparing the same bank-firm relationships before and after the reform across treated and control areas. The granular set of fixed effects included in Equation (1) plays an important role in making this comparison as informative as possible. First, we include bank$\times$month fixed effects ($\alpha_{b,t}$), which absorb all lender-specific shocks that vary over time. These fixed effects are especially important in our setting because banks may adjust credit supply in response to macroeconomic conditions, monetary policy, or internal strategies, unrelated to TicketBAI. Second, we include industry$\times$size-tercile$\times$month fixed effects ($\alpha_{i,s,t}$), which absorb time-varying credit demand factors

24. We also repeat our analysis employing a staggered DiD design in which we explicitly distinguish between the voluntary and mandatory periods. See Section 4.5 for more details.
25. In Appendix B.3, we show that including Gipuzkoa in our baseline analysis does not change our main results.
26. Specifically, we use the `ppmlhdfe` routine in Stata developed by Correia et al. (2020).

common to firms of similar size operating in the same industry (Degryse et al., 2019). In this way, we compare treated and control firms that face similar sectoral and size-specific credit-demand conditions. Third, we include bank×firm fixed effects ($\alpha_{f,b}$), which account for all time-invariant features of a given lending relationship, including non-random matching between banks and borrowers.

Our identifying assumption is that, after absorbing lender-specific shocks, time-varying demand factors common to firms in the same industry and size class, and time-invariant features of bank-firm relationships, credit outcomes for treated and control firms would have followed similar trends absent the reform. In other words, conditional on our set of fixed effects, the introduction of TicketBAI is unrelated to other factors differentially affecting the evolution of credit in treated provinces after January 2022. As discussed in Section 3, the pre-reform covariate balance between treated and control firms supports the plausibility of this assumption (see Table 1).

To further reduce potential bias, we control for a rich set of time-varying firm and bank-firm controls ($X_{f,t-1}$), as well as factors related to the COVID-19 crisis and its policy response. At the firm level, we include measures of size, leverage, liquidity, tangibility, profitability, and age. At the bank-firm level, we include the lagged share of firm f's outstanding credit with bank b covered by ICO guarantees, which captures differences in the composition of borrowing and in banks' effective risk exposure, as well as an indicator for whether the firm is in default on any outstanding credit position with bank b. At the province-industry-month level, we include the number of workers covered by the *Expediente de Regulación Temporal de Empleo* (ERTE) program, which captures sector-specific labor-market support during the pandemic. We also control for a Google Mobility index that tracks changes in visitors to public transport hubs relative to a baseline (from Google COVID-19 Mobility Reports) at the province-month level, to account for heterogeneity in local restrictions to economic activity and in the severity of the pandemic shock. Overall, these controls reduce concerns that our estimates might be driven by differences in firm composition or exposure to COVID-related disruptions.

4.2 Main Results: Full Sample

We begin by examining whether eInvoicing affects firms' use of credit in the full sample. Table 3, Panel A, reports the results from estimating Equation (1) using invoice-based credit, non-invoice-based credit, and total credit as outcomes.

The estimates show a clear increase in invoice credit for treated firms following the introduction of TicketBAI relative to the control group. In column (1), the coefficient on $Post \times Treated$ is 0.0448 and statistically significant at the 1% level. Under a PPML specification, this implies an increase of about $100 \times (\exp 0.0448 - 1) = 4.58\%$ in invoice-based credit.[27] By contrast, columns (2) and (3) show essentially no effect on non-invoice credit or total credit: the estimated effects are -0.05% and 0.02%, respectively, and both are statistically insignificant. Thus, at the bank-firm level, eInvoicing increases the use of invoice-based credit relative to the control group, but without affecting either total borrowing or other forms of credit.[28]

These results are consistent with the idea that eInvoicing makes invoice-based lending more attractive. By improving the informational properties of invoices, the reform appears to strengthen the use of credit instruments based on invoices and receivables. At the same time, the absence of any effect on total credit at the bank-firm level suggests that the increase in invoice credit relative to the control group does not mechanically translate into a broader increase in borrowing within individual lending relationships.

Next, we examine whether these relationship-level effects aggregate to the firm level or are offset by substitution effects across banks. Because our previous estimates capture the average effect across lending relationships, a positive effect at the bank-firm level need not imply higher firm-level borrowing if firms expand invoice credit with some lenders while reducing it with others. To assess whether the reform changes firms' overall borrowing rather than only the composition of credit across lending relationships, we re-estimate the

27. Throughout this section, we report coefficient estimates that have already been transformed into percentage effects, computed as $100 \times \left(\exp \hat{\beta} - 1\right)$.

28. This is likely because invoice-based credit accounts for around 7% of total credit for the average firm in our sample.

Table 3. eInvoicing and Credit Supply

This table reports estimates of β from the following regressions over our main sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. In Panel A, the unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel B, the unit of observation is a firm-month, credit outcomes are aggregated across all banks lending to firm f in month t, and we estimate:

$$\mathbb{E}[Y_{f,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_f + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel A, $Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. In Panel B, $Y_{f,t}$ is the corresponding firm-level amount. The dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. Post_t is an indicator equal to one from January 2022 onward. Treat_p is an indicator equal to one for firms headquartered in treated provinces. Panel A includes industry×size×month, firm×bank, and bank×month fixed effects. Panel B includes industry×size×month and firm fixed effects. In both panels, $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. In Panel A, this vector also includes bank-firm-level controls: the share of bank-firm credit guaranteed by ICO and an indicator for whether the firm is in default with that bank. In Panel B, these two variables are replaced by their firm-level counterparts: the share of the firm's total credit guaranteed by ICO and an indicator for whether the firm is in default with any bank. Average bank controls in Panel B are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. In both panels, $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice	Non-Invoice	Total
	(1)	(2)	(3)
Panel A: Bank-firm level			
Post×Treated	0.0448***	-0.0005	0.0002
	(0.0076)	(0.0021)	(0.0019)
Industry×Size×Month FE	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes
Obs.	552,818	1,872,964	1,899,713
Pseudo R-Squared	0.76	0.95	0.95
Panel B: Firm level			
Post×Treated	0.0240***	-0.0019	-0.0016
	(0.0090)	(0.0019)	(0.0019)
Industry×Size×Month FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes
Avg. Bank Controls	Yes	Yes	Yes
Obs.	286,966	971,356	971,356
Pseudo R-Squared	0.88	0.98	0.98

same specification after aggregating credit outcomes at the firm-month level.[29] Table 3, Panel B, reports these results.

The firm-level evidence closely mirrors the bank-firm results. Column (1) shows that invoice credit increases by 2.43%, again statistically significant at the 1% level.[30] By contrast, columns (3) and (2) show no significant effect on total credit or non-invoice-based credit, with implied effects of −0.16% and −0.19%, respectively. Taken together, these results support the first mechanism discussed above: eInvoicing increases the use of invoice-based lending. At the same time, they provide little evidence that this average increase in invoice credit translates into broader credit growth or substitution with other types of credit.

4.3 Heterogeneity Analysis: Invoice vs Non-Invoice Firms

The estimates reported in Table 3, however, may conceal substantial heterogeneity across firms. First, the reform may matter more for firms already relying on invoice credit because, for these firms, it improves the functioning of an existing financing technology. Second, if invoice data improve lenders' broader assessment of borrower quality, credit effects should be stronger precisely for firms for which invoices are more relevant to screening and monitoring. We explore this heterogeneity by re-estimating Equation (1) on two subsamples of firms, based on their pre-reform use of invoice credit as a proxy of the relevance of invoices for their financing. One limitation of our approach is that we cannot estimate PPML specifications with invoice credit as the dependent variable for the subsample of non-invoice firms, as they always display zero invoice credit in the pre-reform period.[31] Table 4 reports these results.

The estimates reveal marked differences between invoice and non-invoice firms. Panel

29. In this firm-level specification, we include the same set of time-varying firm controls as Equation (1), as well as the share of firms' credit guaranteed by ICO, an indicator for default status with any bank, and the average of bank-level controls weighted by each bank's credit with the firm. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. We also include industry×size-tercile×month and firm fixed effects.

30. Because the bank-firm and firm-level specifications differ in their unit of observation and aggregation, the magnitudes of the respective coefficients are not directly comparable.

31. This follows directly from our definition of non-invoice firms. With no positive pre-treatment observations, the high-dimensional PPML estimator cannot use within-firm or within-relationship variation to identify a treatment effect on the level of invoice credit.

Table 4. eInvoicing and Credit Supply: Invoice vs Non-Invoice Firms

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms over our main sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. Invoice firms are firms with positive invoice-based credit in the pre-reform period; non-invoice firms are all other firms. In Panel A, the unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel B, the unit of observation is a firm-month, credit outcomes are aggregated across all banks lending to firm f in month t, and we estimate:

$$\mathbb{E}[Y_{f,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_f + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel A, $Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. In Panel B, $Y_{f,t}$ is the corresponding firm-level amount. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications using invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. Post_t is an indicator equal to one from January 2022 onward. Treat_p is an indicator equal to one for firms headquartered in treated provinces. Panel A includes industry×size×month, firm×bank, and bank×month fixed effects. Panel B includes industry×size×month and firm fixed effects. In both panels, $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. In Panel A, this vector also includes bank-firm-level controls: the share of bank-firm credit guaranteed by ICO and an indicator for whether the firm is in default with that bank. In Panel B, these two variables are replaced by their firm-level counterparts: the share of the firm's total credit guaranteed by ICO and an indicator for whether the firm is in default with any bank. Average bank controls in Panel B are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. In both panels, $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Panel A: Bank-firm level				
Post×Treated	0.0394***	0.0025	0.0059**	-0.0066***
	(0.0074)	(0.0032)	(0.0030)	(0.0022)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	499,113	714,333	739,989	1,159,071
Pseudo R-Squared	0.75	0.93	0.93	0.97
Panel B: Firm level				
Post×Treated	0.0209**	0.0094***	0.0110***	-0.0138***
	(0.0089)	(0.0026)	(0.0023)	(0.0024)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Avg. Bank Controls	Yes	Yes	Yes	Yes
Obs.	229,364	231,027	231,027	739,196
Pseudo R-Squared	0.88	0.98	0.98	0.98

A of Table 4 shows that, for invoice firms in treated provinces, eInvoicing increases total credit by 0.59% and invoice credit by 4.02% relative to invoice firms in control provinces, with a positive, although statistically insignificant, effect on non-invoice credit. For non-invoice firms in treated provinces, by contrast, total credit falls by 0.66% relative to non-invoice firms in the control group.

We then aggregate the data at the firm level to assess whether these relationship-level effects translate into changes to overall firm borrowing. Panel B shows that they do. For invoice firms, total credit rises by 1.11%, invoice credit by 2.11%, and non-invoice credit by 0.94%, with all three effects statistically significant. For non-invoice firms, total credit falls by 1.37%. These estimates imply that the zero average effect on total credit in the full sample masks substantial heterogeneity across firms.

Taken together, the results in Table 4 help interpret the full-sample evidence in Table 3. The positive average effect on invoice credit is driven by firms that were already using this type of financing before the reform, while the zero average effect on total credit reflects opposite firm-level responses across invoice and non-invoice firms. The positive total-credit effect for invoice firms is also consistent with the idea that improved invoice records support lenders' broader credit risk assessment, but only when invoices are especially informative for screening and monitoring. The negative effect for non-invoice firms suggests the presence of a capacity-constraint channel in the use of information, whereby better information availability for some firms leads to resources being reallocated to those firms, at the cost of fewer resources being allocated to firms that do not experience such an increase in information availability.

These estimates, however, do not tell us whether the increase in invoice credit reflects larger amounts among existing credit relationships or the creation of new invoice-based credit relationships. We study these margins in the next subsection.

4.4 Intensive vs Extensive Margin

We now investigate whether eInvoicing changes the amount of credit granted to firms through an increase in credit of pre-existing lending relationships of a given type, akin to an intensive effect, or through an increase in the probability of originating new relationships of a given loan type (invoice-based or non-invoice-based), akin to an extensive effect. Because both questions concern firms' overall use of different financing instruments across lenders, we conduct this analysis at the firm level.

We begin with the intensive margin. We re-estimate our baseline firm-level specification as in Table 3, Panel B, on a restricted subsample, using total, invoice, and non-invoice credit as dependent variables, constructed by aggregating at the firm level, all firm-bank relationships with positive pre-reform credit of the corresponding type. To illustrate, consider the following example: If a firm used invoice credit with banks A and C before the reform, but not with bank B, the intensive-margin specification tracks post-reform invoice credit granted through the pre-existing invoice-based lending relationship with banks A and C, aggregated at the firm level, while disregarding invoice credit that might be granted by bank B after the reform but that did not previously involve invoice-based financing. In this sense, the intensive-margin estimates capture whether the reform changes credit amounts within pre-existing relationships of a given financing type.

Panel A of Table 5 shows that, in the full sample, the intensive-margin response is concentrated in invoice credit. Once we split the sample into invoice and non-invoice firms, the pattern becomes clearer. For invoice firms, invoice credit rises by 5.63% among relationships that were already using invoice credit before the reform. Total credit and non-invoice credit also increase, by 1.75% and 1.15%, respectively. By contrast, for non-invoice firms, total credit falls by -1.18%, and non-invoice credit falls by -1.14%.[32] These estimates indicate that the credit response due to eInvoicing operates in part through changes in credit amounts within pre-existing relationships.

We then turn to the extensive margin. The question is whether eInvoicing affects the probability that a firm originates a new relationship of a given loan type. Because the formation of new relationships is generally infrequent and lumpy, we aggregate the data

32. By construction, it is not possible to estimate the intensive-margin response for invoice credit for non-invoice firms.

at the firm-quarter level and estimate:

$$Y_{f,q} = \alpha_{i,s,q} + \alpha_{p,i,s} + \beta \cdot Post_q \times Treat_p + \Gamma_f \cdot X_{f,q-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,q-1} + \Gamma_p \cdot X_{p,i,q} + \varepsilon_{f,q}, \quad (2)$$

where $Y_{f,q}$ is an indicator equal to one if firm f originates a new total, invoice, or non-invoice loan in quarter q with any bank with which the firm did not have that type of loan relationship before. Returning to the invoice-credit example, the extensive margin captures whether a firm is more likely, after the reform, to originate a new invoice loan with a bank that was not previously extending invoice credit to that firm. The industry×size×quarter fixed effects control for time-varying credit demand factors common to firms of similar size in the same industry, while the province×industry×size fixed effects absorb persistent local differences in the propensity to originate new loans. In addition, we include the same controls as in our baseline firm-level specification.

Panel B of Table 5 reports the results. We find that invoice firms are more likely to establish a new invoice relationship, at the 10% significance level. By contrast, there is no meaningful extensive-margin effect for non-invoice firms.

Taken together, these results suggest that the increase in invoice credit for invoice firms reflects both an intensive-margin effect, through larger credit amounts within pre-existing invoice relationships, and an extensive-margin effect, through a greater likelihood of originating new invoice-based relationships. For non-invoice firms, the decline in credit appears to operate mainly through the intensive margin rather than through a reduction in the origination of new relationships.

Table 5. eInvoicing and Credit Supply: Intensive and Extensive Margins

This table reports estimates of the effect of eInvoicing on intensive and extensive credit margins over the main sample from January 2021 to December 2024. Panel A reports intensive-margin estimates, where the dependent variables are credit amounts conditional on the firm having positive pre-reform credit of the corresponding type, denoted by $y^{pre} > 0$. Panel A is estimated using Poisson Pseudo-Maximum Likelihood at the firm-month level:

$$\mathbb{E}[Y_{f,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_f + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

Panel B reports extensive-margin estimates, where the dependent variables are indicators for new credit of the corresponding type, aggregated at the firm-quarter level. Panel B is estimated using OLS:

$$Y_{f,q} = \alpha_{i,s,q} + \alpha_{p,i,s} + \beta \cdot Post_q \times Treat_p + \Gamma_f \cdot X_{f,q-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,q-1} + \Gamma_p \cdot X_{p,i,q} + \varepsilon_{f,q}.$$

Columns 1–3 report estimates for the full sample, columns 4–6 for invoice firms, and columns 7–9 for non-invoice firms. Within each group, the dependent variables are total credit, invoice-based credit, and non-invoice credit, respectively. Column 8 in Panel A is not estimated because non-invoice firms do not have positive pre-reform invoice credit by construction. *Post* is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. In Panel A, $X_{f,t-1}$ denotes previous-year firm controls, which include firm size, leverage, liquidity, tangibility, profitability, age, the share of the firm's total credit guaranteed by ICO, and an indicator for whether the firm is in default with any bank. Average bank controls are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. Covid controls are denoted by $X_{p,i,t}$ and include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. Panel B includes the corresponding quarterly controls. All variables are defined in Table IA.I. Standard errors are reported in parentheses and are clustered at the province×month level in Panel A and at the province×quarter level in Panel B. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Full Sample			**Invoice Firms**			**Non-Invoice Firms**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Intensive margin, conditional on $y^{pre} > 0$									
Dependent variable	Total	Invoice	Non-Invoice	Total	Invoice	Non-Invoice	Total	Invoice	Non-Invoice
Post×Treated	0.0023	0.0548***	-0.0010	0.0173***	0.0548***	0.0114**	-0.0119***	–	-0.0115***
	(0.0027)	(0.0130)	(0.0033)	(0.0034)	(0.0130)	(0.0047)	(0.0035)	–	(0.0035)
Industry×Size×Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes
Covid Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes
Avg. Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	–	Yes
Obs.	966,583	156,330	964,611	230,819	153,328	228,820	734,631	–	734,619
Pseudo R-Squared	0.99	0.93	0.98	0.98	0.93	0.98	0.98	–	0.98
Panel B: Extensive margin									
Dependent variable	New Total	New Invoice	New Non-Invoice	New Total	New Invoice	New Non-Invoice	New Total	New Invoice	New Non-Invoice
Post×Treated	0.0192	0.0754	0.0070	0.2609	0.7002*	0.0632	-0.0139	-0.0441	0.0001
	(0.1203)	(0.0998)	(0.0960)	(0.3361)	(0.3954)	(0.2487)	(0.1140)	(0.0370)	(0.1137)
Industry×Size×Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province×Industry×Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Avg. Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	321,981	321,981	321,981	76,869	76,869	76,869	244,730	244,730	244,730
R-Squared	0.02	0.03	0.02	0.04	0.04	0.04	0.03	0.03	0.03

4.5 Additional Results on Credit

This section presents additional evidence that helps interpret the credit results and assesses several alternative explanations. We first distinguish between the voluntary and mandatory phases of TicketBAI to study how the credit effects evolve over the course of the reform implementation. We then show that the credit effects documented above are not confined to bank relationships that already used invoice-based financing before the reform, but instead appear to operate more broadly at the firm level. We then address the concern that our results reflect newly reported sales as a consequence of improved tax compliance rather than better informational properties of invoices. Finally, we show that the main findings are unchanged when we employ a within-province analysis. Additional robustness checks are reported in the internet appendix.

Voluntary vs Mandatory Periods. In our baseline specification, we treat the start of the voluntary adoption phase as the beginning of the treatment period, both because we do not observe firm-level compliance directly and because doing so is the more conservative choice. To better understand the timing of the reform's effects, we now estimate a specification that distinguishes between the voluntary and mandatory adoption phases. Because this exercise exploits province-specific treatment timing in a staggered fashion, it also allows us to include Gipuzkoa, which we excluded from the baseline analysis because its voluntary phase begins with the start of our sample.

Table 6 reports the results. For invoice firms, the effects already begin to emerge during the voluntary phase: the coefficients on invoice credit, non-invoice credit, and total credit are generally positive relative to the control group. The mandatory phase then adds a further positive and statistically significant effect across all three credit outcomes, at both the bank-firm and firm levels. For non-invoice firms, by contrast, the voluntary phase has no meaningful effect, whereas the mandatory phase is associated with a significant decline in total credit.

Overall, these results suggest that the credit effects of eInvoicing start to materialize during the voluntary phase but become stronger once adoption becomes mandatory, with the negative effects for non-invoice firms appearing only in the latter period. The fact that some effects are already visible during the voluntary phase is also consistent with our baseline assumption that treatment starts with the voluntary period. More generally, the strengthening of the effects as the reform moves from the voluntary to the mandatory phase, that is, as eInvoicing becomes more widely implemented, makes it less likely that other province-specific developments are driving the differential post-reform credit dynamics across treated and control groups.

Within-Firm Analysis. A natural question is whether the increase in invoice credit

Table 6. eInvoicing and Credit Supply: Voluntary vs. Mandatory Periods

This table reports estimates from staggered difference-in-differences regressions over the main sample, including firms headquartered in Gipuzkoa, from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. In Panel A, the unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta_V \cdot \textit{Treated(Voluntary)}_{p,t} + \beta_M \cdot \textit{Treated(Mandatory)}_{p,t} + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel B, the unit of observation is a firm-month, credit outcomes are aggregated across all banks lending to firm f in month t, and we estimate:

$$\mathbb{E}[Y_{f,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_f + \beta_V \cdot \textit{Treated(Voluntary)}_{p,t} + \beta_M \cdot \textit{Treated(Mandatory)}_{p,t} + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel A, $Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. In Panel B, $Y_{f,t}$ is the corresponding firm-level amount. The dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3 for invoice firms; for non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. $\textit{Treated(Voluntary)}_{p,t}$ is an indicator equal to one for treated provinces once firms in province p enter the voluntary eInvoicing regime. $\textit{Treated(Mandatory)}_{p,t}$ is an additional indicator equal to one for treated provinces once eInvoicing becomes mandatory in province p. Therefore, β_V measures the effect of entering the voluntary regime relative to the pre-reform period and relative to the control group, while β_M measures the incremental effect of the mandatory regime relative to the voluntary regime and relative to the control group. Panel A includes industry×size×month, firm×bank, and bank×month fixed effects. Panel B includes industry×size×month and firm fixed effects. In both panels, $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. In Panel A, this vector also includes bank-firm-level controls: the share of bank-firm credit guaranteed by ICO and an indicator for whether the firm is in default with that bank. In Panel B, these two variables are replaced by their firm-level counterparts: the share of the firm's total credit guaranteed by ICO and an indicator for whether the firm is in default with any bank. Average bank controls in Panel B are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. In both panels, $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Panel A: Bank-firm level				
Treated(Voluntary)	0.0274***	0.0089***	0.0094***	0.0018
	(0.0073)	(0.0028)	(0.0024)	(0.0021)
Treated(Mandatory)	0.0404***	0.0056**	0.0072***	-0.0120***
	(0.0061)	(0.0027)	(0.0025)	(0.0021)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	625,655	889,992	922,159	1,421,469
Pseudo R-Squared	0.75	0.93	0.93	0.97
Panel B: Firm level				
Treated(Voluntary)	0.0038	0.0112***	0.0090***	-0.0009
	(0.0080)	(0.0022)	(0.0022)	(0.0026)
Treated(Mandatory)	0.0327***	0.0043**	0.0069***	-0.0160***
	(0.0064)	(0.0019)	(0.0020)	(0.0028)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Avg. Bank Controls	Yes	Yes	Yes	Yes
Obs.	288,149	289,704	289,704	911,105
Pseudo R-Squared	0.88	0.98	0.98	0.98

documented above reflects a shift toward specific lending relationships within the same firm, rather than an effect operating at the firm level. To address this, we estimate the same specification as in Equation (1) including firm×month fixed effects. This is a within-firm specification that compares, for the same firm in the same month, bank relationships that were already associated with invoice use in the pre-reform period ("invoice relationships") with those that were not. Since we require at least one credit relationship with invoice-based lending, this analysis focuses only on invoice firms.

Table 7 shows no differential post-reform increase in either total credit or non-invoice credit for pre-existing invoice relationships relative to other relationships of the same firm. This suggests that the effects we document in our main analysis are not driven by relationships that were already used for invoice-based financing before the reform. Rather, they appear to reflect a broader firm-level effect, not one confined to a particular subset of lending relationships.

Table 7. eInvoicing and Credit Supply: Within-Firm Analysis

This table reports estimates of β from regressions over the sample of invoice firms from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{f,t} + \alpha_{f,b} + \alpha_{b,t} + \gamma \cdot Post_t \times Invoice\ Relationship_{f,b} + \beta \cdot Post_t \times Treat_p \times Invoice\ Relationship_{f,b}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. The dependent variable is total credit in column 1 and non-invoice credit in column 2. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. $Invoice\ Relationship_{f,b}$ is an indicator equal to one for bank-firm relationships with positive invoice-based credit in the pre-reform period. The specification includes firm×month, firm×bank, and bank×month fixed effects. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms	
	Total	Non-Invoice
	(1)	(2)
Post×Treated×Invoice Relationship	0.0013	-0.0063
	(0.0067)	(0.0061)
Firm×Month FE	Yes	Yes
Firm×Bank FE	Yes	Yes
Bank×Month FE	Yes	Yes
Other Interaction Term	Yes	Yes
Obs.	651,711	627,608
Pseudo R-Squared	0.95	0.95

Within-Province Analysis. One concern is that the main estimates may be driven by province-level shocks that differentially affect treated and control provinces over time. To address this, we estimate a triple-difference specification that augments the baseline with an interaction distinguishing invoice from non-invoice firms. This design allows us to include province×industry×size×month fixed effects, which absorb shocks common to firms within the same province, industry, size class, and month. This controls not only for aggregate macroeconomic and industry-size-time variation, but also for time-varying province-specific factors such as regional economic conditions, COVID-19-related policies, local labor-market dynamics, and other fiscal or regulatory changes that may affect all firms in a given local cell. Because the $Post_t \times Treat_p$ interaction varies only at the province-month level, it is absorbed by this fixed-effects structure. Identification thus comes only from whether invoice firms respond differently from non-invoice firms within the same province×industry×size×month cell. The coefficient on $Post_t \times Treat_p \times Invoice\ firm_f$ is thus identified under the weaker assumption that, absent the reform, invoice and non-invoice firms in treated provinces would have evolved similarly relative to their counterparts in control provinces, conditional on this saturated set of local-sectoral-time controls. Table 8 shows that this coefficient remains positive and statistically significant at both the bank-firm and firm level, indicating that the relative expansion in credit for invoice firms is not explained by province-level time-varying shocks shared by all firms in the same local economic environment.

Tax Compliance and Reporting Incentives. One concern is that our estimates may reflect improved tax compliance rather than better informational properties of invoices. Since eInvoicing is designed precisely to reduce underreporting and strengthen tax enforcement, previously hidden sales may emerge after the reform. This would mechanically increase the stock of pledgeable receivables and, in turn, invoice-based borrowing. There is evidence that eInvoicing adoption can indeed improve tax compliance (e.g., Bellon et al., 2022). At the same time, this mechanism is not an obvious explanation for our credit results, because banks care about borrowers' true repayment capacity, not only about what is formally reported to the tax authority (Artavanis et al., 2016).

In our setting, however, there are no reliable granular firm- or industry-level measures of tax evasion that would allow us to test this channel directly. We therefore examine whether

Table 8. eInvoicing and Credit Supply: Within-Province Analysis

This table reports estimates from triple-difference regressions over the main sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The regressions compare the effect of eInvoicing on invoice firms relative to non-invoice firms. In Panel A, the unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{\mathcal{F}} + \gamma \cdot Post_t \times Invoice\ Firm_f + \beta_1 \cdot Post_t \times Treat_p + \beta_2 \cdot Post_t \times Treat_p \times Invoice\ Firm_f + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel B, the unit of observation is a firm-month, credit outcomes are aggregated across all banks lending to firm f in month t, and we estimate:

$$\begin{aligned}\mathbb{E}[Y_{f,t} \mid \cdot] = \ &\exp\{\alpha_{\mathcal{F}} + \gamma \cdot Post_t \times Invoice\ Firm_f + \beta_1 \cdot Post_t \times Treat_p \\ &+ \beta_2 \cdot Post_t \times Treat_p \times Invoice\ Firm_f + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t}\}.\end{aligned}$$

In Panel A, $Y_{f,b,t}$ is the total credit extended by bank b to firm f in month t. In Panel B, $Y_{f,t}$ is firm-level total credit. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. $Invoice\ Firm_f$ is an indicator equal to one for firms with positive invoice-based credit in the pre-reform period. The coefficient β_2 estimates whether the effect of eInvoicing differs between invoice and non-invoice firms, comparing treated and control provinces before and after the reform. The term $\alpha_{\mathcal{F}}$ denotes the fixed effects included in each specification. Column 1 includes industry×size×month fixed effects, while column 2 includes province×industry×size×month fixed effects. Panel A additionally includes firm×bank and bank×month fixed effects; Panel B additionally includes firm fixed effects. In both panels, $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. In Panel A, this vector also includes bank-firm-level controls: the share of bank-firm credit guaranteed by ICO and an indicator for whether the firm is in default with that bank. In Panel B, these two variables are replaced by their firm-level counterparts: the share of the firm's total credit guaranteed by ICO and an indicator for whether the firm is in default with any bank. Average bank controls in Panel B are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. In both panels, $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. In column 2, Covid controls are absorbed by province×industry×size×month fixed effects. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Total	
	(1)	(2)
Panel A: Bank-firm level		
Post×Treated	-0.0124***	
	(0.0027)	
Post×Treated×Invoice Firm	0.0250***	0.0264***
	(0.0045)	(0.0062)
Industry×Size×Month FE	Yes	No
Province×Industry×Size×Month FE	No	Yes
Firm×Bank + Bank×Month FEs	Yes	Yes
Firm + Covid Controls	Yes	Yes
Other Interaction Term	Yes	Yes
Obs.	1,899,713	1,896,474
Pseudo R-Squared	0.95	0.96
Panel B: Firm level		
Post×Treated	-0.0108***	
	(0.0022)	
Post×Treated×Invoice Firm	0.0190***	0.0094**
	(0.0036)	(0.0045)
Industry×Size×Month FE	Yes	No
Province×Industry×Size×Month FE	No	Yes
Firm FE	Yes	Yes
Firm + Covid + Avg. Bank Controls	Yes	Yes
Other Interaction Term	Yes	Yes
Obs.	971,356	964,821
Pseudo R-Squared	0.98	0.99

eInvoicing adoption induced higher reported firm sales in treated provinces relative to the control group. The evidence in Figure 3, where we re-estimate our main specification at the firm level using log revenues as the dependent variable, shows that this is not the case. In addition, in our triple-difference specification, we account for province×industry×size×time fixed effects (see Table 8). To the extent that firms of similar size, in the same province, in the same industry, and in the same month have similar scope for underreporting, these fixed effects firmly attenuate concerns that differential changes in tax evasion drive our results. Overall, increased tax compliance does not appear to be the main driver of our results.

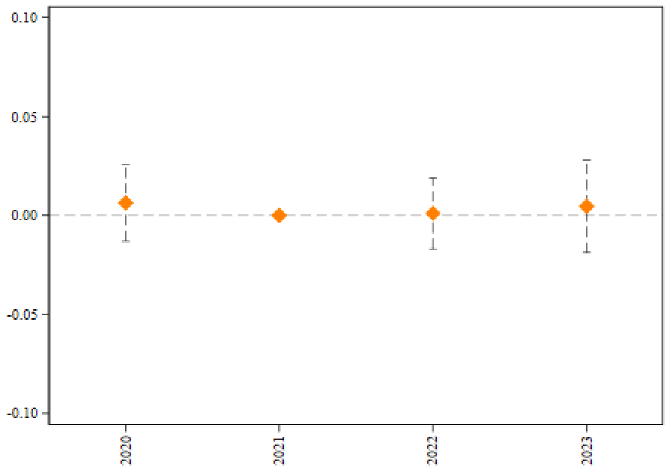


Figure 3. DiD Coefficients of log Revenues.

This figure plots the estimates of β_ℓ from a dynamic difference-in-differences specification estimated for the average firm over our main sample from 2020 to 2023, using OLS.

$$Y_{f,t} = \alpha_{i,s,t} + \alpha_f + \sum_{\ell \neq -1} \beta_\ell \cdot \mathbf{1}\{t - T_p = \ell\} \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \varepsilon_{f,t}.$$

$Y_{f,t}$ is the logarithm of revenues of firm f in year t. $\mathbf{1}\{t - T_p = \ell\}$ is an indicator equal to one when year t is ℓ periods away from the treatment year T_p of province p, with $\ell = -1$ omitted as the reference period. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×year fixed effects ($\alpha_{i,s,t}$) and firm fixed effects (α_f). $X_{f,t-1}$ denotes the vector of lagged firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. All variables are defined in Table IA.I. Confidence intervals are at the 95% level.

In Appendix B, we report a broader set of additional analyses and robustness tests that complement the evidence discussed above. These include repeating the analysis on alternative subsamples of firms (Appendix B.1 and B.2), ensuring that our results are not driven by one specific province or by the exclusion of Gipuzkoa (Appendix B.3), nor by bank-specific sector shifts (Appendix B.4), nor by the VAT-compliance system known as the *Suministro Inmediato de Información* (Appendix B.5). We also discuss potentially alternative clustering schemes for standard errors (Appendix B.6), and report additional heterogeneity analyses (Appendix B.7). Overall, these results show that the credit-market effects of eInvoicing are robust across alternative samples, specifications, and estimation choices.

4.6 Interest Rates

The evidence presented so far demonstrates that the introduction of eInvoicing has significant and heterogeneous effects on credit quantities, but these results do not, by themselves, allow us to distinguish between supply-side and demand-side explanations, as they reflect equilibrium outcomes. The observed changes in invoice credit, non-invoice credit, and total credit following the introduction of TicketBAI could, in principle, reflect shifts in firms' demand for different forms of borrowing due to the implementation, or also changes in lenders' willingness to provide them. Analyzing the joint effects of the introduction on prices (loan rates) and quantities helps to distinguish between these two interpretations. For invoice and non-invoice credit, if changes in credit supply drive the observed increase (decrease) in quantities, treated firms should obtain more (less) credit at lower (higher) interest rates. By contrast, if changes in credit demand are the primary drivers, they would move both prices and quantities in the same direction. It should be noted that for the total cost of credit, the empirical predictions are less straightforward as observed changes need not only reflect shifts in overall credit supply and demand, but may also arise from compositional effects, already documented in our previous results, driven by the relative prices and quantities of invoice and non-invoice credit.

We begin by examining the effects of eInvoicing on the cost of invoice and non-invoice credit. This helps assess whether the quantity effects are consistent with a pure demand explanation or whether the evidence suggests that changes in credit supply also play a significant role. We then examine the overall cost of credit. Formally, we estimate the same specification as in Equation (1) using OLS at the bank-firm-month level from January

2021 to December 2024, replacing the dependent variable with the logarithm of the cost of credit, measured as the credit-weighted average interest rate on outstanding credit:

$$\log\left(R_{f,b,t}\right) = \alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,b,t-1} + \Gamma_p \cdot X_{p,i,t} + \varepsilon_{f,b,t} \quad (3)$$

where the subscripts follow the same notation as in Equation (1), and the dependent variable $\log(R_{f,b,t})$ denotes the logarithm of the interest rate charged by bank b to firm f in month t, computed separately for total credit, invoice credit, and non-invoice credit. As before, β captures the differential change in borrowing costs for firms in the treated provinces after the introduction of eInvoicing relative to firms in control provinces. The fixed effects and control variables are the same as in the baseline credit specification. Standard errors are clustered at the province$\times$month level.

Table 9 reports the effects of eInvoicing on borrowing costs. We begin with invoice and non-invoice credit, since these prices are the most informative for interpreting the quantity results. For invoice firms in treated provinces, the cost of invoice credit declines by approximately 2%, while the cost of non-invoice credit rises by approximately 3.3% relative to invoice firms in control provinces. For non-invoice firms, the cost of total credit also rises by approximately 3.2%. The firm-level results show the same pattern.

Taken together with the quantity results, suggests that a supply story is at play. In equilibrium, the quantity of invoice credit increases while its price decreases. If treated firms simply demanded relatively more invoice credit after the reform, we would instead expect both quantities and prices to rise. At the same time, non-invoice credit becomes more expensive even though its quantity either remains flat or declines relative to the control group, which is again inconsistent with a pure demand-driven explanation. As already discussed, the results for the total cost of credit are less direct to interpret, because total borrowing costs reflect the composition of borrowing across invoice and non-invoice instruments, which our previous credit quantity results show have been altered. In the data, the cost of total credit rises in treated provinces relative to the control group, both for invoice and non-invoice firms. This does not contradict a supply-driven interpretation of the quantity results. Invoice credit is normally more expensive on average than other

Table 9. eInvoicing and Interest Rates

This table reports estimates of β from interest-rate regressions estimated separately over the subsamples of invoice firms and non-invoice firms from January 2021 to December 2024, using OLS. In Panel A, the unit of observation is a bank-firm-month, and we estimate:

$$\log(R_{f,b,t}) = \alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t} + \varepsilon_{f,b,t}.$$

In Panel B, the unit of observation is a firm-month, interest rates are aggregated across all banks lending to firm f in month t using credit-volume weights, and we estimate:

$$\log(R_{f,t}) = \alpha_{i,s,t} + \alpha_f + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_{\bar{b}} \cdot \bar{X}_{b,f,t-1} + \Gamma_p \cdot X_{p,i,t} + \varepsilon_{f,t}.$$

In Panel A, $R_{f,b,t}$ is the credit-weighted average interest rate charged by bank b to firm f in month t, with the loan amounts as weights. In Panel B, $R_{f,t}$ is the corresponding credit-weighted firm-level interest rate. For invoice firms, the dependent variable is the logarithm of the interest rate on invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is the logarithm of the interest rate on total credit in column 4. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. Panel A includes industry×size×month, firm×bank, and bank×month fixed effects. Panel B includes industry×size×month and firm fixed effects. In both panels, $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, and age. In Panel A, this vector also includes bank-firm-level controls: the share of bank-firm credit guaranteed by ICO and an indicator for whether the firm is in default with that bank. In Panel B, these two variables are replaced by their firm-level counterparts: the share of the firm's total credit guaranteed by ICO and an indicator for whether the firm is in default with any bank. Average bank controls in Panel B are denoted by $\bar{X}_{b,f,t-1}$ and are computed as firm-level weighted averages, with weights based on each bank's share of the firm's credit. These controls include bank profitability, liquidity, size, non-performing loan ratio, and capitalization. In both panels, $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Panel A: Bank-firm level				
Post×Treated	-0.0198***	0.0328***	0.0318***	0.0323***
	(0.0056)	(0.0067)	(0.0063)	(0.0056)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	98,145	631,896	650,142	923,898
R-Squared	0.88	0.83	0.83	0.85
Panel B: Firm level				
Post×Treated	-0.0263***	0.0961***	0.0892***	0.0830***
	(0.0054)	(0.0091)	(0.0087)	(0.0078)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Avg. Bank Controls	Yes	Yes	Yes	Yes
Obs.	69,443	220,092	221,966	604,039
R-Squared	0.85	0.84	0.84	0.84

forms of borrowing, so a shift in the composition of credit toward invoice-based instruments can raise the average cost of total credit even if the price of invoice credit itself decreases in relative terms.[33] Overall, the joint behavior of prices and quantities suggests that eInvoicing changes the equilibrium allocation of credit not only through firms' demand, but also through lenders' supply decisions. The next section analyzes more directly whether these supply-side effects operate through changes in lenders' information production.

5 eInvoicing and Lenders' Information Production

The evidence in Section 4 shows that eInvoicing reshapes credit allocation: invoice firms in treated provinces obtain more total and invoice-based credit and face a lower cost of invoice-based borrowing relative to comparable firms in control provinces. One plausible interpretation of these patterns is that the reform changes the information available to lenders by making invoice records more standardized, verifiable, and usable for lending purposes. If eInvoicing improves the informational content of invoices in ways relevant to credit risk assessment, banks should be able to differentiate more effectively among borrowers and allocate credit accordingly. In this section, we test this hypothesis by analyzing the effects of eInvoicing on two complementary measures of banks' information production: the cross-sectional dispersion of interest rates and banks' internal risk assessments, and the accuracy of these internal risk assessments in predicting ex-post defaults.

5.1 Dispersion

We first focus on the cross-sectional dispersion of both loan rates and banks' internal PDs. When banks rely on a coarse information set, borrowers who are observably similar tend to receive similar rates and risk assessments, thereby compressing the cross-section. However, when lenders can instead use richer, more informative signals, differences among borrowers become more salient, and prices and PDs can vary more widely across otherwise

33. The average interest rate is 4.43% for invoice credit and 3.70% for non-invoice credit, while the average interest rate at the firm level is 3.71%. See Appendix A.3.

similar firms (Cornell and Welch, 1996). If eInvoicing improves the information available to lenders, banks should be able to tailor rates and internal risk assessments more closely to borrower-specific signals, thereby increasing cross-sectional dispersion (Skrastins and Vig, 2019; Cerqueiro et al., 2011; Doerr et al., 2023).

5.1.1 Methodology

To test this hypothesis, we study how the dispersion of loan rates and internal risk assessments changes across treated and control provinces over time. Formally, we estimate the following specification at the province-quarter level:

$$Log\left(y_{p,t}\right) = \beta \cdot Post_t \times Treat_p + \alpha_p + \alpha_t + \Gamma \cdot X_{p,t} + \varepsilon_{p,t} \tag{4}$$

where p denotes province and t the year-quarter, and $Log(y_{p,t})$ is the log of the interquartile range or the standard deviation of either loan interest rates or banks' internal PDs across all credit relationships in a given province-quarter.[34] The coefficient of interest, β, captures whether cross-sectional dispersion changes differentially in treated provinces relative to the control group after the reform. The vector $X_{p,t}$ includes province-quarter controls meant to absorb differences in local credit-market conditions, borrower composition, and the intensity of COVID-19-related disruptions.[35] Because the informational content of eInvoicing is likely most relevant for firms that already relied on invoice-based credit before the reform, we estimate Equation (4) separately for invoice and non-invoice firms.

We then pool the two groups and estimate a specification that allows the treatment effect to differ across firm types while absorbing province×firm-type and firm-type×quarter

34. We conduct this analysis at the province-quarter level because PDs are reported only quarterly. Specifically, we compute measures of dispersion for interest rates by considering only new loans and averaging the rates at the bank-firm-quarter level, using loan amounts as weights; for the dispersion of PDs, we instead consider all outstanding bank-firm relationships, as PDs are updated quarterly. See Appendix C.2 for more details.
35. Specifically, the province-quarter controls include: Log(total credit), the number of firms, the corporate credit-market HHI, the share of ICO-guaranteed credit, the shares of credit to the main sectors of economic activity – Agriculture, Mining, Manufacturing, Construction, Retail and Wholesale, Transport, and Hospitality – as well as the Google Mobility Index and the total number of workers covered by the ERTE program. All province-quarter controls are constructed within the estimation sample used in each analysis to compute the dispersion measures.

fixed effects:

$$
\begin{aligned}
\log(y_{p,g,t}) = {} & \beta_1 \cdot Post_t \times Treat_p + \beta_2 \cdot Post_t \times Treat_p \times Invoice\ Firms_g \\
& + \alpha_{p,g} + \alpha_{g,t} + \Gamma_1 \cdot X_{p,t} + \Gamma_2 \cdot X_{p,t} \times Invoice\ Firms_g + \varepsilon_{p,g,t}
\end{aligned}
\tag{5}
$$

where p province, g indexes firm type, and t year-quarter. $X_{p,t}$ is the same vector of controls as in Equation (4). This specification allows us to test directly whether the change in dispersion after the reform differs between invoice and non-invoice firms, after accounting for persistent differences across provinces within each firm type and time-specific shocks common to a given firm type across all provinces. The interaction between $X_{p,t}$ and *Invoice Firms*$_g$ allows the control variables to have different coefficients across invoice and non-invoice groups, making the estimates of Equation (5) comparable to the ones of Equation (4).

5.1.2 Results

Table 10 reports the results of estimating Equation (4) and Equation (5). The estimates reveal marked heterogeneity across firm types, consistent with the credit allocation results documented in Section 4. Across both panels and both dispersion measures, the signs of the estimated coefficients point to the same pattern: dispersion widens for invoice firms in treated provinces relative to control provinces after the reform, while it narrows for non-invoice firms. This is consistent with lenders differentiating more across invoice firms and less across non-invoice firms after the introduction of eInvoicing. Because the analysis is conducted at the province×quarter level, however, the effective sample size is limited, and statistical significance is not achieved uniformly across specifications, especially when dispersion is measured by the standard deviation, which is more sensitive to outliers.[36]

Panel A reports the results when using the dispersion of loan interest rates as the dependent variable. Under the interquartile-range measure (columns (1)–(3)), the coefficient in column (1) implies that the dispersion of interest rates increases by $100 \times (\exp 0.29 - 1) = 34\%$ for invoice firms in treated provinces after the reform relative to the control group, whereas the coefficient in column (2) implies a decline of $100 \times (\exp -0.19 - 1) = -17\%$

36. Results are similar without taking the log of the dependent variables. See Table IA.XII in Appendix C.1.

for non-invoice firms.[37] The triple interaction in column (3) confirms that the difference between the two groups is both economically and statistically significant. The standard-deviation specifications (columns (4)–(6)) deliver the same qualitative picture, although with more noise: the coefficient in column (4) implies a small increase of 2% for invoice firms, the coefficient in column (5) implies a decline of 17% for non-invoice firms, and the triple interaction in column (6) confirms that the difference across the two groups remains statistically significant at the 10% level.

Panel B repeats the exercise using the dispersion of banks' internal PDs. Under the interquartile-range measure (columns (1)–(3)), the coefficient in column (1) implies that PD dispersion increases by 15% for invoice firms in treated provinces after the reform relative to invoice firms in control provinces. By contrast, the coefficient in column (2) implies a decline of 13% for non-invoice firms relative to the control group. The triple interaction in column (3) again confirms that the difference between the two groups is large and statistically significant at the 1% level. The standard-deviation specifications (columns (4)–(6)) preserve the same sign pattern, although with more noise: the coefficient in column (4) implies a small increase of 3% for invoice firms, the coefficient in column (5) implies a decline of 15% for non-invoice firms, and the triple interaction in column (6) confirms that the difference across the two groups remains statistically significant at the 5% level.

Taken together, these results suggest that eInvoicing changes how banks use information to price and assess borrowers, and that this effect differs sharply across firm types. For invoice firms, lenders appear to assign more borrower-specific prices and internal risk assessments after the reform, widening the cross-sectional dispersion of both interest rates and PDs. For non-invoice firms, by contrast, the reform appears to compress the dispersion, consistent with a more uniform treatment of borrowers whose invoice records are less directly relevant for lending decisions. Overall, these results are consistent with eInvoicing affecting how banks use information in pricing and risk assessment, especially for firms whose financing is more closely tied to invoices.

37. Throughout this section, we report coefficient estimates that have already been transformed into percentage effects, computed as $100 \times \left(\exp \hat{\beta} - 1\right)$.

Table 10. Dispersion of Interest Rates and Probabilities of Default

This table reports estimates of the effect of eInvoicing on the dispersion of interest rates and banks' probability-of-default (PD) estimates at the province-quarter level. Panel A uses the log dispersion of interest rates as the dependent variable; Panel B uses the log dispersion of banks' PD estimates. Columns 1–3 measure dispersion using the interquartile range, while columns 4–6 measure dispersion using the standard deviation. Columns 1 and 4 restrict the sample to invoice firms; columns 2 and 5 restrict the sample to non-invoice firms; columns 3 and 6 use the full sample and estimate separate effects for invoice and non-invoice firms. For columns 1, 2, 4, and 5, we estimate:

$$\log(y_{p,t}) = \beta \cdot Post_t \times Treat_p + \alpha_p + \alpha_t + \Gamma \cdot X_{p,t} + \varepsilon_{p,t}.$$

For columns 3 and 6, we estimate the corresponding stacked specification:

$$\log(y_{p,g,t}) = \beta_1 \cdot Post_t \times Treat_p + \beta_2 \cdot Post_t \times Treat_p \times Invoice\ Firms_g + \alpha_{p,g} + \alpha_{g,t} + \Gamma_1 \cdot X_{p,t} + \Gamma_2 \cdot X_{p,t} \times Invoice\ Firms_g + \varepsilon_{p,g,t}.$$

In these equations, y is either the interquartile range or the standard deviation of interest rates or PDs within a province-quarter cell. In the stacked specification, g indexes whether the dispersion measure is computed among invoice firms or non-invoice firms, and $Invoice\ Firms_g$ is an indicator equal to one for the invoice-firms group. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for treated provinces. The vector $X_{p,t}$ includes log total credit, the number of firms, the corporate credit-market HHI, the share of ICO-guaranteed credit, the shares of credit to Agriculture, Mining, Manufacturing, Construction, Retail and Wholesale, Transport, and Hospitality, the Google Mobility index, and the total number of workers covered by the ERTE program, all constructed within the sample used to compute the dispersion measures. All variables are defined in Table IA.I. Heteroskedasticity-robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Log(Interquartile Range)			Log(Standard Deviation)		
	Invoice	Non-Invoice	All	Invoice	Non-Invoice	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Interest Rates						
Post×Treated	0.29***	-0.19***	-0.19***	0.02	-0.19**	-0.19**
	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Post×Treated×Invoice Firms			0.48***			0.21*
			(0.10)			(0.12)
Province FE	Yes	Yes	No	Yes	Yes	No
Quarter FE	Yes	Yes	No	Yes	Yes	No
Province×Invoice FE	No	No	Yes	No	No	Yes
Invoice×Quarter FE	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	96	96	192	96	96	192
R-Squared	0.80	0.81	0.84	0.68	0.81	0.89
Panel B: Probabilities of Default						
Post×Treated	0.14***	-0.14**	-0.14**	0.03	-0.16**	-0.16**
	(0.04)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
Post×Treated×Invoice Firms			0.29***			0.20**
			(0.08)			(0.09)
Province FE	Yes	Yes	No	Yes	Yes	No
Quarter FE	Yes	Yes	No	Yes	Yes	No
Province×Invoice FE	No	No	Yes	No	No	Yes
Invoice×Quarter FE	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	96	96	192	96	96	192
R-Squared	0.79	0.75	0.77	0.78	0.65	0.77

5.2 Predictive Accuracy of Probablities of Default

To complement the evidence on dispersion, we next examine the accuracy of banks' internal PDs in predicting ex-post defaults. This is arguably a more direct measure of the quality of banks' information production, as it captures banks' ability to produce internal risk assessments that more accurately predict the realization of credit risk.

5.2.1 Methodology

To capture how well banks' PDs can predict borrowers' credit risk, we employ the Area Under the Receiver Operating Characteristic curve (AUROC) as a measure of the quality of banks' information production (Howes and Weitzner, forthcoming; Beyhaghi et al., 2024). The AUROC quantifies the extent to which banks' PDs discriminate across borrowers with different ex-post default outcomes. Intuitively, it reflects the probability that a defaulting borrower is assigned a higher PD than a non-defaulting borrower. An AUROC of 0.5 indicates no discriminatory power (i.e., predictions are no better than a random coin toss), whereas a value of 1 reflects perfect rank ordering.

For this analysis, we rely on the PDs that banks employing the IRB approach for capital requirements (IRB banks) must report to the Bank of Spain.[38] Hence, these data are available at the bank-firm-quarter level only for IRB banks and the subset of firms borrowing from at least one of these banks.[39] To construct the AUROC, we compute the Receiver Operating Characteristic (ROC) curve by comparing banks' predicted PDs with one-year-ahead realized defaults. For each possible PD threshold, borrowers are classified as defaulting or non-defaulting depending on whether their PD exceeds the threshold. At each threshold, we compute the true positive rate (the share of actual defaulters correctly classified) and the false positive rate (the share of non-defaulters incorrectly classified as defaulters). Plotting the true positive rate against the false positive rate across all possible thresholds yields the ROC curve. The area under this curve summarizes predictive

38. In Appendix C.2, we provide additional information about PDs and validate them as a reliable proxy for predicting default.
39. In Appendix B.1 we show that the main results of Section 4 hold for this restricted sample of firms.

accuracy as a single statistic: higher AUROC values indicate better performance of the PDs in distinguishing future defaulters from non-defaulters.

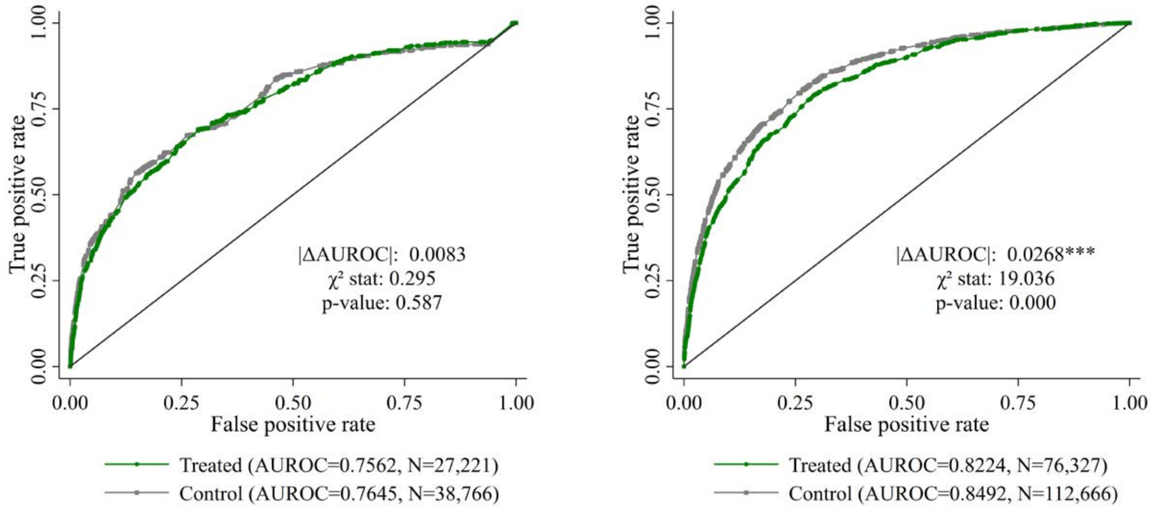
Our objective is to test whether the quality of banks' information production – measured as their ability to rank borrowers by risk – changes differentially in response to the eInvoicing mandate. To this end, we compute the AUROC separately for treated and control firms in the pre-reform and post-reform periods, and compare the resulting values across treated and control provinces before and after the introduction of TicketBAI, in the spirit of a graphical DiD.

5.2.2 Main Results

Figure 4 displays the results of the AUROC-based analysis. Both the left and right panels show two curves, one for treated firms and one for control firms. For each group, the relevant metric is the area under the curve (i.e., the AUROC), and our comparison focuses on how this area differs between treated and control firms before and after the reform. The green curve corresponds to firms located in the treated provinces (Biscay and Álava), while the grey curve corresponds to firms located in the control provinces (Burgos, Cantabria, La Rioja, and Navarre). The left panel restricts the sample to bank-firm-quarter observations in 2021, i.e., the period preceding the introduction of TicketBAI in the provinces of Biscay and Álava, and the right panel focuses on the bank-firm-quarter observations from 2022 to 2024.

Figure 4 shows that the predictive accuracy of banks' internal PDs improved over time in both treated and control provinces. In 2021, before the reform, predictive performance was very similar across the two groups: the difference in AUROC between treatment and control is -0.008 and statistically indistinguishable from zero. In the post-reform period, by contrast, the difference widens to -0.027 and becomes statistically significant at the 1% level, indicating that the improvement in predictive performance was less pronounced for firms in treated provinces. This difference is also economically meaningful, as even a 0.01 improvement in AUROCs *"is considered a noteworthy gain in the credit scoring industry"* (Iyer et al., 2016, p. 1565).

All Firms



(a) 2021 **(b)** 2022-2024

Figure 4. Differences in Predictive Quality of PDs before and after eInvoicing

Notes: This figure presents the AUROC-based analysis of the predictive quality of the probabilities of default (PD) internally estimated for credit to firms located in treated and control provinces. The larger the area under the curve, the higher the accuracy of the estimated PDs.

5.2.3 Invoice vs Non-Invoice Firms

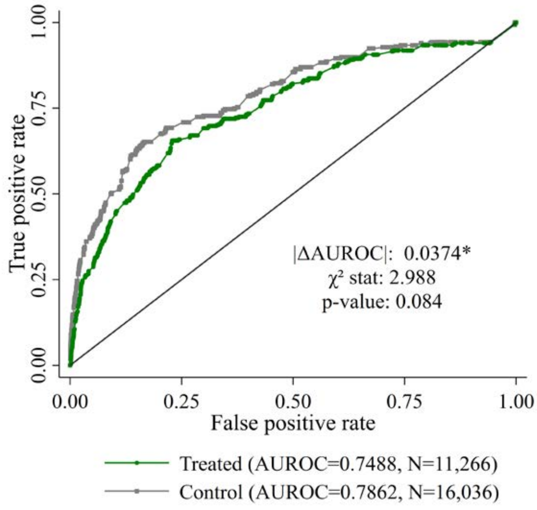
As in the previous sections, the aggregate results in Figure 4 mask important heterogeneity across firms. The informational effects of eInvoicing are unlikely to be uniform, since their relevance to lenders should depend on whether invoice records are actually useful for assessing borrower risk. For firms that use invoice-based credit, the reform may improve lenders' internal risk assessments more directly. For firms that do not use invoice-based credit, by contrast, any informational effect is likely to be more muted and could even be negative if banks reallocate screening and monitoring capacity towards borrowers for whom invoice records are more informative. To examine whether the aggregate pattern in Figure 4 reflects these differences across firms, we split the sample into invoice and non-invoice firms and repeat the AUROC analysis separately for each group, in the spirit of a graphical triple-difference design. Figure 5 reports the results: the top row shows the pre-and post-reform AUROCs for invoice firms in treated and control provinces, while the bottom row reports the corresponding comparison for non-invoice firms.

Consistent with our previous results, two different patterns emerge depending on the firm's type. For invoice firms, the control group exhibits a higher AUROC than the treated group in the pre-reform period, with an economically large and statistically significant gap of 0.037.[40] After the reform, however, this gap shrinks markedly and becomes statistically indistinguishable from zero, implying a substantial relative increase in the information quality of banks for invoice firms. For non-invoice firms, the opposite pattern emerges. Before the reform, treated and control firms display similar AUROCs, whereas in the post-reform period the treated curve shifts below the control curve, generating a difference of -0.052 that is statistically significant at the 1% level. Taken together, these results indicate that the reform is associated with an improvement in banks' ability to predict default for invoice firms, but with a deterioration for non-invoice firms. This result is aligned with the evidence on credit allocation in Table 4: lenders expand credit to invoice firms, for which the information embedded in invoice records appears more relevant for assessing credit risk, while outcomes move in the opposite direction for non-invoice firms.

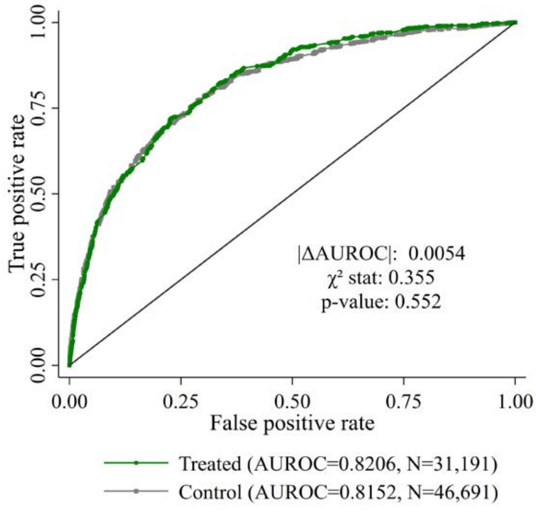
These findings are consistent with frictions in banks' information production. The reform appears to increase the informational value of invoice records for invoice firms, but the corresponding decline in predictive accuracy for non-invoice firms suggests that banks may not be able to expand information production uniformly across all types of credit. One possible interpretation is that banks adjust their information-processing technologies, rating methodologies, or analysts' attention toward the type of lending for which the reform makes information more valuable. If information-processing capacity is limited, such a reallocation can generate negative spillovers for borrowers or credit products for which invoice data are less informative. This interpretation is consistent with a recent literature emphasizing capacity constraints and limited attention in lending decisions (see,

40. This difference may reflect the fact that, in this graphical exercise, we cannot control for differences in firm characteristics between treated and control provinces. In Section 5.2.4, we show that this pre-period gap is substantially reduced when we apply a matching procedure based on firm and bank-firm characteristics.

Invoice Firms

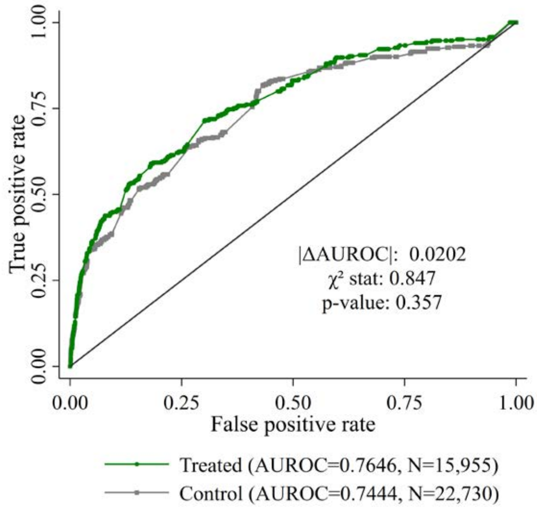


(a) 2021

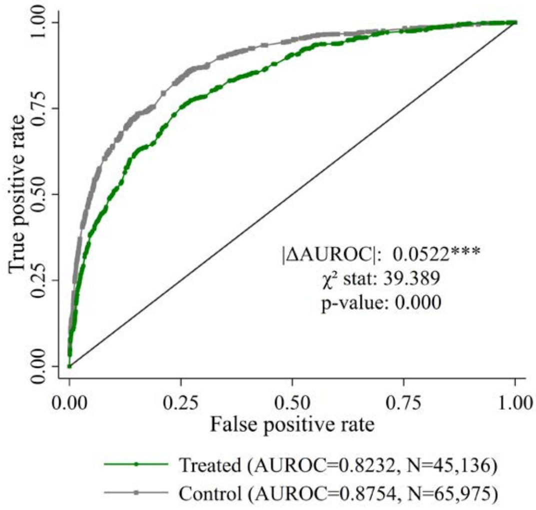


(b) 2022-2024

Non-Invoice Firms



(c) 2021



(d) 2022-2024

Figure 5. Differences in Predictive Quality of PDs before and after eInvoicing, for invoice and non-invoice firms.

Notes: This figure presents the AUROC-based analysis of the predictive quality of the probabilities of default (PD) internally estimated for credit to firms located in treated and control provinces for invoice and non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

e.g. Campbell et al., 2019; Mariathasan and Zhuk, 2022).

5.2.4 Robustness Tests

The intrinsic design of AUROCs raises two potential concerns. First, the AUROC is computed on raw samples, and we cannot control for additional covariates or fixed effects as we would normally do in a regression framework. Thus, cross-sectional differences in borrower composition and the differential evolution of these characteristics over time across treated and control groups could explain our results. Second, the post-reform window overlaps with the tail end of government-guaranteed (ICO) lending issued during the COVID-19 pandemic and other policies to stabilize the economy, including default moratoria. If these programs weakened banks' incentives to produce information, and if their effects differed systematically across treated and control groups, they could affect the measured change in AUROC for reasons unrelated to eInvoicing. To mitigate these concerns, we conduct a battery of robustness checks.

First, we assess whether cross-sectional differences across treated and control firms drive our results. To this end, in Figure IA.III we re-estimate the AUROCs on a matched sample in which treated and control firms are balanced on observables. The matching procedure is implemented in two stages using bank–firm–quarter information from 2021Q4. We first form exact-match cells defined by the firm's industry, size tercile, invoice indicator, and bank identifier, thereby restricting comparisons to highly homogeneous groups of bank–firm pairs. Within each exact-match cell, we then perform nearest-neighbor propensity-score matching based on firm-level financial characteristics, including liquidity, tangibility, and ROA, as well as the bank's IRB PD estimate for the firm. The final matched sample includes only bank–firm–quarter observations for which a match is found in the exact same cell. Table IA.XIV shows that this procedure substantially improves the balance of observables across treated and control firms. Figure IA.III shows that matching reduces pre-existing differences in AUROCs, especially for invoice firms, and strengthens the estimated post-reform effects for this group, without materially affecting the results for non-invoice firms.

Second, to mitigate the concern that our results could reflect differential compositional shifts across treated and control provinces, in Figure IA.IV we re-estimate the AUROCs on the subsample of *stayers*, defined as bank–firm relationships observed in both the pre-period and the post-period, so that the comparison is not driven by entry or exit.

As an additional check, in Figure IA.V and Figure IA.VI, we assess whether the results depend on cohort composition by splitting relationships according to the first observed quarter of each bank–firm relationship. Specifically, we distinguish relationships that begin in 2019 or earlier from those that begin in 2020–2021, ensuring that each cohort contains bank–firm–quarter observations in both the pre- and post-treatment periods.

Finally, to rule out that government-guaranteed lending drives our results, in Figure IA.VII, we re-estimate AUROCs after excluding bank–firm–quarter observations in which all outstanding loans are either ICO-guaranteed or under COVID-19 payment moratoria.

Across all of these checks, the main conclusions of Figure 5 remain unchanged: banks' ability to predict default improves for invoice-firms in treated provinces relative to invoice-firms in control provinces, and deteriorates for treated non-invoice firms relative to control non-invoice firms.

6 Conclusions

Digitalization is reshaping how firms operate, transact, and obtain external finance. While a large literature has examined the digital execution of payments, digital invoicing has received surprisingly low attention from finance scholars. Electronic invoicing changes the properties of these records by making them standardized, traceable, verifiable, and harder to falsify, thereby altering the information set that firms can credibly share with their financiers. With a growing number of jurisdictions — including the European Union — moving toward mandatory eInvoicing as a foundational pillar of their digital strategy, understanding how this reshapes credit markets is a first-order question for both research and policy.

Exploiting the regional rollout of TicketBAI in Spain's Basque Country together with administrative credit-register and balance-sheet data, this paper provides the first systematic evidence that eInvoicing has a meaningful impact on firms' financing. Overall, three main findings stand out. First, eInvoicing led to an expansion of credit for firms that were already using invoice-based credit, driven by both higher invoice-based credit volumes and a higher likelihood of using such instruments. By contrast, firms that did not rely on invoice-based credit experienced a reduction in total credit, with no offsetting increase in the take-up of invoice-related arrangements. Second, these effects are supply-driven, with lenders shifting their credit provision towards firms that were already using invoice-based financing, arguably because banks can assess their credit risk relatively better. Indeed, our third main finding suggests that our results are consistent with an information channel operating through banks' information sets. Specifically, eInvoicing widens the cross-sectional dispersion of interest rates and internal probabilities of default, whereas the predictive accuracy of banks' risk assessments improves for firms already using invoice-based financing relative to the control group. In other words, banks are better able to discriminate and to predict ex-post default of firms using invoice-based credit, and thus redirect credit towards them and away from firms whose invoicing information is less relevant.

These results speak to a broader debate on how digitalization is reshaping financial intermediation. A common thread across recent reforms — from open banking, to consumer data rights, to digital identity and unified payment infrastructures — is that policymakers seek to lower the cost of producing and sharing standardized, verifiable digital records about firms and households, with the explicit goal of unlocking better credit decisions and broader financial inclusion. Our evidence supports the view that such standardization can indeed sharpen lenders' screening and reallocate credit toward more deserving borrowers, but it also delivers a cautionary message: the gains from digitalization in credit markets are not uniform. When new digital records are more informative for some borrowers than for others, lenders' improved ability to discriminate generates winners and losers, and the reform may tighten credit precisely for the firms whose activity is least represented in the new data. From a policy perspective, this suggests that the design of eInvoicing and other open-data mandates should be evaluated not only by their average effects but also by their distributional consequences, and that complementary tools — such as broader-coverage data-sharing frameworks, public credit guarantees, or targeted support for under-represented segments — may be useful to mitigate adverse, unintended reallocations during the transition.

We leave to future work several questions that our setting and sample period do not allow us to address fully, including the long-run effects of eInvoicing on firm investment, growth, and exit; whether better invoice information levels the playing field for new entrants; and the real effects that may arise as banks gradually internalize the richer information environment.

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Internet Appendix

for

“Digitalization and Credit Markets: Evidence from eInvoicing”

Alejandro Casado, Marco Giometti, Jose E. Gutierrez

David Martinez-Miera, Alexandra Matyunina, Tammaro Terracciano

A Appendix: Data

A.1 Variables definitions

Table IA.I. Variable definitions

Loan-level variables (Source: Central Credit Register (CIRBE), Bank of Spain)	
Loan Size	Outstanding used and unused committed credit granted under the loan.
Default	Indicator variable equal to 1 if the loan is currently classified as doubtful or past due 90 days or more.
One-year-ahead Realized Default	Indicator variable equal to 1 if the loan is classified as doubtful or past due 90 days or more over the next 4 quarters.
Default Rate	Share of the number of loans in default relative to the total number of loans outstanding.
Interest Rate	Interest rate charged on the loan.
Maturity	Loan maturity, measured in months.
Invoice-based Loan	Indicator variable equal to 1 if the loan is invoice-based (see Appendix A.2).
ICO Loan	Indicator variable equal to 1 if the loan is secured by a public guarantee from the Spanish ICO.
Probability of Default (PD)	Probability of default of risk holders over a one-year period (Regulation (EU) No 575/2013) (bank-firm level information).
Bank Share in Firm Credit	Proportion of the total credit volume granted to a firm by a specific bank within a given period (bank-firm level information).
Firm-level variables (Source: Mercantile Register, CIRBE and Financial Statements, Bank of Spain & Spanish Social Security Institute)	
Industry	One of the sectors in the 2-digit CNAE 2009 industry classification
N. Loans	Number of outstanding loans granted to the firm.
N. Bank Relationships	Number of banks with which the firm has outstanding credit relationships.
N. IRB Bank Relationships	Number of IRB banks with which the firm has outstanding credit relationships.
Invoice Firm	Indicator variable equal to 1 if the firm has at least one outstanding invoice-based loan in 2021
Total Assets	Firm total assets as of year-end.
Size	Logarithm of total assets.
Sales	Firm sales as of year-end.
Leverage	Ratio of book total debt to total assets as of year-end.
Liquidity	Ratio of liquid assets to total assets as of year-end.
Tangibility	Ratio of tangible fixed assets to total assets as of year-end.
Profitability	Return on assets, calculated as earnings divided by total assets as of year-end.
Age	Firm age, measured as the difference between the current year and the year of incorporation.
Total Credit	Used and unused committed credit granted by Spanish banking institutions.
Invoice Credit	Used and unused committed credit described in Appendix A.2 granted by Spanish banking institutions.
Non-Invoice Credit	Difference between total credit and invoice-based credit.
Share of ICO-guaranteed Credit	Share of the firm's total credit that is guaranteed by the Instituto de Crédito Oficial.
ERTE	Number of employees covered by the ERTE temporary employment adjustment scheme (province-industry-month level information).
New Relationship	Indicator equal to one if the firm originates a new loan with a bank not previously providing credit.
New Invoice Relationship	Indicator equal to one if the firm originates a new invoice-based loan with a bank not previously providing invoice credit.
New Non-Invoice Relationship	Indicator equal to one if the firm originates a new non-invoice loan with a bank not previously providing non-invoice credit.
Weighted Average Bank Controls:	(weights: each bank's share of the firm's credit)
Profitability	Return on assets, calculated as net income divided by total assets.
Liquidity	The ratio of cash holdings, cash balances with central banks, and other demand deposits to total assets.
Size	The logarithm of the bank's total assets.
Non-performing Loan Ratio	Non-performing loans divided by total assets.
Capitalization	Common Equity Tier 1 (CET1) capital divided by total assets.
Province-level variables (Source: CIRBE and Spanish Mercantile Register, Bank of Spain; Google)	
N. Firms	The number of firms with outstanding committed credit in the province.
Total Credit	Amount of total credit granted to firms in the province.
Share of SMEs	Share of firms in the province classified as small or medium-sized enterprises.
IRB Banks' Market Share	Share of total provincial corporate credit granted by IRB banks (calculated only on credit to SMEs).
Corporate Credit Market HHI	Herfindahl-Hirschman Index of the corporate credit market in the province (calculated only on credit to SMEs).
Interquartile Range	The difference between the 75th and 25th percentiles of the distribution of interest rates on newly originated loans or of probabilities of default for existing bank–firm relationships within a given quarter.
Standard Deviation	The standard deviation of the distribution of interest rates on newly originated loans or of probabilities of default for existing bank–firm relationships within a given quarter.
Share of Credit to Agriculture	Share of provincial corporate credit granted to SMEs in agriculture.
Share of Credit to Mining	Share of provincial corporate credit granted to SMEs in mining.
Share of Credit to Manufacturing	Share of provincial corporate credit granted to SMEs in manufacturing.
Share of Credit to Construction	Share of provincial corporate credit granted to SMEs in construction.
Share of Credit to Retail/Wholesale	Share of provincial corporate credit granted to SMEs in retail and wholesale trade.
Share of Credit to Transport	Share of provincial corporate credit granted to SMEs in transport.
Share of Credit to Hospitality	Share of provincial corporate credit granted to SMEs in hospitality.

A.2 Invoice-based credit definition

We define *invoice-based credit* as the set of credit operations reported to the Bank of Spain's Central Credit Register (CIR) under the heading *crédito comercial* (trade credit). Following the official Bank of Spain (BOE) reporting instructions, trade credit corresponds to credit granted on the basis of the purchase of collection rights (bills, invoices, or other documents) that arise from the deferral of payments associated with the sale of goods or the provision of services. Operationally, these credit lines encompass the discounting of commercial paper, factoring, and reverse factoring (payments to suppliers).

The BOE classifies these operations along two dimensions. The first is *recourse*: an operation is classified as *with recourse* (*con recurso*) when the assignor of the collection rights substantially retains the risks and rewards of the receivables, or when the reporting entity does not acquire control over their cash flows, so that the assignor is treated as the party bearing the direct credit risk; it is classified as *without recourse* (*sin recurso*) in the opposite case, and the obligor is then treated as the party bearing the direct credit risk. The second dimension applies within non-recourse operations and distinguishes *reverse factoring* (*pago a proveedores*), in which the reporting entity enters the operation through a contract with the obligor to pay its suppliers at maturity, from *factoring*, which covers all other non-recourse operations; each is further labelled as *with disbursement* (*con inversión*) when the entity may advance funds to the assignor before maturity, or *without disbursement* (*sin inversión*) when it is obligated to pay only on the maturity date. For all trade-credit operations, a credit limit is reported only when the entity has a committed line with the client.

Based on this taxonomy, the BOE categories that we aggregate under our definition of invoice-based credit are the following: discounting of commercial paper (*descuento de papel comercial*); other recourse operations (*resto de las operaciones*); reverse factoring with disbursement (*pago a proveedores con inversión*); reverse factoring without disbursement (*pago a proveedores sin inversión*); factoring with disbursement (*factoring con inversión*); factoring without disbursement (*factoring sin inversión*); and collection rights on regulated tariffs (*derechos de cobro sobre tarifas reguladas*).

A.3 Additional Summary Statistics

Table IA.II. Summary Statistics: Full Sample

This table presents summary statistics for the full sample period, January 2021 to December 2024. Panel A reports statistics at the bank-firm level, Panel B at the firm level, and Panel C at the province level. Panels A and B report all observations for our main sample at the firm-month level. Panel C reports observations at the province-quarter level. All variables are defined in Table IA.I. Column (1) reports the number of observations, Column (2) the mean, Column (3) the standard deviation, Column (4) the minimum, Columns (5)-(7) the 25th, 50th (median), and 75th percentiles, and Column (8) the maximum. Credit variables are expressed in thousands of euros. Shares, ratios, and probabilities are expressed as fractions, except where otherwise noted.

	N	Mean	SD	Min	25%	Median	75%	Max
Panel A: Bank-Firm Level								
Total credit, thousand €	1,899,601	201.79	362.83	0.69	18.55	69.21	207.59	2,284.22
Invoice credit, thousand €	1,899,601	16.12	61.21	0.00	0.00	0.00	0.00	434.87
Non-Invoice credit, thousand €	1,899,601	180.9	326.27	0.00	15.30	61.18	190.00	2,048.46
Probability of default, %	910,584	5.86	17.94	0.03	0.30	0.97	2.69	100.00
Interest rate, %	1,575,288	3.73	3.27	0.60	1.76	2.88	4.74	23.80
Maturity, months	1,624,866	80.98	54.44	3.07	51.07	62.80	97.40	305.10
Share of invoice-based credit	1,899,601	0.07	0.20	0.00	0.00	0.00	0.00	1.00
Share of ICO-guaranteed loans	1,899,601	0.31	0.40	0.00	0.00	0.00	0.70	1.00
Panel B: Firm Level								
Total credit, thousand €	971,356	398.38	910.76	1.50	23.84	90.32	312.67	6,055.05
Invoice credit, thousand €	971,356	28.76	121.28	0.00	0.00	0.00	0.00	915.93
Non-Invoice credit, thousand €	971,356	359.72	801.63	1.20	22.72	85.73	294.38	5,310.99
Interest rate, %	827,567	3.71	3.29	0.65	1.87	2.93	4.54	23.80
Interest rate, invoice credit, %	73,842	4.43	2.33	0.70	2.51	4.32	5.95	11.50
Interest rate, non-invoice credit, %	825,656	3.70	3.30	0.65	1.86	2.90	4.51	23.80
Share of invoice firms	971,356	0.24	0.43	0.00	0.00	0.00	0.00	1.00
Age	971,356	17.74	11.38	0.00	8.00	17.00	25.00	121.00
Leverage	971,356	0.31	0.29	0.00	0.07	0.24	0.47	1.52
Liquidity	971,356	0.64	0.30	0.01	0.41	0.70	0.90	1.00
Tangibility	971,356	0.26	0.27	0.00	0.04	0.17	0.42	0.97
Profitability	971,356	0.02	0.15	-0.84	-0.01	0.02	0.07	0.46
ERTE	971,356	74.02	401.77	0.00	0.00	5.00	21.00	10,246.00
New relationship, % (quarterly)	322,608	2.26	14.87	0.00	0.00	0.00	0.00	100.00
New invoice relationship, % (quarterly)	322,608	1.24	11.07	0.00	0.00	0.00	0.00	100.00
New non-invoice relationship, % (quarterly)	322,608	2.04	14.15	0.00	0.00	0.00	0.00	100.00
Panel C: Province Level								
Share of SMEs	288	0.95	0.01	0.92	0.94	0.96	0.96	0.97
Total market share of the IRB banks (SME)	288	0.40	0.06	0.27	0.36	0.40	0.43	0.53
Corporate credit market HHI	288	0.13	0.03	0.09	0.11	0.12	0.16	0.17
Share of credit to:								
Agriculture	288	0.01	0.00	0.00	0.01	0.01	0.01	0.02
Mining	288	0.002	0.00	0.00	0.00	0.00	0.00	0.00
Manufacturing	288	0.18	0.03	0.13	0.15	0.17	0.20	0.26
Construction	288	0.06	0.02	0.03	0.05	0.06	0.06	0.17
Retail/Wholesale	288	0.12	0.03	0.07	0.10	0.13	0.14	0.18

Table IA.III. Descriptive Statistics: Invoice vs. Non-Invoice Firms

This table presents firm-level descriptive statistics for invoice firms versus non-invoice firms. All variables are measured as of the end of 2021, the pre-reform period, and are defined in Table IA.I. Column (1) reports the mean for invoice firms, Column (2) reports the mean for non-invoice firms, Column (3) reports the difference in means between invoice and non-invoice firms, and Column (4) reports the p-value of a two-sided test for the equality of means across the two groups. Standard deviations appear in parentheses, except for N, for which the relative share appears in parentheses. For the tests of equality of means, standard errors are clustered at the province$\times$industry level. p-values are reported to two decimal digits, with values below 0.01 reported as < 0.01. ***, **, * indicate statistical significance at the 1%, 5% and 10%, respectively.

	Invoice Firms	Non-Invoice Firms	Diff.	P-value
	(1)	(2)	(3)	(4)
N	5,182 (23.34%)	17,016 (76.66%)		
N. loans	14.83 (18.85)	4.25 (7.06)	10.58	$< 0.01^{***}$
N. bank relationships	3.87 (2.53)	1.76 (1.13)	2.11	$< 0.01^{***}$
N. IRB bank relationships	1.88 (0.94)	1.25 (0.53)	0.63	$< 0.01^{***}$
Total assets, million €	2.97 (5.28)	1.22 (2.99)	1.74	$< 0.01^{***}$
Sales, million €	3.43 (6.17)	0.83 (2.06)	2.60	$< 0.01^{***}$
Leverage	0.35 (0.24)	0.31 (0.31)	0.04	$< 0.01^{***}$
Liquidity	0.71 (0.23)	0.61 (0.31)	0.10	$< 0.01^{***}$
Tangibility	0.23 (0.21)	0.27 (0.29)	-0.04	$< 0.01^{***}$
Profitability	0.01 (0.12)	0.02 (0.17)	-0.01	$< 0.01^{***}$
Age	20.85 (12.09)	16.29 (10.80)	4.56	$< 0.01^{***}$
Share of ICO guaranteed credit	0.43 (0.31)	0.31 (0.38)	0.12	$< 0.01^{***}$

Table IA.IV. Descriptive Statistics: Invoice vs. Non-Invoice Firms, Treated vs. Control

This table presents firm-level descriptive statistics for invoice firms (Panel A) versus non-invoice firms (Panel B), split by treated and control groups. All variables are measured as of the end of 2021, the pre-reform period, and are defined in Table IA.I. Column (1) reports the mean for invoice firms, Column (2) reports the mean for non-invoice firms. Column (3) reports the difference in means between invoice and non-invoice firms, and Column (4) reports the p-value of a two-sided test for the equality of means across the two groups. Standard deviations appear in parentheses, except for N, for which the relative share appears in parentheses. For the tests of equality of means, standard errors are clustered at the province×industry level. p-values are reported to two decimal digits. ***, **, * indicate statistical significance at the 1%, 5% and 10%, respectively.

	Treated	Control	Diff.	P-value
	(1)	(2)	(3)	(4)
Panel A: Invoice Firms				
N	2,249 (43.40%)	2,933 (56.60%)		
N. loans	14.38 (18.78)	15.17 (18.89)	-0.79	0.31
N. bank relationships	3.79 (2.51)	3.93 (2.54)	-0.14	0.14
N. IRB bank relationships	1.89 (0.95)	1.88 (0.94)	0.01	0.93
Total assets, million €	2.76 (5.05)	3.12 (5.44)	-0.36	0.41
Sales, million €	3.14 (5.87)	3.64 (6.39)	-0.50	0.20
Leverage	0.35 (0.26)	0.35 (0.23)	0.00	0.68
Liquidity	0.74 (0.23)	0.69 (0.23)	0.05	0.05*
Tangibility	0.20 (0.21)	0.25 (0.21)	-0.05	0.04**
Profitability	0.01 (0.14)	0.02 (0.11)	-0.01	0.02**
Age	20.15 (11.60)	21.37 (12.41)	-1.22	0.31
Share of ICO guaranteed credit	0.40 (0.31)	0.45 (0.30)	-0.05	0.03**
Panel B: Non-Invoice Firms				
N	7,525 (44.22%)	9,491 (55.78%)		
N. loans	4.26 (8.93)	4.25 (5.12)	0.01	0.97
N. bank relationships	1.74 (1.13)	1.78 (1.13)	-0.04	0.43
N. IRB bank relationships	1.26 (0.55)	1.24 (0.52)	0.02	0.34
Total assets, million €	1.29 (3.29)	1.17 (2.74)	0.12	0.54
Sales, million €	0.90 (2.38)	0.77 (1.76)	0.12	0.45
Leverage	0.29 (0.31)	0.33 (0.31)	-0.04	0.16
Share of liquid assets	0.64 (0.32)	0.59 (0.31)	0.05	0.42
Share of tangible assets	0.24 (0.28)	0.30 (0.28)	-0.06	0.05*
Return on assets	0.02 (0.19)	0.03 (0.16)	-0.01	0.58
Age	16.07 (10.55)	16.46 (10.99)	-0.39	0.56
Share of ICO guaranteed credit	0.26 (0.36)	0.35 (0.39)	-0.09	0.02**

B Appendix: Credit Market Dynamics

In this appendix, we report a set of supplementary analyses and robustness checks that complement the credit-market evidence in Section 4. Throughout, we focus on estimates at the bank-firm-month for the split between invoice and non-invoice firms. The goal is to assess whether the our main findings are robust to alternative samples, geographic restrictions, fixed-effects specifications, and other potential confounding factors, and to provide additional evidence on firm heterogeneity. Taken together, the results support the robustness of the credit-market effects documented in the main text.

B.1 Subsample of Firms with PDs

Because the information-production analysis in Section 5.2 is necessarily restricted to firms for which at least one IRB bank reports a usable PD, we re-estimate the main credit specifications on this subsample to confirm that our results hold. Table IA.V shows that the results are very similar to the baseline ones: invoice firms experience an increase in invoice credit and total credit, whereas non-invoice firms experience a decline in total credit.

Table IA.V. Robustness: Firms Borrowing from IRB Banks

This table reports estimates of β over the subsample of firms with at least one credit relationship with an IRB bank, from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The unit of observation is a bank-firm-month, and we estimate:

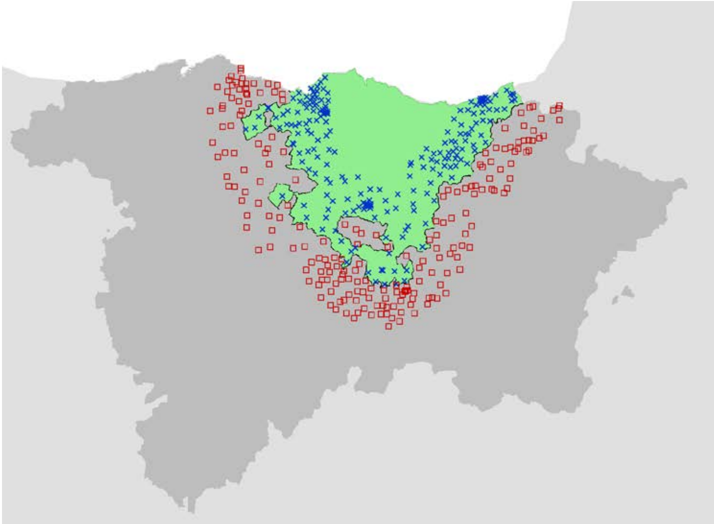
$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

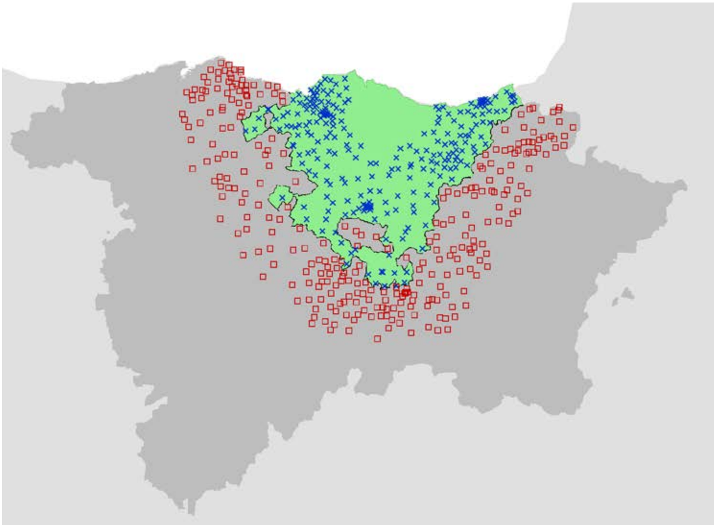
	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Post×Treated	0.0452***	-0.0001	0.0055*	-0.0046*
	(0.0082)	(0.0032)	(0.0031)	(0.0027)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	424,874	611,488	633,634	646,549
Pseudo R-Squared	0.74	0.93	0.93	0.96

B.2 Subsample of Firms near the Basque Country Border

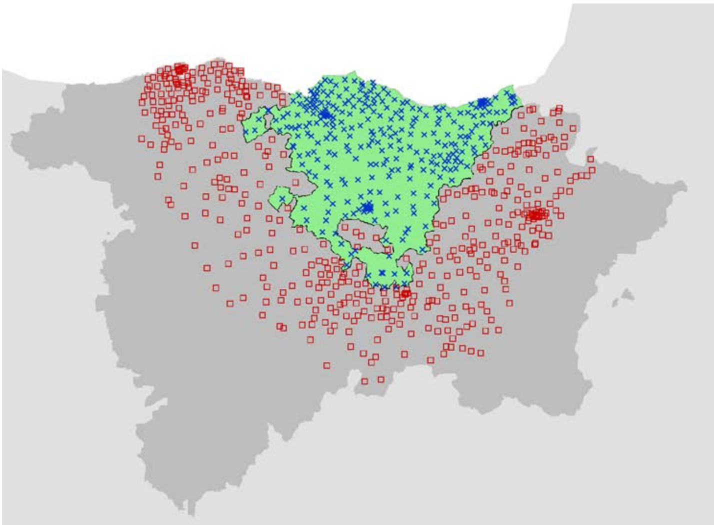
To further mitigate concerns about unobserved heterogeneity between treated and control provinces, we restrict the comparison to firms located near the Basque Country border. The underlying idea is that firms on either side of the border are more likely to face similar local economic conditions, so that comparing nearby treated and control firms provides a more demanding test of the reform's effects. We therefore restrict the sample ex ante to zip codes within a certain distance (either 20, 30, or 50 km) from the Basque Country border and re-estimate the baseline bank-firm specifications.[41] Figure IA.I illustrates the geographic distribution of the resulting samples, and Table IA.VI reports the estimates. The main findings of the paper are unchanged.



(a) Within 20 km of the Basque Country land border



(b) Within 30 km of the Basque Country land border



(c) Within 50 km of the Basque Country land border

Figure IA.I. Geographic distribution of firms by distance from the Basque Country land border.

Notes: The green area is the Basque Country (treatment); the medium-grey area covers the four adjacent control provinces (Cantabria, Burgos, La Rioja, and Navarra). Blue Xs mark firms located inside the Basque Country; red squares mark firms in the control provinces. Panels (a)–(c) show the subsample of firms whose zipcode centroid lies within 20, 30, and 50 km of the Basque Country's land border, respectively.

Table IA.VI. Robustness: Firms Near the Basque Country Border

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms located within a given distance from the land border of the Basque Country, from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. Each panel corresponds to a different distance bandwidth. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Panel A: Bandwidth = 20 km				
Post×Treated	0.0298**	0.0207***	0.0156***	-0.0191***
	(0.0141)	(0.0049)	(0.0045)	(0.0033)
Obs.	195,800	280,989	291,901	514,210
Pseudo R-Squared	0.75	0.94	0.93	0.97
Panel B: Bandwidth = 30 km				
Post×Treated	0.0666***	0.0133***	0.0157***	-0.0251***
	(0.0143)	(0.0044)	(0.0037)	(0.0031)
Obs.	254,577	363,379	377,038	622,062
Pseudo R-Squared	0.75	0.94	0.93	0.97
Panel C: Bandwidth = 50 km				
Post×Treated	0.0518***	0.0076**	0.0117***	-0.0107***
	(0.0078)	(0.0031)	(0.0030)	(0.0026)
Obs.	406,851	578,405	599,556	951,466
Pseudo R-Squared	0.75	0.94	0.93	0.97
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes

B.3 Leave-One-Out Province Analysis

Because our identification relies on a small number of treated and control provinces, one may worry that our results may be driven by idiosyncratic dynamics in a single province. To assess this, we re-estimate the baseline specification while sequentially excluding each province from the sample. As a complementary check, we also report estimates from a staggered design that includes Gipuzkoa. This province is excluded from the baseline analysis because its voluntary phase begins in 2021, leaving no reliable pre-reform period in our sample (see Section 2). However, the staggered design allows treatment timing to vary across provinces according to the start of TicketBAI in each province. Table IA.VII shows that the main patterns remain overall stable across these alternative samples. This indicates that the documented effects are not driven by any single province, nor by the exclusion of Gipuzkoa from the baseline design.

Table IA.VII. Robustness: Leave-One-Out Estimates and Gipuzkoa Inclusion

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms, using Poisson Pseudo-Maximum Likelihood. Panel A re-estimates the baseline specification, excluding one province at a time from the main sample from January 2021 to December 2024. In these specifications, treatment starts in January 2022 for the treated provinces (Álava and Biscay). Panel B includes Gipuzkoa in the sample and estimates a staggered difference-in-differences specification in which treatment starts in each treated province when its voluntary TicketBAI period begins. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Treatment}_{p,t} + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

In Panel A, $\mathit{Treatment}_{p,t}$ equals $\mathit{Post}_t \times \mathit{Treat}_p$, where Post_t is an indicator equal to one from January 2022 onward and Treat_p is an indicator equal to one for firms headquartered in treated provinces. In Panel B, $\mathit{Treatment}_{p,t}$ is an indicator equal to one once province p enters the voluntary TicketBAI period. $Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Panel A: Leave-One-Out Estimates (common treatment date)				
Excluding Álava	0.0519***	-0.0111***	-0.0029	-0.0079***
	(0.0079)	(0.0027)	(0.0030)	(0.0027)
Excluding Biscay	0.0227**	0.0249***	0.0209***	-0.0041
	(0.0098)	(0.0037)	(0.0039)	(0.0028)
Excluding Burgos	0.0395***	0.0065**	0.0088***	-0.0062***
	(0.0078)	(0.0032)	(0.0031)	(0.0023)
Excluding Cantabria	0.0406***	0.0076**	0.0109***	-0.0078***
	(0.0082)	(0.0033)	(0.0032)	(0.0022)
Excluding Navarre	0.0231***	-0.0055*	-0.0041	-0.0062***
	(0.0076)	(0.0032)	(0.0029)	(0.0022)
Excluding La Rioja	0.0455***	0.0020	0.0066**	-0.0039*
	(0.0072)	(0.0033)	(0.0032)	(0.0022)
Panel B: Including Gipuzkoa (province-specific treatment dates)				
Including Gipuzkoa	0.0268***	0.0090***	0.0096***	0.0016
	(0.0069)	(0.0029)	(0.0025)	(0.0021)

B.4 Accounting for Bank-Specific Sector Shocks

A potential concern is that the baseline fixed-effects structure may not fully absorb time-varying credit-supply shocks at the bank-sector level. To address this, we re-estimate the baseline specification using a more demanding fixed-effects combination that includes bank×industry×month variation. Table IA.VIII shows that the estimated effects remain very similar to the baseline ones. This indicates that the main results are unlikely to be driven by bank-specific shifts in lending to particular sectors that may correlate with treatment exposure.

Table IA.VIII. Robustness: Bank-Industry-Month Fixed Effects

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms over our main sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,i,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. Post_t is an indicator equal to one from January 2022 onward. Treat_p is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×industry×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Post×Treated	0.0421***	0.0033	0.0066**	-0.0093***
	(0.0072)	(0.0032)	(0.0031)	(0.0022)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Industry×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	487,754	705,681	731,281	1,150,450
Pseudo R-Squared	0.77	0.94	0.94	0.97

B.5 SII - *Suministro Inmediato de Información*

Another potential concern is that our results may be confounded by the *Suministro Inmediato de Información* (SII), a Spanish VAT reporting system that requires certain firms to report billing data to the tax authority within 4 days.[42] We view this as unlikely for three reasons.

42. Specifically, the eligible entities are: large businesses (i.e. firms with turnover above €6 million); VAT groups; firms registered with REDEME (monthly VAT return registry); holders of tax warehouses for gasoline, diesel or

First, the SII entered into force in 2017, well before the start of our sample, and applies in both treated and control provinces. Second, our fixed-effects structure absorbs persistent firm-level differences related to SII eligibility. Third, Table IA.IX shows that our main results remain unchanged when we exclude firms most likely affected by the SII, namely those with turnover above €6 million and those in fuel warehousing-related industries.[43] These results indicate that the documented credit effects are unlikely to be driven by the SII.

Table IA.IX. Robustness: Excluding Firms Potentially Subject to SII

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms over a restricted sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The sample excludes firms with turnover above €6 million and firms in 2-digit NACE-2009 industries 19, 46, 47, and 52, which are potentially subject to the *Suministro Inmediato de Información* regime. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot Post_t \times Treat_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Post×Treated	0.0335***	0.0003	0.0058**	-0.0036**
	(0.0115)	(0.0027)	(0.0024)	(0.0016)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	192,879	296,791	305,002	641,201
Pseudo R-Squared	0.76	0.96	0.95	0.98

biofuels included in the objective scope of the Hydrocarbon Tax; and entrepreneurs or professionals who extract these products from tax warehouses (from 01/01/2025). Additional information about the SII can be found at `https://sede.agenciatributaria.gob.es/Sede/en_gb/iva/suministro-inmediato-informacion/informacion-general.html`.

43. The law does not explicitly mention the specific NACE sectors; we manually ascribe them. To be conservative, we discard the following 2-digit NACE codes: 19 - Manufacture of coke and refined petroleum products; 46 - Wholesale trade, except for motor vehicles and motorcycles; 47 - Retail trade, except for motor vehicles and motorcycles; 52 — Warehousing and support activities for transportation.

B.6 Alternative Clustering of Standard Errors

We cluster standard errors at the province-time level in our main specifications (Equation (1)). This choice reflects three features of our design. First, while the eInvoicing reform was introduced at the province level, its implementation varied over time across voluntary and mandatory phases, so that the relevant policy variation in our estimating equations is defined at the province-month level. Second, our preferred specifications include a rich set of fixed effects—industry×size-tercile×month, bank×month, and firm×bank—which absorb the main channels through which province-level or aggregate shocks could affect credit outcomes. The industry×size-tercile×month effects in particular control for industry-time credit demand, while also absorbing size-specific shocks, including any potential differential exposure defined at the firm-size level that overlaps with our sample period. Third, we report a more demanding triple-interaction specification with province×month×industry×size-tercile fixed effects (Table 8), which absorbs shocks at the most granular implementation-exposure cell. After this conditioning, identification comes from residual variation between invoice and non-invoice firms within the same province-time-industry-size tercile, alleviating concerns regarding the autocorrelation of the residuals. This inference approach is consistent with the design-based perspective of Abadie et al. (2023), who emphasize that clustering choices should be guided by the sampling and treatment-assignment process rather than by residual correlation alone.

Nevertheless, to address possible remaining concerns, we discuss alternative clustering choices and report their p-values in Table IA.X. In the first alternative, we use heteroskedasticity robust standard errors, which allow us to be agnostic about the level of the treatment and the possible autocorrelation of the errors. The significance of these estimates is consistent with the baseline results in Table 4.

Next, we cluster standard errors at the industry-time and province-industry-time levels, since the timing of reform implementation depended on the industry (see Section 2). The caveat of this approach is that our industry variable is the standard NACE classification, while TicketBAI uses IAE, for which there is no direct mapping. Nonetheless, also in these cases, our estimates are consistent with the baseline results in Table 4.

Finally, we focus on the province dimension alone. The issue with this approach is that the conventional cluster-robust inference is not reliable, because the design contains only six provinces, two of which are treated. Our baseline province-month clustering matches the observed policy-exposure cell, but it does not by itself address arbitrary serial correlation in province-level shocks. We therefore treat province-level inference as the relevant conservative benchmark. MacKinnon and Webb (2020) show that, with very few treated clusters, conventional cluster-robust t-tests can severely over-reject; the usual restricted wild cluster bootstrap can severely under-reject, while unrestricted variants can over-reject. For PPML, residual-based additive wild-bootstrap pseudo-outcomes are also not straightforward because they may be negative, and score/multiplier variants do not eliminate the finite-sample concern created by having 6 provinces, of which only 2 are treated. We therefore report exact province-level randomization inference over all $\binom{6}{2} = 15$ two-province placebo assignments, using the cluster-robust t-statistic as the test statistic, in line with the recommendation of MacKinnon and Webb (2020).

It is important to underscore how stringent this procedure is in our setting. With only 15 admissible treatment-pair assignments, the exact two-sided p-value is necessarily a multiple of 1/15, so the smallest value the test can ever produce is $1/15 \approx 0.067$. As a mechanical consequence, statistical significance at the conventional 5% level is *unattainable* by design, regardless of the magnitude or precision of the underlying effect; the most decisive verdict the procedure can return is significance at the 10% level, and only when the actual Álava–Biscay assignment yields a more extreme $|t|$ than *all fourteen* alternative two-province placebos. The procedure is therefore heavily skewed toward non-rejection: it requires the realized treatment to produce a uniformly more extreme test statistic than every other province pair, including pairs that mix one treated and one control province and that are mechanically correlated with the actual treatment. Failure to reject under this test is consequently weak evidence of a null, while a rejection – even at 10% – constitutes unusually strong evidence in favour of an effect. Against this backdrop, the realized assignment delivers RI-$t = 1/15 \approx 0.067$ for invoice-based credit, the minimum attainable exact p-value in this design. We interpret this as the strongest evidence that the randomization-inference procedure can provide for the existence of a true effect, while acknowledging that the six-province design necessarily limits the resolution of any inference procedure built on the province-level permutations.

Table IA.X. eInvoicing and Credit Supply: Inference Robustness

This table reports the coefficient on Post×Treated from the regression specified in Equation (1), separately for invoice firms (columns 1–3) and non-invoice firms (column 4). All specifications are estimated using Poisson-Pseudo Maximum Likelihood at the bank-firm-month level and include Industry × Size × Month, Firm × Bank, and Bank × Month fixed effects, firm controls, and Covid controls. The first row reports the point estimate, mechanically identical across inference choices. Subsequent rows report two-sided p-values (in parentheses) under alternative variance–covariance assumptions: heteroskedasticity-robust (unclustered) inference; cluster-robust standard errors at the indicated level (using the normal-distribution reference); and randomization inference at the province level following MacKinnon and Webb (2020), enumerating all $\binom{6}{2} = 15$ placebo treatment-pair assignments and computing $p^*_t = \#\{|\hat{t}^*_r| \geq |\hat{t}|\}/15$ from the cluster-robust t-statistic. The smallest p-value the randomization-inference procedure can deliver in this design is $1/15 \approx 0.0667$.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Post×Treated	0.0394	0.0025	0.0059	-0.0066
Standard Errors (p-values are reported):				
Heteroskedasticity Robust	(0.0000)	(0.3667)	(0.0309)	(0.0133)
Clustered at Industry × Month Level	(0.0000)	(0.4096)	(0.1056)	(0.0031)
Clustered at Province × Industry × Month Level	(0.0000)	(0.4262)	(0.0496)	(0.0021)
Clustered at Province Level (with only 6 clusters):				
Conventional Approach	(0.0140)	(0.8583)	(0.6161)	(0.1026)
Randomized-inference Approach (RI-t)	(0.0667)	(0.9333)	(0.6000)	(0.2000)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	499,113	714,333	739,989	1,159,071
Pseudo R-Squared	0.75	0.93	0.93	0.97

B.7 Additional Firm Heterogeneity

We also explore whether the effects of eInvoicing vary across firms with different pre-reform characteristics. To do so, we split the sample along several firm-level dimensions (size, age, tangibility, and receivables intensity) measured before the reform, and re-estimate the baseline specification within each subsample. Table IA.XI reports the results and complements the main heterogeneity analysis in the paper, which focuses on the distinction between invoice and non-invoice firms.

Table IA.XI. Additional Heterogeneity by Firm Characteristics

This table reports estimates of β from regressions estimated separately by firm characteristics over the main sample from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. Each panel splits firms at the median of a pre-reform firm characteristic: size in Panel A, age in Panel B, tangibility in Panel C, and receivables over sales in Panel D. Within each panel, columns 1–3 report estimates for firms above the median, while columns 4–6 report estimates for firms below the median. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. The dependent variable is invoice-based credit in columns 1 and 4, non-invoice credit in columns 2 and 5, and total credit in columns 3 and 6. Post_t is an indicator equal to one from January 2022 onward. Treat_p is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Above the Median			**Below the Median**		
	Invoice	Non-Invoice	Total	Invoice	Non-Invoice	Total
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Size						
Post×Treated	0.0420***	0.0017	0.0019	0.1507***	-0.0174***	-0.0113***
	(0.0077)	(0.0020)	(0.0019)	(0.0219)	(0.0034)	(0.0033)
Obs.	457,003	1,238,729	1,261,831	94,564	633,754	637,413
Pseudo R-Squared	0.74	0.94	0.94	0.74	0.94	0.94
Panel B: Age						
Post×Treated	0.0058	0.0073***	0.0057**	0.1004***	-0.0156***	-0.0099***
	(0.0105)	(0.0023)	(0.0024)	(0.0125)	(0.0033)	(0.0029)
Obs.	347,370	1,001,225	1,020,424	202,546	870,505	878,000
Pseudo R-Squared	0.76	0.95	0.95	0.76	0.96	0.96
Panel C: Tangibility						
Post×Treated	0.0187	0.0040*	0.0025	0.0612***	-0.0079***	-0.0055*
	(0.0152)	(0.0022)	(0.0026)	(0.0103)	(0.0029)	(0.0032)
Obs.	286,275	980,084	991,348	256,791	851,220	866,387
Pseudo R-Squared	0.75	0.95	0.95	0.77	0.96	0.96
Panel D: Receivables/Sales						
Post×Treated	0.0499***	0.0008	0.0013	-0.0015	0.0036	0.0061
	(0.0090)	(0.0021)	(0.0024)	(0.0167)	(0.0036)	(0.0042)
Obs.	387,909	1,024,861	1,042,598	151,697	797,159	805,617
Pseudo R-Squared	0.75	0.95	0.95	0.77	0.96	0.96

C Appendix: Banks' Information Production

C.1 Robustness Tests: Dispersion

Table IA.XII estimates the same specifications of Table 10 but without taking the logarithm of the interquartile range or the standard deviation.

Table IA.XII. Robustness: Dispersion of Interest Rates and Probabilities of Default (levels)

This table reports estimates of the effect of eInvoicing on the dispersion of interest rates and banks' probability-of-default (PD) estimates at the province-quarter level. Panel A uses the dispersion of interest rates as the dependent variable; Panel B uses the dispersion of banks' PD estimates. Columns 1–3 measure dispersion using the interquartile range, while columns 4–6 measure dispersion using the standard deviation. Columns 1 and 4 restrict the sample to invoice firms; columns 2 and 5 restrict the sample to non-invoice firms; columns 3 and 6 use the full sample and estimate separate effects for invoice and non-invoice firms. For columns 1, 2, 4, and 5, we estimate:

$$y_{p,t} = \beta \cdot Post_t \times Treat_p + \alpha_p + \alpha_t + \Gamma \cdot X_{p,t} + \varepsilon_{p,t}.$$

For columns 3 and 6, we estimate the corresponding stacked specification:

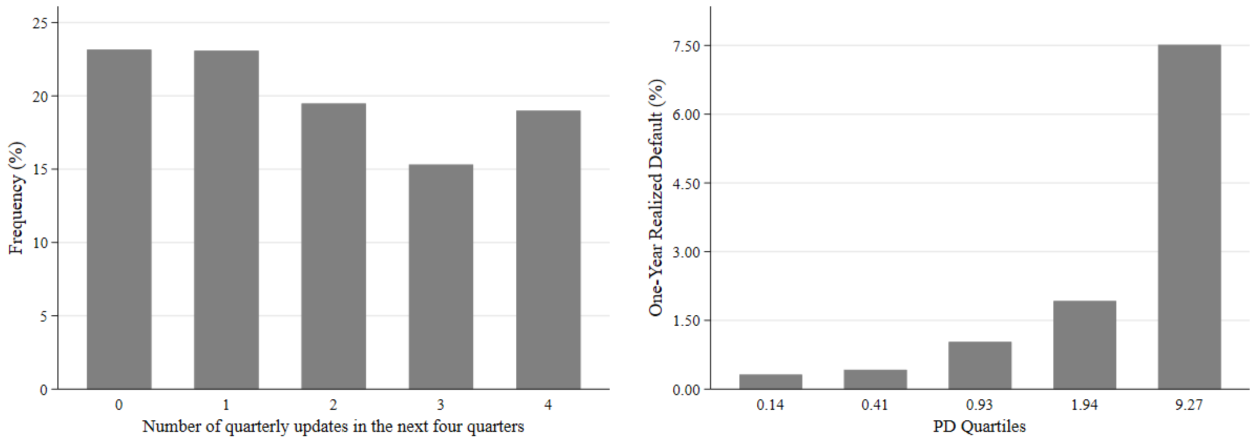
$$y_{p,g,t} = \beta_1 \cdot Post_t \times Treat_p + \beta_2 \cdot Post_t \times Treat_p \times Invoice\ Firms_g + \alpha_{p,g} + \alpha_{g,t} + \Gamma_1 \cdot X_{p,t} + \Gamma_2 \cdot X_{p,t} \times Invoice\ Firms_g + \varepsilon_{p,g,t}.$$

In these equations, y is either the interquartile range or the standard deviation of interest rates or PDs within a province-quarter cell. In the stacked specification, g indexes whether the dispersion measure is computed among invoice firms or non-invoice firms, and $Invoice\ Firms_g$ is an indicator equal to one for the invoice-firms group. $Post_t$ is an indicator equal to one from January 2022 onward. $Treat_p$ is an indicator equal to one for treated provinces. The vector $X_{p,t}$ includes log total credit, the number of firms, the corporate credit-market HHI, the share of ICO-guaranteed credit, the shares of credit to Agriculture, Mining, Manufacturing, Construction, Retail and Wholesale, Transport, and Hospitality, the Google Mobility index, and the total number of workers covered by the ERTE program, all constructed within the sample used to compute the dispersion measures. All variables are defined in Table IA.I. Heteroskedasticity-robust standard errors are reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Interquartile Range			Standard Deviation		
	Invoice	Non-Invoice	All	Invoice	Non-Invoice	All
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Interest Rates						
Post×Treated	29.59***	-32.85***	-32.85***	5.06	-39.87**	-39.87**
	(10.16)	(11.30)	(11.30)	(11.71)	(19.53)	(19.53)
Post×Treated×Invoice Firms			62.44***			44.93*
			(15.20)			(22.77)
Province FE	Yes	Yes	No	Yes	Yes	No
Quarter FE	Yes	Yes	No	Yes	Yes	No
Province×Invoice FE	No	No	Yes	No	No	Yes
Invoice×Quarter FE	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	96	96	192	96	96	192
R-Squared	0.80	0.83	0.85	0.69	0.82	0.90
Panel B: Probabilities of Default						
Post×Treated	32.94***	-29.37**	-29.37**	41.00	-91.27*	-91.27*
	(9.82)	(12.89)	(12.89)	(35.04)	(47.97)	(47.97)
Post×Treated×Invoice Firms			62.31***			132.28**
			(16.20)			(59.40)
Province FE	Yes	Yes	No	Yes	Yes	No
Quarter FE	Yes	Yes	No	Yes	Yes	No
Province×Invoice FE	No	No	Yes	No	No	Yes
Invoice×Quarter FE	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	96	96	192	96	96	192
R-Squared	0.78	0.76	0.78	0.78	0.64	0.76

C.2 Basic Facts about PDs

In this subsection, we validate that, in our sample, banks' internal estimates of borrowers' probability of default measure banks' private information.[44] We present two facts. First, PDs are updated frequently even after loan origination, suggesting that banks' information production plays an active role throughout the loan's lifespan. Figure IA.IIa shows the quarterly frequency of PD updates over a year. Banks change the PDs for approximately 77% of their outstanding loans over a year. Second, PDs are related to ex-post defaults. Figure IA.IIb shows that higher values of the PDs are positively correlated with an increased likelihood of ex-post default.



(a) Frequency of Banks' PD Updates **(b)** Ex-Post Loan Defaults vs Ex-Ante PDs

Figure IA.II. Probabilities of Defaults (PDs): updates and validity.

To show this more formally, we estimate the following regression at the loan level, following the same approach as Howes and Weitzner (forthcoming):

$$Default_{l,t+4} = \beta \cdot \mathrm{PD}_{l,t} + \Omega \cdot X_l + \delta_{b,t} + \gamma_{i,t} + \sigma_{b,p} + \varepsilon_l \tag{6}$$

where l, b, t, i and p index loan, bank, quarter, industry, and province, respectively. $Default_{l,t+4}$ is a dummy variable that equals one if loan i outstanding at t enters default or becomes doubtful within the subsequent four quarters. $\mathrm{PD}_{l,t}$ denotes the one-year probability of default estimated by bank b at quarter t for loan l. X_l is a vector of firm- and loan-level characteristics, including firm size (measured as the logarithm of total assets), leverage (total debt to total assets), profitability (EBITDA to total assets), asset tangibility (tangible assets to total assets), the current ratio, the logarithm of one plus the firm's age. Loan-level contract controls include the

44. Howes and Weitzner (forthcoming) and Casado and Martinez-Miera (2025) show this for the United States and Spain, respectively.

logarithm of the loan credit volume in euros (including both drawn and undrawn amrounts for credit lines), a binary indicator for whether the loan is secured with collateral, the proportion of the total credit volume granted to a firm by a specific bank within a given period, and loan type fixed effects. $\delta_{b,t}$ are bank×quarter fixed effects, $\gamma_{i,t}$ are industry×quarter fixed effects, and $\sigma_{b,p}$ are bank×province fixed effects. The coefficient of interest is β, which measures the expected change in realized default (in percentage points) per one-point increase in the loan's PD. Estimating Equation (6) with and without the loan interest rate as a control allows us to gauge whether the PD has predictive power beyond the credit risk information already embedded in the interest rate. As expected, the PD has a positive and highly significant correlation with the loan default, even after accounting for interest rates (see Table IA.XIII).

Table IA.XIII. Predictive Power of PDs

This table reports the coefficients of the loan-level regression specified in Equation (6), estimated by OLS. The dependent variable is a dummy variable equal to 1 if the loan enters default or becomes doubtful within the subsequent 4 quarters. Contract controls include the logarithm of the loan credit volume in euros (including both drawn and undrawn amounts for credit lines), a binary indicator for whether the loan is secured with real collateral, and the share of the total credit volume granted to a firm by a specific bank within a given period. Firm controls include leverage, current ratio, tangibility, ROA, log total assets, and log(1 + firm age). When we control for the interest rate, we also include an indicator for the loan's interest-rate structure, which equals 1 for fixed-rate loans and 0 otherwise. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province × quarter level. ***, **, * indicate statistical significance at the 1%, 5% and 10%, respectively.

	Loan Default			
	(1)	(2)	(3)	(4)
PD	0.6601***	0.6501***	0.5976***	0.6040***
	(0.0218)	(0.0215)	(0.0213)	(0.0195)
Interest Rate				0.1040***
				(0.0244)
Fixed Interest Rate Indicator				0.1730*
				(0.0886)
Industry×Quarter FE	Yes	Yes	Yes	Yes
Bank×Province FE	Yes	Yes	Yes	Yes
Bank×Quarter FE	Yes	Yes	Yes	Yes
Loan Type FE	No	Yes	Yes	Yes
Contract Controls	No	Yes	Yes	Yes
Firm Controls	No	No	Yes	Yes
Obs.	876,271	876,231	876,231	587,815
R-Squared	0.08	0.08	0.09	0.10

C.3 Robustness Tests: AUROCs

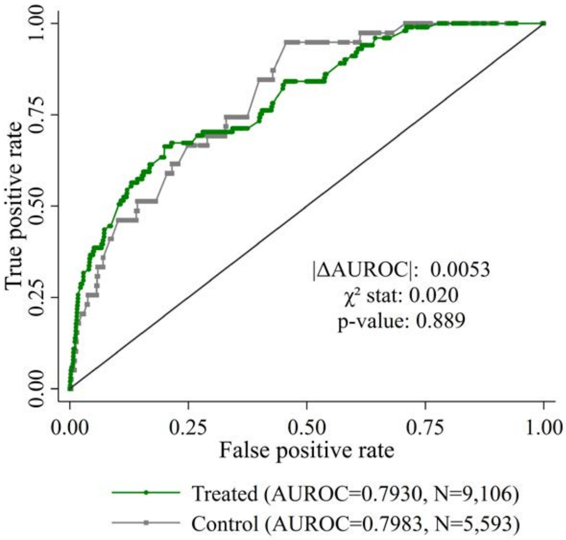
C.3.1 Matched/Reweighted Samples

Table IA.XIV. Descriptive Statistics for the AUROC Sample: Invoice vs. Non-Invoice Firms, Treated vs. Control (Pre- and Post-Matching)

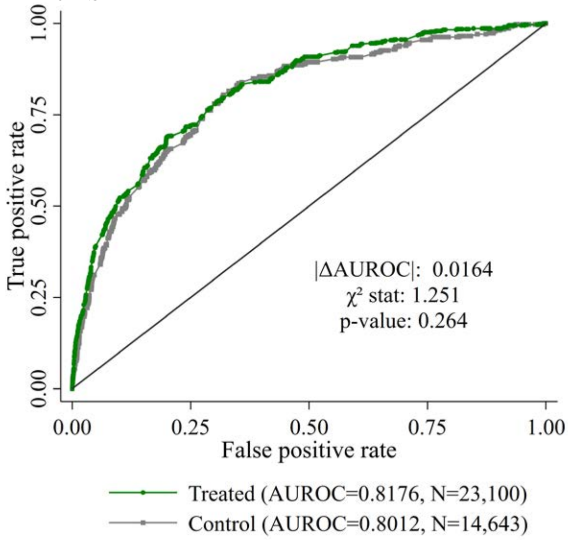
This table presents descriptive statistics for the AUROC sample. All variables are measured as of the end of 2021, the pre-reform period, and are defined in Table IA.I. Panel A reports pre-matching statistics, while Panel B reports post-matching statistics. Within each panel, observations are further split into Invoice firms and Non-Invoice firms, and into control and treated groups. Columns (1) and (2) report the mean for the treated and control groups, respectively. Column (3) reports the difference between the treated and control groups, and Column (4) reports the p-value of a two-sided test for the equality of means across the treated and control groups. Standard deviations appear in parentheses, except for N, for which the relative share appears in parentheses. p-values are reported to two decimal digits. ***, **, * indicate statistical significance at the 1%, 5% and 10%, respectively.

	Treated	Control	Diff.	P-value
	(1)	(2)	(3)	(4)
Panel A: Pre-Matching				
Invoice Firms				
N	2,720 (40.71%)	3,962 (59.29%)		
Total credit, thousand €	335.09 (560.41)	396.20 (666.66)	-61.11	0.15
Median PD	2.80 (7.05)	2.35 (5.84)	0.45	0.03**
Size	7.47 (1.35)	7.54 (1.37)	-0.07	0.62
Leverage	0.39 (0.22)	0.39 (0.21)	0.00	0.80
Share of liquid assets	0.73 (0.22)	0.68 (0.22)	0.05	0.07*
Share of tangible assets	0.20 (0.20)	0.25 (0.21)	-0.05	0.03**
Return on assets	-0.00 (0.11)	0.01 (0.07)	-0.01	0.05**
Age	21.49 (12.09)	23.01 (12.66)	-1.52	0.23
Share of ICO guaranteed credit	0.49 (0.40)	0.48 (0.40)	0.01	0.67
Non-Invoice Firms				
N	3,859 (41.03%)	5,547 (58.97%)		
Total credit, thousand €	201.17 (536.34)	183.24 (460.92)	17.93	0.48
Median PD	2.11 (6.00)	1.96 (5.76)	0.15	0.47
Size	6.41 (1.54)	6.28 (1.47)	0.13	0.37
Leverage	0.37 (0.31)	0.38 (0.30)	-0.01	0.71
Share of liquid assets	0.63 (0.31)	0.59 (0.30)	0.04	0.47
Share of tangible assets	0.24 (0.27)	0.31 (0.28)	-0.07	0.03**
Return on assets	-0.02 (0.19)	-0.01 (0.16)	-0.01	0.50
Age	17.23 (11.38)	17.77 (11.62)	-0.54	0.57
Share of ICO guaranteed credit	0.44 (0.44)	0.47 (0.44)	-0.03	0.45
Panel B: Post-Matching				
Invoice Firms				
N	2,407 (61.78%)	1,489 (38.22%)		
Total credit, thousand €	338.21 (570.92)	380.07 (599.84)	-41.86	0.37
Median PD	2.16 (4.40)	2.14 (4.26)	0.02	0.90
Size	7.51 (1.32)	7.53 (1.32)	-0.02	0.93
Leverage	0.38 (0.21)	0.39 (0.21)	-0.01	0.71
Share of liquid assets	0.74 (0.21)	0.73 (0.21)	0.01	0.71
Share of tangible assets	0.20 (0.19)	0.21 (0.18)	-0.01	0.66
Return on assets	0.01 (0.08)	0.01 (0.07)	0.00	0.65
Age	21.91 (12.15)	22.99 (12.28)	-1.08	0.42
Share of ICO guaranteed credit	0.48 (0.40)	0.51 (0.40)	-0.03	0.29
Non-Invoice Firms				
N	3,390 (62.16%)	2,064 (37.84%)		
Total credit, thousand €	189.47 (503.55)	187.27 (494.43)	2.19	0.94
Median PD	1.81 (4.67)	1.67 (3.56)	0.14	0.48
Size	6.37 (1.54)	6.30 (1.50)	0.07	0.71
Leverage	0.37 (0.31)	0.37 (0.30)	0.00	1.00
Share of liquid assets	0.64 (0.31)	0.63 (0.30)	0.01	0.86
Share of tangible assets	0.24 (0.27)	0.26 (0.26)	-0.02	0.55
Return on assets	-0.01 (0.17)	-0.01 (0.16)	0.00	0.99
Age	17.15 (11.32)	18.04 (11.63)	-0.89	0.41
Share of ICO guaranteed credit	0.45 (0.44)	0.48 (0.45)	-0.03	0.51

Invoice Firms

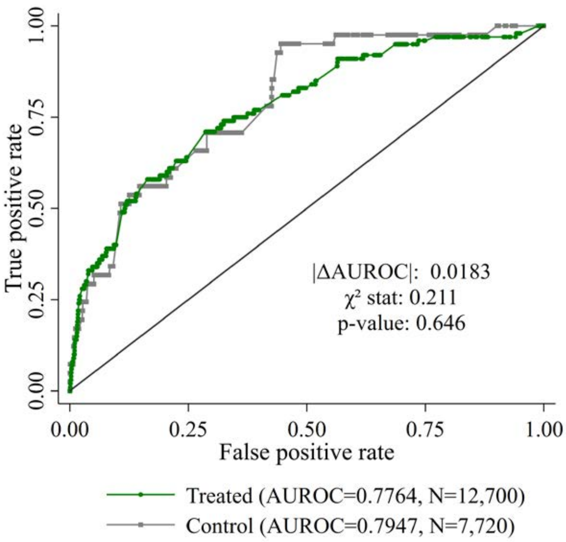


(a) 2021

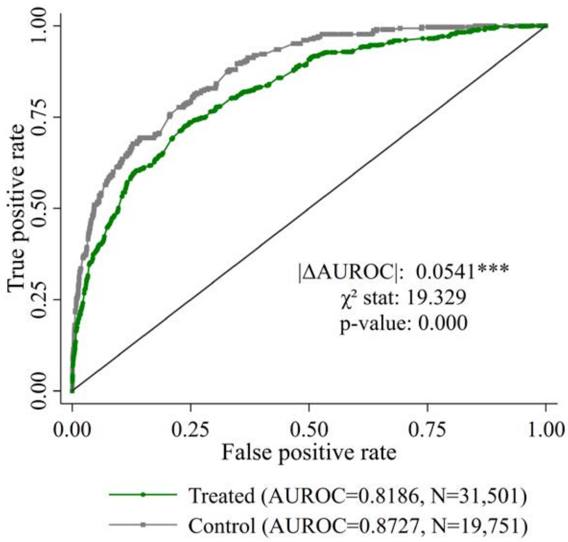


(b) 2022-2024

Non-Invoice Firms



(c) 2021



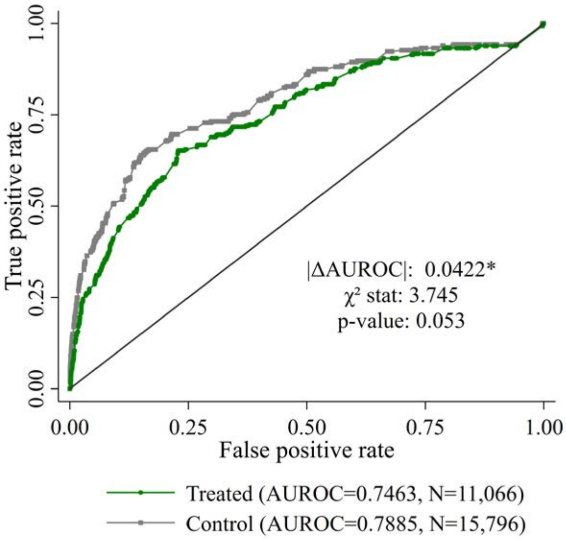
(d) 2022-2024

Figure IA.III. Robustness: Differences in Predictive Quality of PDs before and after eInvoicing, Matched/Reweighted Sample.

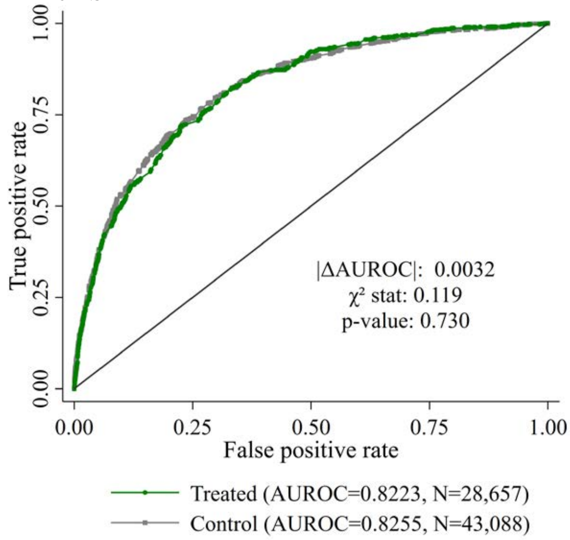
Notes: This figure replicates the AUROC-based analysis in Figure 5 on a matched/reweighted sample in which treated and control firms are balanced on observable characteristics. The top row reports AUROCs for invoice firms, and the bottom row for non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

C.3.2 Stayers Samples

Invoice Firms

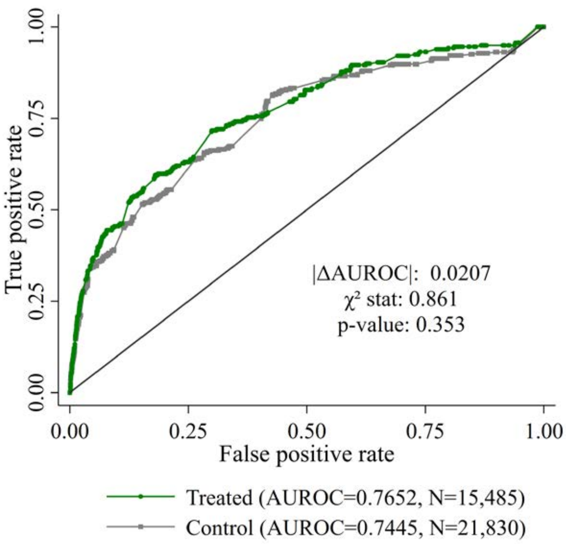


(a) 2021

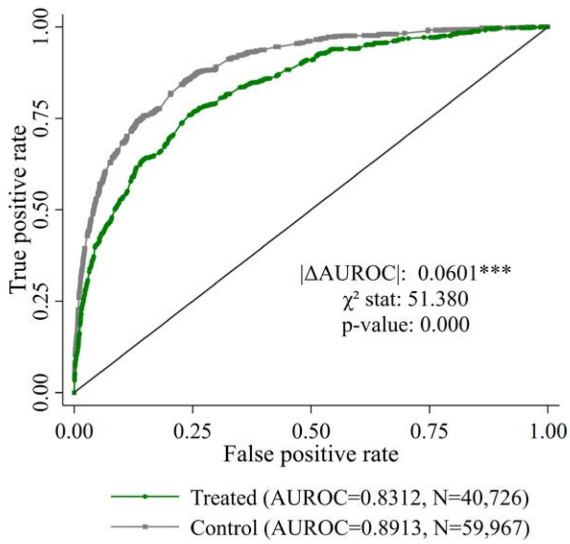


(b) 2022-2024

Non-Invoice Firms



(c) 2021



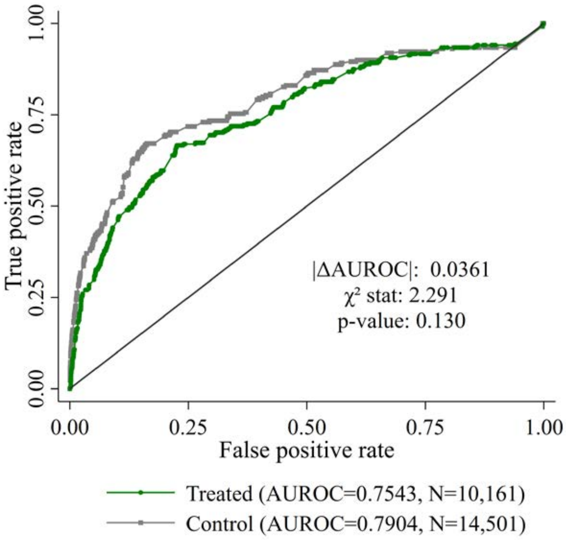
(d) 2022-2024

Figure IA.IV. Robustness: Differences in Predictive Quality of PDs before and after eInvoicing, Stayers Only.

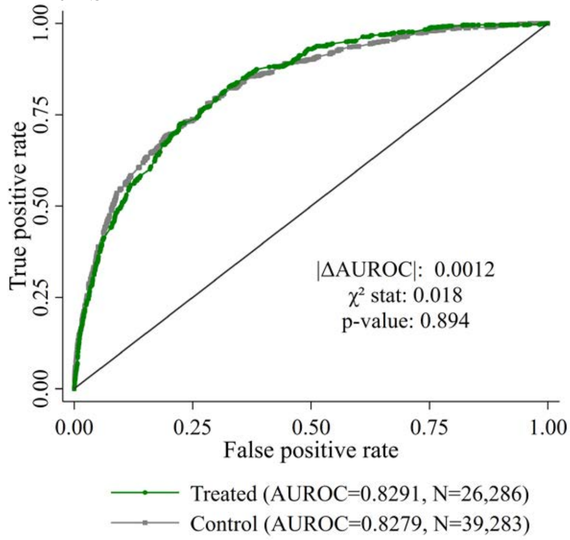
Notes: This figure replicates the AUROC-based analysis in Figure 5, restricting the sample to *stayers*, i.e., firms that are present in the data both before and after the reform. The top row reports AUROCs for invoice firms, and the bottom row for non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

C.3.3 Same-Cohort Samples

Invoice Firms

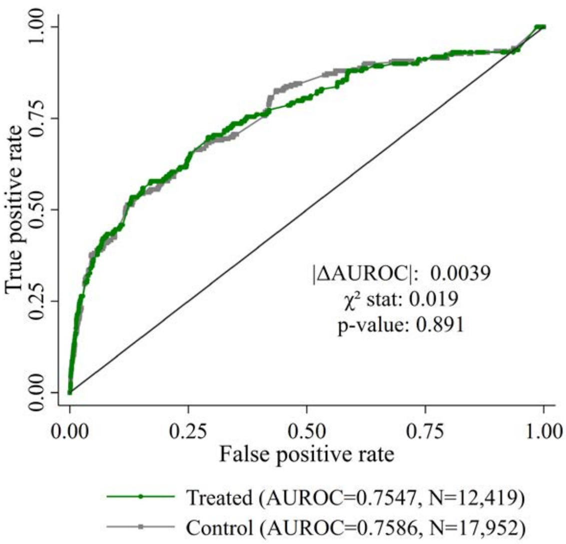


(a) 2021

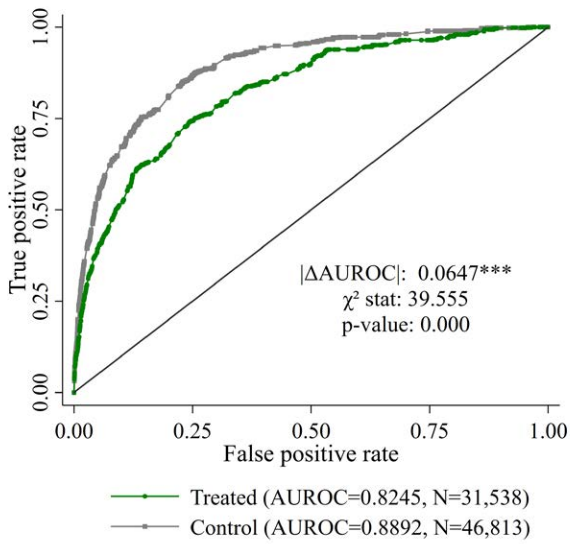


(b) 2022-2024

Non-Invoice Firms



(c) 2021

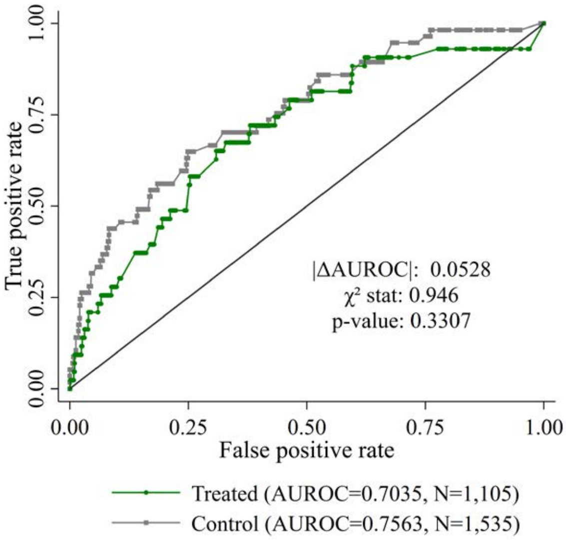


(d) 2022-2024

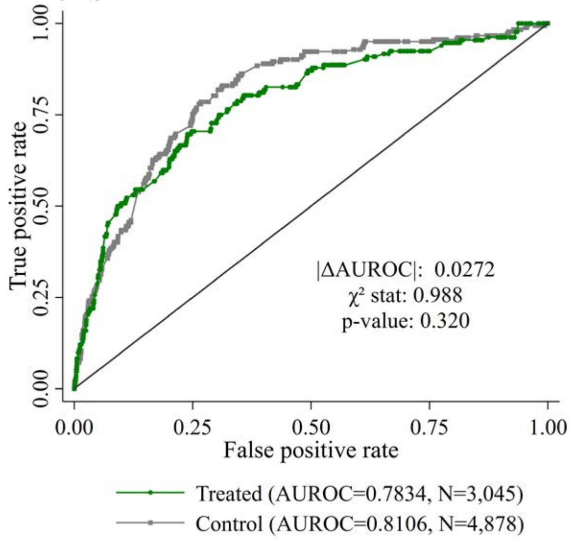
Figure IA.V. Robustness: Differences in Predictive Quality of PDs before and after eInvoicing, by Adoption Cohort. Relationships Starting in 2019 or Earlier.

Notes: This figure replicates the AUROC-based analysis in Figure 5 for bank-firm relationships that begin in 2019 or earlier. The top row reports AUROCs for invoice firms, and the bottom row for non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

Invoice Firms

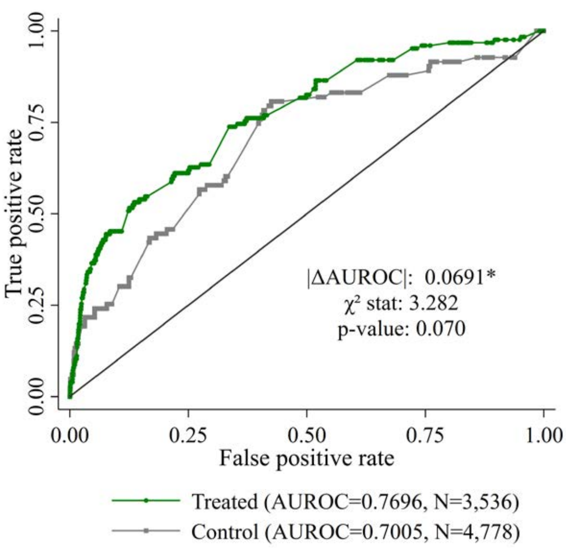


(a) 2021

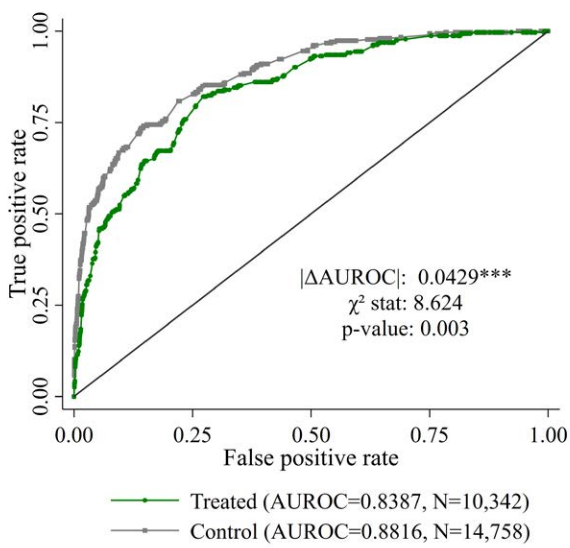


(b) 2022-2024

Non-Invoice Firms



(c) 2021



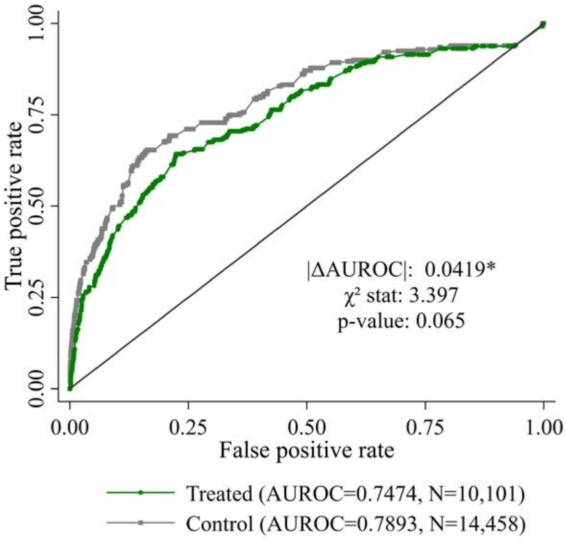
(d) 2022-2024

Figure IA.VI. Robustness: Differences in Predictive Quality of PDs before and after eInvoicing, by Adoption Cohort. Relationships Starting in 2020–2021.

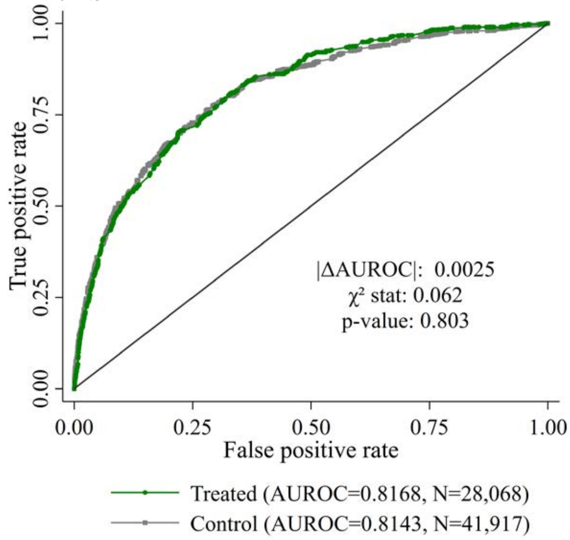
Notes: This figure replicates the AUROC-based analysis in Figure 5 for bank-firm relationships that begin in 2020-2021. The top row reports AUROCs for invoice firms, and the bottom row for non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

C.3.4 Samples without ICO-guaranteed or Moratoria-related Loans

INVOICE FIRMS

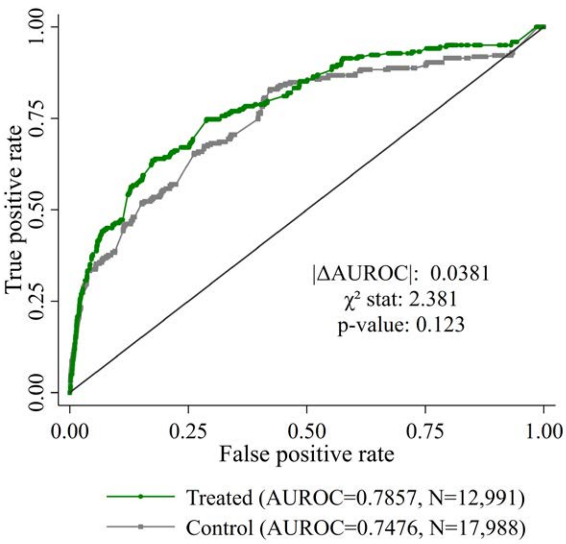


(a) 2021

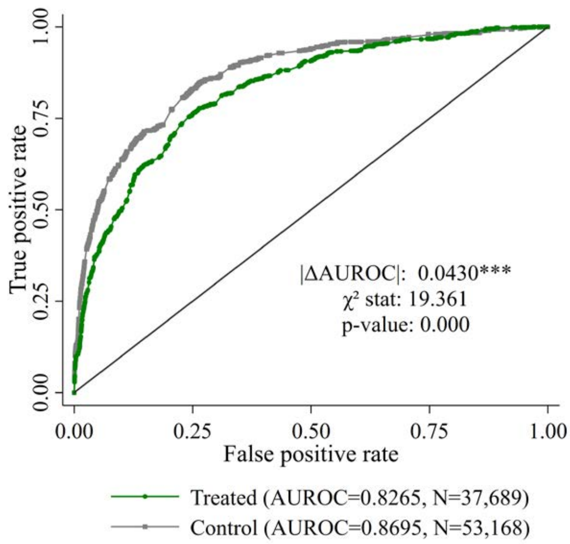


(b) 2022-2024

NON-INVOICE FIRMS



(c) 2021



(d) 2022-2024

Figure IA.VII. Robustness: Differences in Predictive Quality of PDs before and after eInvoicing, Dropping ICO-Guaranteed Loans, and the Ones Subject to the Moratoria.

Notes: This figure replicates the AUROC-based analysis in Figure 5 after dropping all bank-firm-quarter observations in which credit consists solely of ICO-guaranteed loans or loans affected by the default moratoria. The top row reports AUROCs for invoice firms, and the bottom row for non-invoice firms. The larger the area under the curve, the higher the accuracy of the estimated PDs.

C.4 Credit Analysis with AUROC Matching Procedure

In this section, we repeat our main credit analysis on a sample of firms obtained using the same matching procedure as in the AUROC-based analysis. Specifically, we match bank-firm-quarter observations across treated and control groups, applying the procedure described in Section 5.2.4 and employing the same set of variables, except for banks' IRB PD estimates for the firm. The results are reported in Table IA.XV, and they are consistent with our main analysis.

Table IA.XV. Robustness: Matched Samples

This table reports estimates of β from regressions estimated separately for invoice firms and non-invoice firms over matched samples from January 2021 to December 2024, using Poisson Pseudo-Maximum Likelihood. The unit of observation is a bank-firm-month, and we estimate:

$$\mathbb{E}[Y_{f,b,t} \mid \cdot] = \exp\left\{\alpha_{i,s,t} + \alpha_{f,b} + \alpha_{b,t} + \beta \cdot \mathit{Post}_t \times \mathit{Treat}_p + \Gamma_f \cdot X_{f,t-1} + \Gamma_p \cdot X_{p,i,t}\right\}.$$

$Y_{f,b,t}$ is the amount of credit extended by bank b to firm f in month t. For invoice firms, the dependent variable is invoice-based credit in column 1, non-invoice credit in column 2, and total credit in column 3. For non-invoice firms, the dependent variable is total credit in column 4. We cannot estimate specifications with invoice-based credit as the dependent variable for non-invoice firms because, by construction, these firms have no positive invoice-based credit in the pre-reform period. Post_t is an indicator equal to one from January 2022 onward. Treat_p is an indicator equal to one for firms headquartered in treated provinces. The specification includes industry×size×month, firm×bank, and bank×month fixed effects. $X_{f,t-1}$ denotes the vector of previous-year firm controls, which includes firm size, leverage, liquidity, tangibility, profitability, age, the share of bank-firm credit guaranteed by ICO, and an indicator for whether the firm is in default with that bank. $X_{p,i,t}$ denotes Covid controls, which include the number of employees covered by the ERTE temporary employment adjustment scheme in the firm's province-industry-month cell and the Google Mobility index at the province-month level. All variables are defined in Table IA.I. Standard errors (in parentheses) are clustered at the province×month level. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Invoice Firms			**Non-Invoice Firms**
	Invoice	Non-Invoice	Total	Total
	(1)	(2)	(3)	(4)
Post×Treated	0.0366***	0.0086***	0.0086***	-0.0125***
	(0.0090)	(0.0031)	(0.0032)	(0.0024)
Industry×Size×Month FE	Yes	Yes	Yes	Yes
Firm×Bank FE	Yes	Yes	Yes	Yes
Bank×Month FE	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Covid Controls	Yes	Yes	Yes	Yes
Obs.	246,877	360,738	369,462	575,027
Pseudo R-Squared	0.75	0.93	0.93	0.97

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