BigTech Credit and Platform Economic Activity*

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Abstract

BigTech firms increasingly integrate financial services into their platform ecosystems, raising a central question: how this integration affects firms' core business activity. Using a large-scale field experiment at one of the world's largest e-commerce platforms (over 2.5 million individual-month observations), we estimate the causal effects of marginal expansions in access to embedded credit. We find that credit access increases user economic activity—raising intensity and persistence, stabilizing demand, and expanding consumption scope—with little evidence of consumption upgrading or increased delinquency. Our results show that embedded credit reinforces the platform's core operations, highlighting complementarities in modern digital ecosystems.

Keywords: BigTech, Consumer Credit, FinTech Lending, Field Experiment, Household Finance, Credit Supply

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1. INTRODUCTION

A central question in economics concerns which activities should be organized within firm boundaries rather than through arm's-length market transactions (Coase, 1937; Grossman and Hart, 1986; Hart and Moore, 1990). This question has become newly important in the digital economy, where large technology (BigTech) companies increasingly integrate financial services into their platform ecosystems. Unlike traditional financial technology (FinTech) firms, BigTech firms originate outside finance and build on established platforms and extensive customer networks. They embed financial services within their broader digital ecosystems, creating organizational forms that span technology and finance and attract significant regulatory attention worldwide.[1] This development raises a broader firm-boundary question in the digital economy: when financial services are brought inside a platform firm, how do they reshape economic activity within the firm's core business?

This paper studies this question in the context of BigTech credit, specifically a platform's embedded credit product (hereafter, embedded credit), a prominent financial service increasingly provided by BigTech firms. By 2019, global BigTech lending volume had grown to more than twice the combined volume of all other FinTech firms (Cornelli et al., 2023). This expansion has been widely attributed to advantages rooted in BigTech firms' core platforms, such as proprietary data and large user networks (Berg, Fuster, and Puri, 2022; Stulz, 2022; Liu, Lu, and Xiong, 2025). Yet this one-sided view is incomplete. When financial services are brought inside the firm, they reshape activity in the core business, creating feedback effects that are central to the economics of platform firms. We therefore examine how the provision of embedded credit affects economic activity within the platform's core operations.

Studying the effects of embedded lending on core businesses poses two challenges. First, it requires granular data not only on credit applications in a BigTech firm's lending business but also on user activity in its core business. Second, lending decisions are highly endogenous: users self-select into applying for credit, and the firm approves applications based on user characteristics. As a result, simple comparisons between credit recipients and non-recipients yield biased estimates of credit's impact. We tackle these challenges using unique data combined with a field experiment conducted by a major BigTech firm.

[1] See Consumer Financial Protection Bureau (2023), European Supervisory Authorities (2024), Financial Conduct Authority (2023), Financial Stability Board (2019), and Tierno (2022).

Our setting provides a key advantage: we observe user-level activity across both the firm's e-commerce and lending businesses. The firm's lending arm provides unsecured revolving credit for purchases on its e-commerce platform, allowing us to link credit decisions to subsequent behavior. The data cover detailed user characteristics and activity, including demographics, browsing and purchasing behavior, payment outcomes, and credit management. This allows us to measure how credit affects economic activity within the firm's core business, including activity intensity and persistence, demand stability, and consumption scope.

The field experiment we use for identification is a reject inference (RI) program. The firm employs an automated underwriting system (hereafter, the AI model) to make credit approval decisions. The program is designed to generate training data for the AI model. Typically, the model is employed to evaluate *all applications* but is trained only on *approved applications* because their delinquency outcomes can be observed. This mismatch between usage and training may bias the model. To address this concern, the firm launches the RI program, in which a *random* subset of applicants rejected by the AI model is instead approved. The firm then observes delinquency outcomes for these RI-approved applicants to infer counterfactual outcomes for the remaining rejected pool and incorporates this information into the model's training data.

We exploit this random assignment of credit access within the initially rejected pool for causal inference. Specifically, we implement a stacked cohort difference-in-differences (DiD) design by grouping applicants who apply in the same week into cohorts. We designate AI-rejected but RI-approved applicants as the treated group and the remaining AI-rejected applicants as the control group. Our estimates therefore identify the causal effects of expanded access to embedded credit among applicants who would otherwise be denied credit under the current underwriting regime. This group is economically central because it is the set of borrowers directly affected by marginal changes in credit supply and regulatory constraints.

The success of a BigTech firm's core business depends on the level and composition of economic activity on the platform—captured by activity intensity and persistence, demand stability, and the scope of consumption in our setting. Accordingly, we examine how access to the platform's embedded credit product affects economic activity along these dimensions.

The first dimension we study concerns activity intensity and persistence. Activity intensity reflects how often users browse, purchase, and spend on the platform, whereas persistence captures whether these interactions continue over time. This dimension is fundamental to platform performance because it affects not only current revenue but also the accumulation of user histories, the richness of behavioral signals, and the platform's ability to generate value in the long run. We find that, relative to controls, access to embedded credit leads treated users to increase visit frequency and order frequency by 22.5% and 36.6%, respectively. Monthly net spending in the core business rises by 33.1%, driven primarily by higher order volume rather than changes in price sensitivity (discounts), impulsiveness (visit-to-purchase conversion and cancellations), or basket size (spending per order). Besides, access to embedded credit increases the likelihood of repeat purchases by 29.6%.[2]

The second dimension we examine is demand stability. Beyond activity levels, the stability of consumer demand is central to the operational efficiency and valuation of a BigTech firm's core business. We rely on user visits, order placement, and spending as proxies for demand. Volatility in individual browsing patterns can hinder the platform's ability to learn user preferences, reducing recommendation quality and response efficiency (Wei et al., 2025). Similarly, fluctuations in order placement increase demand uncertainty, complicating inventory and logistics planning and raising the risk of stockouts or excess inventory (Chuang, Oliva, and Heim, 2019). Instability in individual spending also amplifies revenue volatility, which can disrupt working capital allocation, increase idiosyncratic risk, and constrain firm growth (Minton and Schrand, 1999; Irvine and Pontiff, 2009; Alnahedh, Bhagat, and Obreja, 2019). We therefore examine whether credit provision stabilizes consumer activity as reflected in visit frequency, order frequency, and spending. We find that access to embedded credit reduces the volatility of users' visit frequency, order frequency, and spending in the core e-commerce business by 6.2%, 8.1%, and 5.5%, respectively.

The third dimension we study is the scope of users' consumption in the core business, a dimension that is particularly relevant for the BigTech firm we examine. E-commerce platforms' comparative advantage over traditional brick-and-mortar retailers lies in their broad assortments (Brynjolfsson, Hu, and Smith, 2003; Quan and Williams, 2018). Whether this advantage translates into business value depends on the platform's ability to expand the range of products and categories purchased by users. We measure consumption scope using four metrics: the size (i.e., number of unique items) of a user's monthly consumption set across products, brands, shops, and categories. We find that expanding access to embedded credit increases users' consumption scope in the core business across all measures by approximately 34.4% to 42.6%.

[2] In Poisson models, percentage effects are calculated as $\exp(\beta) - 1$, where β is the estimated coefficient.

Collectively, the evidence indicates that BigTech financial services complement the e-commerce business by increasing activity intensity and persistence, stabilizing demand, and expanding consumption scope. Event-study analyses show that these effects are not driven by pre-treatment differences but instead emerge immediately after consumers gain access to embedded credit. However, these complementarities may not persist if credit access induces imprudent spending (Bertrand and Morse, 2016) or if borrowers exhibit elevated delinquency risk. We therefore examine these two risks.

First, using detailed order-item-level data and specifications that hold product category constant, we find little evidence that consumers shift toward more expensive items, choose higher-end brands, or shop at more upscale stores following credit access. Second, the 90-day delinquency rate averages 0.9% over the sample period, below the 1.4%–3.3% range reported for major commercial bank credit cards. Moreover, consumers who substantially increase spending after receiving credit do not exhibit higher delinquency rates than those with more modest spending increases.[3] Overall, we find little evidence of consumption upgrading or repayment risk.[4]

Lastly, we present additional analyses to shed light on the mechanisms and economic significance of the observed effects. First, we show that the treatment effects are consistent with a relaxation of consumers' liquidity constraints, which both increases and stabilizes activity on the platform. Second, we examine the role of downstream marketing following credit approval and find that differential marketing exposure is unlikely to be the primary driver of the observed effects. Importantly, we estimate the marginal propensity to consume (MPC) out of embedded credit and find it comparable to, or larger than, estimates for credit cards in the literature. Finally, while we cannot directly observe cross-platform substitution following credit access, this distinction is not central for our within-firm research question. However, it is important for welfare interpretation, as the effects we document may reflect a combination of new consumption and reallocation across platforms.

Our paper makes several contributions. First, we contribute to the literature on BigTech firms' expansion into finance (Stulz, 2022; Tierno, 2022) by providing direct evidence on how

[3] Additional analyses reveal that users with larger credit-induced increases in spending exhibit more sophisticated liquidity management behaviors, including choosing longer loan terms, selecting products with lower interest rates for installment plans, demonstrating higher repayment capacity, and engaging more actively in refinancing.

[4] We caveat this conclusion by noting that it holds under the platform's current credit management regime, including credit limit and interest rate assignment and adjustment.

financial services brought within the firm feed back into platform activity. While prior work shows how platform advantages enable BigTech firms to enter finance (Brunnermeier and Payne, 2025; Li and Pegoraro, 2025; Ouyang, 2026), much less is known about how embedding these services within platform firms feeds back into their non-financial operations.[5] We show that access to the platform's embedded credit product increases economic activity—raising user activity intensity and persistence, stabilizing demand, and expanding consumption scope. These effects arise because embedded credit relaxes users' liquidity constraints *within the platform environment*, thereby deepening their interaction with the platform. Our findings provide causal evidence on how complementarities between financial and non-financial operations arise within modern digital ecosystems. While our setting is an e-commerce platform, the mechanism we document—financial services affecting platform activity through changes in user behavior—is likely relevant to other digital platforms that embed finance.

Second, our paper complements recent work on BigTech credit to small and medium-sized enterprises, which shows that such credit supports firm resilience and performance (Chen et al., 2022; Hau et al., 2024; Liu, Lu, and Xiong, 2025). We instead focus on the other major customer segment of BigTech firms, individual consumers. We find that granting access to BigTech credit within the platform ecosystem increases activity intensity and persistence, reduces demand volatility, and expands consumption scope. In doing so, we show that embedded finance enhances a BigTech firm's core business beyond the direct revenues generated by lending.

More broadly, we contribute to the literature on the real effects of FinTech. Recent studies show that FinTech can stimulate entrepreneurial growth and merchant profitability (e.g., Agarwal et al., 2019; Gopal and Schnabl, 2022; Berg et al., 2025) and improve consumer financial decisions (e.g., D'Acunto, Prabhala, and Rossi, 2019; Fuster et al., 2019; Parlour, Rajan, and Zhu, 2022; Gargano and Rossi, 2024; Rossi and Utkus, 2024). Other research, however, cautions that FinTech may fuel excessive consumption and over-indebtedness (Bu et al., 2022; Buchak et al., 2018; Chava et al., 2021; Di Maggio and Yao, 2021; Wang and Overby, 2022). We contribute to this literature by showing that BigTech lending, as a novel form of FinTech, reinforces the firm's core

[5] Our paper relates to contemporaneous work by Berg et al. (2025), who study how buy-now-pay-later serves as a tool for price discrimination and increases merchant sales. Our focus differs in two key respects. First, we examine the integration of financial services with the core business within BigTech firms. Second, this integration operates through a feedback loop driven by the accumulation of user histories (continued online shopping and revolving credit) rather than through transaction-level credit provision used primarily as a pricing instrument at the point of sale.

operations without inducing a systematic shift toward more expensive goods or higher delinquency rates.

Finally, our evidence on the synergy between financial services and core businesses within BigTech firms has important policy implications. Regulators worldwide are increasingly attentive to BigTech firms' economic significance and conglomerate structure spanning technology and finance. This structure raises questions about whether entity-based oversight and greater coordination between financial and non-financial authorities (e.g., banking and financial, antitrust and competition, data protection, cybersecurity, and consumer protection) are needed (see Section 2.1 for details).[6] While regulatory design ultimately rests with policymakers, informed policymaking requires credible evidence on the existence and magnitude of the interactions between BigTech firms' financial and non-financial operations. Our findings provide such evidence by quantifying an ecosystem-level feedback loop, which is relevant to debates on whether oversight should be calibrated at the ecosystem level and whether coordination across financial and non-financial regulators is warranted in assessing BigTech firms' systemic impact.

2. INSTITUTIONAL BACKGROUND

2.1 BigTech Firms' Financial Expansion

BigTech firms play a central role in the global economy. According to Tierno (2022), they operate four of the five most-visited websites in the United States (U.S.), serve millions of consumers through mobile payment applications, and account for more than 70% of the U.S. cloud computing market. The average BigTech firm has a market capitalization of $1.51 trillion, and four of the ten largest U.S. companies are BigTech firms.

Over the past decade, these firms have expanded their presence in financial services. U.S.-based technology companies such as Alphabet (Google), Amazon, Meta (Facebook), Apple, and Microsoft offer products ranging from payments and credit to insurance, crowdfunding, and stablecoins. Major Chinese firms such as Alibaba and Tencent have expanded further into areas including asset management, while firms such as NTT Docomo and Rakuten in Japan, Vodafone in the United Kingdom (U.K.), and Samsung in South Korea are also active in financial services.

BigTech firms that provide financial services are often considered a subset of FinTech, but

[6] See Agur, Ari, and Dell'Ariccia (2025) for a model of how BigTech firms exploit payment data for credit advantages, creating privacy trade-offs and a rationale for regulating intrusive practices.

benefit from three distinct advantages (Financial Conduct Authority, 2023). First, they have access to vast customer networks and proprietary data generated on their non-financial platforms. Second, they have stable access to low-cost capital, supported by their core business operations. Third, their technological infrastructure enables efficient processing and analysis of large volumes of data. As illustrated in Figure 1, these strengths in BigTech firms' non-financial businesses allow them to launch and scale financial services rapidly, which can in turn reinforce their core operations (Financial Stability Board, 2019).

However, these features of BigTech firms pose challenges for regulators. Traditional regulatory frameworks are typically activity-based (e.g., banking regulations for banks, telecommunications rules for telecom firms), whereas BigTech firms operate across multiple sectors within integrated groups, potentially creating regulatory gaps.[7] In response, regulators worldwide are increasingly adopting entity-based approaches that impose requirements at the group level, regardless of how activities are structured across subsidiaries.[8] Effective oversight, however, requires a clear understanding of the interactions between BigTech firms' financial and core operations.

2.2 The BigTech Lending Business

Lending is a major financial service provided by BigTech firms. BigTech lending has expanded rapidly since 2013, growing from \$11 billion in 2013 to \$397 billion in 2018, exceeding the total credit issued by other FinTech firms (\$297 billion) and further widening the gap in 2019 (\$572 billion versus \$223 billion) (Cornelli et al., 2023). In China alone, BigTech credit grew by 37% in 2021, nearly triple the 13% growth rate of bank credit (De Fiore, Gambacorta, and Manea, 2024). In practice, BigTech firms extend credit to users either directly through financial subsidiaries or in partnership with traditional financial institutions.[9]

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[7] For example, a BigTech firm may comply with banking regulations in providing payment services while using the resulting data to gain advantages in other markets, such as e-commerce or advertising. Similarly, a BigTech firm operating in search or social media may adhere to privacy regulations yet still leverage its data to offer preferential credit terms or build off-balance-sheet exposures, potentially distorting credit markets and amplifying systemic risk.

[8] The U.S. has introduced legislative proposals and antitrust reforms targeting anti-competitive practices by large, multi-sector platforms. China has implemented comprehensive regulations for internet companies, requiring firms such as Ant Group and Tencent to establish financial holding companies and comply with rules for nonbank payment providers. Similarly, the European Union's Digital Markets Act and Digital Services Act introduce extensive requirements for the governance, risk management, and supervision of BigTech "gatekeepers" (see also Crisanto et al., 2021; Bains, Sugimoto, and Wilson, 2022; Tierno, 2022).

[9] Examples include Ant Group (Alibaba) and WeBank (Tencent) in China; Amazon's lending operations in the U.S., the U.K., and other countries; Google's lending initiatives in India; Vodafone and Safaricom's joint services in Africa; Grab and Go-Jek's offerings in Southeast Asia; and Mercado Libre's platform in Latin America.

The BigTech firm we study is headquartered in China, and its core business is e-commerce. Leveraging advanced data analytics and logistics technologies, the firm operates nationwide in mainland China and offers a broad range of products spanning consumer electronics, home appliances, groceries, apparel, cosmetics, and books. With annual revenues exceeding $100 billion, it ranks among the world's largest online retail platforms.[10]

The firm operates a financial services division alongside its e-commerce business, offering unsecured revolving credit to e-commerce platform users. Customers apply for credit through a one-click online process. The firm's proprietary AI model evaluates applications and, if approved, assigns credit limits and interest rates within seconds. Credit usage is fully integrated into the platform, allowing users to select it at checkout and repay in installments. Monthly statements are issued by the firm, and users may repay in full or roll over balances through 3-, 6-, 12-, or 24-month installment plans. The firm regularly reviews users' credit performance to adjust limits and interest rates.

3. THE SETTING FOR IDENTIFICATION

3.1 Reject Inference

Figure 2 illustrates the architecture of the automated underwriting system and the role of the RI program. The firm's proprietary AI model first classifies credit applicants as approved or rejected. For approved applicants (left branch), realized repayment outcomes (compliance or default) are observed and used as training data for the model. Because these outcomes are only observed for *approved applicants*, the training data is a selected sample, introducing bias when the model is applied to the *full applicant pool* (Anderson, 2007; Thomas, Crook, and Edelman, 2017).

To address this selection problem, the firm implements the RI program (right branch), under which a *random subset* of initially rejected applicants is approved for credit. The realized repayment outcomes of these RI-approved applicants provide information on the performance of rejected applicants and enable the firm to infer counterfactual delinquency risk for those who remain rejected. The firm incorporates both observed outcomes from approved applicants and inferred outcomes for rejected applicants into model training, thereby mitigating model bias.

The RI program also allows the firm to evaluate its credit policy. Credit providers face a

[10] See the ranking of publicly traded e-commerce firms by revenue available at: https://companiesmarketcap.com/e-commerce/largest-e-commerce-companies-by-revenue/.

trade-off between the benefits of approving more applicants and the costs associated with expanded lending. In the BigTech context, this trade-off is more nuanced because approved users are not only borrowers in the lending business but also customers of the firm's core business. The benefits of approval therefore extend beyond interest income to include increased spending, greater engagement, and potentially stronger customer retention on the platform. At the same time, the costs go beyond loan defaults and funding expenses, as expanded credit may generate operational frictions for the core business, such as higher customer service costs, increased product returns, and reputational risks if credit is extended to unsuitable users. The RI program provides a structured setting for the firm to assess this trade-off across the distribution of applicants who would otherwise be excluded, spanning both those near the approval threshold and those further below it.

The firm implemented this field experiment (the RI program) from April to September 2020. During this period, its proprietary AI model continued to make real-time credit decisions, while applications rejected by the model were subsequently processed through the RI program, which randomly determined whether to approve them for credit. Approvals through RI and the AI model are indistinguishable to applicants in both format and timing.

Figure IA1 in the Internet Appendix presents weekly statistics for the program. Panel A plots the weekly volume of AI-rejected applications, which remains stable over time, with no clustering around specific events.[11] This pattern mitigates concerns that time-specific shocks drive our results. Panel B plots the weekly average internal credit score of AI-rejected applicants. Scores ranged narrowly from 677 to 685, indicating stable applicant quality over time. The firm initially adopted a conservative credit policy during the experiment.[12] The RI approval rate increased from 14% to 27% over the sample period (Panel C), average initial credit limits rose from 289 to 341 RMB (Panel D), and monthly interest rates declined from 1.17% to approximately 1.12% before stabilizing (Panel E).[13]

3.2 Identification Strategy

[11] For confidentiality, weekly volumes are normalized by the total number of AI-rejected applications during the experiment and reported as percentages.
[12] Week-by-week analyses indicate that our results are robust across both early and later phases of the RI program (see Section 5.1).
[13] For context, average monthly spending on the e-commerce platform in our sample is 190 RMB, while monthly per capita disposable income in China in 2020 was 2,682 RMB. In the same year, China's central bank capped credit card annual interest rates between 12.775% and 18.25%, and FinTech credit products, such as peer-to-peer lending, carried annual rates ranging from 8% to 24% (Deer, Mi, and Yu, 2015).

To achieve its objectives, the RI program features quasi-random assignment, similar to the field experiments in Aydin (2022) at a retail bank and Karlan and Zinman (2010) at a consumer finance company. First, credit approval within the RI program is randomized among AI-rejected applicants. Second, credit access is the sole experimental intervention.[14] Third, applicants are unaware of whether approval results from the AI model or the RI program, as notifications are identical in format and delivered within seconds of application. This design minimizes the scope for behavioral responses to experimental awareness. Fourth, the staggered timing of approvals under the RI program helps mitigate potential confounding factors, as any such factor would need to vary over time and across individuals in a manner aligned with the timing of approvals.

We leverage these features to identify how access to the firm's embedded credit product affects activity on its e-commerce platform.[15] Specifically, we implement a stacked cohort DiD design. Applicants who apply in the *same week* and are rejected by the AI model are grouped into the same cohort. Within each cohort, AI-rejected but RI-approved applicants constitute the treatment group, while the remaining rejected applicants serve as the control group. Each applicant appears in only one cohort and is not reused across cohorts, which avoids bias from time-varying treatment effects (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022).[16] We further exclude applicants who switch treatment status to prevent contamination of the estimates.[17] Within each cohort, we compare changes in core business activity before and after credit approval for treated applicants relative to controls. Formally, we estimate the following model using a panel of individual-month observations:

$$Outcome_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t} \quad (1)$$

where c indexes cohorts, i consumers, and t calendar months. The term $\alpha_{c,i}$ denotes cohort-by-

[14] All platform-initiated actions other than credit approval, such as credit limit and interest rate assignment, targeted advertising, and customer support, remain independent of the RI program and follow standard operating procedures. Accordingly, the estimated treatment effects of BigTech credit are conditional on the firm's existing operational practices, including its policies for adjusting credit limits and interest rates. We further discuss the implications of this experimental design for interpreting the causal effects in Section 7.3.

[15] Reject inference is a standard tool used by credit providers that rely on scoring models and is widely adopted by both FinTech firms and traditional banks to improve model performance (Anderson, 2007; Thomas, Crook, and Edelman, 2017). Despite its widespread use, the exogenous variation generated by reject inference has not been exploited for causal inference in empirical research. To our knowledge, this study is the first to do so.

[16] Treated applicants in early cohorts are not reused as controls in later cohorts. Likewise, control applicants are not reused across cohorts, even if they submit new applications and get rejected in subsequent weeks.

[17] In rare cases, treated applicants had previously held and closed BigTech credit prior to the RI program, and some control applicants in early cohorts were subsequently approved by either the AI model or the RI program during the sample period.

individual fixed effects that control for time-invariant consumer characteristics, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects that capture time-varying shocks common to all consumers within a cohort.[18] $Outcome$ represents a set of platform-side outcome variables that capture the extent to which the firm's lending business feeds back into its core business. $Treated$ is an indicator for RI-approved applicants, and $Post$ is an indicator equal to one for the application month and subsequent months. The coefficient β captures the average treatment effect of being granted access to the firm's embedded credit product on the outcome variable. We estimate Equation (1) using Poisson regressions to obtain valid semi-elasticity estimates (Cohn, Liu, and Wardlaw, 2022; Chen and Roth, 2023).[19, 20] Standard errors are clustered at the cohort-by-individual level to account for serial correlation. Definitions of all variables are provided in Appendix A. In specifications using order-item-level data, we adjust the outcome variables and fixed effects accordingly, as described in the relevant sections.

A key implication of this design is that our estimates pertain to applicants in the reject-inference pool. Accordingly, our results should be interpreted as the causal effects of expanding embedded credit access to applicants who would otherwise be denied credit, rather than the effects of universally granting credit to all platform users. This group is economically important because it is the population most directly affected by marginal changes in credit supply and regulatory constraints on BigTech lending. Accordingly, our results speak to the question of firm boundaries in the context of embedded credit in the digital economy.

4. SAMPLE CONSTRUCTION AND DESCRIPTION

4.1 Sample Construction

We begin our sample construction with applicants who applied for BigTech credit between April and September 2020 and were initially rejected by the AI model (i.e., the RI pool). The firm provides access to a random sample of this pool. We further restrict the sample to applicants with

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[18] Because applicants are not reused across cohorts, cohort-by-individual fixed effects are equivalent to individual fixed effects in our specification.

[19] These studies show that using log-transformed nonnegative dependent variables can introduce bias when the underlying variables are right-skewed or contain a large proportion of zeros. In our setting, outcomes such as monthly visits, order counts, and spending frequently take the value zero. We therefore use Poisson regressions as our baseline specification. Our results are robust to estimation using ordinary least squares (OLS).

[20] In Poisson regressions with a log link, coefficients are interpreted as semi-elasticities: a one-unit increase in the regressor changes the dependent variable by $\exp(\beta) - 1$ in percentage terms. Throughout the paper, we apply this transformation and report effect sizes as percentage changes.

complete demographic information (e.g., gender, account age, user age, and location) who are active on the platform (Agarwal, Qian, and Zou, 2021).[21] For each applicant, we construct a monthly panel from April 2019 to March 2021 by aggregating activity within the BigTech ecosystem. Each individual contributes up to 24 monthly observations.[22, 23]

We further implement one-to-one matching between treated and control applicants within each weekly cohort. The RI experiment is designed to randomize approval among AI-rejected applicants; however, the exact operational algorithm governing assignment is proprietary and not disclosed to us. We therefore conduct balance tests on applicants' demographics, credit demand and risk, and online shopping behavior. Consistent with random assignment, most pre-application differences between treated and control applicants are economically small, although some are statistically significant due to the large sample size.[24] To improve covariate balance and estimation precision, we apply nearest-neighbor propensity score matching without replacement within each cohort, pairing each treated applicant with one control applicant. Our main results are robust to alternative specifications without matching. Appendix B provides details on the balance tests and matching procedure.

Our final sample consists of 59,904 treated and 59,904 control applicants, with approximately 1.29 million individual-month observations in each group.[25] In some analyses, we aggregate user activity to the weekly level to construct a user-week panel that captures more immediate responses. For a 10% random subsample of applicants in the main sample, we obtain order-item-level purchase histories for more detailed analysis (hereafter, the detailed subsample).

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[21] We define consumers as active if they have browsing activity in more than half of the pre-application months. Our results are robust to alternative definitions and to dropping this filter (results untabulated).

[22] For applicants who apply in the first month of the RI program, we observe up to 12 months before and 12 months after application. For those who apply in the last month of the RI program, we observe up to 17 months before and 7 months after application. Some consumers signed up for their e-commerce accounts after April 2019; for these users, observations begin in the month of account registration and extend through March 2021, resulting in fewer than 24 monthly observations.

[23] This lengthy sample period is important for our analysis. Short pre-event periods typically provide low power to assess the parallel trends assumption (Roth, 2022; Hribar, McInnis, and Wang, 2025), while short post-event periods may not be long enough for time-varying treatment effects to stabilize, potentially leading to underestimated effect sizes (Goodman-Bacon, 2021; Baker et al., 2022). Our sample includes up to 17 months of pre-event and up to 12 months of post-event data, providing a sufficiently long pre-treatment window to assess baseline differences between treated and control groups, allowing us to distinguish between short-term and persistent effects, and ensuring adequate post-treatment exposure for potential adverse outcomes to materialize.

[24] For example, the difference in internal credit scores is statistically significant but only 4.5 relative to a mean of approximately 686, indicating a negligible economic difference.

[25] The number of observations varies across tables because variables are measured at different levels, over different samples, or across different time periods for different analyses. In regression tables, the number of observations may differ further because singleton observations are dropped.

These samples are described in detail in the relevant sections.

4.2 Applicant Characteristics and Purchase Composition

Panel A of Table 1 reports basic demographic characteristics of the sample applicants. Approximately 66% of applicants are male, with an average age of 29 and an average platform tenure (i.e., account age) of 31 months. Figure IA2 in the Internet Appendix provides additional details on the geographic, age, and consumption composition of the sample. The geographic distribution broadly aligns with China's population size and mobile internet usage, with Guangdong, Shandong, Sichuan, Jiangsu, and Henan accounting for the largest shares.[26] The age distribution is concentrated among younger users, with individuals aged 20–29 representing the largest group and those over 60 the smallest.[27] These patterns suggest that the sample broadly resembles China's internet user population along observable dimensions such as geographic and age. In terms of consumption, electronics and digital products account for the largest share (24.7%), followed by food and beverage (15.9%), home and living (15.7%), and beauty, personal care, and health (15.7%), while jewelry and watches represent the smallest share (1.5%). Additional user characteristics are discussed in the relevant sections.

5. EFFECTS OF EMBEDDED CREDIT ON PLATFORM ACTIVITY

5.1 Effects on Activity Intensity and Persistence

The first way embedded credit may reinforce a platform's core business is by changing how intensely and how persistently economic activity occurs on the platform. Activity intensity captures how frequently users browse and purchase, as well as how much they spend on the platform in a given period. Persistence captures whether those interactions translate into continued platform use over time. These dimensions are central to the performance of digital platforms because they shape not only contemporaneous revenue but also the accumulation of user histories, the quality of behavioral signals, and the platform's ability to sustain ongoing engagement.

We therefore examine four primary outcomes that capture activity intensity and persistence: $Visits$, defined as the number of product-page visits in a given month (i.e., web traffic); $Orders$,

[26] This geographic distribution is broadly consistent with 2019 provincial population data from the National Bureau of Statistics and mobile internet user data from the China Digital Economy Research Database.

[27] This pattern is consistent with the age distribution of internet users reported by the China Internet Network Information Center (2019). It also reflects the fact that the "post-90s" (born after 1990) and "post-00s" (born after 2000) generations have the highest rates of online shopping in China (China Internet Network Information Center, 2024).

the number of orders placed in that month; $Spending$, net monthly spending on the platform; and $Retention$, an indicator equal to one if a consumer makes a purchase in a given month (i.e., month 0) and makes at least one additional purchase in months 3 to 5 thereafter.

Summary statistics for these variables are reported in Table 1, Panel B. On average, users make 63.5 product-page visits per month. Conditional on purchasing, users place 1.4 orders per month. Average net monthly spending is 192 RMB. Approximately 29% of users make a purchase and return within the subsequent rolling-window quarter.

Table 2 reports generalized DiD estimates from Poisson regressions based on Equation (1). Credit access increases browsing activity, order placement, net monthly spending, and the likelihood of repeat purchases by 22.5%, 36.6%, 33.1%, and 29.6%, respectively.[28]

Figure 3 presents dynamic treatment effects relative to the application month. Pre-treatment coefficients are centered around zero across all outcomes, supporting the parallel trends assumption. For $Visits$, $Orders$, $Spending$, and $Retention$, we observe immediate and persistent increases following credit approval, with effects remaining economically and statistically significant for up to 12 months (and up to six months for $Retention$).

To better understand the drivers underlying the effects on intensity and persistence, we also examine $Conversion$, the fraction of visits that result in purchases; $Discount$, discounts as a share of order value; $Cancellation$, cancellations as a share of order value; and $SpendingPerOrder$, net spending per order after accounting for discounts and cancellations. Event studies (Figure IA4 in the Internet Appendix) indicate short-term pulses but no persistent changes in impulsiveness (conversion and cancellations), price sensitivity (discounts), or basket size (spending per order).[29]

Overall, the evidence indicates that access to the firm's embedded credit strengthens the platform's economic activity by increasing user activity intensity and persistence, rather than by materially altering impulsiveness, price sensitivity, or inducing larger per-order purchases. The main effect is not that users shift toward more expensive baskets, but that they return more often, place more orders, and spend more frequently on the platform. This pattern is more consistent with

[28] We also estimate treatment effects separately by cohort and plot the estimates. As presented in Figure IA3 in the Internet Appendix, effect sizes are stable over time and do not appear to be driven by a small number of cohorts.

[29] $Discount$ declines slightly on impact but quickly reverts to baseline. $Cancellation$ shows a brief spike but quickly returns to pre-treatment levels. $Conversion$ and $SpendingPerOrder$ are modestly elevated post-treatment but not statistically different from zero.

a liquidity-driven mechanism rather than increased consumer impulsiveness.[30]

5.2 Effects on Demand Stability

A second way embedded credit may reinforce the core platform is by stabilizing user demand. Volatility in consumer activity can impose substantial operational and financial costs on e-commerce platforms. Fluctuations in browsing behavior can reduce the informativeness of behavioral signals used to learn user preferences, potentially degrading recommendation quality and service responsiveness (Wei et al., 2025).[31] Variability in order placement complicates logistics and inventory planning, increasing the likelihood of stockouts or excess inventory (Chuang, Oliva, and Heim, 2019). Fluctuations in individual spending also amplify revenue volatility, which can disrupt cash flow management (Minton and Schrand, 1999), increase idiosyncratic firm risk (Irvine and Pontiff, 2009), and constrain investment and growth (Alnahedh, Bhagat, and Obreja, 2019). In contrast, more stable consumer activity facilitates effective user preference learning, improves demand forecasting, and supports more predictable fee revenue, thereby enhancing overall efficiency.

We measure volatility along these three dimensions, namely, browsing, ordering, and spending, using a user-week panel that spans 24 weeks before and 24 weeks after the credit application week.[32] For each week, volatility is defined as the standard deviation of activity over the prior 12 weeks, normalized by the corresponding mean. This normalization facilitates comparisons before and after credit access that shifts activity levels as shown in Section 5.1. We construct three measures: $VisitVolatility$, $OrderVolatility$, and $SpendingVolatility$ (see Table 1, Panel B for summary statistics).

Table 3 reports generalized DiD estimates based on Equation (1). Across all three measures, the interaction term ($Treated \times Post$) is negative and statistically significant at the 1% level. In economic terms, credit access reduces the volatility of visit frequency, order frequency, and spending by 6.2%, 8.1%, and 5.5%, respectively. Figure 4 presents dynamic treatment effects. Because volatility is constructed using a 12-week rolling window, we observe 12 pre-treatment

[30] Section 6 presents additional evidence that is inconsistent with consumer impulsiveness, and Section 7 provides direct support for the liquidity-driven mechanism.

[31] For example, personalized recommendation systems (e.g., the "similar products you may be interested in" section on an e-commerce product page) typically present a limited, ranked set of items. The size of this set is determined by both the computational constraints of the underlying algorithms and typical user browsing behavior. A sudden increase in browsing intensity may exhaust the available recommendation pool, potentially degrading recommendation quality.

[32] The observation window is chosen to capture immediate responses while maintaining a manageable sample size.

event weeks. We use week -1 as the reference period and omit the transition weeks in which volatility measures span both pre- and post-treatment periods. Pre-treatment coefficients are centered around zero, supporting the parallel trends assumption. Following credit approval, volatility declines immediately and remains persistently lower for at least 24 weeks (approximately six months).

Overall, BigTech credit reduces volatility in consumer activity across browsing, ordering, and spending, thereby making platform demand more stable. Smoother demand patterns support better algorithmic performance and improve inventory management and cash flow planning (e.g., Minton and Schrand, 1999; Chuang, Oliva, and Heim, 2019; Wei et al., 2025). Therefore, our findings suggest that embedded credit affects not only the level of economic activity but also its predictability, an important dimension through which financial integration can reinforce the platform's core operations.

5.3 Effects on Consumption Scope

A third way embedded credit may reinforce the core platform is by expanding the scope of users' activity within the ecosystem. While e-commerce platforms' advantage over traditional retailers lies in their broad product assortments (Brynjolfsson, Hu, and Smith, 2003; Quan and Williams, 2018), this advantage translates into business value only if users expand the range of products they purchase. Greater consumption scope also signals deeper economic integration within the platform, as the firm captures a larger share of consumer spending across product categories (Klemperer and Padilla, 1997; Thomassen et al., 2017).

We measure consumption scope using four user-month-level metrics constructed from order-item-level data (i.e., the detailed subsample): $SKUs$, $Brands$, $Shops$, and $Categories$. An SKU (stock-keeping unit) is a unique identifier assigned to each distinct product and represents the most granular level of product differentiation.[33] Each metric captures the number of unique products, brands, shops, or categories in a consumer's purchases within a given month.

Table 4 reports the effects of BigTech credit on these measures based on Equation (1). Credit access increases consumption scope across all four metrics by approximately 34.4%–42.6%, with effects that are both statistically and economically significant. Figure 5 presents the corresponding dynamic treatment effects. Pre-treatment coefficients are centered around zero,

[33] For example, each specification of an iPhone 16 (defined by its combination of attributes such as color, memory, and processor configuration) corresponds to a unique SKU.

supporting the parallel trends assumption. Following credit access, all four measures exhibit an immediate and persistent increase, with effects remaining stable throughout the 12-month post-treatment period.

Overall, the evidence indicates that BigTech credit expands the breadth of consumer purchases on the platform. These findings suggest that embedded financial services increase platform stickiness and deepen the linkage between the firm's financial and core businesses.

6. ARE THE POSITIVE FEEDBACK EFFECTS SUSTAINABLE?

While the findings in the previous section show that embedded credit reinforces the BigTech firm's core operations, these benefits may not persist if credit access induces undesirable consumer behavior. For example, increased spending may reflect imprudent borrowing or overspending. Similarly, the observed expansion in consumption scope may be driven by impulse buying, as consumers seek variety to satisfy psychological or emotional needs (e.g., Ratner and Kahn, 2002; Yoon and Kim, 2018). We therefore assess two potential risks that could offset the benefits of credit provision.

6.1 Imprudent Spending

A first concern is that BigTech credit may induce imprudent spending (Bertrand and Morse, 2016). We examine whether consumers, after receiving credit, upgrade to *higher-priced versions* of products that *serve similar functions*, e.g., switching to more expensive products, brands, or shops within the same product category.

To study this question, we compile the order-item-level data (i.e., the detailed subsample). We collect all orders placed by subsample users during the sample period. Each order may contain multiple items, and for each item we observe the unit price and quantity purchased, yielding 578,392 order-item observations. Summary statistics are reported in Table 1, Panel C. The average unit price is 229 RMB.

We begin by testing whether the prices of purchased items increase following credit access. The dependent variable, $ItemPrice$, is defined as the unit price of an item in a given order. Table 5 reports the results. In Column (1), we include user, shop, brand, product category, and cohort-date fixed effects, allowing us to compare prices within the same user while holding product characteristics and time constant. In Column (2), we replace user and product category fixed effects with user-by-category fixed effects, focusing on within-consumer variation within a given product category. For example, switching from regular milk to a more expensive organic alternative after receiving credit would be captured in this specification. In both cases, the coefficient on $Treated \times Post$ is statistically insignificant.

We also examine whether consumers upgrade to higher-end brands or shops. To do so, we construct two measures, $BrandRank$ and $ShopRank$, defined as the percentile rank of a brand or shop based on the average unit price of its products. Higher values correspond to more expensive brands or shops. For example, Apple MacBook laptops have higher average unit prices than Acer laptops and therefore receive higher brand ranks. Using the same fixed-effects specifications, Columns (3) – (6) of Table 5 show that the coefficient on $Treated \times Post$ remains statistically insignificant across all specifications.

Overall, these results provide no evidence that BigTech credit induces consumers to purchase higher-priced products or upgrade to more expensive brands or shops for items serving the same function, inconsistent with the notion that BigTech credit leads to imprudent spending.

6.2 Borrower Delinquency

The second risk is borrower delinquency. To assess this, we restrict the sample to post-treatment observations of treated consumers, as delinquency outcomes are observed only for users who receive BigTech credit.

First, we document that the overall delinquency rate among treated consumers is relatively low. We define the monthly delinquency rate as the share of loan principal that is more than 90 days past due, divided by the total outstanding loan principal. This definition is consistent with that used by traditional credit card issuers, such as commercial banks. The average delinquency rate in our sample is 0.9%, compared with a range of 1.4% to 3.3% for major Chinese commercial banks' credit card portfolios in 2020, based on their annual reports. This relatively low rate is inconsistent with the view that BigTech credit leads to higher levels of consumer over-indebtedness than traditional credit providers.

Second, we provide user-level evidence by examining whether credit-induced increases in spending are associated with higher delinquency. If BigTech credit were unsustainable due to excessive spending, consumers experiencing larger treatment effects would be more likely to become delinquent. To test this hypothesis, we measure the credit-induced growth in spending for each treated consumer as the change in average monthly spending before and after treatment, relative to the matched control. We then classify treated consumers into high- and low-spending-effect groups based on the sample median of this growth rate. The indicator variable $HighSpendingEffect$ equals one for consumers in the high-spending-effect group and zero otherwise.

We define two user-month-level indicators of delinquency: $Delinquent60Day$ and $Delinquent90Day$, which equal one if a consumer is more than 60 or 90 days past due on any monthly credit statement, respectively. Univariate comparisons (results untabulated) show that the mean of $Delinquent60Day$ ($Delinquent90Day$) is 0.013 (0.009) for the high-spending-effect group and 0.013 (0.009) for the low-spending-effect group, with differences that are statistically insignificant at conventional levels. We further examine the relationship between delinquency and spending responses by regressing these indicators on $HighSpendingEffect$.[34] Table 6 shows that the coefficient on $HighSpendingEffect$ is statistically insignificant, suggesting no meaningful difference in delinquency risk between consumers who experience large spending increases and those with more modest increases following credit access.[35]

Overall, these results do not indicate material increases in delinquency risk induced by access to embedded credit under the platform's current credit management regime (including credit limit and interest rate assignment and adjustment).

7. SUPPLEMENTARY ANALYSIS AND FURTHER DISCUSSION

7.1 Are the Effects Consistent with Relaxed Liquidity Constraints?

We examine whether the positive feedback effects from embedded BigTech credit to the core business is consistent with a relaxation of consumers' liquidity constraints. To this end, we extend our stacked cohort DiD model to a triple-difference (DDD) specification to test whether the effects of BigTech credit are more pronounced among consumers who are more likely to face ex-ante financial constraints.

We employ two proxies for liquidity constraints. Following Constantinides, Donaldson, and Mehra (2002), we use consumer age, as younger consumers are more likely to face borrowing constraints due to limited savings and shorter credit histories. We also use the firm's internal credit

[34] We use ordinary least squares (OLS) for this analysis given the binary nature of the dependent variables, allowing the coefficients to be interpreted as differences in delinquency probabilities across groups.

[35] In untabulated analysis, we find that consumers with larger spending responses make more prudent financial choices. At purchase, they select longer loan terms and products with lower service fees; after purchase, they are more likely to refinance and repay larger portions of their balances. These patterns are consistent with active liquidity management that supports higher spending without increasing delinquency risk.

score, as prior research shows that individuals with lower credit scores face greater difficulty accessing traditional credit despite their higher borrowing demand (e.g., Agarwal et al., 2018).

We then examine whether treatment effects vary along these dimensions for three key outcomes: net monthly spending ($Spending$), the volatility of spending ($SpendingVolatility$), and the number of unique products purchased ($SKUs$). These outcomes correspond to the three channels through which BigTech credit complements the core business: activity intensity, demand stability, and consumption variety.[36]

Table 7, Panel A reports the results. $YoungAge$ is an indicator equal to one if the applicant is younger than 22 (the typical age of college graduation in China), and $LowCreditScore$ is an indicator equal to one if the applicant's internal credit score is below the sample median at the time of application. The coefficient on the triple-interaction term is statistically significant and carries the expected sign across all specifications. The results indicate that the positive effects of credit on $Spending$ and $SKUs$, as well as the negative (i.e., stabilizing) effect on $SpendingVolatility$, are more pronounced among younger and lower-credit-score applicants. These findings are consistent with a liquidity-based mechanism underlying the complementarity between BigTech credit and the core business.

7.2 Is the Spending Increase Captured from Other Platforms?

In addition to inter-temporal reallocation, the increase in spending may also reflect intra-temporal substitution, such as a shift of purchases from competing platforms or offline retailers. Our data, which are confined to the BigTech firm's ecosystem, do not allow us to directly observe such substitution across platforms.

However, for our primary question, namely, how access to the platform's embedded credit product feeds back into the firm's own core business within firm boundaries, this distinction is not central. Whether spending is newly generated or reallocated from competitors, the resulting increase in activity intensity and persistence, demand stability, and consumption scope represents

[36] In untabulated analyses, we examine additional outcomes from Tables 2–4 and obtain qualitatively similar results.

a direct benefit to the firm's core business.[37] That said, we acknowledge its implication for welfare interpretation, as the effects we document may reflect a combination of new consumption and reallocation across platforms.

7.3 Are the Effects Attributable to Credit Access or Downstream Marketing?

While the RI program exogenously assigns credit access, it does not separately randomize subsequent operational practices such as targeted marketing. As a result, the treatment effect we estimate should be interpreted as the effect of granting access to the platform's embedded credit product under the firm's ordinary business practices, rather than as a mechanically isolated "pure credit" effect stripped of all subsequent platform responses.

We argue that the overall effect is the relevant object of interest for three reasons. First, bundling is not unique to BigTech credit. Estimates of traditional credit card effects, for example, implicitly incorporate the influence of associated rewards programs and marketing tied to card usage. Second, bundling is a defining feature of the BigTech business model. Unlike traditional financial institutions, BigTech firms integrate financial services with their existing platforms, and the complementarities we document arise within the platform ecosystem. Measuring the bundled effect therefore provides a more complete characterization of the role of BigTech lending than isolating individual components. Third, bundled effects are more informative for policy analysis. For competition authorities concerned with market concentration and financial regulators assessing systemic risk, the relevant question is how financial and non-financial operations interact within the firm to generate reinforcing feedback effects at the ecosystem level.

Nevertheless, we provide three pieces of evidence suggesting that differential marketing exposure is unlikely to be the primary driver of the observed effects. First, marketing tools (e.g., limited-time promotions, seasonal campaigns, or coupons tied to credit approval) are typically short-lived by design. Consistent with this, the marketing literature shows that targeted advertising rarely leads to persistent changes in consumer behavior (Sahni, Zou, and Chintagunta, 2017; Sahni, Narayanan, and Kalyanam, 2019; Li et al., 2021). This pattern is not consistent with our dynamic

[37] The spending-shift mechanism is not mutually exclusive with a relaxation of liquidity constraints and may, in fact, be consistent with it. A rational consumer would reallocate spending from other platforms only if doing so generates incremental value. Because we find no evidence that consumers switch to higher-end versions of products serving the same function after receiving BigTech credit, a plausible interpretation of such substitution (if it occurs) is the availability of more favorable or convenient credit relative to outside options. In this sense, reallocating spending to the platform to utilize BigTech credit is consistent with an effective relaxation of the consumer's budget constraint relative to existing credit alternatives. More broadly, spending reallocation (if it occurs) can be viewed as a channel through which the benefits of relaxed liquidity are realized within the platform.

analyses in Figures 3–5, which reveal stable and persistent treatment effects.

Second, marketing is typically associated with shifts toward more premium purchases (Miller et al., 2021). If credit approval in our setting were bundled with enhanced targeted marketing, we would expect treated consumers to upgrade their purchases. However, as shown in Section 6.1, we find no evidence of such upgrading, suggesting that differential marketing exposure is unlikely to account for our results.

Third, we examine whether treatment effects are stronger for individuals more likely to be targeted by marketing strategies. Because digital footprints facilitate user profiling and targeted advertising (Trusov, Ma, and Jamal, 2016; Sun et al., 2024), we construct two indicators, $HighVisits$ and $HighOrders$, each equal to one if a consumer's pre-application visit or order frequency is above the sample median. As reported in Table 7, Panel B, we find that the effects of BigTech credit are attenuated for users with greater digital footprints. This pattern is not consistent with a marketing-based explanation. Instead, it is more consistent with treatment effects being more pronounced among users who face binding liquidity constraints and therefore are more likely to exhibit lower baseline activity in the pre-treatment period.

7.4 Is the Spending Effect of BigTech Credit Large Relative to Credit Cards?

We assess the economic magnitude of the spending effect of the firm's embedded credit product using the MPC, which measures how much additional spending the platform captures for each RMB of credit extended. To estimate the MPC, we extend our DiD specification by interacting $Treated \times Post$ with the initial credit limit (measured in 100 RMB increments) assigned to treated consumers.[38] To minimize confounding from subsequent credit limit adjustments, we focus on a short post-treatment window.

Table 8 reports the results. Column (1) includes the month of credit approval and the following month, while Column (2) excludes the approval month to mitigate potential timing effects within the month. Based on Column (2), the estimated MPC is approximately 0.15, implying that each additional 100 RMB of credit is associated with an increase of about 15 RMB in spending on the platform. This magnitude is economically meaningful and comparable to estimates for credit cards, which range from 0.10 to 0.14 in Gross and Souleles (2002) and 0.11 in

[38] The initial credit limit is set to zero for the control group in this analysis. We use OLS because the interaction with a continuous credit limit allows the coefficient to be interpreted directly as the marginal propensity to consume out of additional credit.

Aydin (2022).

8. CONCLUSION

BigTech firms have become a major force in financial services, expanding rapidly beyond their traditional non-financial activities. This integration highlights a central issue: how bringing financial services within firm boundaries reshapes economic activity in the core business. We study this question using a proprietary dataset from one of the world's largest e-commerce platforms covering more than 2.5 million individual-month observations, and a field experiment that provides exogenous variation in access to the BigTech firm's integrated credit. Leveraging this setting, we estimate the causal effects of expanding credit access among applicants who would otherwise be denied credit. This population is particularly relevant for understanding how marginal changes in the supply of integrated credit affect platform economic activity and for evaluating regulatory interventions.

We find that embedded credit increases the platform's economic activity. It raises user activity intensity and persistence, stabilizes demand, and expands the scope of consumption. At the same time, we find little evidence of consumption upgrading or increased delinquency. Taken together, our results show that embedding financial services within platform firms reinforces core operations, highlighting complementarities within modern digital ecosystems and informing ongoing policy debates on BigTech finance.

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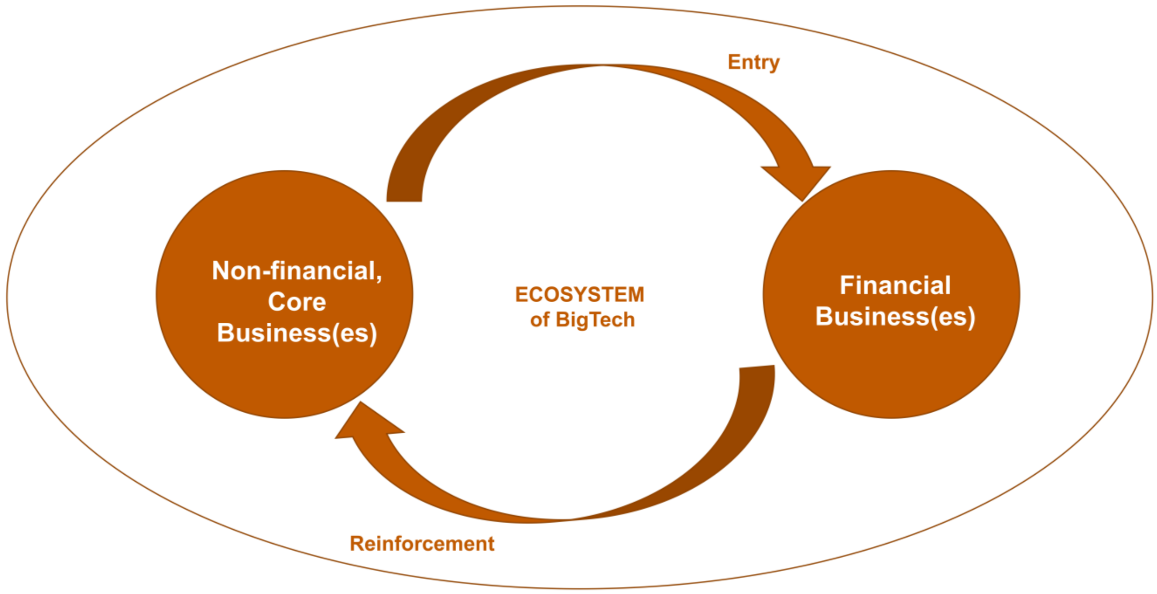
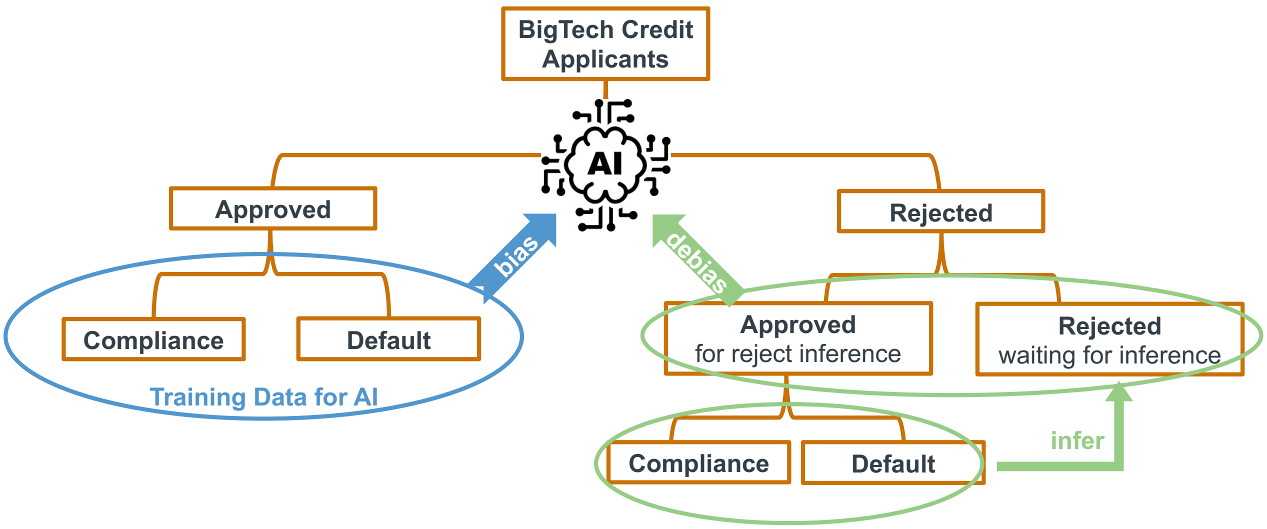
Figure 1. A Self-Reinforcing Feedback Loop in BigTech Firms



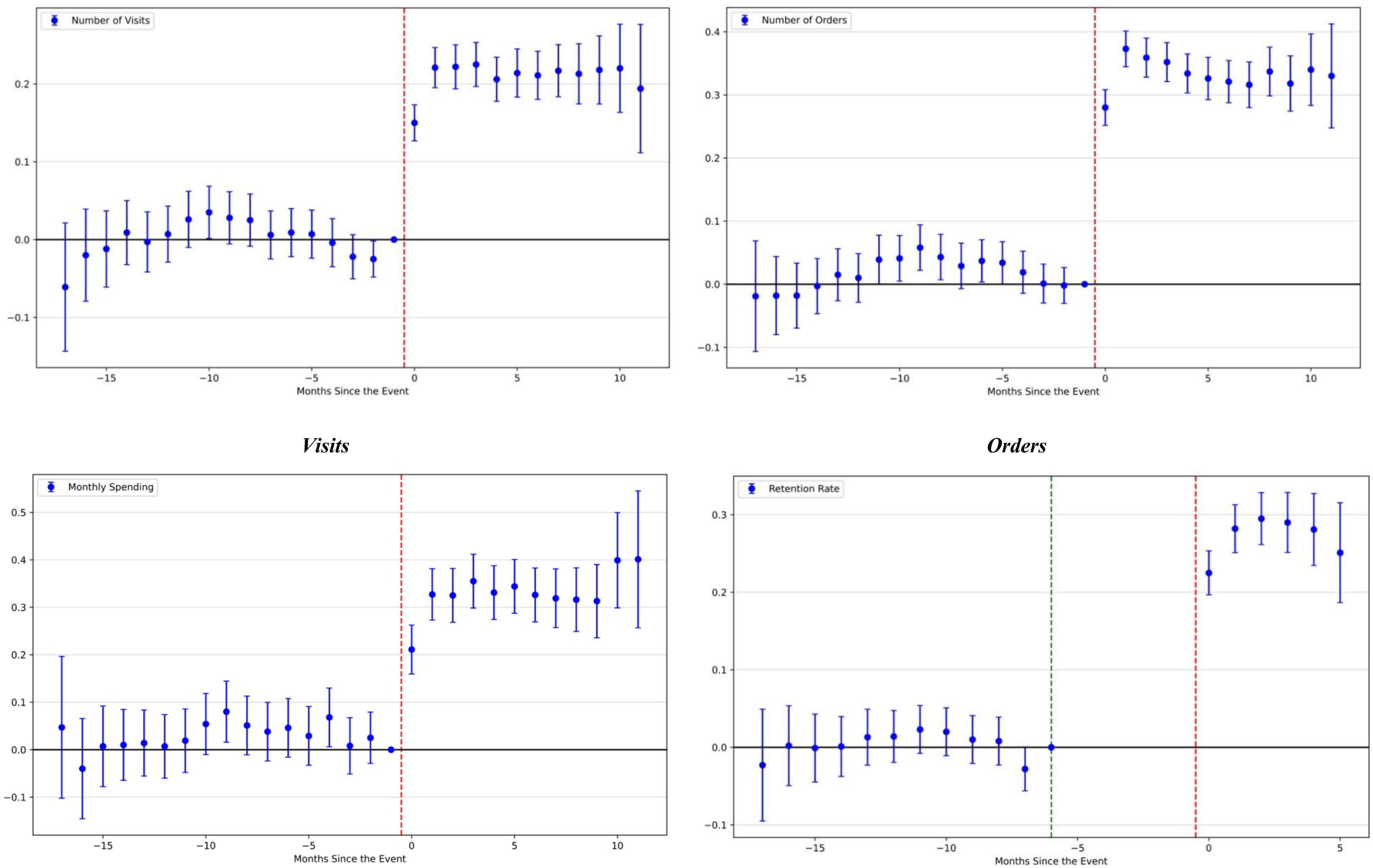
Notes: This figure illustrates a self-reinforcing feedback loop between BigTech firms' non-financial core businesses and their financial services within an integrated ecosystem. Strengths in the core business facilitate entry into financial services ("Entry"). Financial services, in turn, reinforce the core business ("Reinforcement") by increasing user activity, generating additional data, and enhancing monetization. These interactions create a feedback loop that links financial and non-financial operations and contributes to BigTech firms' competitive advantage (Financial Stability Board, 2019). While prior literature has documented the entry of BigTech firms into financial services (Brunnermeier and Payne, 2025; Li and Pegoraro, 2025; Ouyang, 2026), our study focuses on the reinforcement of core business operations by financial services.

Figure 2. Architecture of the Automated Underwriting System



Notes: This figure illustrates the architecture of the automated underwriting system and the role of the RI program. The firm's proprietary AI model first classifies credit applicants as approved or rejected. For approved applicants (left branch), realized repayment outcomes (compliance or default) are observed and used as training data for the AI model. Because these outcomes are only observed for *approved applicants*, the training data is a selected sample, introducing bias when the model is applied to the *full applicant pool* (Anderson, 2007; Thomas, Crook, and Edelman, 2017). To address this selection problem, the firm implements the RI program (right branch), under which a random subset of initially rejected applicants is approved for credit. The realized repayment outcomes of these RI-approved applicants provide information on the performance of rejected applicants and enable the firm to infer counterfactual delinquency risk for those who remain rejected. The firm incorporates both observed outcomes from approved applicants and inferred outcomes for rejected applicants into model training, thereby mitigating model bias.

Figure 3. The Effects of Embedded BigTech Credit on Activity Intensity and Persistence: Event-Study Estimates



Visits

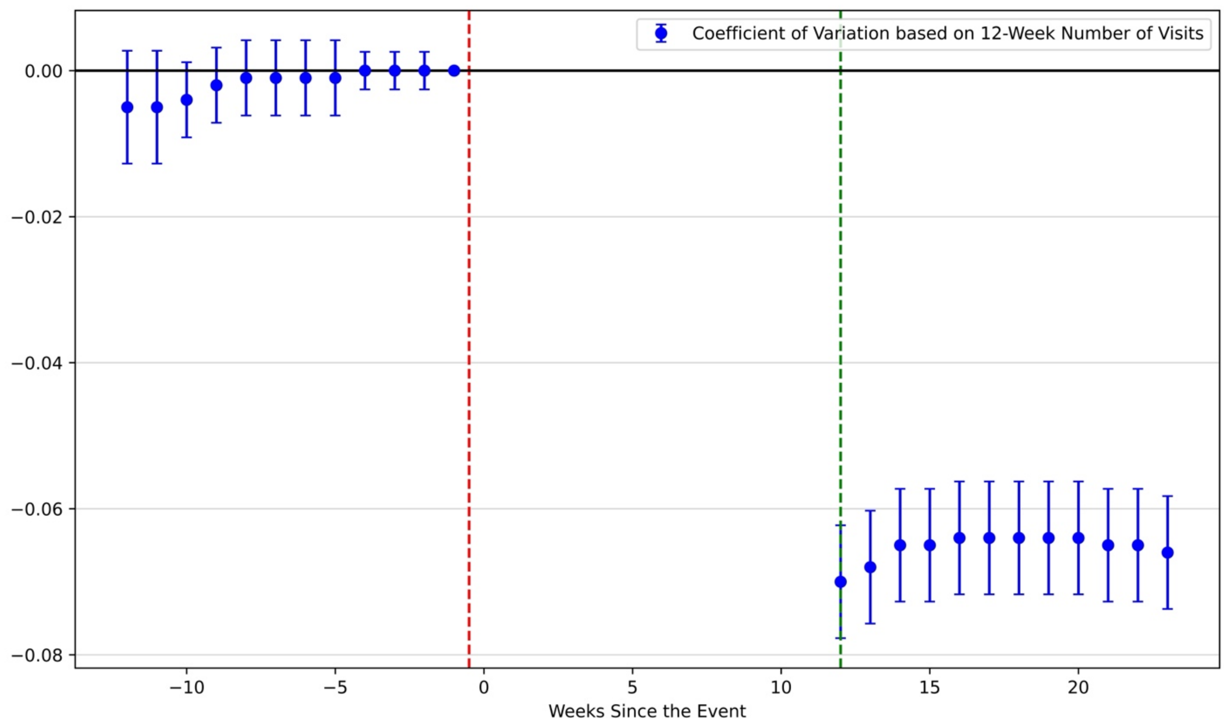
Orders

Spending

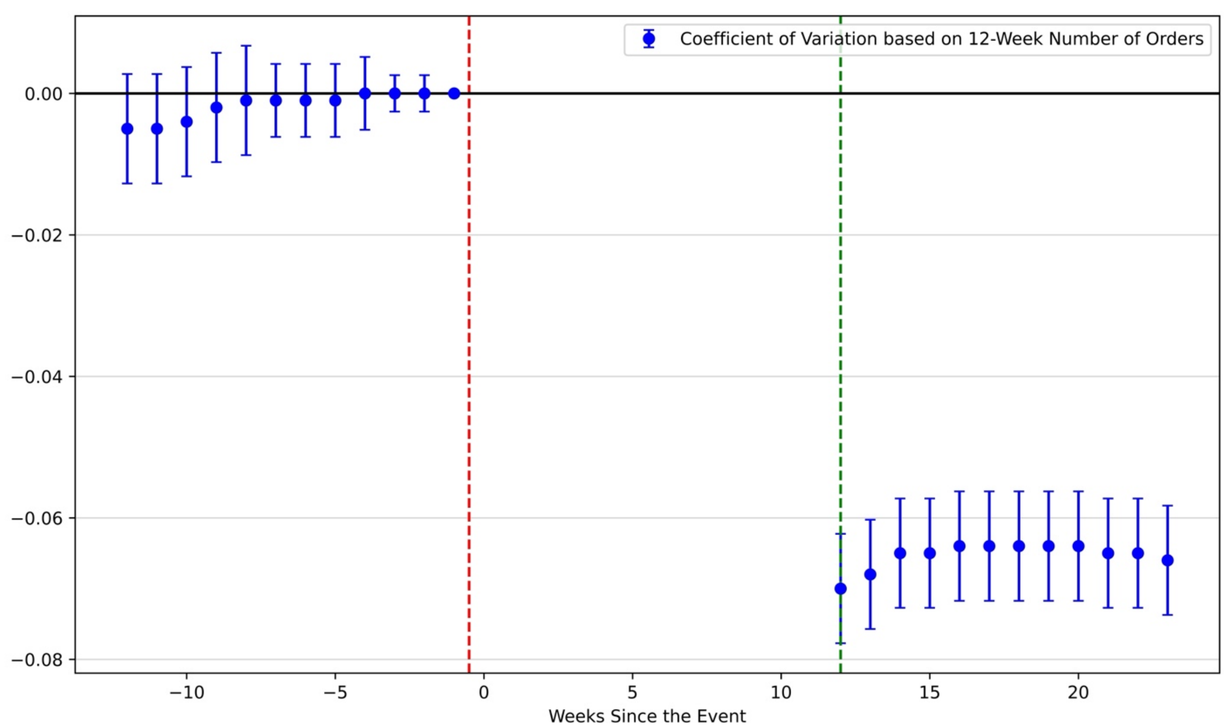
Retention

Notes: This figure presents event-study estimates of the effects of embedded BigTech credit on activity intensity and persistence of platform users. Points represent estimated treatment effects, and vertical bars indicate 99% confidence intervals. The horizontal axis denotes event time in months relative to the credit application month (month 0). The red dashed vertical line at −0.5 denotes the cutoff between pre- and post-treatment periods. Estimates are obtained from the following stacked cohort dynamic DiD specification: $Outcome_{c,i,t} = \sum_{k=-17}^{-2} \beta_k Treated_{c,i} \times 1_{c,k} + \sum_{k=0}^{11} \beta_k Treated_{c,i} \times 1_{c,k} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, t, and k index cohort, consumer, calendar month, and month relative to the application month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $Visits$, $Orders$, $Spending$, and $Retention$ (see Appendix A for definitions). Month −1 is used as the reference point. Coefficients are estimated using Poisson regressions. Standard errors are clustered at the cohort-by-individual level. The first post-treatment month reflects partial exposure, as credit may be granted at any time during that month. Confidence intervals widen toward the two ends of the event window due to the decreasing sample size as the time approaches the two ends of the sample period. For $Retention$, the regression excludes periods in which the measure cannot be constructed consistently, including months for which the calculation of the measure spans both pre- and post-treatment periods (months −5 to −1) and months for which the subsequent quarter is not fully observed to generate the measure (months ≥ 6). Therefore, month −6 (indicated by the green dashed vertical line) is used as the reference point for $Retention$.

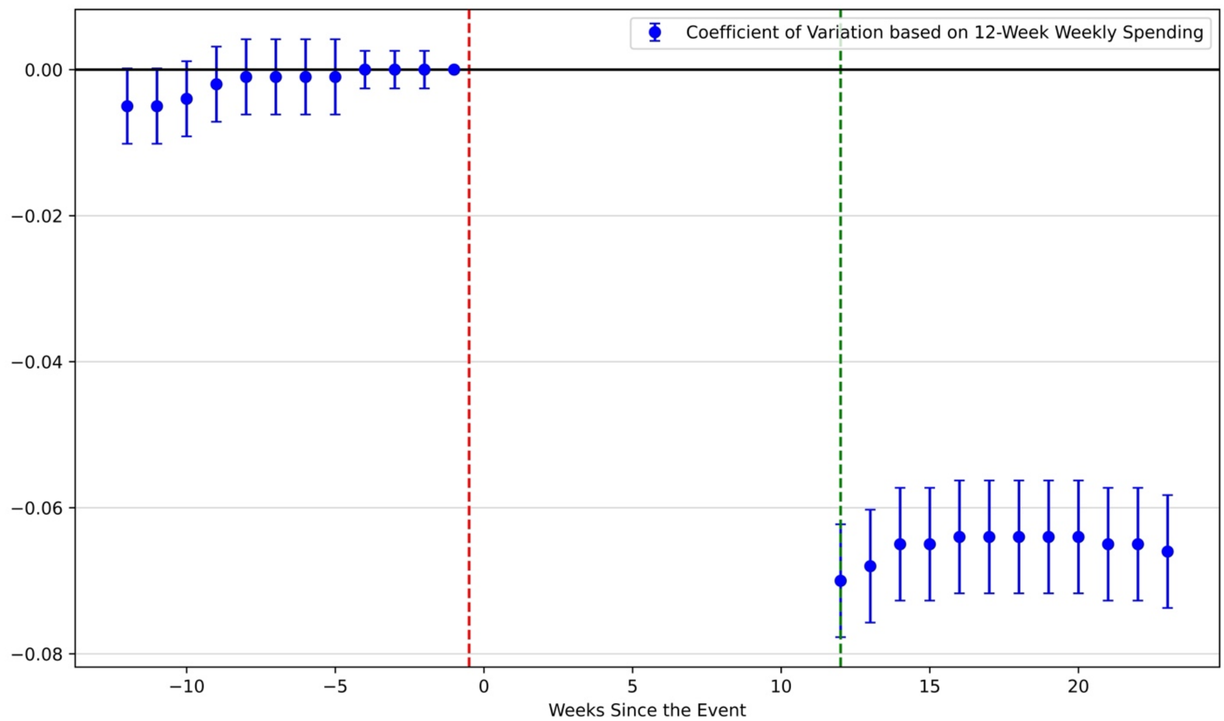
Figure 4. The Effects of Embedded BigTech Credit on Consumer Demand Stability: Event-Study Estimates



VisitVolatility



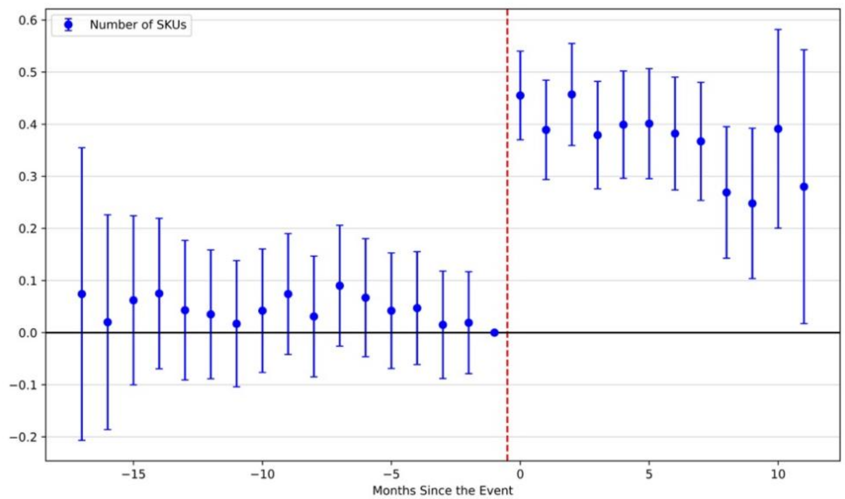
OrderVolatility



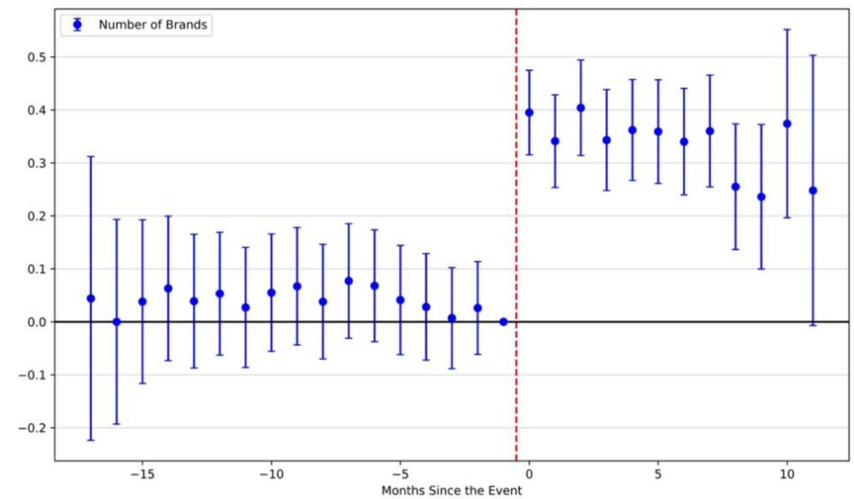
SpendingVolatility

Notes: This figure presents event-study estimates of the effects of embedded BigTech credit on the stability of consumer demand. Points represent estimated treatment effects, and vertical bars indicate 99% confidence intervals. The horizontal axis denotes event time in weeks relative to the credit application week (week 0). The red dashed vertical line at −0.5 denotes the cutoff between pre- and post-treatment periods. Estimates are obtained from the following stacked cohort dynamic DiD specification: $Outcome_{c,i,t} = \sum_{k=-12}^{-2} \beta_k Treated_{c,i} \times 1_{c,k} + \sum_{k=12}^{23} \beta_k Treated_{c,i} \times 1_{c,k} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, t, and k index cohort, consumer, calendar week, and week relative to the application week, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-week fixed effects. The $Outcome$ variables include $VisitVolatility$, $OrderVolatility$, and $SpendingVolatility$ (see Appendix A for definitions). Week −1 is used as the reference point. Coefficients are estimated using Poisson regressions. Standard errors are clustered at the cohort-by-individual level. To capture immediate responses while maintaining a manageable sample size, the regressions use a user-week panel that spans 24 weeks before and 24 weeks after the credit application week. The regressions exclude periods in which the measure cannot be constructed consistently, including weeks for which the calculation of the measure spans both pre- and post-treatment periods (weeks 0 to 11) and weeks for which the weekly data is not fully observed to generate the measure (weeks −24 to −13). Therefore, week 12 (indicated by the green dashed vertical line) is the first post-treatment week for which treatment effects are estimated.

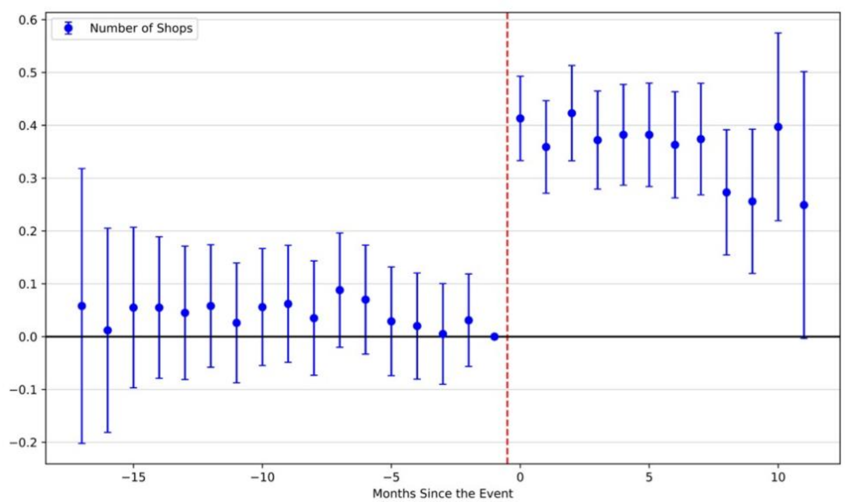
Figure 5. The Effects of Embedded BigTech Credit on Consumption Scope: Event-Study Estimates



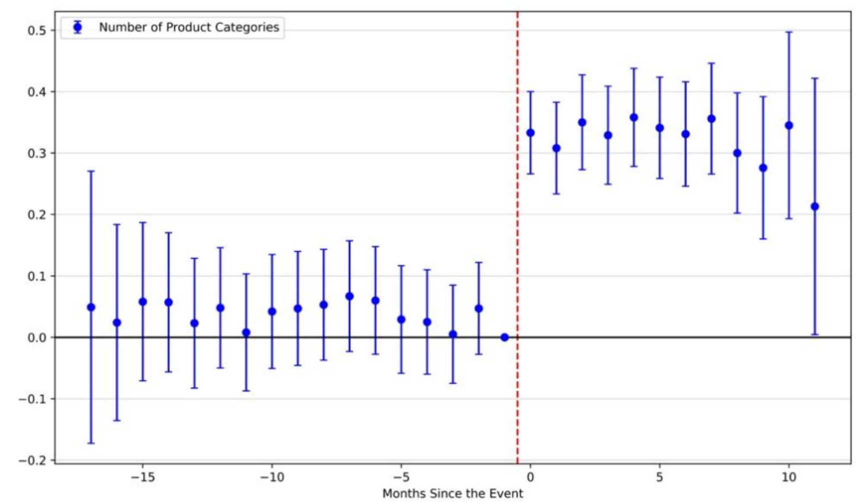
SKUs



Brands



Shops



Categories

Notes: This figure presents event-study estimates of the effects of embedded BigTech credit on consumption scope. Points represent estimated treatment effects, and vertical bars indicate 99% confidence intervals. The horizontal axis denotes event time in months relative to the credit application month (month 0). The red dashed vertical line at −0.5 denotes the cutoff between pre- and post-treatment periods. Estimates are obtained from the following stacked cohort dynamic DiD specification: $Outcome_{c,i,t} = \sum_{k=-17}^{-2} \beta_k Treated_{c,i} \times 1_{c,k} + \sum_{k=0}^{11} \beta_k Treated_{c,i} \times 1_{c,k} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, t, and k index cohort, consumer, calendar month, and month relative to the application month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $SKUs$, $Brands$, $Shops$, and $Categories$ (see Appendix A for definitions). Month −1 is used as the reference point. Coefficients are estimated using Poisson regressions. Standard errors are clustered at the cohort-by-individual level. The first post-treatment month reflects partial exposure, as credit may be granted at any time during that month. Confidence intervals widen toward the two ends of the event window due to the decreasing sample size as the time approaches the two ends of the sample period.

Table 1. Summary Statistics

Panel A. Consumer Demographics

Variable	Count	Mean	Std.	25%	50%	75%
$Male$	119,808	0.661	0.473	0.000	1.000	1.000
Age	119,808	28.882	10.366	20.000	27.000	34.000
$AccountAge$	119,808	31.034	22.069	12.516	26.873	45.237

Panel B. Variables for Complementarity Analysis

Variable	Count	Mean	Std.	25%	50%	75%
$Visits$	2,586,650	63.484	121.708	1.000	17.000	66.000
$Orders$	2,586,650	1.351	2.624	0.000	0.000	2.000
$Spending$	2,586,650	192.003	609.452	0.000	0.000	94.000
$Retention$	1,781,543	0.289	0.453	0.000	0.000	1.000
$VisitVolatility$	6,290,107	1.770	0.732	1.221	1.658	2.236
$OrderVolatility$	4,449,221	2.313	0.845	1.635	2.236	3.317
$SpendingVolatility$	4,027,493	2.560	0.753	1.964	2.610	3.317
$SKUs$	253,664	1.818	3.488	0.000	0.000	2.000
$Brands$	253,664	1.271	2.312	0.000	0.000	2.000
$Shops$	253,664	1.273	2.302	0.000	0.000	2.000
$Categories$	253,664	0.787	1.182	0.000	0.000	1.000

Panel C. Variables for Sustainability Analysis

Variable	Count	Mean	Std.	25%	50%	75%
$ItemPrice$	578,392	229.255	726.438	9.900	36.900	99.900
$BrandRank$	546,094	0.551	0.290	0.317	0.537	0.811
$ShopRank$	546,484	0.521	0.290	0.287	0.476	0.772
$Delinquent60Day$	51,119	0.013	0.112	0.000	0.000	0.000
$Delinquent90Day$	51,119	0.009	0.095	0.000	0.000	0.000

Notes: This table reports summary statistics for the main variables used in the analysis. Panel A presents demographic characteristics of applicants. Panels B and C report summary statistics for outcome variables used in the analyses of complementarity and sustainability, respectively. The number of observations varies across variables because they are measured at different levels, over different samples, or across different time periods for different analyses. Specifically, demographic variables are measured at the individual level. $VisitVolatility$, $OrderVolatility$, and $SpendingVolatility$ are measured at the individual-week level. $ItemPrice$, $BrandRank$, and $ShopRank$ are measured at the order-item level. All other variables are measured at the individual-month level. $Retention$ is not available for the final five months of the sample period, as its construction requires full observation of the subsequent quarter. $Delinquent60Day$ and $Delinquent90Day$ are defined only for treated consumers in the post-treatment periods. $SKUs$, $Brands$, $Shops$, $Categories$, $ItemPrice$, $BrandRank$, and $ShopRank$ are available only for the detailed subsample. In regression tables, the number of observations may differ from the number here because singleton observations are dropped. All continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in Appendix A.

Table 2. The Effects of Embedded BigTech Credit on Activity Intensity and Persistence

Dependent Variable =	(1) $Visits$	(2) $Orders$	(3) $Spending$	(4) $Retention$
$Treated \times Post$	0.203***	0.312***	0.286***	0.259***
	(0.007)	(0.007)	(0.010)	(0.008)
Cohort-Individual FE	Yes	Yes	Yes	Yes
Cohort-Month FE	Yes	Yes	Yes	Yes
Observations	2,586,650	2,572,363	2,562,426	951,416
Pseudo R-squared	0.468	0.280	0.377	0.122

Notes: This table reports stacked cohort DiD estimates of the effects of embedded BigTech credit on consumers' activity intensity and persistence. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $Visits$, $Orders$, $Spending$, and $Retention$ (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. For $Retention$, the regression excludes periods in which the measure cannot be constructed consistently, including months for which the calculation of the measure spans both pre- and post-treatment periods (months -5 to -1) and months for which the subsequent quarter is not fully observed to generate the measure (months ≥ 6).

Table 3. The Effects of Embedded BigTech Credit on Consumer Demand Stability

	(1)	(2)	(3)
Dependent Variable =	$VisitVolatility$	$OrderVolatility$	$SpendingVolatility$
$Treated \times Post$	-0.064***	-0.085***	-0.057***
	(0.003)	(0.003)	(0.002)
Cohort-Individual FE	Yes	Yes	Yes
Cohort-Week FE	Yes	Yes	Yes
Observations	3,161,367	2,224,921	2,024,658
Pseudo R-squared	0.069	0.068	0.050

Notes: This table reports stacked cohort DiD estimates of the effects of embedded BigTech credit on the stability of consumer demand. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar week, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-week fixed effects. The $Outcome$ variables include $VisitVolatility$, $OrderVolatility$, and $SpendingVolatility$. (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. To capture immediate responses while maintaining a manageable sample size, the regressions use a user-week panel that spans 24 weeks before and 24 weeks after the credit application week. The regressions exclude periods in which the measure cannot be constructed consistently, including weeks for which the calculation of the measure spans both pre- and post-treatment periods (weeks 0 to 11) and weeks for which the weekly data is not fully observed to generate the measure (weeks −24 to −13).

Table 4. The Effects of Embedded BigTech Credit on Consumption Scope

Dependent Variable =	(1) $SKUs$	(2) $Brands$	(3) $Shops$	(4) $Categories$
$Treated \times Post$	0.355***	0.316***	0.336***	0.296***
	(0.021)	(0.020)	(0.020)	(0.017)
Cohort-Individual FE	Yes	Yes	Yes	Yes
Cohort-Month FE	Yes	Yes	Yes	Yes
Observations	252,623	252,388	252,531	252,623
Pseudo R-squared	0.289	0.270	0.272	0.194

Notes: This table reports stacked cohort DiD estimates of the effects of embedded BigTech credit on consumption scope. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $SKUs$, $Brands$, $Shops$, and $Categories$ (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Effects of Embedded BigTech Credit on Purchase of Higher-End Products

Dependent Variable =	(1) $ItemPrice$	(2) $ItemPrice$	(3) $BrandRank$	(4) $BrandRank$	(5) $ShopRank$	(6) $ShopRank$
$Treated \times Post$	0.008	0.010	0.001	0.001	0.000	-0.001
	(0.013)	(0.016)	(0.001)	(0.001)	(0.002)	(0.002)
Cohort-Individual FE	Yes	No	Yes	No	Yes	No
Cohort-Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Individual-Category FE	No	Yes	No	Yes	No	Yes
Category FE	Yes	No	Yes	No	Yes	No
Brand FE	Yes	Yes	No	No	Yes	Yes
Shop FE	Yes	Yes	Yes	Yes	No	No
Observations	507,382	492,835	509,083	494,743	511,519	497,811
Pseudo R-squared	0.753	0.763	0.101	0.101	0.103	0.105

Notes: This table reports stacked cohort DiD estimates of the effects of embedded BigTech credit on purchases of higher-end products. The analysis uses order-item-level data, where each observation corresponds to a unique item within an order. Estimates are obtained from the following specification: $Outcome_{c,i,t,j,k,g} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \gamma_j + \lambda_k + \theta_g + \epsilon_{c,i,t,j,k,g}$, where c, i, t, j, k, and g index cohort, consumer, calendar date, product category, brand, and shop, respectively. The $Outcome$ variables include $ItemPrice$, $BrandRank$, and $ShopRank$ (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6. BigTech-Induced Spending Growth and Consumer Delinquency

Dependent Variable =	(1) $Delinquent60Day$	(2) $Delinquent90Day$
$HighSpendingEffect$	0.000	-0.000
	(0.001)	(0.001)
Cohort-Month FE	Yes	Yes
Observations	520,645	520,645
Adjusted R-squared	0.006	0.006

Notes: This table reports the relationship between BigTech-induced spending growth and consumer delinquency. The sample consists of post-treatment individual-month observations for treated consumers. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta \times HighSpendingEffect_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar month, respectively. The term $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $Delinquent60Day$ and $Delinquent90Day$ (see Appendix A for definitions). Coefficients are estimated using OLS, with standard errors clustered at the cohort-by-individual level and reported in parentheses. We use OLS for this analysis given the binary nature of the dependent variables, allowing the coefficients to be interpreted as differences in delinquency probabilities across groups. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Heterogeneity in Treatment Effects and Evidence on Mechanisms

Panel A. Evidence Consistent with Relaxed Liquidity Constraints

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable =	$Spending$	$Spending$	$Spending\ Volatility$	$Spending\ Volatility$	$SKUs$	$SKUs$
$Treated \times Post\ \times YoungAge$	0.268***		-0.038***		0.289***	
	(0.022)		(0.005)		(0.057)	
$Treated \times Post\ \times LowCreditScore$		0.249***		-0.032***		0.314***
		(0.020)		(0.005)		(0.055)
Two-way Interactions	Included	Included	Included	Included	Included	Included
Cohort-Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,562,426	2,562,426	2,024,658	2,024,658	190,087	190,087
Pseudo R-squared	0.377	0.377	0.0505	0.0504	0.289	0.288

Panel B. Evidence Related to Targeted Marketing

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable =	$Spending$	$Spending$	$Spending\ Volatility$	$Spending\ Volatility$	$SKUs$	$SKUs$
$Treated \times Post\ \times HighVisits$	-0.212***		0.008*		-0.347***	
	(0.021)		(0.005)		(0.056)	
$Treated \times Post\ \times HighOrders$		-0.259***		0.005		-0.394***
		(0.022)		(0.005)		(0.056)
Two-way Interactions	Included	Included	Included	Included	Included	Included
Cohort-Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,562,426	2,562,426	2,024,658	2,024,658	190,087	190,087
Pseudo R-squared	0.378	0.380	0.0505	0.0507	0.290	0.295

Notes: This table reports stacked cohort DDD estimates that examine heterogeneity in treatment effects and provide evidence on potential mechanisms. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta_1 \times Treated_{c,i} \times Post_{c,t} \times Group_{c,i} + \beta_2 \times Treated_{c,i} \times Post_{c,t} + \beta_3 \times Treated_{c,i} \times Group_{c,i} + \beta_4 \times Post_{c,t} \times Group_{c,i} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. Panel A examines heterogeneity consistent with a liquidity-based mechanism, using $YoungAge$ and $LowCreditScore$ for $Group$ as proxies for financial constraints. Panel B examines patterns related to differential marketing exposure, using $HighVisits$ and $HighOrders$ for $Group$ as proxies for consumers' digital footprints. The $Outcome$ variables include $Spending$, $SpendingVolatility$, and $SKUs$ (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 8. Marginal Propensity to Consume of Embedded BigTech Credit

	(1)	(2)
Dependent Variable =	$Spending$	$Spending$
Sample	Month <= 1	Month ≠ 0 & <= 1
$Treated \times Post \times InitialCreditLimit$	12.137***	14.852***
	(0.749)	(0.954)
Cohort-Individual FE	Yes	Yes
Cohort-Month FE	Yes	Yes
Observations	1,740,634	1,620,826
Adjusted R-squared	0.205	0.207

Notes: This table reports estimates of the MPC out of gaining access to embedded BigTech credit. Estimates are obtained from the following stacked cohort DiD specification: $Spending_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} \times InitialCreditLimit_{c,i} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c , i , and t index cohort, consumer, and calendar month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. Variable definitions are provided in Appendix A. Column (1) restricts the sample to the month of credit approval and the subsequent month to mitigate the influence of credit limit adjustments. Column (2) further excludes the approval month to address potential timing effects arising from within-month variation in credit access. Variable definitions are provided in Appendix A. Coefficients are estimated using OLS, with standard errors clustered at the cohort-by-individual level and reported in parentheses. The initial credit limit is set to zero for the control group in this analysis. We use OLS because the interaction with a continuous credit limit allows the coefficient to be interpreted directly as the marginal propensity to consume out of additional credit. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix A. Variable Definitions

Variables for BigTech Credit Granting	
$Treated$	Indicator equal to one if a consumer's credit application is rejected by the AI model but approved through the reject inference program, and zero if the application remains rejected.
$Post$	Indicator equal to one for the month of credit application and all subsequent months, and zero otherwise.
Variables for Complementarity Analysis	
$Visits$	Number of product-page visits by a consumer in a given month.
$Conversion$	Number of orders divided by the number of visits in a given month (times 10).
$Orders$	Number of orders placed by a consumer in a given month.
$Discount$	Total discount amount as a share of pre-discount, pre-cancellation order value in a given month.
$Cancellation$	Canceled order value as a share of pre-discount, pre-cancellation order value in a given month.
$SpendingPerOrder$	Average net spending per order in a given month.
$Spending$	Total net spending (excluding discounts and canceled amounts) by a consumer in a given month.
$Retention$	Indicator equal to one if a consumer makes a purchase (i.e., positive out-of-pocket spending) in month *t* and at least one additional purchase during months *t*+3 to *t*+5. This window is defined relative to month *t* rather than by calendar quarters; thus, purchases in months *t*+1 and *t*+2 are not counted. The variable is defined only for months with positive spending and for which the subsequent quarter is fully observable.
$VisitVolatility$	Standard deviation of weekly visits over the prior 12 weeks, normalized by the mean over the same period.
$OrderVolatility$	Standard deviation of weekly orders over the prior 12 weeks, normalized by the mean over the same period.
$SpendingVolatility$	Standard deviation of weekly spending over the prior 12 weeks, normalized by the mean over the same period.
$SKUs$	Number of unique products purchased by a consumer in a given month.
$Brands$	Number of unique brands purchased by a consumer in a given month.
$Shops$	Number of unique shops from which a consumer makes purchases in a given month.
$Categories$	Number of unique product categories purchased by a consumer in a given month.
Variables for Sustainability Analysis	
$ItemPrice$	Unit price of an item purchased in an order.
$BrandRank$	Percentile rank of a brand based on the average unit price of its products.
$ShopRank$	Percentile rank of a shop based on the average unit price of its products.
$Delinquent60Day$ $(Delinquent90Day)$	Indicator equal to one if a consumer is more than 60 (90) days past due on any monthly statement, and zero otherwise.
$HighSpendingEffect$	Indicator equal to one if a treated consumer's credit-induced spending growth (measured as the change in average monthly spending before and after treatment relative to the matched control) exceeds the sample median, and zero otherwise.
Variables for Supplementary Analysis	
$YoungAge$	Indicator equal to one if a consumer's age at the time of application is below 22, and zero otherwise.
$LowCreditScore$	Indicator equal to one if a consumer's credit score at the time of application is below the sample median, and zero otherwise.
$HighVisits$	Indicator equal to one if a consumer's pre-application visit frequency (measured over the prior 12 weeks) exceeds the sample median, and zero otherwise.
$HighOrders$	Indicator equal to one if a consumer's pre-application order frequency (measured over the prior 12 weeks) exceeds the sample median, and zero otherwise.
$InitialCreditLimit$	Credit limit initially assigned to the consumer, measured in 100 RMB increments.

Consumer Demographics and Other Variables for Matching	
$Male$	Indicator equal to one if the consumer is male, and zero otherwise.
Age	Consumer age, measured in years.
$AccountAge$	Number of months since the consumer registered on the e-commerce platform.
$Applications$	Number of credit applications submitted by the consumer at the time of application (including the current application).
$CreditScore$	Internal credit score assigned by the AI model at the time of application.
$PreVisits$	Total number of product-page visits in the 12 months prior to the credit application, scaled by the number of months with nonzero visits in those 12 months.
$PreOrders$	Total number of orders placed in the 12 months prior to the credit application, scaled by the number of months with nonzero orders in those 12 months.
$PreDiscount$	Total discount amount as a share of total order value in the 12 months prior to the credit application.
$PreCancellation$	Total canceled amount as a share of total order value in the 12 months prior to the credit application.
$PreSpending$	Total out-of-pocket spending in the 12 months prior to the credit application, scaled by the number of months with nonzero spending in those 12 months.
$PreCashShare$	Total out-of-pocket payment not made using platform stored balance in the 12 months prior to the credit application, scaled by total out-of-pocket spending in those 12 months.

Appendix B. Propensity Score Matching

As discussed in Section 3, the RI program is designed to randomize credit approval among AI-rejected applicants; however, the implementation details are proprietary and not observable to us. We therefore conduct balance tests on applicants' pre-treatment characteristics to assess whether treated and control groups are comparable. Specifically, we compare demographics (gender, age, and account age), credit demand and risk (application history and internal credit score), and online shopping activity (browsing and purchasing behavior, including visits, orders, spending, discounts, cancellations, and the use of platform stored balance).

Table B1. Balance Check Before Matching

Variable	Mean (RI-Approved)	Mean (AI-Rejected)	Difference
$Male$	0.660	0.731	-0.071***
Age	28.900	28.516	0.384***
$AccountAge$	31.092	36.475	-5.382***
$Applications$	1.273	1.968	-0.695***
$CreditScore$	689.032	684.501	4.531***
$PreVisits$	63.422	74.288	-10.866***
$PreOrders$	1.260	1.492	-0.232***
$PreSpending$	179.901	216.924	-37.022***
$PreDiscount$	0.168	0.160	0.008***
$PreCancellation$	0.310	0.350	-0.040***
PreCashShare	0.947	0.964	-0.017***

Table B1 reports the results for balance tests. Variable definitions are in Appendix A. While many differences are statistically significant due to the large sample size, most are economically modest relative to their means. In particular, the difference in internal credit scores is statistically significant but only 4.5 relative to a mean of approximately 686. Overall, these patterns are broadly consistent with random assignment subject to finite-sample variation.

To further improve covariate balance and ensure comparability between treated and control applicants, we implement nearest-neighbor propensity score matching without replacement within each cohort, pairing each treated applicant with one control applicant. The propensity scores are estimated using a logistic regression that includes the variables listed above as well as applicants' province of residence.

Table B2. Balance Check After Matching

Variable	Mean (RI-Approved)	Mean (AI-Rejected)	Difference
$Male$	0.660	0.662	-0.002
Age	28.900	28.914	-0.014
$AccountAge$	31.092	31.091	0.001
$Applications$	1.273	1.274	-0.001
$CreditScore$	689.032	689.010	0.022
$PreVisits$	63.422	63.001	0.421
$PreOrders$	1.260	1.267	-0.007
$PreSpending$	179.901	181.773	-1.872
$PreDiscount$	0.168	0.168	-0.000
$PreCancellation$	0.310	0.311	-0.000
$PreCashShare$	0.947	0.948	-0.001

Post-matching differences are negligible and reported in Table B2. The matching procedure substantially improves covariate balance across all characteristics, with mean differences close to zero and no statistically or economically meaningful discrepancies between treated and control applicants.

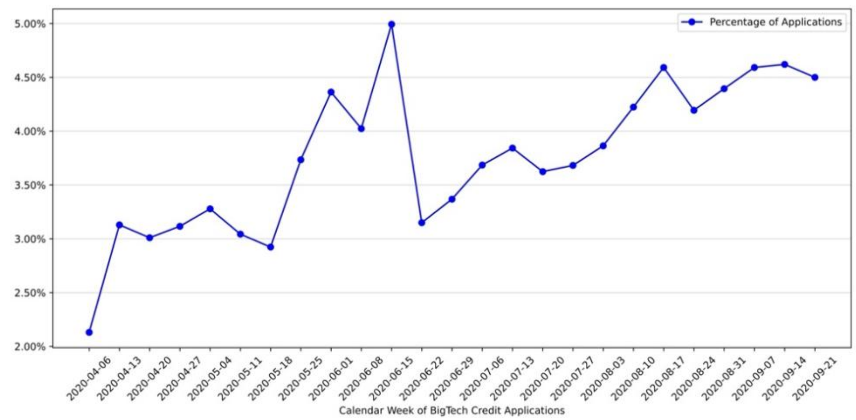
Internet Appendix for

"BigTech Credit and Platform Economic Activity"

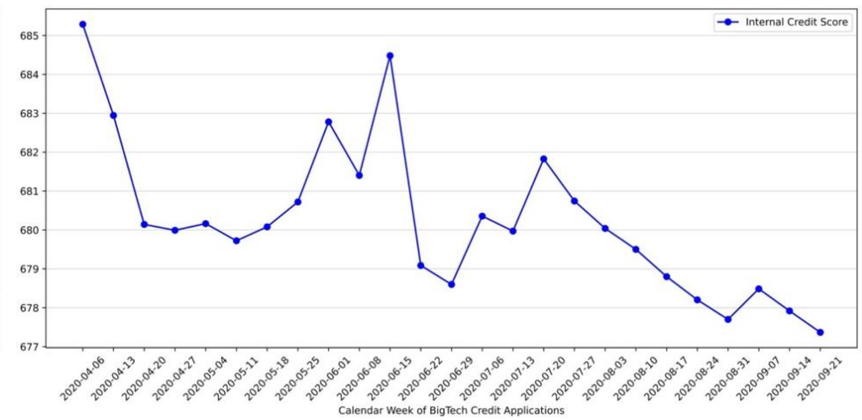
By **Lei Chen**, **Wenlan Qian**, **Albert Di Wang**, and **Qi Wu**

Table of Content

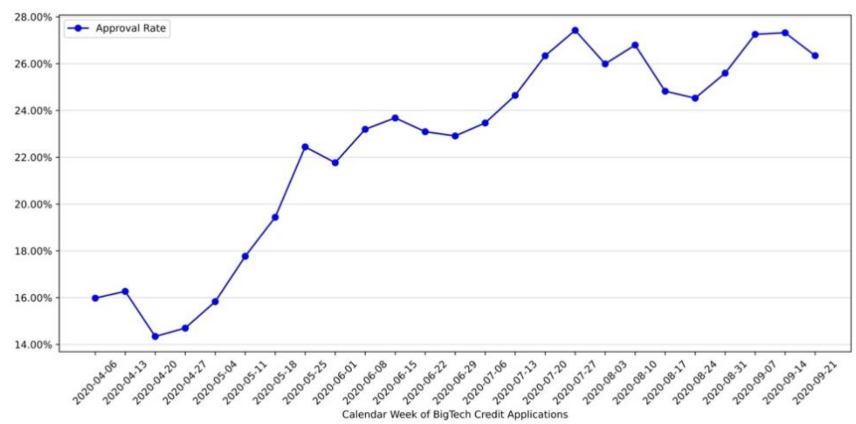
Figure IA1. Time-Series Patterns of Key Metrics in the Reject Inference Program



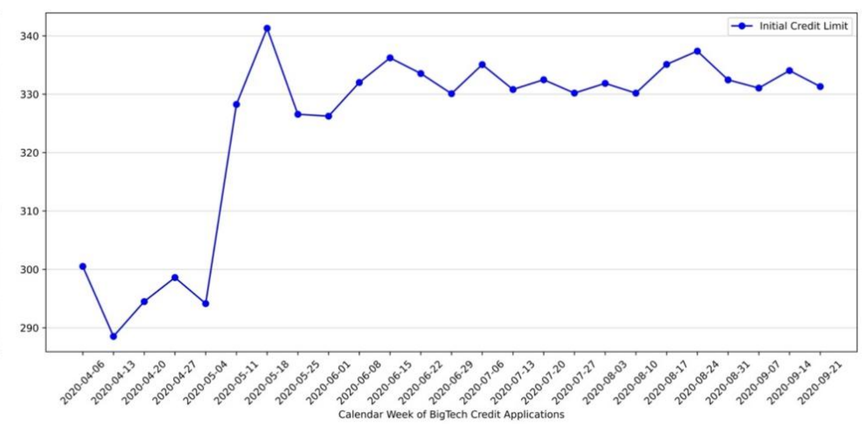
Panel A. Application Volume



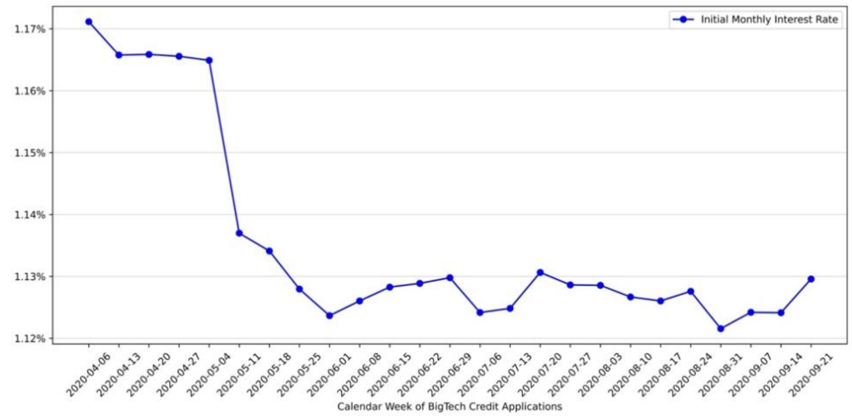
Panel B. Applicants' Credit Scores



Panel C. Approval Rates



Panel D. Initial Credit Limits

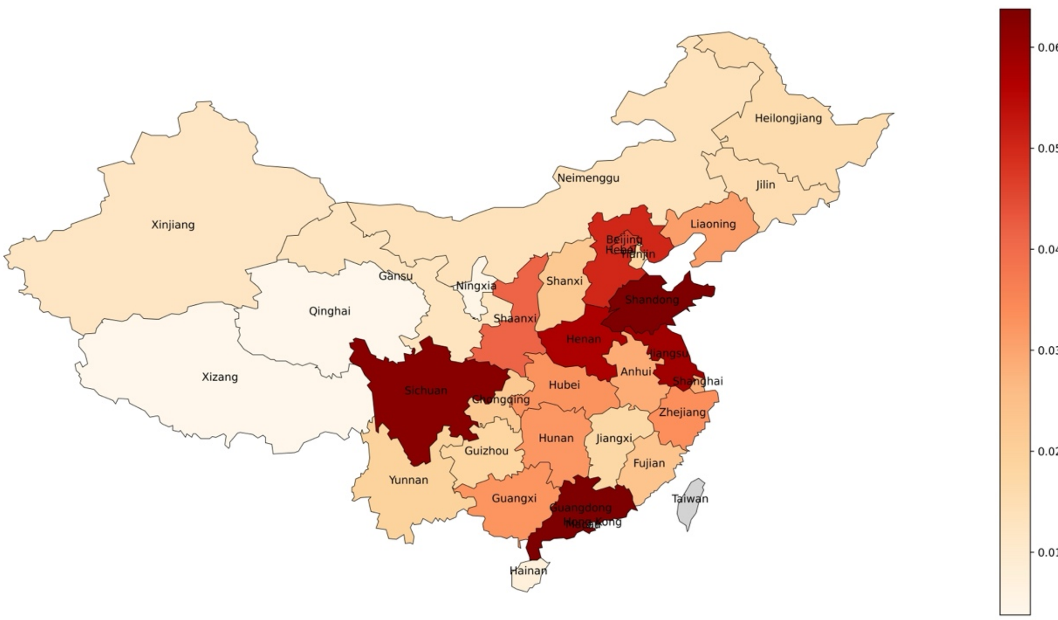


Panel E. Initial Interest Rates

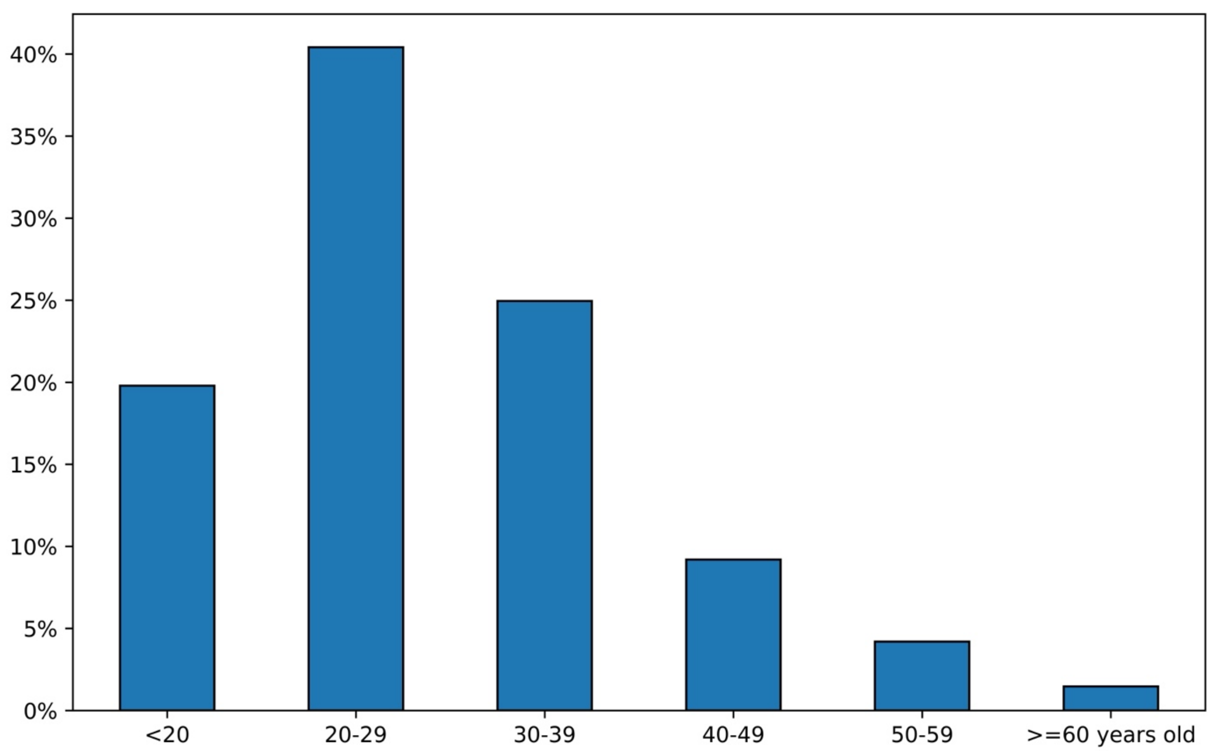
Notes: This figure presents weekly time-series patterns of key metrics associated with the RI program. Panel A shows the volume of AI-rejected applications (normalized as a percentage of total applications for confidentiality). Panel B reports the average internal credit score of AI-rejected applicants. Panel C presents the RI approval rate among initially rejected applicants. Panel D shows the average initial credit limit (in RMB) assigned to approved consumers. Panel E reports the average initial monthly interest rate for approved consumers.

Figure IA2. Geographic, Age, and Consumption Distributions of BigTech Credit Applicants

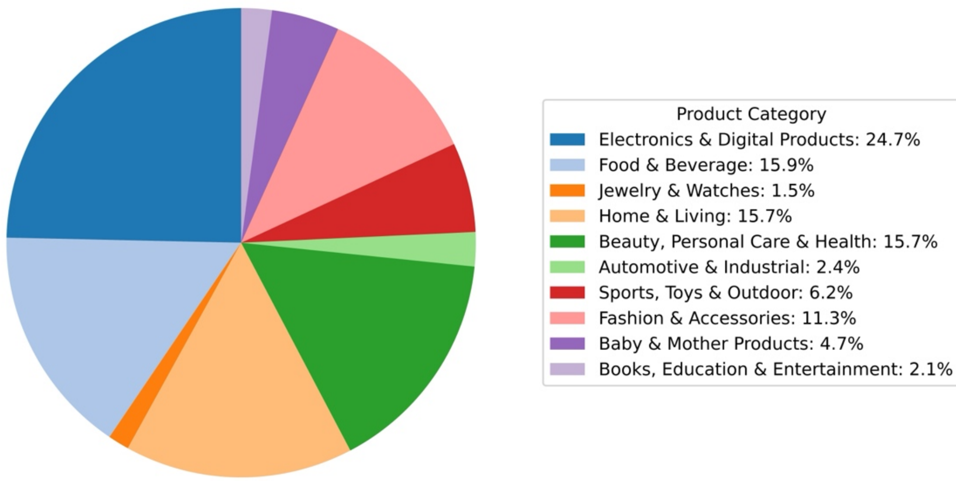
Panel A. Geographic Distribution



Panel B. Age Distribution

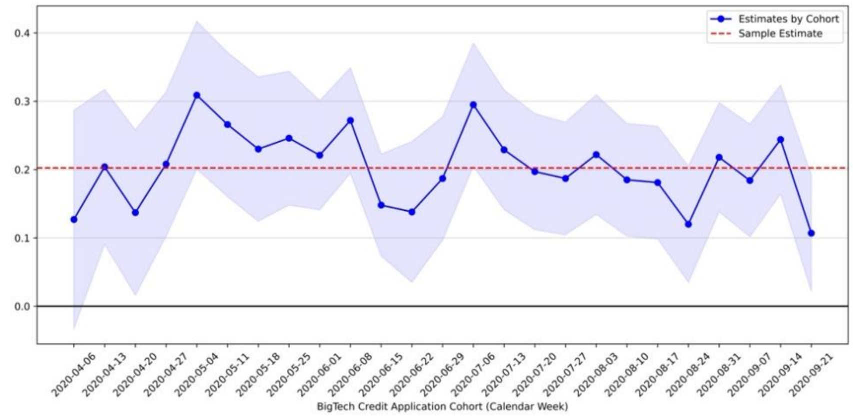


Panel C. Consumption Distribution

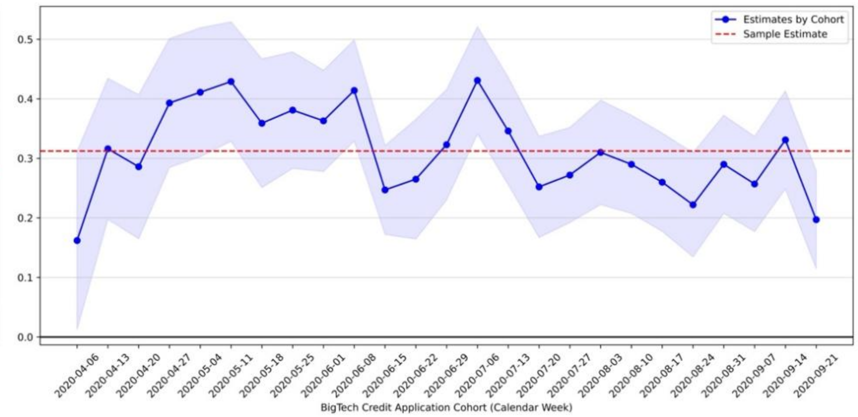


Notes: This figure presents the geographic, age, and consumption distributions of BigTech credit applicants in our sample. Panel A shows the geographic distribution based on applicants' shipping addresses, where the color scale indicates the share of applicants relative to the total sample. Taiwan, Hong Kong, and Macau are excluded due to a limited number of observations. Panel B presents the age distribution of applicants across six age groups. Panel C reports the distribution of purchase frequency across product groups for applicants in the detailed subsample with order-item-level purchase histories. The platform maintains a hierarchical product classification system. For ease of presentation, we aggregate detailed product classes into ten broader categories using a large language model based on semantic similarity and standard e-commerce classification conventions. The resulting categories are: Automotive & Industrial; Baby & Mother Products; Beauty, Personal Care & Health; Books, Education & Entertainment; Electronics & Digital Products; Fashion & Accessories; Food & Beverage; Home & Living; Jewelry & Watches; and Sports, Toys & Outdoor.

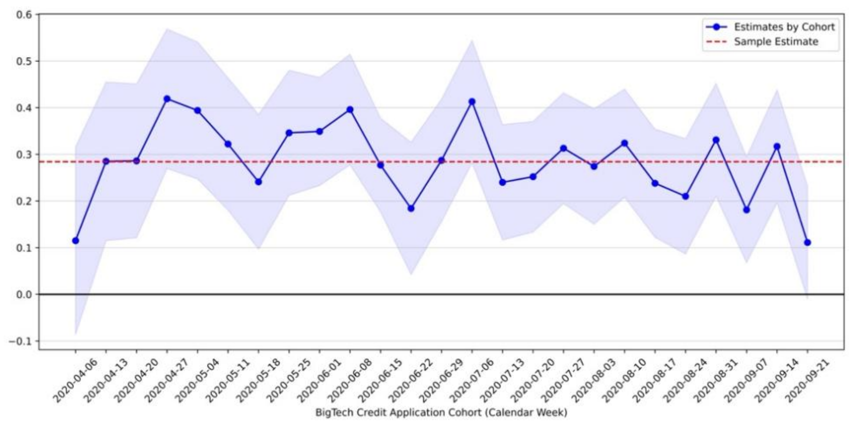
Figure IA3. Effects of BigTech Credit on Activity Intensity and Persistence by Weekly Cohort



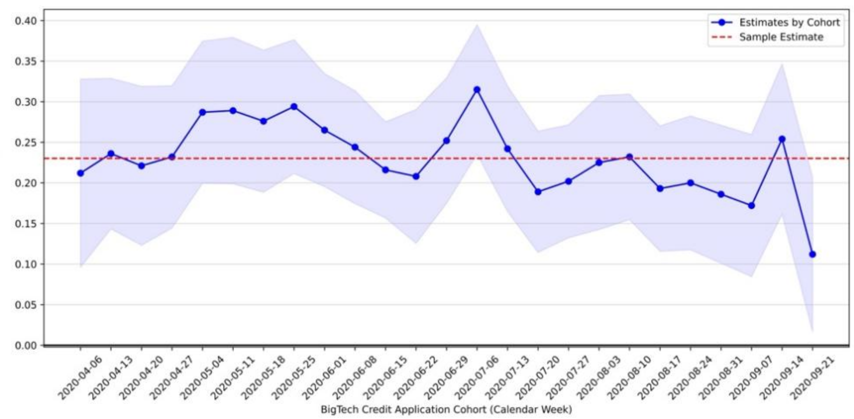
Visits



Orders



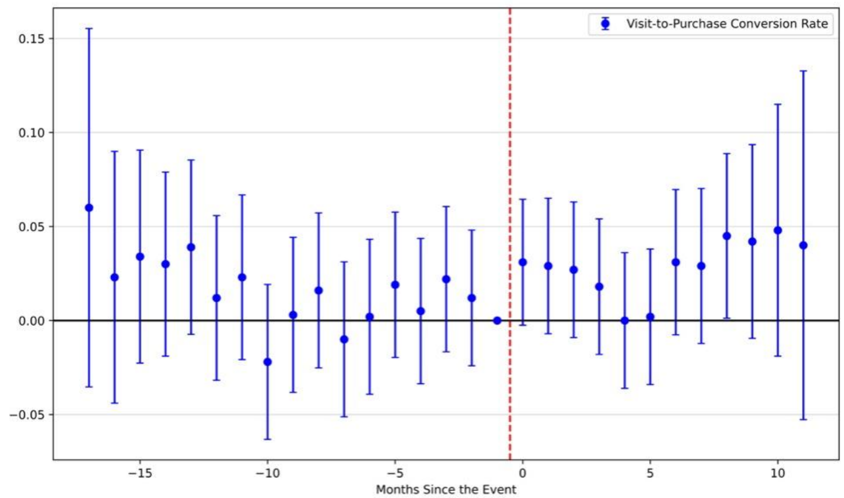
Spending



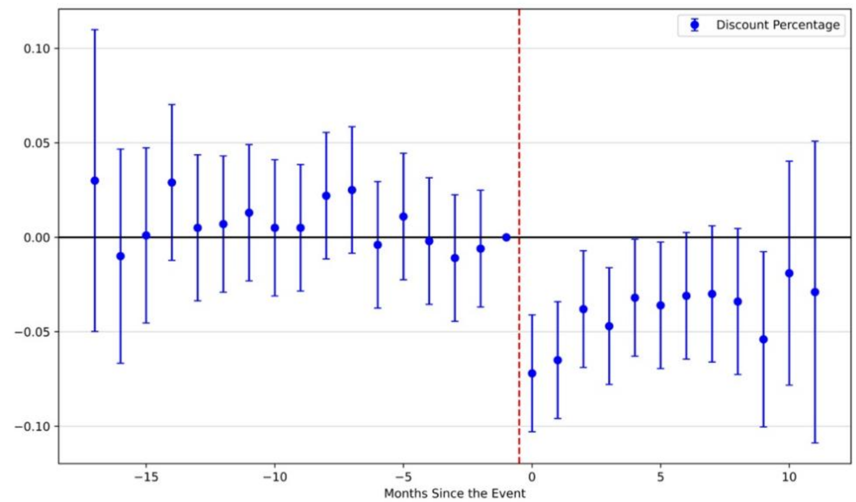
Retention

Notes: This figure presents stacked cohort DiD estimates of the effects of BigTech credit on activity intensity and persistence across cohorts. Points represent estimated treatment effects for each weekly cohort, and shaded areas indicate 99% confidence intervals. Estimates are obtained from the following specification: $Outcome_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, and t index cohort, consumer, and calendar month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $Visits$, $Orders$, $Spending$, and $Retention$ (see Appendix A for definitions). Coefficients are estimated using Poisson regressions, with standard errors clustered at the cohort-by-individual level. For $Retention$, the regression excludes periods in which the measure cannot be constructed consistently, including months for which the calculation of the measure spans both pre- and post-treatment periods (months -5 to -1) and months for which the subsequent quarter is not fully observed to generate the measure (months ≥ 6).

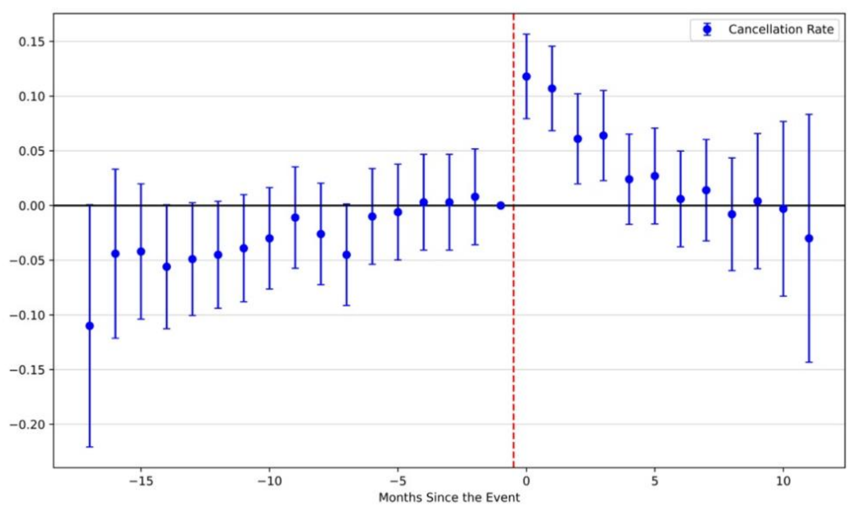
Figure IA4. Effects of BigTech Credit on Impulsiveness, Price Sensitivity, and Basket Size



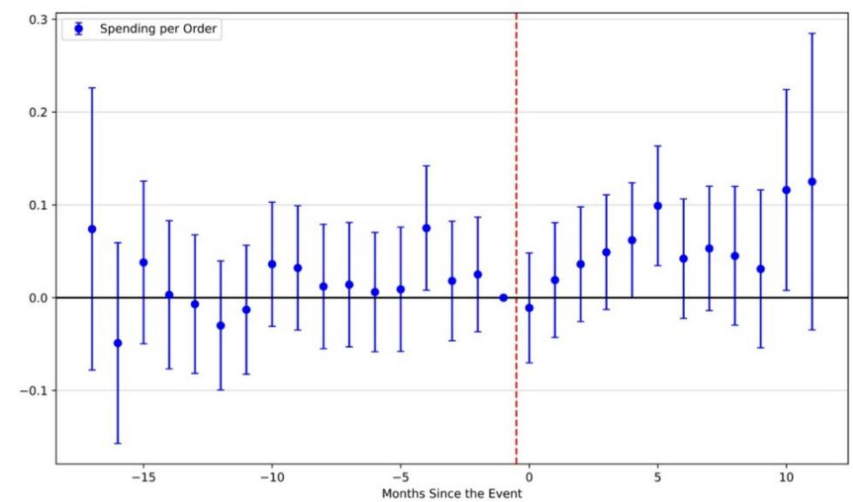
Conversion



Discount



Cancellation



SpendingPerOrder

Notes: This figure presents event-study estimates of the effects of embedded BigTech credit on impulsiveness (visit-to-purchase conversion and cancellations), price sensitivity (discounts), and basket size (spending per order). Points represent estimated treatment effects, and vertical bars indicate 99% confidence intervals. The horizontal axis denotes event time in months relative to the credit application month (month 0). The red dashed vertical line at −0.5 denotes the cutoff between pre- and post-treatment periods. Estimates are obtained from the following stacked cohort dynamic DiD specification: $Outcome_{c,i,t} = \sum_{k=-17}^{-2} \beta_k Treated_{c,i} \times 1_{c,k} + \sum_{k=0}^{11} \beta_k Treated_{c,i} \times 1_{c,k} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c, i, t, and k index cohort, consumer, calendar month, and month relative to the application month, respectively. The term $\alpha_{c,i}$ denotes cohort-by-individual fixed effects, and $\delta_{c,t}$ denotes cohort-by-calendar-month fixed effects. The $Outcome$ variables include $Conversion$, $Discount$, $Cancellation$, and $SpendingPerOrder$ (see Appendix A for definitions). Month −1 is used as the reference point. Coefficients are estimated using Poisson regressions. Standard errors are clustered at the cohort-by-individual level. The first post-treatment month reflects partial exposure, as credit may be granted at any time during that month. Confidence intervals widen toward the two ends of the event window due to the decreasing sample size as the time approaches the two ends of the sample period.