Deciphering the Impact of BigTech Consumer Credit[*]

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First Draft: December, 2023
Current Draft: August, 2024
Preliminary, please do not post online or circulate via other means

[*] We appreciate the helpful comments and suggestions from Neroli Austin (discussant), Lin William Cong, Allen Hu, Aruhn Venkat, Nathan Herrmann, Zirui Wang, Jillian Grennan, Michael Gelman, and workshop and conference participants at the University of Texas at Austin, the 2024 SFS Cavalcade North America, and the 2024 DEFT Academy Summer Institute in Digital Finance.

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Abstract

The last decade has seen a significant boom in large technology companies (BigTechs) entering the credit market by integrating lending with their core business ecosystems. Using a novel dataset from a leading e-commerce BigTech, we address an under-examined question raised by this integration: How does BigTech credit (the lending business) shape users' consumption patterns on the e-commerce platform (the core business)? In a randomized context, our stacked difference-in-differences analyses demonstrate a 19% increase in monthly spending among credit recipients. This is mainly driven by the increase in the purchase frequency, consisting of visit frequency and visit-to-purchase conversion tendency, rather than the amount spent per order. Credit-induced spending is more pronounced among individuals that have less access to traditional bank credit, are less active on the e-commerce platform, or reside in regions with better logistics infrastructure for e-commerce, highlighting the BigTech's financial inclusion role and the synergy effect between its lending and core businesses. Credit recipients also demonstrate increased variety-seeking behavior, choosing a wider range of products and brands. Despite its strong consumption effects, we find no evidence that BigTech credit causes excessive spending, as it does not lead to higher discretionary spending or purchases of more expensive items. Additionally, neither higher spending boost nor higher credit limit is associated with higher delinquency rates.

Keywords: BigTech, FinTech, Consumer Credit, Consumption, Delinquency, Household Finance, China

1. INTRODUCTION

In recent years, large technology companies (BigTechs) such as Apple and Amazon from the United States, and Alibaba and Tencent from China, have expanded beyond their core businesses into the lending market. As a distinctive category within the Financial Technology (FinTech) sector, BigTechs have surpassed other FinTechs in global lending volume, becoming three times larger than all other FinTechs combined as of 2019 (Berg, Fuster, and Puri, 2022; Cornelli et al., 2023). This paper leverages a randomized context with highly granular data from a leading e-commerce BigTech to explore how BigTech credit (the lending business) shapes credit users' consumption patterns on the e-commerce platform (the core business)?

This question is important for two reasons. First, we have little empirical evidence about the implications of BigTechs' integration of their lending and core businesses, despite this being considered a primary source of their competitive advantage in the lending market (Stulz, 2022; Li and Pegoraro, 2022). Unlike traditional financial intermediaries and other FinTechs, BigTechs leverage a data-network-activities feedback loop (BIS, 2019). Specifically, their core businesses serve a large number of users, generating extensive data for their operations. By tapping into the extensive user data they possess, BigTechs are able to integrate lending services into their existing platforms. In turn, the lending business can reinforce the core business, attracting more users, generating more data, and enhancing competitiveness (Doerr et al., 2023). These unique dynamics suggest that the nature and consequences of BigTech lending can differ significantly from those of traditional banks and other FinTechs (Liu, Lu, and Xiong, 2024). Therefore, investigating the interplay between BigTechs' lending and core businesses is essential to understanding the business model and its boom in the global lending market.

Second, understanding the research question is crucial for protecting consumer welfare and ensuring the long-term sustainability of BigTech credit. BigTech credit has the potential to enhance financial inclusion, as BigTechs can leverage predictive algorithms and machine learning on extensive alternative data, giving them an advantage in providing credit to underserved populations (Berg et al., 2020; Wang, Wu, and Zhang, 2019; Balyuk, 2023; Ouyang, 2023). However, it also poses risks of causing excessive spending and over-indebtedness. These risks arise when loans are issued to borrowers ineligible for formal credit or when the ease and speed of accessing credit tempt borrowers to overextend financially (Wang and Overby, 2022; deHaan et al., 2024). This issue is particularly acute among e-commerce BigTechs with integrated lending, as it can distort the behavior of present-biased individuals who struggle to resist extra credit at checkout (Huang, 2022). Therefore, investigating how the increasingly popular BigTech credit shapes credit users' consumption patterns is important for consumer protection.

Nevertheless, addressing the research question faces major challenges. First, it requires extremely comprehensive and granular data. To study the implications of the integration of BigTechs' lending and core businesses, one needs to observe not only user-level credit applications, outcomes, and adjustments within the lending business but also detailed information regarding user activities within the core business on the BigTech platform. Second, lending decisions are highly endogenous. Consumers self-select to apply for BigTech credit, and then the BigTech makes lending decisions based on applicants' profiles and activity history. Consequently, one cannot simply compare consumption patterns between those who receive BigTech credit and those who never apply or are rejected, when estimating the impact of BigTech credit.

To address these challenges, we employ a novel micro-level proprietary dataset from one of the world's largest e-commerce BigTechs. This BigTech extends unsecured revolving credit to consumers. Approved consumers later use the credit for purchases on the e-commerce platform through installment plans and receive monthly credit bills detailing the debt due for that month. We construct a random sample of consumers who applied for BigTech credit between April 2020 and September 2020, and compile a consumer-month panel from April 2019 to March 2021. This long sample period gives us at most 17 (12) months before (after) credit applications to detect any pre-trend or long-term consequences. Our dataset includes information on demographics (age, gender, and location), credit management (credit scores, applications, decisions, and credit limits), and browsing and purchasing activities (visit intensity, visit-to-purchase conversion, and spending) within the BigTech's ecosystem. Additionally, for a random subsample of consumers, we even obtain their detailed order-item level purchase histories (product, shop, brand, category, price, and quantity) to enhance our analysis of BigTech credit's impact on consumption patterns.

For identification, we employ the reject inference (RI) setting in which credit applicants initially rejected by the BigTech's auto-underwriting system (AI model hereafter) are randomly approved and assigned initial credit limits. Typically, the AI model's parameters are estimated using a training sample that excludes rejected applicants due to the absence of their delinquency outcomes, resulting in potential biases when the model is applied to the whole pool of applicants (Hand and Henley, 1993; Crook and Banasik, 2004; Li et al., 2017). To correct this, the BigTech randomly approves a representative sample of AI-rejected applicants, so that the BigTech can learn from their realizations and infer the delinquency outcomes for the rest AI-rejected applicants (i.e., reject inference). Adding these inferred delinquency outcomes of rejected applicants back to the training sample can correct the biases in the AI model. In our study, these AI-rejected but RI-approved applicants comprise the treatment group, while the remaining AI-rejected applicants serve as the control group. Although RI is an essential component for any credit providers that involve credit scoring models in their underwriting (either FinTechs or banks), to our knowledge, our study is the first that proposes utilizing this setting for identification.

To estimate the causal effects of BigTech credit on consumption patterns, we adopt propensity score matching (PSM) and stacked cohort Difference-in-Differences (stacked DiD). Our PSM eliminates observable differences in demographics, demand for BigTech credit, credit risk, and past consumption level and shopping habits through one-on-one matching between treatment and control applicants. Our stacked DiD utilizes the staggered nature of BigTech credit applications to account for any time-varying treatment effects (Baker, Larcker, and Wang, 2022; Goodman-Bacon, 2021). The staggered nature also helps rule out many alternative explanations for our findings. Any viable confounding factor must vary not only over time, but also by consumer, in lockstep with 30,441 treatment events (application approvals) in our study.

Our analysis begins by evaluating the overall effect of BigTech credit on a key aspect of consumption patterns, namely, the spending amount on the e-commerce platform. Both the unconditional plot and the event study reveal a parallel trend in spending for 17 months before treatment and an elevated spending level persisting over 12 months after treatment, underscoring the lasting influence of BigTech credit on consumer expenditures. Consumers who receive BigTech credit experience an average 19% increase in monthly spending compared to those who do not receive credit, suggesting the impact of credit granting is economically meaningful. Through the use of various sample selection approaches and model specifications, we confirm the robustness of our findings.

To shed light on the mechanisms through which BigTech credit boosts consumer spending, we decompose the spending effect into two factors: the average spending per order and the number of orders placed within the month. Our findings reveal that while the provision of credit slightly increases the average spending per order by 3.5%, it significantly increases the number of orders by 20%. This indicates that BigTech credit primarily stimulates spending through an increase in purchase frequency. Further analysis reveals that BigTech credit enhances purchase frequency by increasing both underlying components, visit frequency and visit-to-purchase conversion tendency, by 12% and 8%, respectively.

We conduct three tests to explore the heterogeneity in BigTech credit's impact on spending amount. The first test aims to demonstrate that BigTech credit contributes to financial inclusion. We find that the credit markedly boosts spending, especially among consumers that are younger, have lower credit scores, or reside in areas with limited credit card availability. This suggests that those who typically face greater challenges in accessing traditional credit derive the most significant benefits from BigTech credit.

The other two heterogeneity tests help us gain a deeper understanding of the symbiosis between the BigTech's lending and core businesses. We find that consumers with lower ex ante activeness on the e-commerce platform, as reflected in lower visits, visit-purchase conversion, or consumption level, experience a more significant increase in spending following the receipt of BigTech credit. This finding indicates that the expansion into financial services can reinforce BigTech's data-network-activities feedback loop, which is fundamental to its business model. By incentivizing less active consumers to increase their participation in both financial and non-financial activities on the platform, BigTechs can generate additional data and ultimately enhance their ability to offer competitive services. Last, we show that consumers in regions with well-established logistics infrastructure for e-commerce exhibit a greater increase in spending after receiving BigTech credit, consistent with the synergy effect between the BigTech's lending and core businesses. This finding highlights that the impact of BigTech credit depends on the efficiency with which consumer demand is met, particularly in relation to the BigTech's core services, such as product purchase and delivery in the context of e-commerce. Combined, these findings depict a unique relationship between the BigTech's lending and core businesses: By extending credit, the BigTech boosts demand for their e-commerce business, and the attainability of the e-commerce business, in turn, amplifies the impact of BigTech credit.

In addition to spending amount and its underlying components (e.g., visit intensity, visit-to-purchase conversion tendency, and average order value), our order-item level data allows us to examine variety seeking, another important aspect of consumption patterns. We document that the availability of BigTech credit heightens consumers' propensity to purchase a more diverse array of products from different categories, selecting items from a broader spectrum of brands, and shopping across a multitude of distinct stores.

We further categorize products into discretionary and non-discretionary products and find no evidence that access to BigTech credit results in an increase in spending on discretionary products. Alternatively, we categorize products into daily-use and necessity products and demonstrate that the BigTech credit significantly boosts spending in both of these categories. Additionally, we examine item prices in the order-item data to determine if treated consumers start purchasing more expensive items after receiving BigTech credit, relative to matched control consumers. Again, we do not find evidence that BigTech credit sways consumers to opt for higher-priced goods, even when they purchase products from the same store, brand, and/or product category.

Next, we investigate whether consumers become more likely to default on their credit bills, especially those receiving more BigTech credit or experiencing a larger boost in consumption due to BigTech credit. First, we calculate and find that the delinquency rate fluctuates between 0% and 1.6%, notably lower than the same rate observed in traditional consumer credit providers from the same region, such as commercial banks. Second, we classify each treated consumer into one of two groups: high or low initial BigTech credit limit. If BigTech credit caused excessive spending, the high group would witness a higher likelihood of delinquency. However, we find no evidence of this using various definitions of delinquency. Third, we calculate the growth rate of spending for treated consumers, benchmarked against their paired control consumers. We then classify each treated consumer into one of two groups: high or low spending boost. If BigTech-induced spending resulted in an inability to repay, the high group would witness a higher likelihood of delinquency. Again, we find no evidence of this. Overall, the collective evidence thus far suggests that BigTech credit enables treated consumers to expand their consumption on the e-commerce platform without overextending their repayment capacity.

Our paper extends the nascent literature on BigTech credit in two key ways. First, BigTech starts to dominate the FinTech lending market in recent years (Cornelli et al., 2020; Cornelli et al., 2023; Frost et al., 2019). Although review articles consistently highlight the integration of BigTechs' lending and core businesses as a primary driver of their rapid growth (Berg, Fuster, and Puri, 2022; Stulz, 2022; Allen, Gu, and Jagtiani, 2021), there is little empirical evidence on the implications of this integration. The only exception is Ouyang (2023) which demonstrates how adopting a BigTech's core business (digital wallet) facilitates consumers' access to BigTech credit. Our study shows that BigTech credit, in turn, shapes users' consumption patterns on the BigTech's core business platform. Together, we complete the reinforcing loop of BigTechs' lending and core businesses, painting a full picture of this novel business model.

Second, we identify novel synergies between the lending and e-commerce operations of the BigTech we study. Theories suggest that the synergies are the source of BigTechs' competitive advantage. For instance, BigTechs' control over their core business can enhance their ability to enforce repayment from borrowers by capturing a portion of the borrowers' cash flow within the core business (Li and Pegoraro, 2022; Huang, 2021; Boualam and Yoo, 2022). Additionally, this synergy may simply arise from the information gained by monitoring consumers' activities in the core business or from the inherent value that the core business offers to consumers (Bouvard, Casamatta, and Xiong, 2022; Huang, 2022). Our findings present new synergies within BigTechs' ecosystem: (1) the lending business stimulates the demand for the core business, especially for inactive consumers and (2) the attainability to the core business reinforces this stimulation; the more accessible the core business, the stronger the consumer response to BigTech credit.

Our paper also sheds light on growing concerns about BigTech-induced overspending. The digitalization of our economy has not only transformed the production but also increased the importance of data (Veldkamp and Chung, 2024). While the information superiority of FinTechs over traditional banks is crucial for financial inclusion (Balyuk, 2023), controlling more information also raises concerns about inducing excessive consumption. These concerns are exacerbated by the fact that regulation of FinTechs is significantly less stringent than that of banks (Buchak et al., 2018; Gopal and Schnabl, 2022). BigTechs' business model that integrates both lending and product markets further heightens these concerns (CFPB, 2019, 2023, 2024). Although pinpointing overspending is challenging, our paper thoroughly investigates the derived consequences to detect any potential clues. We find no evidence on affected consumers purchasing more discretionary or expensive items, or defaulting more frequently upon higher credit lmit or consumption boost.[1]

[1] We note the emergence of another stream of literature on Buy-Now-Pay-Later platforms (BNPLs) (Berg et al., 2023; Bian, Cong, and Ji, 2023; deHaan et al., 2024). Unlike BigTechs, BNPLs do not engage in other businesses besides consumer lending. Consequently, they neither shape consumer patterns with a controlled product market nor exhibit the market synergy between the lending and core businesses. Therefore, we consider BNPLs fundamentally different from the BigTech credit.

2. DESIGN AND SAMPLE

2.1 The BigTech and Its Reject Inference Process

The BigTech company we study is a leading e-commerce platform in China with annual revenues exceeding $100 billion, positioning it among the world's largest players. It offers a diverse array of products like electronics, appliances, clothing, cosmetics, books, groceries, etc. The BigTech's financial arm provides consumers with unsecured revolving credit for platform purchases. Access to this BigTech credit is streamlined through a simple, one-click online application process. Within minutes, the company's proprietary AI model assesses the application and, upon approval, sets an initial credit limit. This credit product is fully integrated into the e-commerce platform, allowing consumers to select it as their payment method at checkout. The BigTech issues monthly bills which consumers can repay in full or finance through their preferred installment plans. Credit limits are regularly adjusted based on consumers' recent shopping and repayment behaviors.

For the BigTech, the post-application behavior of approved consumers is a valuable data asset. Specifically, by analyzing the correlation between consumers' pre-application profiles and their actual credit outcomes (such as default frequency, default amounts, recovery rates, etc.), the BigTech can enhance its ability to predict outcomes at the application stage, thereby improving its screening skills. In the context of this BigTech's lending service, the correlation analysis is the training process of its auto-underwriting AI model, and the pre-application profiles and subsequent credit outcomes are the training data for the AI model. By taking in more and more training data, the AI model becomes increasingly powerful at identifying high-quality consumers among future BigTech credit applicants. Figure 1 shows the architecture of the auto-underwriting system. The left branch of the architecture depicts this reinforcing loop of training data generation and AI model improvement.

However, since the AI model is applied to all BigTech credit applicants, including those who are rejected, it becomes biased if trained only on the outcomes of the approved ones. To correct this bias, the company needs to infer the counterfactual outcomes of the rejected consumers and incorporate them into the training dataset to debias the AI model. This process is known as reject inference (RI) (Hand and Henley, 1993; Crook and Banasik, 2004; Li et al., 2017). In our study, the BigTech implements RI by extending credit to a sample of previously rejected applicants and monitoring their repayment performance to gather data. Then, it uses the collected data to infer the counterfactual outcomes for the rest of the rejected applicants. To maximize the efficiency of inference, this sample needs to be representative of the entire pool of rejected applicants. The key to achieving this is approving consumers for the RI purpose randomly. Figure 1 illustrates this RI process in the right branch of the architecture.[2]

2.2 Identification Strategy

The concept of randomly approving AI-rejected consumers for RI purpose aligns closely with the principles of a controlled randomized trial, similar to the field experiments conducted by Aydin (2022) in the context of a retail bank and by Karlan and Zinman (2010) in the context of a consumer finance company. Therefore, we leverage the exogeneity of BigTech credit granting for identification in our study. We designate the AI-rejected but RI-approved applicants as the treatment group, and the remaining AI-rejected applicants as the control group. We note that RI is an essential component for any credit provider that involves credit scoring models in its underwriting. This practice has been studied in applied credit management (Anderson, 2007;

[2] Besides debiasing the auto-underwriting AI model, RI also serves an operational purpose. By extending credit to randomly selected AI-rejected applicants and observing their credit outcomes, the BigTech can assess the unconditional profitability of these applicants. This provides a valuable benchmark for estimating the added value of their proprietary AI model.

Thomas, Crook, and Edelman, 2017) and widely used in the industry by both FinTechs and banks over many years (Upadhyay, 2014; Markov, Seleznyova, and Lapshin, 2022; Sawla, 2023; Bharath, 2023). However, the use of this exogeneity for identification has not been explored yet. To our knowledge, we are the first to leverage the RI procedure that is originally designed for model improvement, to facilitate causal inference.

We would like to highlight several important features of the BigTech's RI implementation that are crucial for our identification strategy. First, RI decisions are seamlessly integrated into the auto-underwriting system, and the decision notifications are indistinguishable from those normally approved by the AI model. This ensures that RI-approved applicants are unaware of the inclusion in the RI program. Second, the RI approval is the only exogenous intervention applied to this subsample of AI-rejected applicants. All subsequent actions initiated by the BigTech like credit adjustments and targeted advertising, are consistent with the company's standard processes with consumers. This isolates the effects attributable to the receipt of BigTech credit and facilitates the detection of any potential overspending induced by the credit. Third, both the selection of who receives credit and the assigned initial credit limits upon RI approval are randomly decided. This ensures that any heterogeneous effects of BigTech credit are not due to the variations in treatment intensity. Fourth, the RI specifically targets AI-rejected applicants, i.e., marginal consumers. To the extent that these consumers are more prone to over-indebtedness and including them is crucial for financial inclusiveness, this RI setting enables us to closely examine these critical issues within this key demographic. Lastly, while the principle behind RI is randomness, the specific process used by the BigTech to select RI-approved applicants is proprietary and has not been disclosed to us. In the sample construction section, we conduct sanity checks to verify the randomness of the selection process.

To estimate the effects of BigTech credit, we use a stacked DiD approach. This method compares the changes in consumption patterns of AI-rejected but RI-approved consumers (treatment group) before and after their applications are approved, relative to consumers who apply for BigTech credit in the same month but are rejected (control group). Given that applications occur in a staggered manner, this comparison ensures that consumers who applied in the same month are in the same cohort. This staggered nature of our setting helps eliminate many alternative explanations for our findings, and this stacked design avoids the bias in our estimates caused by time-varying treatment effects (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022). Notably, any viable confounding factor would need to vary not only over time but also by consumer, in lockstep with 30,441 treatment events (approved applications) in our study. Specifically, we estimate the following model using a panel dataset 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 represents the cohort (with each cohort corresponding to applications made each month over the RI period), i the consumer, t the calendar month, $\alpha_{c,i}$ individual-cohort fixed effects controlling for time-invariant consumer characteristics, and $\delta_{c,t}$ month-cohort fixed effects controlling for consumer-invariant calendar month characteristics. $Outcome$ denotes a series of variables for consumption patterns of the e-commerce platform's consumers, including but not limited to visit intensity (number of visits in a sample month), visit-to-purchase conversion tendency (number of visits scaled by number of orders placed in a sample month), and spending amount (total amount paid for orders placed in a sample month). $Treated$ is a binary variable indicating RI-approved applicants, and $Post$ is a binary variable taking the value of one for the application month and subsequent months. The coefficient β captures the average treatment effect of BigTech credit. We estimate the coefficient using Poisson instead of Ordinary Least Squares (OLS) regressions to obtain valid semi-elasticity estimates since recent studies indicate that log transformations of non-negative dependent variables can bias estimates, particularly when the variables are right-skewed or have a high frequency of zero values (Cohn, Liu, and Wardlaw, 2022; Chen and Roth, 2023).[3] To address potential serial and cross-sectional correlations, we implement two-way clustering of standard errors at the individual-cohort and month-cohort levels. Definitions of all variables are provided in Table A1 of the Appendix.

2.3 Data and Sample

We present the complete sample selection procedure in Table A2 of the Appendix. Our sample construction starts by preparing the BigTech credit applications of treated and control consumers. Specifically, we select a random set of consumers who applied for credit between April 2020 and September 2020 (RI period) and were initially rejected by the AI model. Approximately one-third of these applicants were subsequently approved under the RI program. We designate them as the treatment group, and the rest as the control group. Since we later monitor consumer activities on the e-commerce platform and credit outcomes from April 2019 to March 2021 (sample period), any switch from the treatment to control group or vice versa would bias our estimates. Therefore, we drop consumers who switched between the treatment and control groups during the sample period.[4] We then collect their applications made during the RI period. For the treatment group, we retain only their AI-rejected but RI-approved applications, while for the control group, we keep all their AI-rejected applications.

Next, we merge our application data with demographics (age, gender, and location),

[3] For instance, one of the key aspects of the consumption patterns, spending level, could be zero if no orders are placed in a month. Nevertheless, all our findings are robust to OLS regressions.

[4] For instance, a consumer who was rejected during the RI period (control) can later get treated during the sample period by applying for BigTech credit again and getting approved.

activities on the e-commerce platform (visit intensity, visit-to-purchase conversion, and spending), and credit outcomes (credit scores, applications, decisions, and credit limits) to generate a panel of individual-month observations. We exclude consumers with missing demographics. We aggregate activities on the e-commerce platform at the monthly level during the sample period. Each individual has at most 24 monthly observations in our sample. For applicants from the first cohort (i.e., the first month of the RI period), there are a maximum of 12 monthly observations before and after the application. For those from the last cohort (i.e., the last month of the RI period), there are up to 17 monthly observations before and 7 after the application.[5] We then integrate the panel data with credit account data to obtain treated consumers' credit outcomes. We also follow the literature and exclude inactive consumers from our sample (Agarwal, Qian, and Zou, 2021).[6] This approach allows us to focus on a sample where any impact of BigTech credit on consumption patterns and over-indebtedness is most likely to manifest. Following this step, there are 30,441 and 83,938 consumers in the treatment and control groups, respectively.

It is important to note that our sample period spans from 17 months before to 12 months after BigTech credit applications. This long timeframe is crucial as it (1) allows for a long pre-event period to detect any potential differences across the treatment and control groups, (2) helps determine whether any observed effect is temporary or persistent, and (3) leaves enough post-event time for any potential adverse consequences to surface.

Given that the specific process of RI selection is proprietary and has not been disclosed to us, we conduct sanity checks to assess the randomness of the selection process. Despite the statistically significant differences across many dimensions due to the large sample size, these

[5] Note that if consumers opened their e-commerce accounts after April 2019, their monthly observations start from their account registration month to March 2021, thereby having fewer than 24 monthly observations in our sample.
[6] We consider consumers inactive if they have zero monthly spending in more than one-third of their pre-application sample months. Our findings are robust to other definitions or the exclusion of this filter.

differences are generally modest in economic magnitude. For instance, the difference in internal credit scores—a critical factor in the AI's credit-granting decisions—between the treatment and control groups is only 0.65, with an average score of around 689. This suggests that RI-approved and rejected applicants are highly comparable across various dimensions. However, we do note that RI-approved applicants tend to have younger e-commerce accounts, fewer visits, and lower spending levels.[7] To mitigate observable differences (even economically insignificant ones), we employ nearest-neighbor PSM with logistic regressions to pair treated and control consumers within each cohort, balancing demographics, demand for BigTech credit, credit risk, consumption level, and online shopping habits. For a detailed list of variables used in PSM and their differences across groups before and after matching, please refer to Table A3 in the Appendix. In Table A4, we confirm the randomness of initial credit limit assignments among treated consumers by demonstrating that all PSM variables have small correlation coefficients with the initial credit limit and collectively explain only 3% of its variation.

Ultimately, our sample includes 30,441 treated and 30,441 control consumers with 635,919 and 625,011 monthly observations, respectively. For a random 10% subsample of these consumers, we obtain their detailed order-item level purchase histories (product, shop, brand, category, price, and quantity) to enhance our analysis of BigTech credit's impact on consumption choices. In some other supplemental analyses, we impose additional restrictions on the sample as discussed in detail in the relevant sections below.

2.4 Summary Statistics

We present summary statistics of key variables used in this study in Table 1. Panel A provides the demographic profile of the 60,882 applicants: approximately 65% are male, the

[7] If anything, these differences should work against finding positive impact of BigTech credit on spending level.

average age is about 31 years old, they have been registered on the e-commerce platform for roughly 30 months, and have applied for BigTech credit on average 1.28 times. For treated consumers, the initial credit limit is set at an average of 331 RMB. The RI program sets small initial credit limits to minimize risk exposure. Over the course of our sample period, as detailed in Panel B, the BigTech adjusts these limits, resulting in an average monthly credit limit of 1,278 RMB.

Panel B provides summary statistics for the variables of our individual-month panel. On average, a consumer in our sample visits product pages 91 times per month, places orders at a rate of 0.46 for every 10 product page visits, and makes 2.17 orders per month with an average order spending of 154 RMB, totaling 320 RMB spent per month. The number of observations for most variables aligns with the total individual-months in our panel, which is 1,260,930. However, *Conversion* and *SpendingPerOrder* have fewer observations due to some months having no orders placed, resulting in the absence of denominators needed for these calculations. For credit limit and credit outcomes, there are even fewer observations because only treated consumers have these records.

Panel C provides summary statistics for variables derived from the subsample of consumers with order-item level purchase histories. On average, a consumer in this subsample purchases 1.28 units of an item within an order at an average item price of 128 RMB. Monthly, this consumer typically buys 2.17 stock-keeping units (SKUs, or unique products in the e-commerce's inventory) from 1.23 product categories, 1.74 brands, and 1.77 stores on the e-commerce platform. Their average spending is 61 RMB on discretionary items, 156 RMB on daily use items, and 135 RMB on necessities. Note that *ItemPrice* and *ItemQuantity* are measured at the order-item level, whereas other variables are measured at the individual-month level.

3. HOW BIGTECH CREDIT SHAPES CONSUMPTION PATTERNS?

3.1 The Impact of BigTech Credit on Spending and the Underlying Components

We begin by focusing on the spending level, a key aspect of consumption patterns. We compare *Spending*—the total amount spent during the month, excluding any canceled transactions and discounts—before and after the approval of BigTech credit for the treatment group, relative to the control group. Transactions on the application day and the following day are excluded to prevent the inclusion of consumption potentially planned in anticipation of receiving BigTech credit.

Both unconditional plot and event study are employed to analyze pre-trends and the persistence of the effect of BigTech credit on *Spending*. Panel A of Figure 2 illustrates the average *Spending* in the treatment and control groups separately. Prior to the credit application, both groups display similar spending levels, ranging from 230 RMB to 350 RMB. Upon receiving BigTech credit, the treatment group shows a significant 21% increase in spending to 401 RMB in the application month compared to the previous month, while the control group experienced a modest 7% increase during the same period. Moreover, treated consumers consistently spend more than their control counterparts, indicating the long-lasting impact of BigTech credit on consumer expenditure. These dynamics are further validated through an event study, as depicted in Panel B of Figure 2. The results show that the differences in spending amount for 15 out of 17 pre-event months are statistically insignificant, suggesting no pre-trend, while the differences for all 12 post-event months are statistically significant and large, indicating the long-lasting impact of BigTech credit. Collectively, both the unconditional plot and the event study find no evidence of pre-trend and a significant increase in spending lasting over 12 months following the treatment.

Table 2 Column (1) presents the results of substituting the dependent variable in Eq. (1)

with *Spending*. Consistent with the findings in Figure 2, the coefficient for the interaction term *Treated* * *Post* is positive and significant at the 1% level, indicating that consumers in the treatment group experience a significant 19% increase in their monthly spending following the granting of BigTech credit, relative to consumers in the control group.

We conduct a series of robustness tests through various sample selections and model specifications. The first and second tests use a rolling window instead of calendar month and exclude the initial month of credit approval, respectively, to eliminate any potential confounding factors from the application month. The third test adopts the traditional staggered DiD approach with time fixed effects and individual fixed effects. The fourth test employs the state-of-the-art approach proposed by Callaway and Sant'Anna (2020), which addresses both cohort- and time-varying treatment effects. The fifth test runs on a subsample that excludes the application month and any months after month one post-treatment to eliminate the impact of any credit limit adjustments subsequent to the initial credit assignments. Our findings remain consistent across these tests, as presented in Table A5.

Next, we focus on the underlying components of *Spending* to delve into the mechanisms through which credits drive the increase. *Spending* is decomposed into two components: *Orders* and *OrderSpending*. *Orders* equals the total number of orders made by a consumer in a given month, while *OrderSpending* reflects the average spending amount per order for a consumer in a specific month. Therefore, *Spending* is essentially the product of *Orders* and *OrderSpending*. Eq. (1) is estimated with these two components as the dependent variables, and the findings are presented in Table 2 Columns (2) and (3). In Column (2), the interaction term *Treated* × *Post* displays a positive sign (significant at the 1% level), but the effect size on *OrderSpending* is not very economically significant, with the grant of BigTech credit only escalating the average order spending by 3.5%. In Column (3), the interaction term *Treated* × *Post* continues to display a positive sign (significant at the 1% level). In economic terms, the BigTech credits boost consumers' frequencies of placing orders by 20%. Therefore, BigTech credit enhances spending mainly through an increase in the frequency of purchases rather than an increase in the average spending amount per order.

We further decompose the frequency of purchases, *Orders*, into two steps: *Visits*, denoting the total number of product page visits a consumer makes in a given month, and *Conversion*, determined as the number of orders normalized by 10×*Visits*, serving as a measure of the consumers' inclination in converting visiting to purchasing. The results in Columns (4) and (5) show that the interaction term *Treated* × *Post* is positive and significant at the 1% level in both columns, suggesting that BigTech credit positively influence consumers' visit intensity and visit-purchase conversion tendency.

3.2 Heterogeneities in the Impact of BigTech Credit on Spending Level

3.2.1 BigTech Credit and Financial Inclusion

Our first cross-sectional analysis aims to demonstrate the role of BigTech credit in fostering financial inclusion. We capture consumers' financial constraints in three ways. First, we assume that consumers under 22 years old (i.e., *YoungAge* equals one), a typical age for college graduation, face more challenges in securing credit from traditional financial institutions due to a higher probability of unemployment and a lack of established credit history. Second, we use consumers' credit scores as a proxy for financial constraints. These credit scores are generated by the BigTech company using numerous variables within and external to the firm's ecosystem, including credit bureau reports that banks heavily rely on. These scores are used to evaluate borrowers' propensity to repay loans on time at their applications. We expect that individuals with lower BigTech credit scores (i.e., *LowCreditScore* equals 1) may face higher difficulty accessing traditional credit services. Third, we assess consumers' financial constraints using the UPD Index (Regional Bank Credit Card Development Vitality Index) from China UnionPay (the official payment processing networks in China, like Visa and Mastercard) to measure bank credit accessibility in different provinces. Lower UPD Index scores indicate less developed credit card services, and we determine each consumer's province using their pre-application delivery addresses, positing that consumers in lower-scoring provinces (i.e., *LowUPDIndex* equals 1) may face greater challenges securing bank credit.

Using triple DiD, we confirm that, as shown by the *F*-tests in Panel A of Table 3, the spending increase is more pronounced for consumers who are younger, have lower credit scores, and are located in regions with less developed credit card services. This finding is consistent with the notion that BigTech credit mitigates the potential exclusion or marginalization of consumers by traditional credit systems.

3.2.2 BigTech Synergy between Its Lending and Core Businesses

Next, we explore the heterogeneities in the treatment effect of BigTech credit to understand the interplay between the BigTech's lending and core businesses. First, we focus on consumers' ex ante activeness on the BigTech's e-commerce platform, measuring from three aspects: browsing intensity, visit-to-purchase conversion rate, and spending level. We create three groups of indicators: *HighPastVisits* (*LowPastVisits*) equals one if the monthly average of Visits for 12 months (or less if fewer months available) prior to receiving BigTech credit is above (below) the median, and zero otherwise. We define *HighPastConversion* (*LowPastConversion*) and *HighPastSpending* (*LowPastSpending*) in the same manner. Panel B of Table 3 shows that the increase in spending resulting from BigTech credit is more prominent among consumers with lower levels of past visits, visit-to-purchase conversion, or consumption amount, compared to consumers in the counterpart groups (difference significant at the 1% level). These findings suggest that BigTech credit proves highly effective at engaging and creating the demand of less active consumers on the platform for the core business.

Second, we explore the role of attainability of the core business on the impact of BigTech credit. If the synergy between credit service and core business of the BigTech exists, there should be a more pronounced stimulative effect of BigTech credit on spending when the core business is more attainable. Here, we measure attainability with the development of regional logistics infrastructure. We present the regression results from triple DiD in Panel C of Table 3. Column (1) compares the spending effect of BigTech credit among consumers in the top five provinces with the highest expressway length per capita to those in the bottom five provinces. Top-province consumers (*TopExpressway* equals 1) exhibit a 27% increase in spending, significantly higher than the 14% increase observed in the bottom provinces (*TailExpressway* equals 1). This finding suggests that the effectiveness of BigTech credit is contingent upon the promptness with which consumer demands are met, specifically for core services like product purchase and delivery offered by e-commerce in our case.

Another factor that might influence e-commerce is Internet infrastructure. However, since all BigTech credit applicants must submit their applications online, it implies that consumers in our sample have adequate Internet access for using the e-commerce platform. Therefore, we do not anticipate Internet access to be a binding factor in this context. As a falsification test, we conduct cross-sectional analysis in Columns (2) and (3), measuring regional Internet development using metrics of *TopMobileInternet* (*TailMobileInternet*) and *TopBroadband* (*TailBroadband*). As expected, the impact of BigTech credit does not vary with regional Internet development.

Combined, these findings indicate a unique symbiotic relationship between the BigTech's lending and core businesses. By extending credit, the BigTech boosts demand for their e-commerce business, and the attainability of the e-commerce business, in turn, amplifies the impact of BigTech credit.

3.3 The Impact of BigTech Credit on Variety Seeking

After investigating spending levels and their underlying components (e.g., visit intensity, visit-to-purchase conversion tendency, and spending per order), we shift our focus to variety seeking. As an important aspect of spending patterns, variety seeking reflects consumers' desire for novelty and diversity in their purchases. Thus, a close examination of possible changes in variety seeking provides a more comprehensive understanding of how credit influences consumer behavior and decision-making. We conjecture that the availability of BigTech credit can encourage variety seeking by providing consumers with increased purchasing power and flexibility.

To formally test our conjecture, we construct four metrics using the order-item data: *SKUs*, *Categories*, *Brands*, and *Shops*. *SKUs* refers to the number of unique products that a consumer purchases within a specific month. *SKUs* are unique alphanumeric codes or numbers assigned to each distinct product or item in the e-commerce' inventory, providing the most precise identification of products. *Categories* represents the number of unique product categories that a consumer purchases within a specific month.[8] *Brands* and *Shops* signify the number of brands or shops from which a consumer makes purchases within a specific month.

We conduct a series of stacked DiD analyses using Eq. (1), substituting the dependent variable with each of these metrics. The results from Table 4 consistently suggest a highly

[8] The e-commerce platform in our study categorizes products into three levels. The broadest category definition encompasses 73 categories, while the finest category definition consists of 5,067 categories. Although the main paper presents results based on the broadest definition, our findings remain consistent when utilizing the other two more detailed category definitions.

significant increase (around 20%) in all variety seeking measures upon the granting of BigTech credit. Since consumers' variety seeking can be seen as a signal of impulse buying (Sharma, Sivakumaran, and Marshall, 2010; Punj, 2011; Olsen et al., 2016), the finding documented in this section, along with the overall rise in spending following credit granting shown before, exacerbates the concern regarding BigTech-induced overspending. In the upcoming section, we carry out a series of analyses to investigate this issue.

4. DOES BIGTECH CREDIT CAUSE OVERSPENDING?

Given incomplete information about consumers' financial profiles and utility functions, one cannot be certain whether consumers' spending levels are optimal. As an alternative approach, we infer overspending by analyzing several possible consequences of excessive spending. We achieve this by leveraging our highly granular consumer data, which encompasses both non-financial activities and financial activities within the same ecosystem.

4.1 Consumption Upgrade

If BigTech credit encourages overspending, we might expect treated consumers to engage in consumption upgrades. To explore this possibility, we categorize products as discretionary, daily use, or necessity based on their product category.[9] Using order-item data, we define *DiscretionarySpending* as the total expenditure on discretionary items by a consumer in a calendar month. Similarly, *DailyUseSpending* and *NecessitySpending* are defined to measure expenditures on daily use items and necessities, respectively. As shown in Column (1) of Table 5, the relationship between *Treated * Post* and *DiscretionarySpending* is insignificant. In contrast,

[9] We manually classify product categories into three types (discretionary, daily use, and necessity) based on our best judgment. Our definition for discretionary products encompasses a wide range of product categories, including Digital, Sports & Outdoors, Gifts, Books, Music, Movies & TV, Cultural & Entertainment, Local Living/Travel, Watches, Digital Content, Jewelry & Accessories, Toys & Musical Instruments, Automotive Accessories, Pet Supplies, Alcohol, Automotive, Agricultural Supplies & Gardening, Stamps & Coins, and Art. For details on the product categories classified as daily use and necessity items, please refer to Table A1.

Columns (2) and (3) indicate a positive and significant relationship (at the 5% or 1% level) between *Treated * Post* and both *DailyUseSpending* and *NecessitySpending*. In summary, we find no evidence that access to BigTech credit results in higher spending on discretionary items; instead, it appears to increase spending on daily-use items and necessities. These findings contradict the notion that BigTech credit leads to irresponsible spending by stimulating non-essential item purchases.

Another manifestation of irresponsible spending is that consumers start to opt for higher-end versions of needed items after receiving BigTech credit. To explore this possibility, we analyze whether item prices in their orders have increased for treated consumers post-credit, relative to their matched control counterparts. We use *ItemPrice*, the price of an item in a specific order, as the dependent variable. Our model incorporates additional fixed effects for shop, brand, category, or shop-brand-category. Such fixed effects structures are highly effective in identifying any form of consumption upgrade by comparing *ItemPrice* within the same shop, brand, category, or shop-brand-category. For instance, if a consumer upgrades from a low-end to a high-end cell phone, regardless of brand changes, this transition should be detected by the within-category comparison. Similarly, even if a consumer remains in the same shop-brand-category and only purchases a slightly more expensive item, this subtle shift should be captured by the within shop-brand-category comparison.

Table 6 presents our findings. The interaction term *Treated * Post* consistently shows an insignificant coefficient in all columns, indicating no evidence that BigTech credit prompts consumers to buy more expensive items, even within the same shop, brand, category, or shop-brand-category.[10] These findings are again inconsistent with the notion that BigTech credit leads

[10] Our results reveal that credit receipts improve their consumption set through product variety rather than product quality, as indicated by the little change in item price. Correspondingly, Butler, Demirci, Gurun, and Tellez (2024)

to irresponsible spending by encouraging pricier item purchases.[11]

4.2 Delinquency of BigTech Credit Users

A key consequence of excessive spending is frequent delinquency on credit bills. To assess this, we calculate the company-level delinquency rate for each month, measured as the balance of loan principal overdue by more than 90 days divided by the total balance of the consumer credit loan principal facilitated through the BigTech's financial arm. This definition aligns with those used by traditional credit card issuers, such as commercial banks. We find that the delinquency rate fluctuates between 0% and 1.6% across our sample period, significantly lower than the range between 1.4% and 3.3% in 2020 for major Chinese commercial banks' credit card business. This contradicts the notion that BigTech causes consumer over-indebtedness more than other credit providers.

Furthermore, if BigTech credit causes overspending, we might expect that consumers receiving higher initial credit limits are more likely to default. To test this hypothesis, we categorize treated consumers into high and low groups based on the median of their initial credit limits assigned by the BigTech. The indicator variable *HighInitialCredit* equals one for the high credit limit group and zero otherwise. We define two variables for individual-level credit outcomes: *Delinquency30Day* and *Delinquency90Day*, which are set to one if a consumer is delinquent on a monthly credit bill for more than 30 and 90 days, respectively. We analyze the correlation between these delinquency indicators and the group indicator by regressing the former on the latter. The

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demonstrate that consumers become more price-sensitive and shrink their consumption set in response to financial shocks.

[11] In untabulated result, we also use *ItemQuantity,* the number of items purchased in an order, as the dependent variable. Notably, the interaction term *Treated* * *Post* also yields insignificant results. Since the spending per order can be viewed as the product of *ItemQuantity* multiplied by *ItemPrice*, these outcomes also affirm earlier observations from the user-month data—that the monthly average order amount does not escalate with the credit. They further confirm that the BigTech credit's positive effect on overall spending is primarily driven by an increase in the frequency of order placement by credit recipients.

data for this analysis only includes post-treatment individual-month observations of treated consumers. Table 7 displays regression results. The coefficient for *HigherInitialCredit* is not significant across both columns, indicating no significant difference in the likelihood of default between consumers who received more and less BigTech credit.

Lastly, if BigTech credit causes overspending, we might expect that the spending boost it induces leads to an inability to repay, resulting in a higher likelihood of default among treated consumer who experience higher spending increases. To test this hypothesis, we first calculate the BigTech-induced growth rate of spending for each treated consumer by comparing the average monthly spending before and after treatment, relative to the matched control consumer. We then categorize treated consumers into high and low spending effect groups based on the sample median of the growth rate. The indicator variable *HighSpendingEffect* equals one for the high spending effect group and zero otherwise. In Table 8, we use the same individual-level credit outcomes, and find that the coefficient for *HigherSpendingEffect* is not significant, indicating no significant difference in the likelihood of default between consumers who experienced substantial spending increases and those with minor increases after receiving BigTech credit. These findings provide further evidence against the idea of BigTech credit worsening consumers' financial health.

5. CONCLUSION

In recent years, BigTechs, known for their technology services such as e-commerce, search engines, social media, and hardware and software development, have ventured into the credit services sector, surpassing other FinTechs in global lending volume. BigTechs stand out due to their integrated ecosystems, which combine lending with their core businesses that generate extensive user data for their business extension. This integration raises a key research question that has been underexplored in the literature: How does the lending business shape the spending patterns of consumers on the firms' core business platforms? This question is crucial for understanding the dynamics and risks of BigTechs' business models.

Our paper explores these issues using a novel, detailed dataset from a leading e-commerce BigTech credit provider. Our findings indicate that, compared to control consumers, those who receive BigTech credit demonstrate an approximate 19% increase in monthly spending post-credit granting. Delving into the components of monthly spending, we show that BigTech credit mainly makes an impact by increasing the number of orders placed (via strengthening both visit frequency to the platform and visit-purchase conversion tendency), rather than the amount spent per order.

Heterogeneity analyses indicate that individuals facing challenges in obtaining traditional credit, displaying a lower level of past activeness on the BigTech's core business platform, and residing in areas with advanced logistical systems show a more significant spending increase after receiving BigTech credit. These findings align with the objectives of financial inclusion and highlight the synergistic relationship between the financial and non-financial sectors of BigTech firms.

With access to order-item-level data, we further document that credit recipients tend to purchase from a wider range of product categories and select items from a broader array of brands, shopping across numerous distinct stores. However, this enhanced inclination toward variety-seeking does not equate to reckless spending.

We reach this conclusion by showing that BigTech credit is not associated with increased spending on discretionary products or on more expensive items within the same shop, brand, category, or even shop-brand-category. Furthermore, at the firm level, the BigTech shows a lower level of delinquency rate compared to credit card issuers, and at the individual level, consumers with a greater increase in credit-boosted expenditure do not display a higher likelihood of delinquency than others. Moreover, consumers with higher initial BigTech credit limits are not more likely to default, either.

Overall, our findings underscore that the seamless integration of lending services with the BigTech' core businesses, coupled with its advanced data analytics capabilities, enables it to extend credit to promote financial inclusion and reshape consumption patterns on e-commerce platform without clear signs of inducing consumer over-indebtedness. This highlights the importance of BigTechs in reshaping the financial services landscape and driving economic growth.

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Figure 1. The Architecture of Auto-underwriting System

Notes: This figure presents the architecture of auto-underwriting system. BigTech credit applicants arrive and go through an AI model to get approved or rejected. The left branch of the architecture depicts the reinforcing loop of training data generation and AI model improvement. For the approved applicants, their realized credit outcomes (compliance and default) serve as the training data for the AI model. However, using this training data alone will bias the AI model because the outcomes are only for approved applicants but the AI model will be applied to all applicants. The right branch of the architecture depicts the process of reject inference (RI). The BigTech first select a representative sample of AI-rejected applicants and extend them credit. Then, the credit outcomes of these AI-rejected but RI-approved applicants are used to infer the counterfactual credit outcomes for the rest of the rejected applicants. The credit outcomes of the AI-rejected applicants (either realized or inferred) are then added to the training data to debias the AI model.

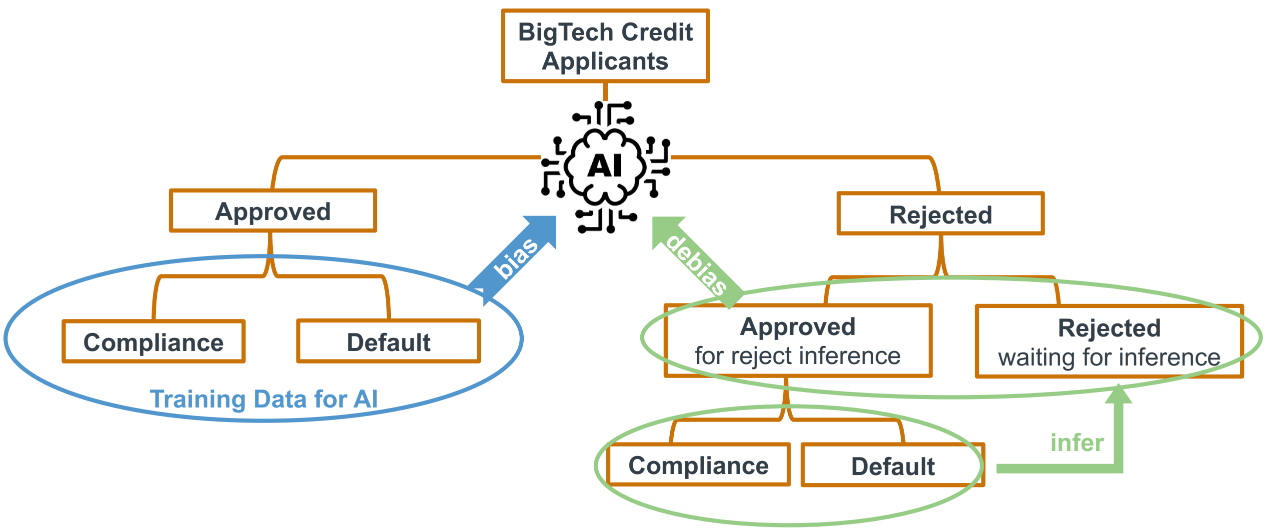
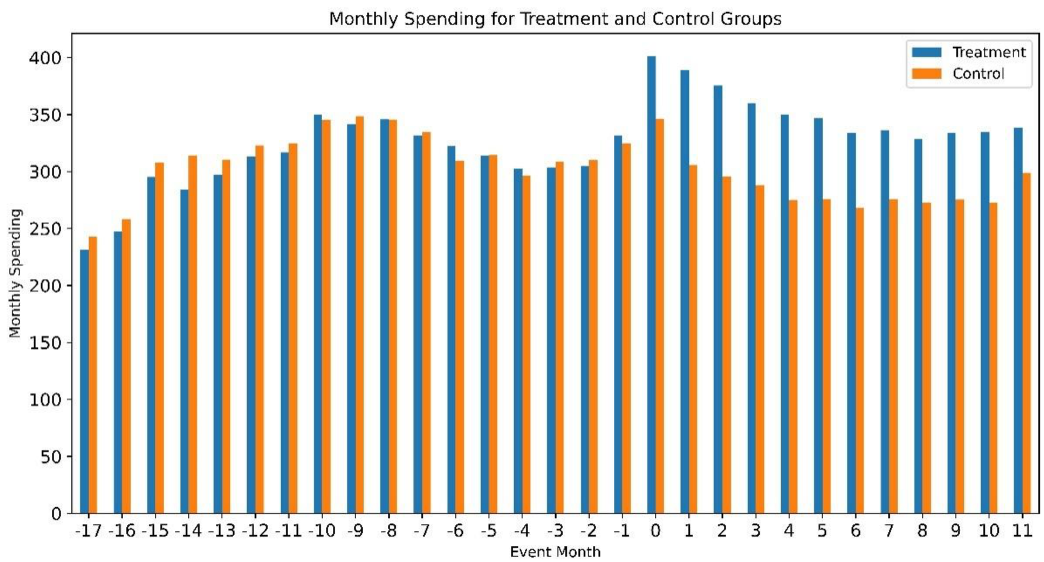


Figure 2. The Unconditional Plot and the Event Study Plot for BigTech Credit's Effect on Spending

Notes: This figure presents the effect dynamics of BigTech credit on spending. Panel A plots the monthly spending before and after credit application for the treatment and control group. The y-axis is the monthly spending measured in RMB. The x-axis is the number of months before or after the application, whereas 0 stands for the month of application. Panel B plots the coefficients β_k from a stacked cohort difference-in-differences analysis using this specification: $Spending_{c,i,t} = \sum_{k=-17}^{-2} \beta_k Treated_{c,i} \times 1_{c,k} + \sum_{k=0}^{11} \beta_k Treated_{c,i} \times 1_{c,k} + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar month, k is the subscript for event month (month relative to the application month), $\alpha_{c,i}$ represents individual-cohort fixed effects, $\delta_{c,t}$ represents month-cohort fixed effects, and $1_{c,k}$ is the indicator that equals one for event month k of cohort c and zero otherwise. We estimate the model using Poisson regression. We cluster standard errors by both individual-cohort and month-cohort.

Panel A. The Unconditional Plot for Spending



Panel B. The Event Study Plot for Spending

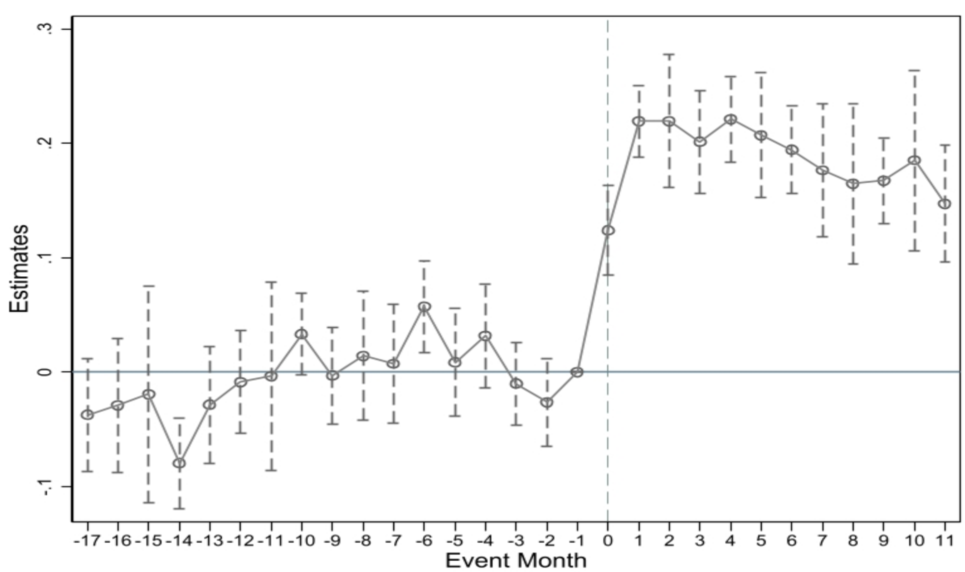


Table 1. Summary Statistics

Notes: This table presents summary statistics of the key variables used in this study. Table A1 presents variable definitons. Panel A focuses on the demographics of applicants in our sample. *InitialCreditLimit* has fewer observations than other variables becaue only treated consumers were assigned initial credit limits. Panel B provides summary statistics for the variables of our individual-month panel. *OrderSpending* and *Conversion* have fewer observations due to some months having no orders placed or no amount spent, resulting in the absence of denominators needed for these calculations. *CreditLimit*, *Delinquency30Day*, and *Delinquency90Day* have fewer observations because only treated consumers have these records. Panel C is for variables derived from the subsample of consumers with order-item level purchase histories. *ItemPrice* and *ItemQuantity* are measured at the order-item level, whereas other variables are measured at the individual-month level. All continuous variables are winsorized at 1% and 99% levels.

Panel A. Consumer Demographics (individual level)

	Count	Mean	Std.	25%	50%	75%
Male	60,882	0.65	0.48	0	1	1
Age	60,882	30.55	10.88	22	28	36
AccountAge	60,882	29.73	24.5	8	25	46
CreditScore	60,882	689.14	36.25	663	688	715
Applications	60,882	1.28	0.77	1	1	1
InitialCreditLimit	30,441	331.04	172.41	200	400	500

Panel B. Consumption Patterns and Credit Outcomes (individual-month level)

	Count	Mean	Std.	25%	50%	75%
Spending	1,260,930	320.15	886.14	0.00	19.90	226.31
OrderSpending	711,815	154.32	288.07	29.90	69.90	147.21
Orders	1,260,930	2.17	3.64	0	1	3
Visits	1,260,930	90.63	167.71	4	29	99
Conversion	1,039,755	0.46	0.79	0.00	0.19	0.54
CreditLimit	264,000	1278.44	2154.34	200	500	850
Delinquency30Day	264,000	0.02	0.15	0.00	0.00	0.00
Delinquency90Day	264,000	0.01	0.10	0.00	0.00	0.00

Panel C. Consumption Patterns based on Order-Item Data (order-item level and individual-month level)

	Count	Mean	Std.	25%	50%	75%
SKUs	125,372	2.17	4.70	0	1	2
Categories	125,372	1.23	1.85	0	1	2
Brands	125,372	1.74	3.34	0	1	2
Shops	125,372	1.77	3.37	0	1	2
DiscretionarySpending	125,372	60.83	463.59	0.00	0.00	0.00
DailyUseSpending	125,372	155.74	2628.85	0.00	0.00	68.51
NecessitySpending	125,372	135.25	550.98	0.00	0.00	79.90
ItemPrice	283,402	127.73	314.74	19.90	42.90	99.00

Table 2. The Impact of BigTech Credit on Consumer Spending and Its Components

Notes: This table presents the effect of BigTech credit on consumer spending and its components. We perform a stacked cohort difference-in-differences analysis using the following specification: $Spending/Component\ of\ Spending_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c.t} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar month, $\alpha_{c,i}$ represents individual-cohort fixed effects, and $\delta_{c,t}$ represents month-cohort fixed effects. The components of spending is either *OrderSpending*, *Orders*, *Visits or Conversion*. Variable definitions are in Table A1. The coefficients are estimated using Poisson regressions. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
		Components of Spending			
Dep. Var.=	***Spending***	***OrderSpending***	***Orders***	***Visits***	***Conversion***
*Treated * Post*	0.190***	0.035***	0.203***	0.116***	0.079***
	(13.837)	(3.255)	(19.655)	(11.427)	(8.751)
Individual FE	YES	YES	YES	YES	YES
Month-Cohort FE	YES	YES	YES	YES	YES
Observations	1,260,930	709,636	1,260,930	1,258,210	1,038,746
Pseudo R-squared	0.377	0.341	0.264	0.511	0.161

Table 3. Heterogeneities in the Impact of BigTech Credit on Consumer Spending
- The Evidence of Financial Inclusion and Synergies between BigTech's Financial and non-Financial Sectors

Notes: This table presents the heterogeneous impact of BigTech credit on spending. In Panel A, we perform a stacked cohort difference-in-differences analysis with group indicators using the following specification: $Spending_{c,i,t} = \beta_1 \times Treated_{c,i} \times Post_{c,t} \times HighConstraint_{c,i} + \beta_2 \times Treated_{c,i} \times Post_{c,t} \times LowConstraint_{c,i} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$. In Panel B, we replace *HighConstraint* (*LowConstraint*) with *HighPriorActiveness* (*LowPriorActiveness*). In Panel C, we replace *HighConstraint* (*LowConstraint*) with *HighCoreInfrastructure* (*LowCoreInfrastructure*). In all specifications, *c* is the subscript for cohort, *i* is the subscript for consumer, *t* is the subscript for calendar month, $\alpha_{c,i}$ represents individual-cohort fixed effects, and $\delta_{c,t}$ represents month-cohort fixed effects. *Constraint* is a list of variables that measure consumers' level of financial constraints. *PriorActiveness* is a list of variables that measure the level of ex ante consumer activeness on the e-commerce platform. *CoreInfrastructure* is a list of variables that measure the level of infrastructure (i.e., logistics and Internet) development for the core business. Variable definitions are in Table A1. Applicable Interactions include any applicable two-way interaction terms between the group indicators and *Treated* or *Post*. We obtain the estimates using Poisson regressions. We perform Wald Chi-squared test to compare the effects of BigTech credit across different groups, and report the p-value of the test in the last row. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Effect Heterogeneities across Financial Constraint

Dependent Variable = *Spending*	**(1)**	**(2)**	**(3)**
*Treated * Post * YoungAge*	0.439***		
	(12.466)		
*Treated * Post * OldAge*	0.130***		
	(9.015)		
*Treated * Post * LowCreditScore*		0.369***	
		(16.188)	
*Treated * Post * HighCreditScore*		0.113***	
		(6.759)	
*Treated * Post * LowUPDIndex*			0.331***
			(4.950)
*Treated * Post * HighUPDIndex*			0.214***
			(9.697)
Applicable Interactions	YES	YES	YES
Individual-Cohort FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	1,260,930	1,260,930	483,674
Pseudo R-squared	0.377	0.377	0.376
p-value ($\beta_1 = \beta_2$)	**0.000**	**0.000**	**0.000**

Panel B. Effect Heterogeneities across Prior Activeness on the Core Business Platform

Dependent Variable = *Spending*	**(1)**	**(2)**	**(3)**
*Treated * Post * LowPastVisits*	0.290***		
	(12.572)		
*Treated * Post * HighPastVisits*	0.146***		
	(9.149)		
*Treated * Post * LowPastConversion*		0.292***	
		(14.996)	
*Treated * Post * HighPastConversion*		0.139***	
		(8.344)	
*Treated * Post * LowPastSpending*			0.386***
			(15.571)
*Treated * Post * HighPastSpending*			0.132***
			(8.661)
Applicable Interactions	YES	YES	YES
Individual-Cohort FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	1,260,930	1,254,882	1,260,930
Pseudo R-squared	0.377	0.380	0.383
p-value ($\beta_1 = \beta_2$)	**0.000**	**0.000**	**0.000**

Panel C. Effect Heterogeneities across Infrastructure Development Levels

Dependent Variable = *Spending*	**(1)**	**(2)**	**(3)**
*Treated * Post * TopHighway*	0.270***		
	(4.113)		
*Treated * Post * TailHighway*	0.136***		
	(5.472)		
*Treated * Post * TopMobileInternet*		0.206***	
		(9.803)	
*Treated * Post * TailMobileInternet*		0.220***	
		(5.098)	
*Treated * Post * TopBroadband*			0.214***
			(6.890)
*Treated * Post * TailBroadband*			0.195***
			(3.779)
Applicable Interactions	YES	YES	YES
Individual-Cohort FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	328,370	584,962	267,325
Pseudo R-squared	0.392	0.381	0.381
p-value ($\beta_1 = \beta_2$)	**0.058**	0.772	0.753

Table 4. The Impact of BigTech Credit on Variety Seeking

Notes: This table presents the effects of BigTech credit on variety seeking behavior. We perform a stacked cohort difference-in-differences analysis using the following specification: $VarietySeeking_{c,i,t} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, where i is the subscript for consumer, c is the subscript for cohort, t is the subscript for calendar month, $\alpha_{c,i}$ represents individual-cohort fixed effects, and $\delta_{c,t}$ represents month-cohort fixed effects. The dependent variable *VarietySeeking* is one of four measures: *SKUs, Categories, Brands,* and *Shops*. Variable definitions are in Table A1. We use the order-item data aggregated at the monthly level. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Dependent Variable	*SKUs*	*Categories*	*Brands*	*Shops*
*Treated * Post*	0.200***	0.180***	0.185***	0.194***
	(5.506)	(6.748)	(5.468)	(5.727)
Individual-Cohort FE	YES	YES	YES	YES
Month-Cohort FE	YES	YES	YES	YES
Observations	125,324	125,324	125,211	125,139
Pseudo R-squared	0.324	0.206	0.285	0.284

Table 5. The Impact of BigTech Credit on Discretionary and Necessary Spending

Notes: This table presents the impact of BigTech credit on the spending on discretionary, daily use and necessity items. The data used here is the order-item data aggregated at the monthly level. We perform a stacked cohort difference-in-differences analysis using the following specification: $SpendingOnCertainItems_{c,i,t} = \beta_1 \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar month, $\alpha_{c,i}$ represents individual-cohort fixed effects, and $\delta_{c,t}$ represents month-cohort fixed effects. The dependent variable *SpendingOnCertainItems* is either *DiscretionarySpending*, *DailyUseSpending*, or *NecessitySpending*. Variable definitions are in Table A1. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	**(1)** *DiscretionarySpending*	**(2)** *DailyUseSpending*	**(3)** *NecessitySpending*
*Treated * Post*	0.138	0.253**	0.175***
	(1.565)	(2.336)	(2.764)
Individual-Cohort FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	111,560	121,518	122,868
Pseudo R-squared	0.390	0.670	0.394

Table 6. The Impact of BigTech Credit on the Price of Items Purchased

Notes: This table presents the impact of BigTech credit on the price of items purchased. The data used here is the order-item data with unique observations at the order-item level. We perform a stacked cohort difference-in-differences analysis using the following specification: $ItemPrice_{c,i,t,j,k} = \beta \times Treated_{c,i} \times Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \zeta_{c,i,j,k} + \varepsilon_{c,i,j,k}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar date, j is the subscript for order, k is the subscript for item, $\alpha_{c,i}$ represents individual-cohort fixed effects, $\delta_{c,t}$ represents date-cohort fixed effects, and $\zeta_{c,i,j,k}$ represents the shop, brand, category, or shop-brand-category fixed effects that the order-item belongs to. Variable definitions are in Table A1. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual-cohort and month-cohort in all regressions. t-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable = *ItemPrice*	**(1)**	**(2)**	**(3)**	**(4)**
*Treated * Post*	-0.020	-0.017	0.024	-0.015
	(-1.263)	(-0.978)	(0.973)	(-0.975)
Individual-Cohort FE	YES	YES	YES	YES
Date-Cohort FE	YES	YES	YES	YES
Shop FE	YES	NO	NO	NO
Brand FE	NO	YES	NO	NO
Category FE	NO	NO	YES	NO
Shop-Brand-Category FE	NO	NO	NO	YES
Observations	250,289	256,971	283,211	231,857
Pseudo R-squared	0.850	0.794	0.484	0.872

Table 7. The Correlation between BigTech Credit' Boosting Impact and Consumer Delinquency

Notes: This table presents the correlation between the boosting impact of BigTech credit on spending and the delinquency of consumers. The data used here is the individual-month observations for treated consumers after they receive BigTech credit. We perform OLS regressions using the following specification: $DelinquencyIndicator_{c,i,t} = \beta \times HighSpendingEffect_{c,i} + \delta_{c,t} + \varepsilon_{c,i,j,k}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar date, and $\delta_{c,t}$ represents month-cohort fixed effects or dummies. The dependent variable *DelinquencyIndicator* is either *Delinquency30Day* or *Delinquency90Day*. Variable definitions are in Table A1. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	**(2)**
Dependent Variable	*Delinquency30Day*	*Delinquency90Day*
HighSpendingEffect	0.001	-0.001
	(0.578)	(-1.402)
Month-Cohort FE/Dummy	YES	YES
Observations	264,000	264,000
Adjuste R-squared	0.004	0.006

Table 8. The Correlation between Initial Credit Limit and Consumer Delinquency

Notes: This table presents the correlation between the BigTech's initial credit limits assigned and the delinquency of consumers. The data used here is the individual-month observations for treated consumers after they receive BigTech credit. We perform OLS regressions using the following specification: $DelinquencyIndicator_{c,i,t} = \beta \times HighInitialCredit_{c,i} + \delta_{c,t} + \varepsilon_{c,i,j,k}$, where *c* is the subscript for cohort, *i* is the subscript for consumer, *t* is the subscript for calendar date, and $\delta_{c,t}$ represents month-cohort fixed effects or dummies. The dependent varibale *DelinquencyIndicator* is either *Delinquency30Day* or *Delinquency90Day*. Variable definitions are in Table A1. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	**(2)**
Dependent Variable	*Delinquency30Day*	*Delinquency90Day*
HighInitialCredit	-0.002	-0.000
	(-1.539)	(-0.262)
Month-Cohort FE/Dummy	YES	YES
Observations	264,000	264,000
Adjusted/Pseudo R-squared	0.004	0.006

Table A1. Variable Definitions

Notes: This table provides the definitions of variables used in this study.

Main Independent Variables	
Treated	A dummy variable which is set to one if a consumer's credit application is rejected by the AI model but approved for the purpose of reject inference, and zero if the application remains rejected by the AI model.
Post	A dummy variable which is set to one if a calendar month is the month a consumer applies for credit or any subsequent months, and zero otherwise.
RepayRate	The proportion of a monthly credit bill amount that is repaid.
RefinanceRate	The amount split into installments scaled by the amount that can be split in a monthly credit bill.
RefundRate	The amount refunded scaled by the amount that can be split in a monthly credit bill.
Main Dependent Variables	
Spending	The total out-of-pocket consumption (i.e., canceled amount and discount are excluded) for a consumer in a month. For the application month, the consumption on the application day and the day after application is excluded.
OrderSpending	The average order value of the orders placed by a consumer in a month. For the application month, the orders made on the application day and the day after application are excluded.
Orders	The number of orders placed by a consumer in a month. For the application month, the orders made on the application day and the day after application are excluded.
Visits	The total number of product page visits made by a consumer in a month. For the application month, the visits on the application day and the day after application are excluded.
Conversion	The number of orders placed by a consumer in a month scaled by 10 * *Visits*.
SKUs	The number of unique products (i.e., stock keeping units or SKUs) involved in a consumer's consumption in a month.
Categories	The number of unique product categories involved in a consumer's consumption in a month.
Brands	The number of unique brands involved in a consumer's consumption in a month.
Shops	The number of unique shops involved in a consumer's consumption in a month.
DiscretionarySpending, DailyUseSpending, NecessitySpending	The spending on discretionary/daily-use/necessity items for a consumer in a month. The discretionary spending encompasses a wide range of product categories, including Digital, Sports & Outdoors, Gifts, Books, Music, Movies & TV, Cultural & Entertainment, Local Living/Travel, Watches, Digital Content, Jewelry & Accessories, Toys & Musical Instruments, Automotive Accessories, Pet Supplies, Alcohol, Automotive, Agricultural Supplies & Gardening, Stamps & Coins, and Art. The daily-use category includes Home Appliances, Apparel & Underwear, Beauty & Skin Care, Home & Daily Use, Kitchenware, Pet Products, Health & Wellness, Shoes, Prescription Medicine, Home Textiles, Household Cleaning & Paper Products, Personal Care and Bags & Leather Goods. The necessity category includes Apparel & Underwear, Mother & Baby, Food & Beverages, Home & Daily Use, Kitchenware, Health & Wellness, Mobile Phones & Communication, Shoes, Fresh Produce, Prescription Medicine, Household Cleaning & Paper Products and Personal Care.
ItemPrice	The price of an item purchased in an order.
Delinquency30Day (*Delinquency90Day)*	A dummy variable that equals one if a consumer's payment for a monthly credit bill is more than 30 days (90 days) overdue, and zero otherwise.
CreditLimit	The credit limit the BigTech assigns to a consumer in a month.
Variables for Heterogeneity Analysis	
YoungAge (OldAge)	A dummy variable that equals one if a consumer's age is (not) smaller than 22, and zero otherwise.
LowCreditScore	A dummy variable that equals one if a consumer's credit score for application is

(HighCreditScore)	below (above) the median, and zero otherwise.
LowUPDIndex (HighUPDIndex)	A dummy variable that equals one if a consumer resides in a province with a below-(above-)median UPD Index. We obtain UPD Index (Regional Bank Credit Card Development Vitality Index) from China UnionPay.
HighPastVisit (LowPastVisit)	A dummy variable that equals one if a consumer's mean *Visits* in the 12-month-window prior to credit granting is above (below) the median, and zero otherwise. In the case where the user account is less than 12 months old, the average is calculated for as many months as possible.
HighPastConversion (LowPastConversion)	A dummy variable that equals one if a consumer's mean *Conversion* in the 12-month-window prior to credit granting is above (below) the median, and zero otherwise. In the case where the user account is less than 12 months old, the average is calculated for as many months as possible.
HighPastSpending (LowPastSpending)	A dummy variable that equals one if a consumer's mean *Spending* in the 12-month-window prior to credit granting is above (below) the median, and zero otherwise. In the case where the user account is less than 12 months old, the average is calculated for as many months as possible.
TopHighway (TailHighway)	A dummy variable that equals one if a consumer is from a province in which expressway road length scaled by the population is among the top (bottom) five provinces, and zero otherwise.
TopMobileInternet (TailMobileInternet)	A dummy variable that equals one if a consumer is from a province whose number of mobile internet users scaled by the population is among the top (bottom) five provinces, and zero otherwise.
TopBroadband (TailBroadband)	A dummy variable that equals one if a consumer is from a province where broadband access per capital is among the top (bottom) five provinces, and zero otherwise.
HighSpendingEffect	A dummy variable only for treated consumers. We define a treated consumer's average spending in the periods before and after credit application as a and b, respectively. Correspondingly, we identify the spending of the paired control consumer's spending during the same periods as c and d. We then compute the formula [(b-a)-(d-c)]/a to measure the change in spending. *HighSpendingIncrease* takes one if the treated consumer's spending increase is above the sample median among treated consumers, and zero otherwise.
HighInitialCredit	A dummy variable that equals one if a consumer's initial credit limit is above the median among all treated consumers.
HighAverageCredit	A dummy variable that equals one if a consumer's average credit limit across post-treatment months is above the median among all treated consumers.
Other Variables	
Male	A dummy variable that equals one if a consumer is a male, and zero otherwise.
Age	The age of a consumer measured in years.
AccountAge	The number of months since a consumer signed up with the e-commerce platform.
CreditScore	The internal credit score generated by the AI model for application.
Applications	The number of applications made by a consumer at application (current application included).
InitialCreditLimit	The initial credit limit granted to a consumer when the application is approved.
CreditBill	A dummy variable that equals one if a consumer utilizes any credit and has a positive credit bill amount to repay in a month, and zero otherwise.

Table A2. Sample Construction Procedure

Notes: This table outlines the procedure for constructing the individual-month panel. Our sample comprises AI-rejected applicants from April to September 2020 (RI period). One-third were approved under the RI program, forming the treated group, while the rest serves as controls. We exclude any consumers who switched groups during the sample period from April 2019 to March 2021. We collect their applications made during the RI period. We merge the application data with demographics (age, gender, location), e-commerce activities (visit intensity, conversion rates, spending), and credit outcomes (credit scores, decisions, limits) to create a panel of individual-month observations. Consumers with missing demographics are excluded. We aggregated e-commerce activities at the monthly level, with each consumer having up to 24 monthly observations. We integrate the panel data with credit account data to obtain the credit outcomes of treated consumers. We exclude inactive consumers to focus on those most likely to exhibit impacts of BigTech credit on consumption patterns and overspending. To mitigate any observable differences across the treatment and control group, we also implement month-by-month PSM to pair each treated consumer with the "nearest" control consumer.

		Approved for Reject Inference			Rejected by AI Model		
#	**Steps**	**# Users**	**# Apps**	**# User-Months**	**# Users**	**# Apps**	**# User-Months**
1	Select applications between April 2020 and September 2020	129,813	129,813	-	340,314	445,457	-
2	Drop applicants with missing demographic information	128,232	128,232	-	328,972	431,573	-
3	Create user-month panel from April 2019 to March 2021	128,232	128,232	2,789,975	328,972	431,573	9,704,460
4	Merge with shopping data to obtain visit and consumption information	128,232	128,232	2,789,975	328,972	431,573	9,704,460
5	Merge with credit data to obtain credit usage and outcomes	128,232	128,232	2,789,975	321,310	422,883	9,502,220
6	Drop inactive users by pre-application spending	30,441	30,441	635,919	83,938	110,473	2,412,557
7	Perform month-by-month PSM without replacement	30,441	30,441	635,919	30,441	30,441	625,011

Table A3. Balance Check for PSM Variables

Notes: This table compares the means of variables used for the propensity score matching between the treatment group (AI-rejected RI-approved consumers) and the control group (AI-rejected consumers). The comparison is made both before and after the propensity score matching. *PreVisits*, *PreOrders*, and *PreSpending* are the total number of product visits, orders placed, and amount spent in the 12-month window prior to credit application scaled by the number of months with nonzero visits, orders, and spending, respectively. *PreDiscount* and *PreCancel* are the total discount and canceled amount scaled by the total order amount in the 12-month window prior to credit application, respectively. Definitions of other variables are in Table A1 of the Appendix.

	Variable	Mean (RI-Approved)	Mean (AI-Rejected)	Difference
Before Matching	*Male*	0.64	0.70	-0.06***
	Age	30.50	30.46	0.04
	AccountAge	29.74	36.68	-6.94***
	Applications	1.28	1.92	-0.64***
	CreditScore	689.11	688.46	0.65***
	PreVisits	89.94	108.69	-18.75***
	PreOrders	2.17	2.51	-0.34***
	PreSpending	320.45	378.74	-58.28***
	PreDiscount	0.19	0.18	0.01***
	PreCancel	0.27	0.30	-0.03***
After Matching	*Male*	0.64	0.65	0.00
	Age	30.50	30.60	-0.10
	AccountAge	29.74	29.73	0.01
	Applications	1.28	1.28	0.00
	CreditScore	689.11	689.17	-0.06
	PreVisits	89.94	89.76	0.18
	PreOrders	2.17	2.17	0.00
	PreSpending	320.45	315.64	4.81
	PreDiscount	0.19	0.19	0.00
	PreCancel	0.27	0.27	0.00

Table A4. Randomness of Initial Credit Limit Assignment

Notes: This table provides evidence of the randomness in the assignment of initial credit limits. It includes (1) the Pearson correlation coefficients between the initial credit limit (*InitialCreditLimit*) and applicants' demographic characteristics and behavior patterns on the BigTech's e-commerce platform prior to credit application, and (2) the adjusted R-squared from regressing *InitialCreditLimit* on these variables (shown in the bottom row). *PreVisits*, *PreOrders*, and *PreSpending* are the total number of product visits, orders placed, and amount spent in the 12-month window prior to credit application scaled by the number of months with nonzero visits, orders, and spending, respectively. *PreDiscount* and *PreCancel* are the total discount and canceled amount scaled by the total order amount in the 12-month window prior to credit application, respectively. Definitions of other variables are in Table A1 of the Appendix.

InitialCreditLimit	**Pearson Correlation Coefficient**
Male	-0.06
Age	0.07
AccountAge	0.05
Applications	-0.04
CreditScore	0.15
PreVisits	0.02
PreOrders	0.01
PreSpending	0.03
PreDiscount	0.01
PreCancel	-0.03
Adj. R2 of regressing *InitialCreditLimit* on all above variables	0.03

Table A5. Robustness Checks for the Effects of BigTech Credit on Spending

Notes: This table presents the robustness checks for the effects of BigTech credit on consumer spending. Column (1) uses an alternative definition of *Spending*, which is the total out-of-pocket consumption for a consumer in a rolling-window month rather than a calendar month. The rest columns use the original definition of *Spending*. Column (2) excludes the application month for the regression. Column (3) uses the traditional staggered DID with two-way fixed effects. Column (4) employs the state-of-the-art approach proposed in Callaway and Sant'Anna (2020) to address heterogeneities of the treatment effect. Column (5) excludes the application month and all relative months after post-treatment month 1 for the regression. All columns use Poisson regression except Column (4) which uses OLS. Variable definitions are in Table A1. We cluster standard errors by both individual-cohort and month-cohort in all regressions. *t*-statistics are presented in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep.Var. = *Spending*	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Specification	Rolling-window	Month≠0	Staggered DID	C&S DID	Month≠0 & Month<=1
Method	Poisson	Poisson	Poisson	OLS	Poisson
*Treated * Post*	0.207***	0.200***	0.193***	43.338***	0.229***
	(16.190)	(14.337)	(12.680)	(6.300)	(15.144)
Individual-Cohort FE	YES	YES	YES	-	YES
Month-Cohort FE	YES	YES	NO	-	YES
Month FE	NO	NO	YES	-	NO
Observations	1,300,723	1,200,048	1,260,930	961,502	772,098
Pseudo R-squared	0.372	0.378	0.376	-	0.389