Deciphering the Impact of BigTech Consumer Credit*

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Abstract

The last decade has witnessed a significant surge in large technology companies (BigTechs) entering the credit market. Using a proprietary dataset from a leading e-commerce BigTech and a novel reject inference setting—where credit applicants initially rejected by the BigTech's auto-underwriting system are randomly approved—we examine how BigTech credit shapes users' spending behavior. Our stacked difference-in-differences analyses reveal a 19% increase in spending among credit recipients. This is primarily driven by increased purchase frequency, consisting of higher visit frequency and improved visit-to-purchase conversion rates. Credit-induced spending is more pronounced among individuals likely excluded from traditional bank credit, less active on the platform, or residing in regions with better logistics infrastructure for e-commerce, indicating BigTechs' role in financial inclusion and the synergy between the lending and core businesses. Credit recipients demonstrate increased consumption variety by choosing a wider range of products, stores, and brands, suggesting that BigTech credit amplifies the value e-commerce adds to the consumer experience. Despite strong consumption effects, we find no evidence of overspending. Our research demonstrates the implications of BigTechs integrating their lending and core businesses, and helps explain the surge in popularity of BigTechs in the FinTech lending sector.

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; Liu, Lu, and Xiong, 2024). This paper leverages a randomized context with highly granular data to explore how BigTech credit, originating from the firm's lending business and integrated into its broader ecosystem, shapes credit users' spending behavior.

Addressing this research question sheds light on the interplay between BigTechs' lending and core businesses, which is essential to understanding this innovative business model and their rapid expansion in the global lending market. While the integration of lending and core businesses is considered the primary source of BigTechs' competitive advantage in the lending market, empirical evidence about the implications of such integration remains scarce. Prior research theorizes that, unlike traditional financial intermediaries and other FinTechs, BigTechs operate on a Data-Network-Activities feedback loop (BIS, 2019). In this loop, data fuels consumer network growth, enabling more activities, which in turn generates more data, creating a self-reinforcing cycle that continuously strengthens their market presence. This unique feedback loop further allows BigTechs to seamlessly integrate lending services into their existing platforms, leveraging extensive user data. As a result, the lending business not only benefits from but also reinforces the core business by attracting more users, generating richer data, and enhancing product/service competitiveness. These unique dynamics suggest that the nature and consequences of BigTech lending can differ significantly from those of traditional banks and other FinTechs (Stulz, 2022;

Liu, Lu, and Xiong, 2024).

Moreover, addressing our research question provides valuable insights into the ongoing discussion about the benefits and risks of this increasingly popular form of credit. On the one hand, BigTech credit has a significant potential to boost financial inclusion. Similar to other FinTechs, BigTechs can effectively serve underbanked populations by leveraging vast alternative data and sophisticated predictive algorithms (Berg et al., 2020). Unlike other FinTechs, their unique access to core business platforms provides them with extensive customer bases and detailed behavioral insights, likely enhancing their ability to identify and address unmet financial needs. On the other hand, BigTech credit poses risks of 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 BigTech credit tempt borrowers to overextend themselves financially. This issue is particularly acute among e-commerce BigTechs with integrated lending, which may alter consumer behavior by offering readily accessible credit at checkout, challenging individuals' financial self-control.

Despite its importance, addressing the research question faces two major challenges. First, it requires extremely comprehensive and granular data not only on user-level credit applications and delinquency outcomes within a BigTech's lending business, but also on user activities within its core business platform, especially considering that consumer spending is a multi-faceted concept. 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 consumer spending between those who receive BigTech credit and those who do not, when estimating the impact of BigTech credit.

To address the first challenge, 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 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, credit applications, application decisions, credit limits, and delinquency outcomes), 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 history (product, shop, brand, category, price, and quantity) to enhance our analysis of BigTech credit's impact.

To address the second challenge, we utilize the reject inference (RI) setting in which credit applicants initially rejected by the BigTech's auto-underwriting system (hereafter AI model) are randomly approved and assigned initial credit limits. The AI model is typically trained on data without rejected applicants since their delinquency outcomes are not observed, potentially causing bias when the AI model is applied to all applicants. To mitigate this, the BigTech randomly approves some rejected applicants, observes their outcomes, and infers outcomes for the rest. 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. We then apply the stacked cohort difference-in-differences (stacked DiD) method (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022) to these two groups to infer the causal effects of BigTech credit on consumer spending. It is also worth noting that while reject inference is an essential component for any credit

provider using credit scoring models in their underwriting (whether FinTechs or banks), to our knowledge, we are the first to introduce this setting for identification.

Our analysis begins by evaluating the overall effect of BigTech credit on a key aspect of consumer spending behavior, namely, the monthly spending level 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 the 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.

Next, 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 rate, by 12% and 8%, respectively.

We investigate the mechanisms underlying BigTech credit's impact on spending through three heterogeneity tests. First, 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 BigTech credit's role in advancing financial inclusion by benefiting demographics that typically face greater challenges in accessing traditional consumer credit.

The other two heterogeneity tests help us gain a deeper understanding of the symbiosis between the BigTech's lending and core businesses. Consumers who are less active on the e-commerce platform prior to credit application, as reflected in fewer visits, lower visit-to-purchase conversion, or lower spending levels, experience a more significant increase in spending following the receipt of BigTech credit. This is consistent with lending business reinforcing the BigTech's core business. This finding also demonstrates an enhanced Data-Network-Activities feedback loop that is fundamental to BigTechs' business model: by incentivizing less active consumers with credit to increase their participation in both financial and non-financial activities within the ecosystem, BigTechs can generate additional data and ultimately enhance their ability to offer competitive services.

Furthermore, 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 result suggests that the impact of BigTech credit depends on the efficiency with which consumer demand is met, particularly in relation to a BigTech's core services like product purchase and delivery in the context of e-commerce. Collectively, 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 accessibility to the e-commerce business, in turn, amplifies the impact of BigTech credit.

In addition to analyzing spending level and its underlying components (i.e., visit intensity, visit-to-purchase conversion rate, number of orders placed, and average spending per order), our order-item level data enables us to provide new insights into consumption of variety, an aspect of spending behavior that is crucial for consumer welfare and utility maximization (Sevilla, Lu, and

Kahn, 2019; Li, 2021). E-commerce offers a great opportunity for consumers to embrace consumption of variety because it provides a wider product range than brick-and-mortar stores and reduces purchasing costs, such as traveling and searching costs, that typically hinder diverse buying patterns (Bronnenberg, 2015; Quan and Williams, 2018). We find that the availability of BigTech credit significantly expands users' consumption sets, causing them to purchase a wider variety of products across different categories, choose items from a broader range of brands, and shop at a more diverse array of stores. This finding indicates that BigTech credit amplifies the benefits provided by e-commerce, thus adding value for consumers. Conversely, enhanced consumption of variety may signal impulsive spending (Ratner and Kahn, 2002; Yoon and Kim, 2018; Huang and Dong, 2019), raising concerns about consumer over-indebtedness induced by BigTech credit. However, additional analyses find no evidence that BigTech credit sways consumers to purchase more discretionary items or opt for higher-priced goods that serve the same function (i.e., within the same store, brand, product category, or even store-brand-category), suggesting that impulsive spending is unlikely.

Lastly, we examine the delinquency outcome of the BigTech credit. First, we note that the monthly delinquency rate of BigTech credit is quite low, peaking at only 1.6% during the sample period. More importantly, we investigate whether consumers become more likely to default on their credit bills when they receive larger amounts of BigTech credit or experience a more substantial boost in spending due to BigTech credit. We classify each treated consumer into high or low initial BigTech credit limit groups. If BigTech credit causes excessive spending, the high group should witness a higher likelihood of delinquency. However, we find no evidence of this using various definitions of delinquency. Furthermore, we calculate the growth rate of spending for treated consumers, benchmarked against their paired control consumers, and classify them into

high or low spending boost groups. Again, we find no evidence that the high group experiences a higher delinquency rate. Additional analyses suggest that this group also possess a greater capacity to repay their debt and more actively manage their financial obligations.

Overall, the collective evidence suggests that BigTech credit has a significant impact on credit users' spending behavior in multiple ways. These changes include increased activeness (e.g., more frequent visits and higher visit-to-purchase conversion rates), expanded spending level, and enhanced consumption of variety on the e-commerce platform. All these changes help the BigTech company expand its financial and non-financial businesses and accumulate user data, enhancing its Data-Network-Activities feedback loop. Notably, these changes occur without evidence of indulgent consumption or consumers overextending their repayment capacity.

Our paper contributes to the literature in three key ways. First, BigTechs have begun to dominate the FinTech lending market in recent years (Berg, Fuster, and Puri, 2022; Cornelli et al., 2023). In the meantime, a nascent literature on BigTech credit has emerged (e.g., Chen et al., 2022; Berg et al., 2023; Bian, Cong, and Ji, 2024; Hau et al., 2024; Liu, Lu, and Xiong, 2024). Despite BigTechs' presumed substantial impact on consumer finance (Stulz, 2022), an in-depth analysis of how BigTech consumer credit affects various key aspects of users' spending behavior is still lacking. By leveraging unique, granular data from a leading e-commerce BigTech and utilizing the setting of reject inference for identification, our paper provides new insights on the nature of the spending response to BigTech credit and two key underlying economic channels (financial inclusion and synergy).

Second, although review articles consistently highlight the interplay of BigTechs' lending and core businesses as the primary driver of their rapid growth (Berg, Fuster, and Puri, 2022; Stulz, 2022), there is little empirical evidence on that. We identify novel synergies between the lending

and e-commerce operations of the BigTech. 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, 2024). Additionally, these synergies 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; Ghosh, Pulak, and Vallee, 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 accessibility to the core business reinforces this stimulation: the more accessible the core business, the stronger the consumer response to BigTech credit. Thus, this research enhances our understanding of BigTech lending's unique business model.

More broadly, we contribute to the research on the real effects of FinTech. Emerging literature demonstrates that FinTech stimulates entrepreneurial growth (e.g., Agarwal et al., 2019; Gopal and Schnabl, 2022; Hau et al., 2024) and enhances consumer financial decisions (e.g., D'Acunto, Prabhala, and Rossi, 2019; Fuster et al., 2019; Gargano and Rossi, 2024; Rossi and Utkus, 2024; Agarwal et al., 2024; Parlour, Rajan, and Zhu, 2022). However, prior studies also suggest that FinTechs may encourage excessive consumption and over-indebtedness (Buchak et al., 2018; Chava et al., 2021; Di Maggio and Yao, 2021; Wang and Overby, 2022). This concern is further intensified by the emergence of BigTechs, a distinct category of FinTech which blur finance-technology boundaries and potentially evade oversight (CFPB, 2023, 2024). Our study enriches the understanding of how BigTech, and FinTech more generally, influences spending behaviors. Additionally, our sample constitutes of marginal consumers whose credit applications

were initially rejected by AI, thus an investigation of their spending behavior and borrowing outcomes offers valuable insights into the potential risk of BigTech lending.

2. RESEARCH 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.

The post-application behavior of approved consumers is a valuable data asset to the BigTech company. Specifically, by analyzing the correlation between consumers' pre-application profiles and post-application credit outcomes (such as default frequency, default amounts, recovery rates, etc.), the BigTech can enhance its ability to assess credit risk at the application stage, thereby enhancing 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 as well as the post-application credit outcomes are the valuable training data for the AI model. By processing increasing amounts of training data, the AI model becomes increasingly powerful at identifying high-quality consumers among future applicants. Figure 1

shows the architecture of the auto-underwriting system. The left branch of the architecture depicts this 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 sample of approved consumers. 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) (Anderson, 2007; Thomas, Crook, and Edelman, 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 RI, this sample of AI-rejected but RI-approved applicants needs to be representative of the entire pool of AI-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 Identification Strategy

The concept of randomly approving AI-rejected consumers for RI purposes aligns closely with the principles of a controlled randomized trial, similar to the field experiments conducted by Aydin (2022) at a retail bank and by Karlan and Zinman (2010) at a consumer finance company. Therefore, we leverage the exogeneity in the RI setting 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 utilizes credit scoring models in its underwriting. This practice has been studied in applied credit management and is widely used in the industry by both FinTechs and banks over many years (Anderson, 2007; Thomas, Crook, and

Edelman, 2017). 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 targeted advertisements, exclusive offers, rewards programs, and customer support, are consistent with the company's standard operating procedures. 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. Since these consumers are less likely to have access to alternative credit and are more prone to over-indebtedness, this RI setting enables us to closely examine BigTech credit's role in financial inclusion and the potential financial vulnerability it may induce within this key demographic. Last, while the principle behind RI implementation 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 spending patterns of AI-rejected but RI-approved consumers (treatment group) before and after their application approvals, 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 apply 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:

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

where c represents the cohort (with each cohort corresponding to applications made during a month), i the consumer, t the calendar month, $\alpha_{c,i}$ individual-cohort fixed effects controlling for time-invariant consumer characteristics, and $\delta_{c,t}$ calendar month-cohort fixed effects controlling for consumer-invariant calendar month characteristics. $Outcome$ denotes a series of variables for spending behavior of the e-commerce platform users, including but not limited to visit intensity (the number of visits in a sample month), visit-to-purchase conversion rate (the number of orders placed scaled by the number of visits in a sample month), and spending level (the 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 regressions instead of Ordinary Least Squares (OLS) regressions on log-transformed outcomes to obtain valid semi-elasticity estimates (Cohn, Liu, and

Wardlaw, 2022; Chen and Roth, 2023).[1] 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 selecting a random set of consumers who applied for BigTech 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 during this period of time would bias our estimates. Therefore, we drop consumers who switched between the two groups during the sample period.[2] 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 (application dates and decisions, credit scores, assigned credit limits) with demographics (age, gender, and location), activities on the e-commerce platform (visit intensity, visit-to-purchase conversion, and spending), and credit outcomes (delinquencies) 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

[1] These 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. In our case, spending level could be zero if no orders are placed in a month. So, we primarily use Poisson regressions. Nevertheless, all our findings are robust to OLS regressions.

[2] 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.

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 12 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.[3] 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 in our sample (Agarwal, Qian, and Zou, 2021).[4] This approach allows us to focus on a sample where any impact of BigTech credit on spending 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 for three reasons. First, it allows for a long pre-event period to detect any potential differences across the treatment and control groups. Second, it helps determine whether any observed effect is temporary or persistent. Third, it 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 differences are generally modest in economic magnitude. Most importantly, 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

[3] Note that some consumers opened their e-commerce accounts during the sample period. Their monthly observations start from their account registration month to the end of the sample period, leading to fewer than 24 monthly observations for each of them in our sample.

[4] 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.

RI-approved and rejected applicants are highly comparable across various dimensions, many of which remain unobservable to researchers.[5] However, we do note that RI-approved applicants tend to have younger e-commerce accounts, fewer visits, and lower spending levels.[6] To mitigate observable differences (even economically insignificant ones), we employ nearest-neighbor propensity score matching (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 history (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 average age is about 31 years old, they have been registered on the e-commerce platform for

[5] Credit scores by major Chinese BigTechs (including the one under study) are derived from upwards of tens of thousands of variables, including users' demographic details, online behaviors, credit bureau reports, digital footprints, and more.
[6] If anything, these differences should make us underestimate the impact of BigTech credit on spending levels.

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.

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 0.46 orders 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 number of 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 in Panel A and credit outcomes in Panel B, 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 history. 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* is measured at the order-item level, whereas other variables are measured at the individual-month level.

3. HOW BIGTECH CREDIT SHAPES CONSUMER SPENDING?

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

We begin by focusing on the spending level, a key aspect of consumer spending. We compare *Spending*—the total amount spent during the month after applying discounts and

excluding any canceled transactions (i.e., out-of-pocket spending)—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 rolling-window instead of calendar months 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. Our findings remain consistent across these tests, as presented in Table A5 Panel A. Furthermore, one may be concerned that as the AI model evolves over time due to the implementation of the RI program, it could undermine the comparability of applicants across different cohorts. To address this, we run the regression for each RI cohort separately, and the results, as shown in Panel B, do not indicate a clear monotonic change in the impact of the BigTech credit.

Next, we focus on the underlying components of *Spending* to decipher how BigTech credit drives the increase. *Spending* is decomposed into two components: *Orders* and *SpendingPerOrder*. *Orders* equals the total number of orders made by a consumer in a given month, while *SpendingPerOrder* reflects the average spending amount per order for a consumer in a specific month. Therefore, *Spending* is essentially the product of *Orders* and *SpendingPerOrder*. 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 coefficient of the interaction term *Treated* * *Post* is positive (significant at the 1% level), but the effect size is not large. The grant of BigTech credit only escalates the average order spending by 3.5%. In Column (3), the coefficient of the interaction term *Treated* * *Post* is also positive (significant at the 1% level). In economic terms, the BigTech credit boosts 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 components: *Visits*, the number of product page visits a consumer makes in a given month, and *Conversion*, the number of orders placed in a given month normalized by 10 * *Visits*, capturing consumers' inclination in converting visiting to purchasing. The results in Columns (4) and (5) show that the coefficient of the interaction term *Treated* * *Post* is positive and significant at the 1% level in both columns, suggesting that BigTech credit increases consumers' visit intensity and visit-to-purchase conversion rate by 12% and 8%, respectively.

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 consumer 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 Wald tests in the last row of Panel A of Table 3, the BigTech's spending effect is more pronounced for consumers who are younger, have lower credit scores, and reside in regions with less developed credit card services. This finding is consistent with the notion that BigTech credit mitigates the potential exclusion of consumers by traditional credit systems.

3.2.2 BigTech Synergies 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: visit 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 visit intensity, visit-to-purchase conversion rate, or spending, compared to consumers in the counterpart groups (differences significant at the 1% level). These findings suggest that BigTech credit proves highly effective at stimulating the demand of less active

consumers for the core business.

Second, we explore the role of accessibility to 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 stimulation of BigTech credit on spending when the core business is more attainable. Here, we measure accessibility of e-commerce services by 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, consumers in our sample should already 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 mobile and broadband Internet penetration rates. 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 Consumption of Variety

After investigating spending levels and their underlying components (e.g., visit intensity, visit-to-purchase conversion rate, purchase frequency, and spending per order), we shift our focus to consumption of variety. Variety consumption plays a crucial role in enhancing consumer welfare and facilitating utility maximization (Sevilla, Lu, and Kahn, 2019; Li, 2021), making it an important aspect of spending behavior to examine.

E-commerce provides an ideal setting for this examination due to its ability to offer a much more diverse range of products to consumers than physical stores (Quan and Williams, 2018). Moreover, e-commerce has made the benefits of consumption of variety more accessible by significantly reducing purchasing costs that typically hinder diverse buying patterns, such as traveling, searching for products, waiting in queues, or negotiating with sales agents (Bronnenberg, 2015). We expect that BigTech credit amplifies these benefits by providing consumers with increased purchasing power and flexibility. Specifically, we hypothesize that the availability of BigTech credit will encourage consumers to purchase a wider range of products.

To formally test our conjecture, we construct four metrics using the order-item data to proxy for consumption of variety: *SKUs*, *Categories*, *Brands*, and *Shops*. SKU (stock-keeping unit) is a unique alphanumeric code or number assigned to each distinct product or item in the e-commerce' inventory, providing the most precise identification of products. *SKUs* refers to the number of unique products that a consumer purchases within a specific month. *Categories* represents the number of unique product categories that a consumer purchases within a specific month.[7] *Brands* and *Shops* counts the number of brands and shops from which a consumer makes

[7] The e-commerce platform in our study has three definitions for product categorization. The broadest category definition encompasses 73 categories, while the finest category definition consists of 5,067 categories. Although the

purchases within a specific month, respectively.

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 significant increase (around 20%) in all consumption of variety metrics upon the granting of BigTech credit. These outcomes point to BigTech credit as a catalyst for consumers to better leverage the benefits of retail e-commerce.

However, enhanced consumption of variety can also be seen as a signal of impulse buying as consumers pursue variety seeking to fulfill psychological and emotional needs, such as compensating for a low sense of personal control or satisfying their desire to impress others (Ratner and Kahn 2002; Yoon and Kim 2018; Huang and Dong, 2018). When considered alongside the overall rise in spending following credit granting, this interpretation aligns with BigTech inducing overspending. To determine which scenario is more plausible, we conduct further analyses in the following sections, examining (1) two additional aspects of consumer spending patterns that may associate with overspending, namely discretionary spending and purchase of pricier products and (2) a potential adverse outcome of using credit for excessive consumption, namely delinquencies.

3.4 Purchase of Discretionary Products

If BigTech credit encourages overspending, we might expect treated consumers to engage in more discretionary spending. To explore this possibility, we categorize products as discretionary, daily use, or necessity based on their product category.[8] Using order-item data, we define

main paper presents results based on the broadest definition, our findings remain consistent when utilizing the other two more detailed category definitions.

[8] 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.

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, Columns (2) and (3) indicate a positive and significant relationship (at the 5% and 1% level, respectively) 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 increases 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.

3.5 Purchase of Pricier Products

Another manifestation of potential overspending is that consumers start to opt for higher-end versions of needed items that serve the same function 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 indulgent consumption upgrades by comparing *ItemPrice* within the same shop, brand, category, or shop-brand-category. For instance, if a consumer upgrades from basic milk to organic milk, 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, such as choosing a more expensive laundry detergent with added features (e.g., extra softness) from the same brand and the

same store, 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.[9] These findings are again inconsistent with the notion that BigTech credit leads to irresponsible spending by encouraging pricier item purchases.[10]

3.6 Delinquency of BigTech Credit Users

A key consequence of excessive spending is frequent delinquency on credit bills. To assess this, we calculate monthly delinquency rate for treated consumers at the company level, 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 is relatively low, with 1.6% being the highest across our sample period. In contrast, it ranges between 1.4% and 3.3% in 2020 for major Chinese commercial banks' credit card businesses. This contradicts the notion that the 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

[9] Our results reveal that credit recipients improve their consumption set through product variety rather than product quality, as indicated by the little change in item price. Correspondingly, Butler et al. (2024) demonstrates that consumers become more price-sensitive and shrink their consumption set in response to financial adversities.

[10] In untabulated results, we also use the number of items purchased in an order as the dependent variable. Notably, the interaction term *Treated * Post* also yields insignificant coefficients. Since the *SpendingPerOrder* can be viewed as the product of the number of items purchased in an order multiplied by *ItemPrice*, these outcomes also affirm earlier observations from the user-month data—that the monthly average order amount does not escalate upon the receipt of BigTech credit. These results 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.

limits assigned by the BigTech. The indicator variable *HighCreditLimit* equals one for the high credit limit group and zero otherwise. We define two variables for individual-level delinquency outcomes: *Delinquency30Day* and *Delinquency90Day*, which are set to one if a consumer is delinquent on any 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 data for this analysis only includes post-treatment individual-month observations of treated consumers because only they have the delinquency outcomes. Table 7 displays the regression results. The coefficient for *HighCreditLimit* 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 delinquency outcomes, and find that the coefficient for *HighSpendingEffect* 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.

To corroborate our finding that BigTech-induced spending increases do not lead to higher

default rates, we investigate whether consumers in the high spending effect group possess greater repayment ability. In the lending services offered by the BigTech, there are three repayment methods: repaying with cash from a debit card, selecting an installment plan to refinance the amount not-yet-split in a credit bill, or returning the purchased goods within a specified time frame. Based on this, we create three variables to assess consumers' repayment abilities: *RepayRate*, defined as the proportion of the monthly credit bill amount that is repaid with cash; *RefinanceRate*, defined as the amount split into installments scaled by the amount that can be split in the credit bill; and *RefundRate*, defined as the amount refunded scaled by the amount that can be split in the credit bill. Figure A1 plots consumers' repayment ability in each month post-treatment for high and low spending effect groups. The high spending effect group has consistently higher average repayment ability across all three measures and in all post-treatment months. These patterns indicate that consumers who substantially increase their spending are also those better equipped to manage their debt. This explains why a BigTech-induced increase in spending does not escalate the default rate.

4. CONCLUSION

In recent years, BigTechs have surpassed other FinTechs in global lending volume due to their competitive advantages derived from the integration of their lending and core businesses. This integration raises a key research question that has been underexplored in the literature but crucial for understanding BigTechs' rapid growth: How does BigTech credit, originating from the firm's lending business, influence credit users' spending on the BigTech's core business platform?

Answering this research question faces two major challenges: the need for comprehensive, granular data within a BigTech's ecosystem, and the endogeneity of BigTech credit granting. To address these challenges, we employ a proprietary order-level dataset from a major e-commerce

BigTech, and utilizes the reject inference setting where AI-rejected applicants are randomly approved for BigTech credit. This dataset guarantees an in-depth analysis on the impact of BigTech credit, and this setting facilitates the use of stacked cohort difference-in-differences as our identification strategy.

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 (by strengthening both visit frequency to the platform and visit-to-purchase conversion rate), rather than the amount spent per order.

Heterogeneity analyses indicate that individuals that face more challenges in obtaining traditional credit, are less active on the BigTech's core business platform, and reside in areas with better logistics infrastructure for e-commerce, show a more significant spending increase after receiving BigTech credit. These findings align with BigTechs' role of promoting financial inclusion and highlight the synergistic relationship between BigTechs' lending and core businesses.

With access to order-item level data, we further document that credit recipients tend to purchase from a wider range of product categories, select items from a broader array of brands, and shop across more distinct stores. However, this enhanced inclination toward consumption of variety does not equate to irresponsible spending, as corroborated by little evidence on discretionary spending, purchase of more expensive items, or credit delinquencies. Specifically, BigTech credit does not cause increased spending on discretionary products or on more expensive items even comparing within the same shop-brand-category. At the firm level, the BigTech shows a rather low level of delinquency rate, and at the individual level, consumers with higher initial credit limits or greater increases in credit-boosted expenditure do not display a higher likelihood

of default than others.

Overall, we show that BigTech credit has profound impacts on consumer spending. Our research underscores that the seamless integration of lending services with BigTechs' core services, the essence of their unique business model, creates novel synergies within their ecosystem and promotes financial inclusion without clear signs of inducing consumer over-indebtedness.

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Figure 1. The Architecture of Auto-underwriting System

Notes: This figure presents the architecture of the BigTech's 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 loop of training data generation and AI model improvement. For the approved applicants, their realized delinquency 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 only exist 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 selects a representative sample of AI-rejected applicants and extend them credit. Then, the delinquency outcomes of these AI-rejected but RI-approved applicants are used to infer the counterfactual outcomes for the rest of the rejected applicants. The outcomes of the AI-rejected applicants (either realized or inferred) are then added to the training data to debias the AI model.

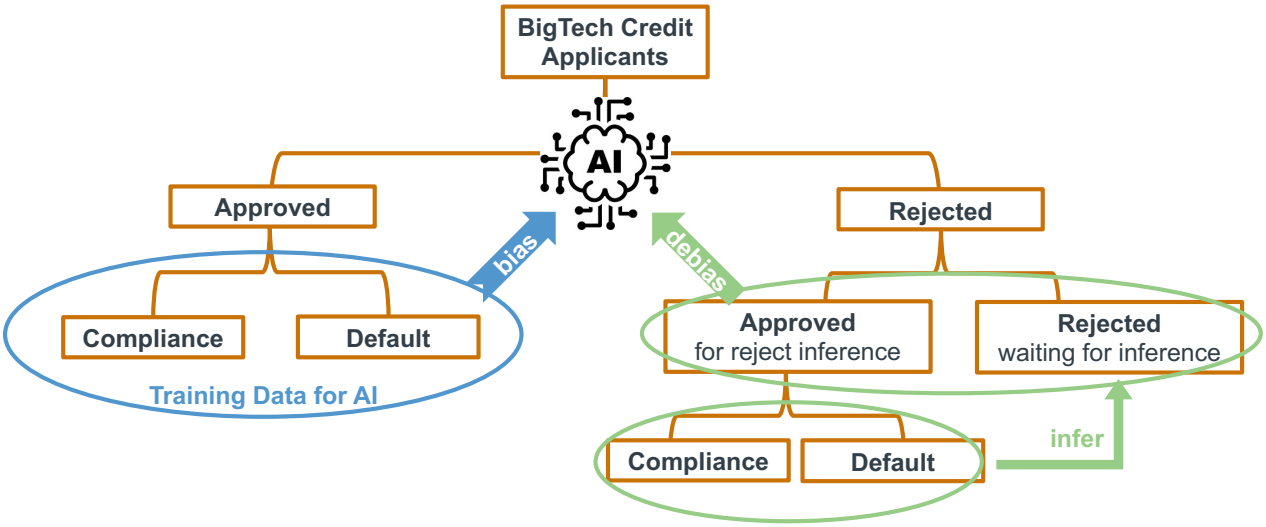
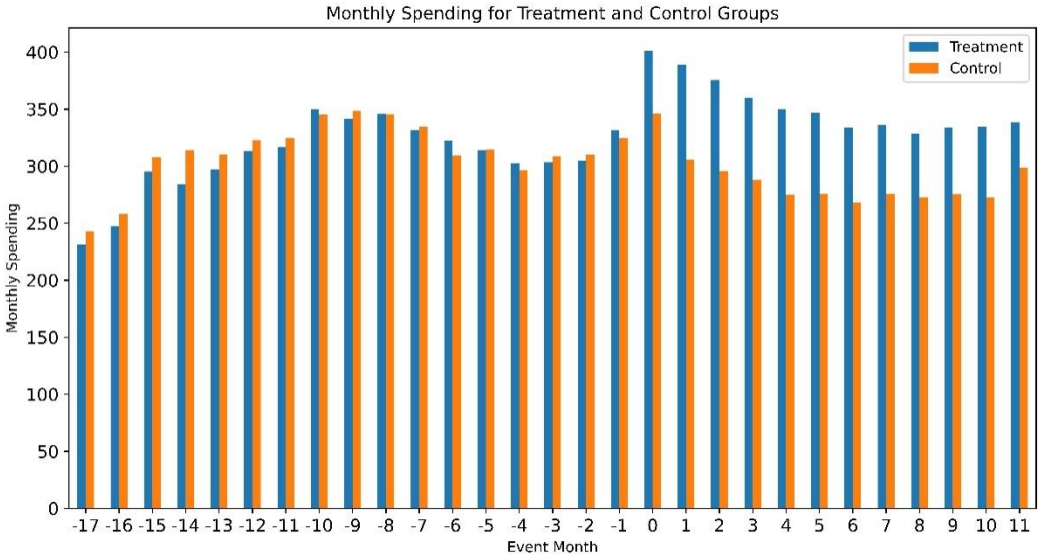


Figure 2. The Unconditional Plot and the Event Study Plot for BigTech Credit's Effect on Spending

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

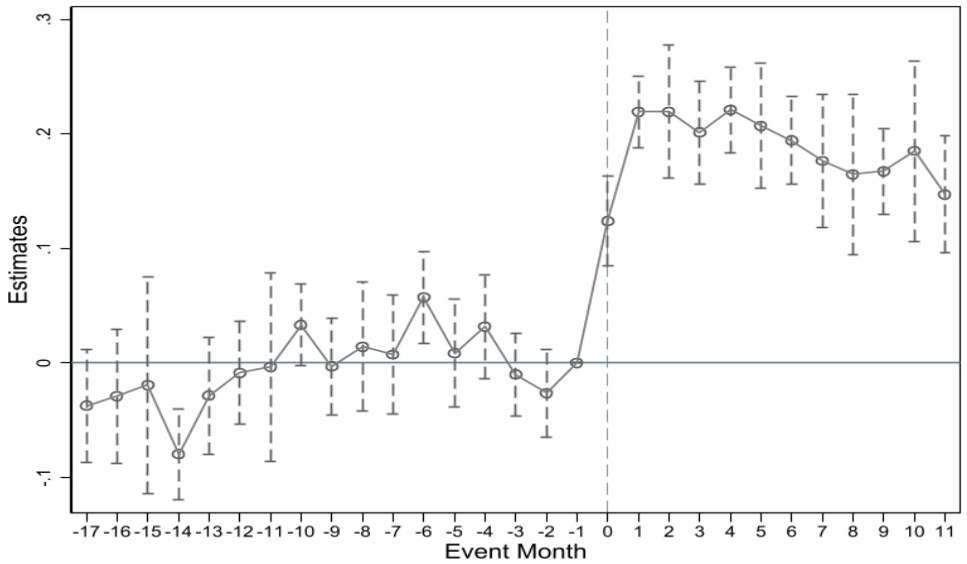


Table 1. Summary Statistics

Notes: This table presents summary statistics of the key variables used in this study. Table A1 presents variable definitions. Panel A focuses on the demographics of applicants in our sample. *CreditLimit* has fewer observations than other variables because only approved consumers were assigned credit limits. Panel B provides summary statistics for the variables of our individual-month panel. *SpendingPerOrder* and *Conversion* have fewer observations due to some months having no orders placed or no page visits made, resulting in the absence of denominators needed for these calculations. *Delinquency30Day* and *Delinquency90Day* have fewer observations because only approved consumers have these records. Panel C is for variables derived from the subsample of consumers with order-item level purchase history. *ItemPrice* is 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
CreditLimit	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
SpendingPerOrder	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
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 (individual-month level and order-item 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 Underlying Components

Notes: This table presents the effect of BigTech credit on consumer spending and its underlying components. We perform a stacked cohort difference-in-differences analysis using the following specification: *Spending/Component of Spending*$_{c,i,t} = \beta$ * *Treated*$_{c,i}$ * *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 are either *SpendingPerOrder*, *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)**
		Underlying Components of Spending			
Dependent Variable	*Spending*	*SpendingPerOrder*	*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-Cohort 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: $Spending_{c,i,t} = \beta_1 * Treated_{c,i} * Post_{c,t} * HighConstraint_{c,i} + \beta_2 * Treated_{c,i} * Post_{c,t} * LowConstraint_{c,i} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$. 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 tests to compare the effects of BigTech credit across different groups, and report the p-value of the tests in the last row of each panel. 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 Supporting the Core Business

Dependent Variable = *Spending*	**(1)**	**(2)**	**(3)**
*Treated * Post * TopExpressway*	0.270***		
	(4.113)		
*Treated * Post * TailExpressway*	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 Consumption of Variety

Notes: This table presents the effects of BigTech credit on consumption of variety. We perform a stacked cohort difference-in-differences analysis using the following specification: $ConsumptionVariety_{c,i,t} = \beta * Treated_{c,i} * Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, 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 *ConsumptionVariety* 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 for this analysis. 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: $SpendingOnCertainItems_{c,i,t} = \beta_1 * Treated_{c,i} * Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, 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.

	(1)	**(2)**	**(3)**
Dependent Variable	*DiscretionarySpending*	*DailyUseSpending*	*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: $ItemPrice_{c,i,t,j,k} = \beta * Treated_{c,i} * Post_{c,t} + \alpha_{c,i} + \delta_{c,t} + \zeta_{c,i,j,k} + \varepsilon_{c,i,j,k}$, 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. 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 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: $DelinquencyIndicator_{c,i,t} = \beta * HighCreditLimit_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar month, and $\delta_{c,t}$ represents month-cohort fixed effects. 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.

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

Table 8. The Correlation between BigTech-induced Spending Growth and Consumer Delinquency

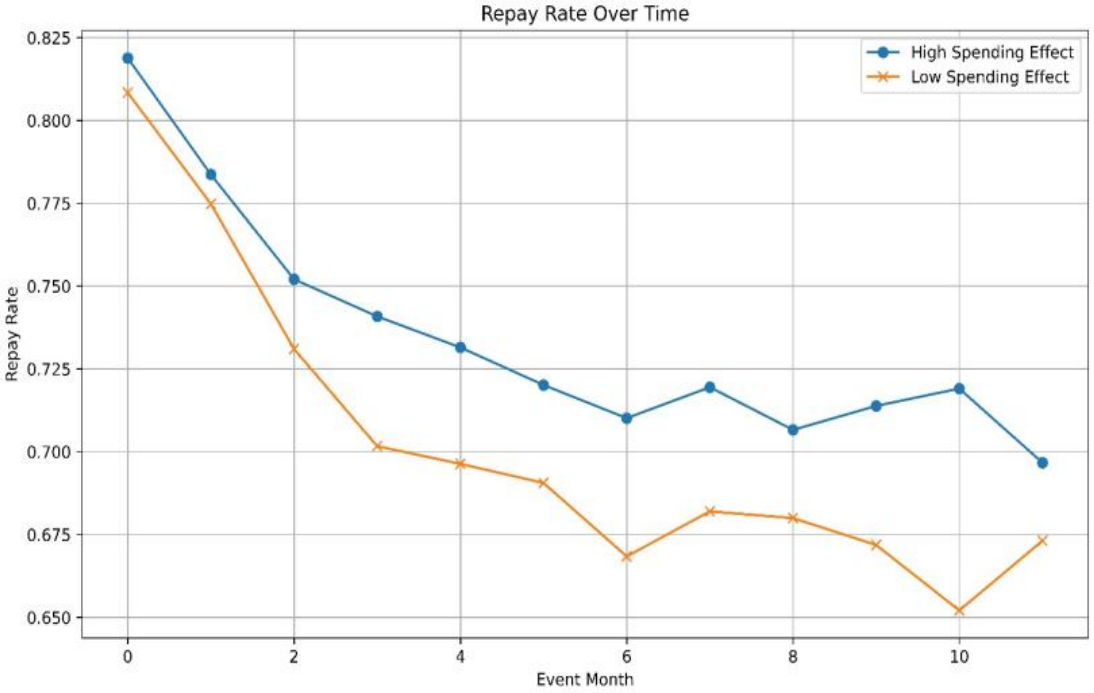
Notes: This table presents the correlation between the BigTech-induced spending growth and the delinquency of consumers. The data used here is the individual-month observations for treated consumers after they receive BigTech credit. We perform OLS regressions using the following specification: $DelinquencyIndicator_{c,i,t} = \beta * HighSpendingEffect_{c,i} + \delta_{c,t} + \varepsilon_{c,i,t}$, where c is the subscript for cohort, i is the subscript for consumer, t is the subscript for calendar month, and $\delta_{c,t}$ represents month-cohort fixed effects. 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	YES	YES
Observations	264,000	264,000
Adjusted R-squared	0.004	0.006

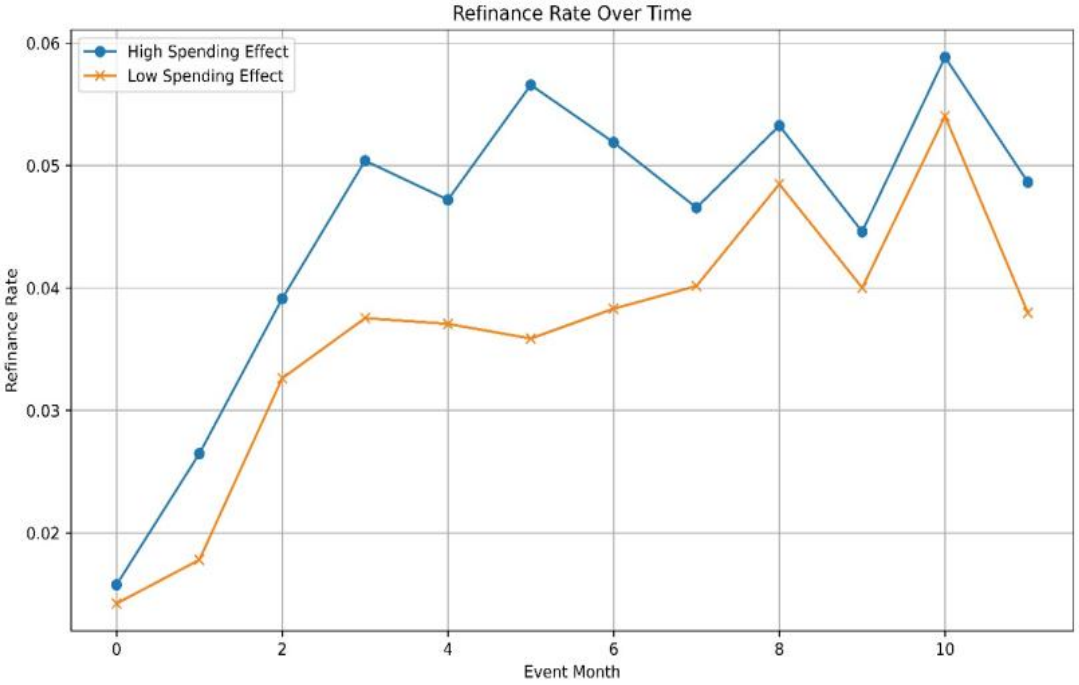
Figure A1. Repayment Ability of Consumers with High vs Low BigTech-induced Spending Growth

Notes: This figure plots consumers' repayment ability in each month post-treatment for the high spending effect group (*HighSpendingEffect*=1) and the low spending effect group (*HighSpendingEffect*=0). Panels A, B, and C measure repayment ability by *RepayRate*, *RefinanceRate* and *RefundRate*, respectively. Variable definitions are in Table A1.

Panel A. Repay Rate



Panel B. Refinance Rate



Panel C. Refund Rate

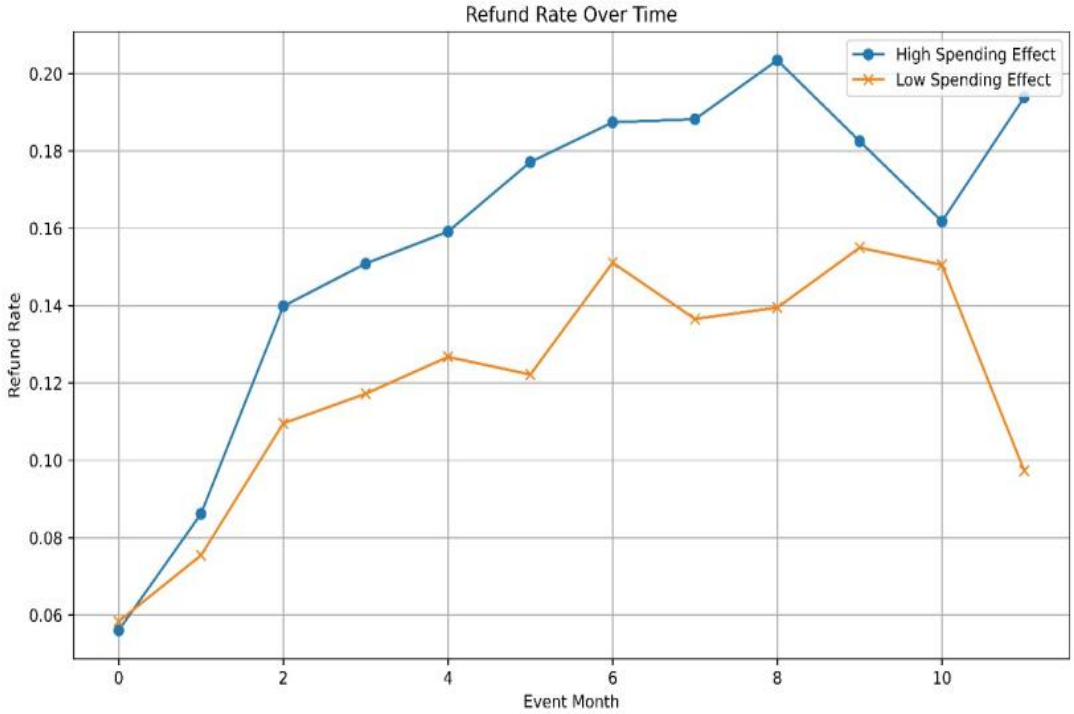


Table A1. Variable Definitions

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 BigTech credit or any subsequent months, and zero otherwise.
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.
SpendingPerOrder	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.
Variables for Heterogeneity Analysis	
YoungAge (OldAge)	A dummy variable that equals one if a consumer's age is (no) smaller than 22, and zero otherwise.
LowCreditScore (HighCreditScore)	A dummy variable that equals one if a consumer's credit score for application is 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	A dummy variable that equals one if a consumer's mean *Visits* in the 12-month-

(LowPastVisit)	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.
TopExpressway (TailExpressway)	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 [(b-a)-(d-c)]/a to measure the relative growth rate in spending. *HighSpendingEffect* takes one if the treated consumer's spending increase is above the sample median among treated consumers, and zero otherwise.
HighCreditLimit	A dummy variable that equals one if a consumer's initial credit limit 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).
CreditLimit	The initial credit limit granted to a consumer when the application is approved.
RepayRate	The proportion of a monthly credit bill amount that is repaid with cash.
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.

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 (application dates and decisions, credit scores, assigned credit limits) with demographics (age, gender, and location), activities on the e-commerce platform (visit intensity, visit-to-purchase conversion, and spending), and credit outcomes (credit scores, applications, credit limits, and delinquencies) 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.

		AI-Rejected but RI-approved (Treatment Group)			**Remaining AI-Rejected (Control Group)**		
#	**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 (remaining 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 (*CreditLimit*) 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 *CreditLimit* 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.

CreditLimit	**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 *CreditLimit* 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. Panel A adopts alternative sample selections and model specifications. Specifically, 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. Panel B presents regression results for each RI cohort. All columns use Poisson regression except Column (4) in Panel A 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.

Panel A. Alternative Sample Selections and Model Specifications

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

Panel B. Regression Results for Each RI Cohort

Dep.Var. = *Spending*	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Sample	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6
*Treated * Post*	0.124***	0.216***	0.191***	0.221***	0.192***	0.177***
	(3.153)	(5.971)	(5.447)	(6.602)	(6.439)	(6.080)
Individual FE	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES
Observations	118,157	155,398	249,804	211,607	253,346	272,618
Pseudo R-squared	0.417	0.391	0.372	0.388	0.365	0.348