Deciphering the Impact of BigTech Consumer Credit

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Current Draft: December 11, 2023

Preliminary, please do not post online or circulate via other means

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Abstract

This study investigates the impact of BigTech credit on consumer spending by leveraging a detailed, novel dataset from a leading BigTech ecosystem. In a near-random assignment setting, we show that credit recipients increase their (online) spending by 20% per month, with those with lower credit rating, more financial constraints, and less attached to the platform exhibiting more significant increases. Notably, we document a stronger effect among credit recipients in locations with more mature logistics and delivery services. The BigTech credit expands the scope of consumption by increasing the number of orders placed, frequency of store visits, and conversion efficiency for purchases. Credit recipients tend to purchase from a wider product range and from a broader array of brands. On the other hand, discretionary spending shows no visible increase and we do not observe over-indebtedness or a higher delinquency rate among recipients with higher credit lines.

Keywords: FinTech, Consumption, BigTech, BNPL, Consumer Credits, Consumer Behaviors, Big Data

1. INTRODUCTION

Over recent years, BigTech firms, which are involved in various technology-related services, such as e-commerce, search engines, social media, hardware, and software, have expanded their presence in the domain of credit services, outstripping the lending volumes of other FinTech entities like peer-to-peer and marketplace lending platforms. Since 2018, the lending totals of BigTech firms have soared, surpassing $700 billion globally by 2020 (Beck et al., 2022; Cornelli et al., 2023). While there is a rich vein of academic work focusing on BigTech's financing of small businesses (Hau, Huang, Shan, and Sheng, 2019; Huang et al., 2020; Chen, Huang, Lin, and Sheng, 2022; Hau et al., 2022; Gambacorta, Huang, Li, Qiu, and Chen, 2023), there remains a scarcity of studies examining their role in consumer finance—a realm where they are thought to have substantial influence (Stulz, 2022). In particular, there has been a gap in the literature regarding how exactly BigTech credit, and more broadly FinTech credit, impacts consumer activities, largely due to data constraints.

This paper seeks to bridge that research gap with a detailed, novel dataset from a leading BigTech firm administering one of the world's most prominent business-to-consumer (B2C) e-commerce platforms. Our dataset intertwines three types of granular information within the firm's ecosystem: consumer demographics, online activity metrics on the e-commerce platform (including page views, order placements, and purchase records), and credit data (details on credit issuance, utilization, and defaults). This alignment presents a significant opportunity to take a closer look and perform a comprehensive analysis on the precise effect of BigTech credit on consumer behavior, as the key to BigTech lending's success—compared to traditional banks and other FinTechs—is their proprietary access to detailed user data within their established

ecosystems such as e-commerce, social media, and internet search (Bank for International Settlements, 2019; Financial Stability Board, 2020; Liu, Lu, and Xiong, 2022).

Specifically, our analyses are built on two datasets. The first dataset comprises of monthly spending for each consumer, including details such as the spending amount per month, visiting frequency per month, and number of orders placed per month. The second dataset (i.e., the order-item level dataset) consists of more granular information on consumers' purchases at the order-item level, detailing items purchased, quantities, transaction prices and etc.

For empirical identification, we utilize reject inference (RI), a method employed by the BigTech firm in our study to enhance its AI credit assessment model. Specifically, RI randomly selects a group of applicants who were initially rejected and grants them credit. Consequently, the subset of RI-approved applicants becomes more comparable to those outright rejected than to those approved directly (for more details, see Section 3.1). We harness this RI methodology for our analysis, creating a distinction between consumers initially declined but later accepted via RI (treatment group) and those consistently rejected by both the AI model and RI (control group). Employing this framework in a stacked cohort difference-in-difference analysis using our proprietary dataset, we are able to draw causal implications regarding the impact of BigTech credit on consumer behaviors.

Our research indicates that consumers granted BigTech credit exhibit an approximate 20% increase in spending per month post-credit application relative to non-credit recipients. This augmented spending trajectory sustains over the subsequent twelve months, showcasing the enduring effect of BigTech credit on consumer expenditure. Through the implementation of alternate model specifications and sample selection methods, we affirm the robustness of our results.

We carry out a range of cross-sectional studies to scrutinize the varying impacts of BigTech credits on consumer expenditure. We evaluate individuals' personal attributes, finding that male consumers, those with less favorable credit ratings, or individuals showing lesser engagement with the e-commerce platform, witness a more significant rise in spending relative to their counterparts. Next, we observe that consumers, who are presumably under more considerable financial constraints, display a more pronounced uptick in expenditure after receiving credit. This suggests that BigTech credit serves as an avenue for financial inclusion. Lastly, in regions where logistical frameworks are more advanced—enabling quicker delivery of goods—the boost in spending linked to credit availability is more noticeable. Such an outcome underscores a unique aspect of BigTech, distinguishing it from other FinTech entities: the symbiosis between financial services and intrinsic non-financial business pursuits (Cornelli et al., 2020). Essentially, the influence of BigTech credit hinges on the speed with which consumer demand—specifically for the BigTech's fundamental offering (like the procurement and receipt of products in this instance)—is fulfilled.

Examining the factors driving the spending amount, our findings demonstrate that BigTech credit increases consumers' inclination to visit the shopping platform, improves conversion rates from visits to purchases, and increases the number of orders placed. However, BigTech credit does not have a significant effect on the amount spent per order. These results suggest that the primary impact of BigTech credit on spending is achieved by increasing the number of orders made, rather than increasing the amount spent per order, through greater visit frequency and improved conversion efficiency.

Shifting to the order-item-level data, we note that BigTech credit's availability heightens consumers' propensity for variety seeking. In particular, credit recipients are inclined to purchase a more diverse array of products from different categories, selecting items from a broader spectrum

of brands and shopping across a multitude of distinct stores. This heightened appetite for variety raises concerns about the potential for consumers with BigTech credit to overspend. To address these concerns, we perform various analyses to illuminate the matter.

Firstly, given that our data allows us to identify the exact items purchased by consumers, we can manually categorize the products into discretionary and non-discretionary types. Our findings show no indication that access to BigTech credit leads to an increase in discretionary spending.

Secondly, we examine the quantity and price of products within each order. We uncover that BigTech credit's impact on the price of items purchased is negligible, implying that the credit does not sway consumers to opt for higher-priced goods within the same shop, brand, or category. Furthermore, we ascertain that BigTech credit does not elevate the number of products bought per order neither. Since the spending per order can be viewed as the product of quantity and the price of items in an order, these outcomes also affirm earlier observations from the user-month data—that the monthly average order amount does not escalate with BigTech credit. They further confirm that the credit's positive effect on overall spending is primarily driven by an increase in the frequency of order placement by the recipients.

Our third analysis targets the likelihood of delinquency (i.e., accounts overdue by more than 30 days). We discover that the delinquency rate is fairly low, staying around 2.3% throughout the year subsequent to credit issuance. Among those consumers who default, the majority settle their outstanding debts within 90 days past the payment due date. Additionally, we identify no notable distinction in the rates of delinquency between consumers with higher and lower credit limits. Aligned with these three sets of analyses, we find no evidence to support the concern that BigTech credit might aggravate consumer overspending.

Our paper offers several key contributions to the literature. Primarily, we enhance the burgeoning research surrounding BigTech finance. BigTech corporations are uniquely positioned to rival traditional banks and conventional FinTech platforms in extending credit, capitalizing on their data-rich environments, advanced technology, and extensive scale (Hau, Huang, Shan, and Sheng, 2019; Stulz, 2022; Li and Pegoraro, 2023). Emerging literature underscores BigTech's role in business lending, demonstrating that BigTech entities bolster financial inclusion and enhance the performance of Micro and Small-Sized Enterprises (MSEs) (e.g., Hau, Huang, Shan, and Sheng, 2019; Huang et al., 2020; Beck, Gambacorta, Huang, Li, and Qiu, 2022; Chen, Huang, Lin, and Sheng, 2022; Hau et al., 2022; Liu, Lu, and Xiong, 2022; Gambacorta, Huang, Li, Qiu, and Chen, 2023). While BigTech's potential in consumer finance is noted (Stulz, 2022), related research is sparse, possibly due to data scarcity. Two contemporaneous works, by Ouyang (2023) and Bian et al. (2023), are the exceptions.[1] Our research seeks to demystify the precise mechanisms through which BigTech credit stimulates consumer spending by executing an array of detailed analyses leveraging data not previously available to researchers.

As the financial activities of BigTech are considered a particular subset of FinTech innovations (Frost et al., 2019), our paper contributes to the expanding research on FinTech lending, especially concerning consumer credit. Previous studies, on the one hand, have largely agreed that FinTech firms, including peer-to-peer or marketplace lending platforms, can serve as

[1] Our study distinguishes itself from these works in several respects. Both studies concentrate on the payment services of BigTech, the entry points for these entities into the financial sector (Liu, Lu, and Xiong, 2022). Specifically, Ouyang (2023) illustrates, through an exogenous shock to cashless payment activities, that the adoption of cashless payment facilitates access to BigTech consumer credit; Bian et al. (2023) showcase how embracing internal payment solutions, particularly buy-now-pay-later (BNPL) models, extends credit to typically underserved consumers. In contrast, our investigation delves into the influence of BigTech credit on consumer activity in-depth. While both works skim the surface of credit's impact on spending, our unique, granular order-item level purchase data enables us to present various new findings that explain how BigTech credit exerts its influence. Moreover, they analyze Alibaba's BigTech consumer credit product, Huabei, which is accessible beyond Alibaba's e-commerce ecosystem. Our study, however, examines a BigTech consumer credit service that is utilized exclusively within the company's own e-commerce platform. As Alibaba and the firm we study exemplify two different BigTech lending models, their research and ours are mutually informative and complementary.

alternatives or complements to traditional bank lending by providing credit to financially constrained borrowers or those without credit histories, thereby promoting financial inclusion; on the other hand, these studies still debate regarding the economic impact of FinTech lending, particularly in terms of default rates (e.g., Tang, 2019; Agarwal, Alok, Ghosh, and Gupta, 2021; Bao and Huang, 2021; Chava, Ganduri, Paradkar, and Zhang, 2021; Di Maggio and Yao, 2021; Di Maggio, Ratnadiwakara, and Carmichael, 2022; Wang and Overby, 2022). Yet, these studies do not address the effects of FinTech or BigTech credit on consumer behaviors.

Further, the BigTech credit we examine is an example of Buy-Now-Pay-Later (BNPL), a fast-growing FinTech payment option which provides short-term credit for retail purchases. Therefore, our study is also an addition to this emerging field of research (e.g., deHaan et al., 2022; Berg et al., 2022; Di Maggio, Katz, and Williams, 2022; Bian, Cong, and Ji, 2023; Guttman-Kenney et al., 2023). Most importantly, instead of providing BNPL as a one-time service at checkout, an increasing number of major retailers worldwide are shifting towards offering a full-service online shopping experience and integrate BNPL into their self-contained ecosystem.[2,3] This shift suggests that data in the hands of retailers can be used to inform the design of user interfaces and user experiences to guide consumers towards desired actions, a practice known as data harvesting

[2] There are two main BNPL models: the pure-play merchant partner model and the app-driven model. The pure-play merchant partner model involves BNPL providers partnering with merchants to integrate their solution into the merchant's payment infrastructure, utilizing the existing bank card network. The app-driven model strengthens the relationship between the consumer and the BNPL lender, as consumers often start and complete their purchase journey within the lender's self-contained ecosystem. This model has the potential to significantly impact consumer behavior, increasing the likelihood of habitual BNPL usage (CFPB, 2022), and is also adopted by the firm we study.

[3] Di Maggio, Katz, and Williams (2022) note that this shift implies that BNPL providers can leverage substantial amounts of proprietary data on customer's shopping and spending patterns, making BNPL increasingly distinct from standard credit cards. They also highlight other distinguishing features of BNPL compared to standard credit cards. First, instead of offering a revolving line of credit, BNPL products are structured as installment loans with a down payment due at sale and a fixed repayment schedule. Second, BNPL loans are offered through retailers and tied to the purchase of a particular product. Third, BNPL companies often provide more lenient lending terms, with no or limited credit checks, often zero interest, minimal fees, and no or limited negative reporting to credit bureaus. BNPL companies make money by charging merchants fees of around 5-8%, significantly higher than the 2-3% charged by credit card companies.

(CFPB, 2022; Consumer Reports, 2023). Consequently, our research is timely, as we leverage proprietary, highly-granular data on consumers' digital footprints and shopping patterns to demonstrate that BNPL offered by a leading e-commerce do not necessarily encourage overspending or lead to over-indebtedness among consumers.

2. RELATED LITERATURE

The activities of BigTech in finance can be considered a particular subset of broader FinTech innovations (Frost et al., 2019). Thus, this section begins with a review of the literature on BigTech credit. We then move on to a review of FinTech-related research, with a focus on the segment of FinTech consumer lending, as it is closely related to our research. For a comprehensive review of FinTech literature, we refer the readers to Allen, Gu, and Jagtiani (2021) and Berg, Fuster, and Puri (2022).

2.1 BigTech Credits

2.1.1 The Macro Drivers of BigTech Credit

Several papers assess the economic and institutional factors driving the growth and adoption of BigTech credit using country-level data. Cornelli et al. (2020) and Cornelli et al. (2023) find that the volume of both FinTech and BigTech lending is larger in countries with higher GDP per capita, where banking sector mark-ups are higher, banking regulation is less stringent, business operations are easier, investor protection disclosure and judicial system efficiency are more advanced, and bond and equity markets are more developed. This suggests that these alternative credits complement more traditional credit markets, rather than substitute for them. Similarly, Frost, Gambacorta, Huang, Shin, and Zbinden (2019) find that the drivers of BigTech credit are similar to those of FinTech credit in terms of economic activity, financial regulation, and competitiveness.

2.1.2 The Advantage of BigTech Firms in Credit Provision

Hau, Huang, Shan, and Sheng (2019) argue that due to both cheaper (online) distribution channels and better credit analysis based on abundant data generated on their own trading platforms, BigTech firms have the capacity to compete with traditional banks for credit provision. Stulz (2022) claims that BigTech firms have potentially big advantages compared to banks and to FinTech firms, including the technical know-how and up-to-date systems that FinTech firms aspire to, the scale of large banks, absence of the legacy nor the organizational issues that banks have, and most importantly the access to data that neither banks nor FinTech firms have, which allow them to replace traditional banks, especially in consumer finance. Li and Pegoraro (2023) theorize that a unique feature of the BigTech platform is that it is the monopolistic provider of a valuable marketplace, allowing it to lend to small businesses of high credit risk, who are traditionally denied credits by banks.

Supporting the conjecture that BigTech firms have information and modeling advantages, Huang et al. (2020) find that BigTech firms can better predict loan defaults during both normal times and periods of large exogenous shocks than banks; Gambacorta et al. (2023) show that BigTech credit performs better than bank credit on average ex-post in terms of defaults and firm performance.

2.1.3 BigTech Credit Offered to Micro, Small-Size Enterprises (MSEs)

Several studies have focused on BigTech credit extended to Micro and Small Enterprises (MSEs). Building on the advantages of BigTech firms identified earlier, these studies highlight the potential for BigTech to complement traditional banks in promoting financial inclusion and improving the performance of MSEs. Notably, all of the literature in this area utilizes data from Mybank, a lending institution backed by Alibaba. Mybank provides credit to MSEs operating

within Alibaba's ecosystem, encompassing online borrowers on platforms such as Taobao and Tmall, as well as offline borrowers utilizing the digital payment platform Alipay.[4]

In particular, Hau, Huang, Shan, and Sheng (2019) demonstrate that BigTech credit increases the availability of credit to borrowers with lower credit scores and provides relatively more credit to those with lower credit scores. Similarly, Huang et al. (2020) report that BigTech's proprietary information can complement or, when necessary, substitute credit history in risk assessment, enabling unbanked firms to access credit, particularly smaller firms in smaller cities. Beck, Gambacorta, Huang, Li, and Qiu (2022) find that transaction data generated through QR codes allows micro firms to obtain credit from both BigTech companies and banks, indicating that digital payment footprints contribute to financial inclusion by mitigating information asymmetry. Additionally, they note positive effects of access to BigTech credit on vendors' sales, even during the Covid-19 pandemic. Chen, Huang, Lin, and Sheng (2022) demonstrate that access to BigTech credit significantly reduces the sales volatility of MSEs, with a stronger effect observed for enterprises with fewer alternative sources of financing, as well as a reduced likelihood of bankruptcy or business exit for those with FinTech credit access. Hau et al. (2022) document that access to BigTech credit from the same e-commerce platform boosts the sales growth, transaction growth, and consumer satisfaction of small e-commerce firms. Liu, Lu, and Xiong (2022) reveal that the lending model of BigTech firms differs from that of conventional banks, as BigTech loans tend to be smaller with higher interest rates, and borrowers tend to repay them far earlier and more frequently, focusing on short-term liquidity needs rather than long-term financing. Gambacorta, Huang, Li, Qiu, and Chen (2023) find that BigTech credit does not correlate with local business

[4] Details about Alibaba Group and its ecosystem can be found in the papers reviewed in this section (e.g., Liu, Lu and Xiong, 2022).

conditions and house prices, unlike cyclical bank credit, suggesting that it reduces the importance of the collateral channel and makes lending more responsive to changes in firms' business activities.

2.1.4 BigTech Credit Offered to Consumers

In contrast to the extensive research on BigTech business credits, there are fewer studies on BigTech consumer credits. Ouyang (2023) demonstrates that the adoption of cashless payments, such as Huabei offered by Alibaba's Alipay, enhances individual consumers' access to credit, particularly for those who were previously financially underserved. Furthermore, Ouyang's findings contradict Huang's (2022) theory, as they do not indicate that increased payment flow leads to a rise in compulsive spending, such as on items like cigarettes, games, lotteries, or live streaming services.

2.2 FinTech Consumer Credit

Based on data from FinTech firms in various countries (e.g., the US, Germany, and India), multiple studies have reached a consensus that unconventional data play a crucial role in enabling FinTech firms to evaluate consumer credit risk, thereby promoting financial inclusion. Berg, Burg, Gombović, and Puri (2020) demonstrate that even basic online interactions, such as accessing or registering on an e-commerce website, influence credit access and decrease default rates. Similarly, Agarwal, Alok, Ghosh, and Gupta (2021) illustrate that alternative credit scoring based on mobile and social footprints can extend credit access to individuals without traditional credit scores without negatively impacting default outcomes, with the greatest benefit seen for underprivileged borrowers. Bao and Huang (2021) find that FinTech companies are more inclined to broaden credit access for new and financially constrained borrowers following the onset of the COVID-19

pandemic, underscoring the advantages of FinTech credit during a crisis.[5] Moreover, Di Maggio, Ratnadiwakara, and Carmichael (2022) report that utilizing alternative data to assess borrowers' creditworthiness leads to expanded credit access, particularly for individuals with limited credit histories and lower credit scores, without an increase in default rates. Additionally, Ghosh, Vallee, and Zenga suggest that consumers who rely more on cashless payments are more likely to obtain loans with lower interest rates and higher amounts, and are also less likely to default.

Several studies have delved into the dynamics and distinctions between banks and FinTech firms in the consumer credit market. Tang (2019) finds that peer-to-peer (P2P) lending serves as a substitute for bank lending for infra-marginal bank borrowers, while also complementing bank lending for small loans. In contrast, Di Maggio and Yao (2021) reveal that FinTech lenders are inclined to extend credit to high-risk borrowers who may have been turned down by traditional lenders due to a lack of soft information, and that the pricing strategies of FinTech lenders are likely to consider this adverse selection. Chava, Ganduri, Paradkar, and Zhang (2021) document that consumers borrowing from marketplace lending platforms tend to have lower credit scores and higher default rates in the long run compared to similar bank loan applicants, indicating that FinTech lenders face greater information frictions than banks, which engage in relationship lending to access unique borrower information (e.g., transaction history, soft information) and build relationship capital. Additionally, Balyuk (2023) demonstrates that banks expand credit access for consumers who have obtained FinTech loans, highlighting the potential of FinTech to mitigate imperfections in the consumer credit market and generate information spillovers to traditional credit intermediaries.

[5] However, the authors also note that in stark contrast to bank loans, which show no significant change, the delinquency rate of FinTech loans triples after the pandemic outbreak, indicating the potential fragility of such institutions.

Some researchers have found evidence suggesting that FinTech lending may potentially worsen, rather than improve, the financial health of borrowers. For instance, Di Maggio and Yao (2021) demonstrate that FinTech borrowers experience delinquency at significantly higher rates, with these borrowers using the additional credit not for consolidating existing debt but to support additional expenditures. Similarly, Wang and Overby (2022) utilize a difference-in-differences approach to reveal that state approval of a major online lending platform to issue peer-to-peer loans results in an increase in bankruptcy filings.

Buy-Now-Pay-Later (BNPL)

A segment of FinTech consumer credit is the Buy-Now-Pay-Later (BNPL) model, offering a short-term credit option that enables consumers to defer payments at the point of sale. BNPL typically represents itself as an interest-free loan repaid in one or more installments and, as a relatively unregulated innovation, provides convenient access to credit for retail purchases. Instant decisions are made using alternative data without a hard credit check.[6]

There has been a growing interest in archival research on BNPL, reflecting its increasing popularity and usage. Driven by widespread concerns about potential misuse of BNPL due to its ease-of-use and ubiquity (e.g., CFPB, 2023; Johnson, 2023; Her Majesty's Treasury, 2023; Reed, Duckworth, and Brown, 2023), most academic studies on BNPL focus on how this FinTech innovation affects consumer welfare. Specifically, Di Maggio, Katz, and Williams (2022) demonstrate that BNPL leads to high consumption, particularly in the retail sector, even among consumers who are not liquidity-constrained. They conclude that BNPL increases the chances of

[6] The popularity of BNPL has surged due to the increasing global trend of online shopping, particularly during the Covid-19 pandemic, where cashless payments are preferred. For instance, a survey by the Consumer Financial Protection Bureau (CFPB) in 2023 revealed that 17% of respondents had made at least one purchase using BNPL between February 2021 and February 2022. This surge in demand has led to BNPL providers experiencing high transaction volume and value. According to CB Insights (2021) and Research and Markets (2023), global BNPL payments are projected to surpass $527 billion by 2023 and reach $1 trillion by 2025.

negative outcomes such as overdraft fees, which reduces consumer price elasticity on covered items, leading to increased near-term spending possibly at the expense of longer-term liquidity. Additionally, deHaan et al. (2022) find that new BNPL users experience rapid increases in bank overdraft charges, as well as credit card interest and fees, suggesting that BNPL is detrimental to users' financial health. Moreover, using a stylized general equilibrium model, Huang (2022) shows that E-commerce platforms with an integrated consumer lending function are more inclined to provide credit than standalone banks. While the additional consumer credit might have positive externalities, it may also amplify the distortion of consumers' present bias. Guttman-Kenney et al. (2023) document that a significant minority of UK consumers use credit cards to fund their BNPL purchases, particularly among younger consumers and those residing in economically deprived areas, raising doubts about these consumers' ability to pay for BNPL.

In contrast, some research show that on average, consumers are using BNPL responsibly. Papich (2023), by exploiting geographic and temporal variation in the availability of BNPL at Walmart in the US, offers causal evidence that although BNPL access leads consumers to increase their utilization of other forms of credit, consumers' ability to repay their debts actually improves with BNPL access. Using Chinese data, Bian, Cong, and Ji (2023) demonstrate that BNPL significantly boosts consumer spending; yet, consumers, particularly those who solely rely on BNPL for credit access, exercise caution to avoid overspending and becoming overly indebted.[7]

Different from the above research, Berg et al. (2023) contribute to the debate on BNPL from a merchant's viewpoint. Using experimental data from a German online furniture retailer, they find that BNPL increases the merchant's sales by 20%, attributed to both the intensive and

[7] Bian, Cong, and Ji (2023) adopt a broader perspective by examining e-wallets that offer external payment options (such as linked debit and credit cards) where e-wallets act as a conduit to banks, as well as internal payment options (e.g., e-wallet balance, e-wallet savings, and BNPL) where e-wallets operate as an independent ecosystem. Their findings reveal that BNPL crowds out other e-wallet payment options and expands FinTech credit to underserved consumers.

extensive margin. Additionally, Berg et al. (2023) demonstrate that BNPL costs arising from payment defaults are considerably lower than the costs associated with other popular payment methods, which the authors believe explains the global surge in BNPL popularity in e-commerce.

3. BACKGROUND, IDENTIFICATION STRATEGY AND DATA

3.1 Background

We obtain our proprietary data from a prominent B2C online retailer in China.[8] This company offers an extensive range of products, including electronics, appliances, clothing, cosmetics, books, groceries, and more. In 2022, it serves nearly 600 million active users and its net online sales surpassed 900 billion yuan or 130 billion US dollars, making it one of the largest retailers globally.

The company operates a FinTech division that offers credit for online purchases on its platform. Consumers are required to submit an application to access the credit. The credit product is smoothly integrated into the company's online shopping platform, enabling approved consumers to select it as a payment option during checkout. At checkout, consumers can use their approved credit limit and choose their preferred installment plan.[9] The proprietary data on credit approval and usage is sourced from this specific FinTech division.

The credit application process is quick and efficient. Customers can initiate it with a simple click, and within minutes, the proprietary AI model considers user demographics and historical shopping behavior to generate important outputs. These outputs include an application score, an

[8] Driven by the development of digital technologies and the rapidly expanding consumer market, China has witnessed significant growth in online retail sales, consistently ranking first worldwide (UNCTAD, 2021). By 2022, the total online retail sales in China reached nearly 13.8 trillion RMB or 2 trillion US dollars (National Bureau of Statistics of China, 2023).

[9] Before choosing to utilize the credit option, customers can view the applicable interest rates based on their credit assessment and selected repayment plan. Furthermore, after choosing to utilize the credit option, customers can track their credit account, monitor their current balance, and manage their repayment schedule through a credit management platform.

application decision, and a designated credit limit. The application score, which the applicant cannot see, provides a comprehensive assessment of the applicant's credit risk, with higher scores indicating a lower likelihood of delinquency or default. The application decision is binary, resulting in either approval or rejection. Upon approval, consumers are granted an initial credit limit, representing the maximum amount they can charge to their revolving consumer credit account.[10]

The proprietary AI model was developed using the repayment histories of approved applicants. It utilizes cutting-edge machine learning algorithms to establish a relationship between consumers' digital footprints and delinquent behaviors. As a result, this trained model can evaluate credit risks associated with new applicants and make informed credit decisions. However, a significant challenge lies in the model's generalizability. Since there is no credit performance data for rejected applicants, the AI model is trained exclusively on the subset of approved consumers rather than the entire consumer population. Consequently, when applied to new applicants from the broader population (usually with worse credibility due to adverse selection), the model encounters considerable difficulties in terms of generalizability and predictive accuracy.

To tackle this issue, the company has implemented a pilot Reject Inference (RI) program since 2020. The RI program introduces a filter that is applied before the proprietary AI model delivers its decision to the consumer. This filer operates seamlessly in real-time, converting a random group of the applicants who were initially rejected by the AI model into RI-approved applicants, all without any observable difference or notification for users. These RI-approved applicants are granted a small initial credit limit, usually below 500 RMB. The company then

[10] For rejected consumers, their application scores are re-evaluated based on the latest shopping behaviors whenever they submit a new application. The credit limit for approved consumers, on the other hand, is periodically updated based on both the latest shopping behaviors and repayment behaviors.

continuously monitors the credit performance of these RI-approved applicants and utilizes their credit behavior to infer the potential credit performance of other rejected applicants. By analyzing the actual and inferred future credit performance of AI model-approved, RI-approved and rejected applicants, the company enhances the training process of its proprietary AI model. This approach helps improve the model's accuracy and effectiveness in evaluating credit risks for a broader range of applicants.[11]

3.2 Identification Strategy

Since the AI model cannot differentiate between RI-approved applicants and other rejected applicants, and the RI program selects applicants to represent the entire population rather than solely focusing on maximizing profits, it offers a valuable opportunity to study the influence of FinTech credit on consumer behaviors. Specifically, our identification hinges on comparing outcome variables between two groups: the treatment group comprising consumers whose credit applications initially get rejected by the AI model but later get approved by the RI program (i.e., the RI-approved applicants), and the control group comprising consumers whose applications are rejected by both the AI model and the RI program (i.e., other rejected applicants).[12]

Furthermore, given that credit applications and decisions occur at different times for various consumers, our setting embodies a typical staggered setting. To account for both the time-varying

[11] Although RI is a common practice, particularly among FinTech and BigTech firms, its application remains largely unexplored in academic settings. By implementing RI, we bridge a gap in literature that often focuses solely on loans directly approved by machine-learning algorithms, constrained by data availability. Additionally, our study reveals that consumers initially declined but subsequently accepted via RI—the treatment group—demonstrate increased spending without signs of overspending or excessive indebtedness. This suggests that RI not only augments the predictive precision of credit models but also promotes fairer distribution of FinTech and BigTech credit, potentially advancing financial inclusion.

[12] Previous studies in this domain have compared the spending behavior of approved and rejected applicants, which has posed limitations due to the endogeneity issue. This arises from the fact that high-income individuals are more likely to be approved and tend to spend more. As a result, these studies may not accurately capture the true impact of FinTech credit on consumer spending behavior.

nature and individual specificity of the credit effect, we employ a stacked cohort difference-in-difference analysis for a panel of individual-month observations. In our analysis, we run regressions according to the following specifications:

$$\text{Spending}_{i,c,t} = \beta \times \text{Treatment}_i \times \text{Post}_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t} \quad (1)$$

where i is the subscript for the consumer, c is the subscript for the cohort (with one cohort representing applications made each month during a six-month application period, see the details below), t is the subscript for the calendar month, α_i represents individual fixed effects that account for time-invariant individual characteristics, $\delta_{t,c}$ represents month-cohort fixed effects that account for individual-invariant time trends with respect to cohort C. *Spending* refers to the total consumption by a consumer in a specific calendar month, excluding the consumption on the application day and the day after application. We do not consider the consumption on these two days to mitigate the concern that consumers' credit applications may be triggered by their planned consumption. *Treatment* is a dummy variable that is set to one if a consumer is an RI-approved applicant and zero if she is any other rejected applicant. *Post* is a dummy variable set to one if the calendar month is the month a consumer applies for credit or any subsequent months, and zero otherwise. β captures the average effect on consumption in the treatment group relative to the control group after the approval of consumer credit.

We estimate Eq. (1) using both the ordinary least squares (OLS) and the Poisson method. In cases where the dependent variable is non-negative, researchers often estimate linear regressions of the log of 1 plus the outcome. However, recent research has indicated that this approach yields problematic results that cannot be interpreted as a percentage effect or elasticity (e.g., Silva and Tenreyro, 2006; Campbell and Mauand, 2021; Chen and Roth, 2023). Therefore, we rely on Poisson regression, which provides valid semi-elasticity estimates and standard errors even when

the outcome variable is continuous and the dependent variable exhibits highly right-skewed distributions with a concentration of values at zero (Silva and Tenreyro, 2011; Cohn, Liu, and Wardlaw, 2022). To adjust for possible cross- sectional and serial correlations, we cluster standard errors by individual and month-cohort.

3.3 Data

We begin by categorizing applicants into the treatment group and the control group. Out of approximately 6 million consumer credit applications received by the company from 5 million unique consumers during a six-month period (from April 1, 2020, to September 30, 2020), we randomly select information from 10% of them. We then remove applicants with more than one applications during the whole sample period.[13] Additionally, we remove applicants with missing demographic information. We also remove inactive applicants who have zero monthly spending in more than 33% of their pre-application sample months.

Next, we conduct nearest-neighbor propensity scoring matching without replacement for each month to ensure that each treated applicant is paired with a control applicant who submitted their credit application in the same month and shares the most similar characteristics.[14] In the end, our sample consists of 58,688 applicants, with half in the treatment group and the other half in the control group.

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[13] Each consumer may have multiple applications because (1) she may submit a new application after being rejected in previous ones or (2) she may apply to reopen her consumer credit account after closing and deleting a previously approved credit account. This data filter suggests that we exclude those who change their membership between the treatment group (including applications approved for RI), the control group (containing rejected applications), and the remaining group (consisting of applications automatically approved by the assessment model).

[14] This involves running a logistic regression model in a specific month to determine the approval of the credit application based on factors such as age, gender, application score generated by the assessment model, account history (measured by the number of days since registration), and average monthly spending prior to the credit application. We match each applicant in the treatment group with a control applicant who has the nearest matching score without replacement. This process is repeated for each month from April 2020 to September 2020. After completing these steps, there are no statistical differences in any of the matching variables mentioned above between the two groups of applicants.

Next, we calculate the total consumption made by each applicant in each month. The consumption data is available from April 1, 2019, to March 31, 2021. For applicants from the first cohort (i.e., April 2020), there are a maximum of 12 monthly consumption observations before and after the applications. For applicants from the last cohort (i.e., September 2020), there are a maximum of 18 monthly consumption observations before and 6 after the applications. However, not every applicant needs to have all 24 monthly observations from April 2019 to March 2021 if they registered their shopping account after April 2019. In such cases, the monthly consumption observations would start from their registration month until March 2021.[15] In the main analysis, we use a total of 1,171,667 individual-month consumption observations from an 18-month period. This period starts from 6 months prior to the credit application and ends at 12 months after the credit application. In addition to consumption, we collect information on various other variables, including the number of visits, the number of orders placed, and the extent of discounts, among others. For comprehensive definitions of these variables, please refer to Appendix A.

Table 1 showcases a summary of the main variables. In Panel A, demographic information is provided for the 58,688 applicants. Notably, 62% of these applicants are male, and the average age is around 31 years. The average account age for applicants is approximately 27 months. The average credit limit is a mere 166 RMB, with a minimum limit of 0 (indicating rejection) and a maximum of 500 RMB. These figures reflect the primary objective of using the RI program for data collection rather than profit generation by the company. Moving to Panel B, summary statistics are presented for variables that vary on a monthly basis. Metrics such as monthly visits,

[15] Our sample period for studying credit's influences spans from six months before to twelve months after credit applications, much longer than prior studies (e.g., one month in Berg et al., 2022, and two months in Bian, Cong, and Ji, 2023). This extended timeframe is crucial as it helps to determine whether BigTech credit or BNPL's boosting effect on spending is is temporary or persistent. A sufficiently long sample period is also preferable when studying any adverse consequences of Fintech/BigTech credit or BNPL product, such as overspending and delinquency.

orders, and spending are based on 1,172,262 individual-month observations. However, only around 631,000 observations are available for the other three variables. This discrepancy arises from months in which no activity occurred, resulting in unavailable denominators for calculating values in these cases.

Insert Table 1 about here

4. RESULTS

4.1 The Effects of BigTech Credits on Spending

To demonstrate the impact of credit, we first compare the monthly spending of the treatment and control groups. Figure 1 reveals that both groups have similar spending levels ranging from 230 yuan to 330 yuan prior to credit application. An univariate analysis (results untabulated) indicates that there is no statistically significant difference between the two groups in terms of spending, except for the fact that consumers in the treatment group spent significantly *less* than those in the control group during the thirteenth, twelfth, and second month before credit application ($month_{-13}$, $month_{-12}$, and $month_{-2}$). The most noteworthy change in spending patterns occurs when the treatment group's credit applications are approved (for reject inference). In comparison to the previous month ($month_{-1}$), consumers who are granted credit experience a substantial increase in their monthly spending, with an approximately 23% rise to 356 yuan in $month_0$. Conversely, consumers who do not receive credit see no increase in their monthly spending during the application month. Interestingly, consumers in the treatment group consistently spend more than their counterparts in the control group, indicating the long-lasting impact of credit on consumption.

Insert Figure 1 about here

Figure 1 provides initial evidence supporting the parallel hypothesis underlying the difference-in-difference method. To formally test this hypothesis, we employ a dynamic model. In

this model, we replace $Treatment_i \times Post_{t,c}$ in Eq. (1) with a series of interaction terms between $Treatment_i$ and $Month_t$, where t ranges from -17 to +11, with $Month_0$ being the application month. As presented in Figure 2, the coefficients of most interaction terms are both insignificant and of small magnitude for the periods preceding the application month. However, in contrast, the interaction terms consistently exhibit a positive and statistically significant impact since the credits are granted. These findings hold true whether we estimate the model using OLS (Panel A) or the Poisson model (Panle B). The results from Figure 2 verify the patterns observed in Figure 1 conditioning on certain fixed effect structures and strongly support the notion that credits enhance consumer spending.

Insert Figure 2 about here

In Table 2, Panel A, we present the results of estimating Eq (1). Column 1 employs OLS regression, while Column 2 uses Poisson regression. Regarding Column 1, in comparison to consumers who do not receive credits, consumers who are granted credits spend approximately 70 yuan more per month after their credit applications, which is statistically significant at the 1% level. Consistent with the findings in Figure 1, Column 2 demonstrates that consumers in the treatment group, when compared to consumers in the control group, experience a significant 23% increase in their monthly spending following the granting of credit by the BigTech firm (significant at the 1% level).[16]

Panel B of Table 2 presents the results of estimating the following model: $Spending_{i,c,t} = \beta \times CreditLimit_i \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$ (2). Eq. (2) is an expanded version of Eq. (1) with a three-way interaction term. *CreditLimit* represents the initial credit limit

[16] We also estimate the intensive margin by running an OLS regression on positive spending observations and the extensive margin by using a dummy variable set to 1 if spending is positive and 0 otherwise as the dependent variable. Our untabulated results indicate that the coefficient of the interaction term is 69.188 and 0.075 (both significant at the 1% level), respectively.

granted to a consumer, scaled by 100. In Column 1, the OLS model is used, while in Column 2, the Poisson model is used. Therefore, we can conclude that compared to consumers receiving no credits from the e-commerce, consumers would spend approximately 15 yuan or 4.7% more per month for every 100 yuan credit they receive (significant at the 1% level).

The BigTech firm consistently updates the credit limit for each consumer who receives credits. In our sample, we observe a steady increase in the average credit limit from 377 yuan in the month when the credit is granted ($Month_0$) to 2,839 yuan in the eleventh month following the credit issuance ($Month_{11}$). In an additional robustness test (unreported results), we substitute *CreditLimit* with the consumer's most recent credit limit in a particular month and observe that the three-way interaction term is also positive (significant at the 1% level), except that its coefficient decreases to 1.915 and 0.005 when using OLS and Poisson models, respectively.

Insert Table 2 about here

4.2 The Heterogeneity in the Effect of BigTech Credits on Spending

Next, we conduct a series of cross-sectional analyses to examine the variation in the effects of credits on the key variable of our interest, *Spending*. To accomplish this, we introduce the model as follows: $Spending_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} \times XSVariable_i + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where XSVariable represents a specific list of variables utilized in our cross-sectional analyses. For the sake of simplicity, we have included only the results from the Poisson model in the main paper, while the robust results from the OLS model are presented in the online appendix (see Table A?). This practice is consistently followed throughout the subsequent analysis.

We begin by examining customers' personal attributes, including demographics, creditworthiness, and shopping characteristics. The findings are presented in Table 3.

We examine two demographic factors: gender and age. To categorize customers, we divide them into two groups based on gender (males and females) and four age groups (Groups 1-4). Group 1 comprises the 25% youngest customers from our sample (age < 23), while Group 4 consists of the 25% oldest customers (age > 37). In Column 1, we observe that the coefficients of the two three-way interaction terms, *Treatment * Post * Male* and *Treatment * Post * Female*, are both positive and significant at the 1% level. This suggests that credits increase spending regardless of gender. Notably, males are more influenced by credits compared to females. The *F*-test in the bottom row confirms that the coefficient of *Treatment * Post * Male* is significantly larger than that of *Treatment * Post * Female*. From an economic standpoint, this difference is also significant, with male consumers increasing their spending by approximately 11 percentage points more than female consumers following the allocation of credits.

Column 3 reveals that the impact of credits on spending varies based on the creditworthiness of consumers. Individuals with lower creditworthiness, as indicated by a lower application score determined by the retailer's credit assessment system, experience a 36.3% increase in their spending after receiving credits. This increase is approximately 18 percentage points higher than the increase observed among consumers with higher creditworthiness (statistically significant at the 1% level).

Columns 4-6 present the outcomes of cross-sectional analyses that specifically examine consumers' shopping characteristics including visiting frequency, conversion rate, and past spending level. These characteristics are likely to be associated with consumers' previous engagement. *HighVisit* and *LowVisit* are indicators which take a value of one if a user's monthly average visiting frequency in the 12-month window prior to receiving credits is above or below the median respectively, and zero otherwise. Similarly, *HighConversion*, *LowConversion*,

HighPastSpending, and *LowPastSpending* are defined in a similar manner. We discover that the increase in spending resulting from credits is more prominent among the low-visit, low-conversion, and low-past-spending group compared to other groups. These differences remain statistically significant at the 1% level, as demonstrated by the *F*-values in the final row. These findings strongly suggest that the effects of credits are most influential among consumers who were less active, providing compelling evidence for the effectiveness of the BigTech credit.

Insert Table 3 about here

The second set of cross-sectional analyses focuses on the level of financial constraint. Since we do not have information on each consumer's personal financial constraint, we rely on the wealth or financial development of the area in which the consumer is based. In other words, we assume that consumers from provinces where per capita disposable income, per capita household consumer expenditure, or the financial inclusion index rank among the bottom five provinces are likely to face stronger financial constraints. Data on income and expenditure are sourced from the National Bureau of Statistics of China. The financial inclusion index is obtained from a research of the People's Bank of China (2015). Following the framework of the G20's Global Partnership for Financial Inclusion (GPFI), the index incorporates 19 indicators under three dimensions of financial inclusion: accessibility, usage, and quality of financial services. This index is available for each province in China, with a lower score indicating poorer development of financial inclusion.

We anticipate and observe results that support the notion that consumers facing more severe financial constraints experience a greater increase in their spending following the grants of credits. A closer examination of Table 4, Column 1 reveals that the coefficient of *Treatment * Post * LowIncome* is significantly lower compared to that of *Treatment * Post * RestIncome*. This indicates that credits have a more pronounced impact on spending for consumers residing in

economically disadvantaged areas as opposed to those in other areas. Furthermore, our findings remain consistent when we measure financial constraint using consumption level (Column 2) or the financial inclusion index (Column 3). Collectively, our findings indicate that BigTech credit plays a significant role in promoting financial inclusion.

Insert Table 4 about here

The final set of cross-sectional analyses focuses on the level of logistic and Internet infrastructure development. These tests are based on the understanding that consumers' inclination to make purchases on the e-commerce platform may depend on the speed at which they receive their parcels and the convenience of their Internet usage.

In Table 5, Column 1 (2), we compare the effects of credits on spending between consumers in the top 5 provinces with the highest per capita total road (highway) length and consumers in other provinces. It is evident that the former group experiences a more significant increase in spending compared to the latter group (with the difference being significant at the 1% or 5% level). Our results suggest that the impact of BigTech credit relies on the speed with which consumer demand, particularly for the core offerings of BigTech companies like product procurement and delivery in this case, is met. This is also consistent with the notion that the interdependence between financial services and inherent non-financial commercial activities represents a distinctive characteristic of BigTech that sets it apart from other FinTech entities (Cornelli et al., 2020).

In contrast, according to Column 3 (4), the effects of credits on spending do not differ between consumers in the top 5 provinces with the highest Internet penetration rate (broadband access rate) and consumers in other provinces. These findings imply that Internet infrastructure does not play a role in moderating the impact of credits on spending. One possible explanation for this is that

using the e-commerce platform only requires basic Internet connection from the consumers' perspective, and all consumers in our sample already have access to such basic connection.

Insert Table 5 about here

4.3 The Effect of BigTech Credits on Spending -Robustness Checks

We conduct a series of robustness tests to validate our main finding, and the outcomes are presented in Table 6. To begin, Column 1 introduces an alternative definition in which *Spending* represents the total consumption for a consumer in a specific event month, rather than in a specific calendar month. In other words, $Spending_0$ reflects the consumption for a consumer in the thirty-day window immediately following the credit application. Moreover, $Spending_t$ represents the consumption for a consumer in the t+1'th thirty-day window after (before) the credit application when t is positive (negative). Column 2, on the other hand, excludes the application month from our sample. These two robustness tests mainly aim to eliminate any potential confounding factors from the application month, where some purchases occur before the credit is granted, while others happen afterward. We discover that the *Treatment* * *Post* interaction continues to exhibit a positive sign (significant at the 1% level) with a magnitude comparable to that observed in the previous tables.

Columns 3-5 employ alternative identification methods. In Column 3, we utilize the traditional staggered Difference-in-Differences (DID) approach with two-way fixed effects, including both time fixed effects and individual fixed effects. Column 4 utilizes the regular cohort approach, which addresses the issue of multiple treatment timings (as described by Bertrand, Duflo, and Mullainathan in 2004). Lastly, Column 5 applies the state-of-the-art approach proposed by Callaway and Sant'Anna in 2020, which addresses both cohort- and time-varying treatment effects. It's important to note that all columns utilize Poisson regression, except for Column 5, which

employs OLS regression. This is due to the absence of a Poisson version of the Callaway and Sant'Anna (2020) method. We find that our inferences remain consistent across all approaches, further strengthening the robustness of our findings.

Insert Table 6 about here

4.4 The Effects of BigTech Credits on other Shopping Behaviors

We further investigate how BigTech credits impact consumers' behaviors beyond final spending. Table 7 presents the outcomes obtained from estimating Eq. (1), with the dependent variable replaced by *Visits*, *Conversion*, *Orders*, *OrderSpending*, and *Discount* in Columns 1-5, respectively.

Visits represents the total number of visits made by a consumer in a given month. *Conversion* is calculated as the number of orders scaled by 10**Visits*, which measures how effectively the retailer converts visitors into consumers. By construction, *Orders* equals to 10**Visits* multiplied by *Conversion*, measuring the total number of orders made by a consumer in a given month. *OrderSpending* is the average spending amount per order for a consumer in a particular month. As our primary dependent variable, *Spending*, is essentially the product of *Visits*, *Conversion*, and *OrderSpending*, the results in Table 7 provide insights into the mechanisms through which credits drive an increase in spending.

Lastly, *Discount* is calculated as the total discount amount scaled by the total order amount for a specific month. It is important to note that the discount information is openly disclosed to consumers at the checkout page, prior to finalizing their payment. This ensures that the *Discount* variable accurately measures consumers' responsiveness to discounts during the purchasing process.

In Column 1, the positive and significant coefficient of the interaction term *Treatment * Post* suggests that credits have a positive impact on consumers' inclination to visit the shopping platform. In Column 2, the positive and significant coefficient of the same interaction term indicates that, holding the number of visits constant, credits facilitate the conversion of visits into purchases. Moving to Column 3, the positive coefficient of *Treatment * Post* shows that credits effectively increase the number of orders placed. This is unsurprising, as *Orders* is a product of *Visits* and *Conversion*. However, it is worth noting that in Column 4, there is no economically significant association between *Treatment * Post* and *OrderSpending*. Credit granting only increases the average order spending by 2%. Altogether, these results imply that the access to credits primarily enhances spending by increasing the number of orders made (through an increase in both visit frequency and conversion efficiency) rather than by increasing the amount spent per order.

Lastly, in Column 5, *Treatment * Post* exhibits a negative coefficient (significant at the 1% level). This finding suggests that credits diminish consumers' sensitivity to discounts. It aligns with the notion that credits effectively reduce searching costs for consumers.

Insert Table 7 about here

4.5 BigTech Credits and Spending: Results from Order-level Data

The results presented above are derived from our low-frequency user-month data. However, in order to gain deeper insights into the impacts of credits on spending, especially the possibility of overspending, we further analyze a more detailed and granular dataset. Given the vast size of the data, we randomly select 10% of the treated applicants and their corresponding matched control applicants from our main sample. This subset allows us to examine specific information regarding

every purchase made by these consumers. We capture details such as the timing of purchases, the specific items purchased, the quantity or amount bought, and the corresponding purchase price.

4.5.1 The Effects of BigTech Credits on Purchasing Variety Seeking

We proceed to conduct a stacked difference-in-difference analysis using Eq. (1), with a slight modification to the dependent variable, *Spending*. Instead, we substitute *Spending* with one of the following variables: *SKUs*, *Categories*, *Brands* and *Shops*.

The variable *SKUs* refers to the number of unique products that a consumer purchases within a specific month. *SKUs* are unique alphanumeric codes or numbers assigned to each product or item in a retailer's inventory. In our dataset, *SKUs* provide the most precise identification of products, even accounting for differences in attributes such as weight and color, which result in distinct SKUs.

Categories represents the number of unique product categories that a consumer purchases within a specific month. The e-commerce in our study categorizes products into three levels. The broadest category encompasses 73 categories, including electronics, personal care, sports, toys, and more. In contrast, the finest category consists of 5,067 categories. While the main paper presents results based on the broadest category, our findings remain consistent when utilizing the other two more detailed categories (results untabulated).

Additionally, we evaluate two additional variables: *Brands* and *Shops*. These variables signify the number of brands or shops from which a consumer makes purchases within a specific month.

Based on the findings presented in Table 8, the interaction term *Treatment* * *Post* consistently exhibits a positive coefficient across all four columns, indicating a statistically significant relationship at the 1% level. This outcome suggests that the provision of credits amplifies customers' inclination for variety seeking. Specifically, customers tend to purchase a higher

number of distinct products, across a wider range of categories, from a greater number of diverse brands, and across multiple distinct shops. The growing desire for a diverse array of options raises concerns about the potential for consumers with BigTech credit to overspend. To explore this, we carry out a series of analyses to illuminate the issue in the upcoming sections.

Insert Table 8 about here

4.5.2 Discretionary Spending

We manually determine whether the consumer's purchased product falls under the category of discretionary items based on its product category.[17] We create a dummy variable called *Discretionary*, which equals one if the product is discretionary and zero otherwise. Using a logit regression with Discretionary as the dependent variable, we find that *Treatment* * *Post* is not significant (Table 9), indicating that credits does not increase the likelihood of discretionary spending.

Insert Table 9 about here

4.5.3 Product Quantity and Product Price

Previously, Table 7 suggests that the increase in customer spending is primarily driven by an increase in the number of orders placed, rather than an increase in spending per order. However, the findings are based on monthly data, providing limited insight into whether recipients of credit display an increasing inclination to purchase more expensive goods.

Therefore, we analyze highly-granular data and introduce two variables: *ItemQuantity* and *ItemPrice*. These variables represent the number of items and the average price of the items purchased in an order, respectively. We use them as dependent variables in Table 10 Panel A and

[17] The discretionary items include Digital, Sports & Outdoors, Gifts, Books, Music, Movies & TV, Entertainment, Local Living/Travel, Watches, Digital Content, Jewelry & Accessories, Toys & Musical Instruments, Automotive Accessories, Pet Supplies, Alcohol, Automotive, Agricultural Supplies & Gardening, Stamps & Coins, Art, and so on.

Panel B. In this analysis, conducted at the order level, we include customer fixed effects, date-cohort fixed effects, as well as shop/brand/category fixed effects in Columns 1-3, respectively.

Notably, the interaction term *Treatment * Post* yields economically insignificant results in Panel A. Credit granting merely increases the quantity of goods purchased within the same shop/brand/category by about 1%. The interaction term *Treatment * Post* yields insignificant results in Panel B. This suggests that credits have little effect on the quantity or price of products purchased in an order. In other words, there is no evidence to indicate that credits lead to purchase upgrades within the same shop, brand, or category. This finding corroborates with the previous result from Table 7, Column 4, that credit does not increase the average spending amount per order in a given month. Combined with the results documented in Table 9, we conclude that it appears that consumers receiving credits do not purchase more expensive or discretionary items despite expanding their shopping choices.

Insert Table 10 about here

4.6 Credits Usage and Delinquency

Finally, we analyze the usage of credits and the delinquency rate over time. According to Table 11, the average credit limit monotonically increases since the credit was issued. This suggests that the e-commerce closely monitors consumer behavior and raises the credit limit for (some) consumers, likely based on their increased shopping activity and strong creditworthiness. Additionally, we observe that the proportion of the credit line utilized remains relatively stable, ranging from 10% to 12% for the majority of the observed period.

More importantly, we examine the delinquency rate, defined as the ratio of accounts that are more than 30 days overdue to total accounts. This definition aligns with that of Alibaba's Huabei

according to the Ant Group prospectus, and the work of Berg et al. (2023).[18,19]. Our study reveals that the delinquency rate remains fairly low, averaging around 2.3% throughout the one-year period after credit issuance. This rate is comparable to similar financial products globally, as evidenced by Berg et al. (2023), who report a default rate slightly under 2%, and Alibaba, which indicates Huabei's default rate fluctuates between 1.08% and 3.01%.

Additionally, our data show that among consumers who default, the overwhelming majority settle their debts in full within 90 days of the initial due date, and there is no notable distinction in the rates of delinquency between consumers with high credit lines and those with lower credit limits. Specifically, Table 12 examines delinquency outcomes in individuals with above- and below-median initial credit limits. In Panel A, the correlation between *CreditLimit* and other variables remains consistently low. This indicates that the credit line assigned for reject inference appears to be random, allowing us to effectively split the sample into *HighCreditLimit* and *LowCreditLimit* groups. Panel B illustrates that delinquency distributions remain stable over time, showing no significant differences between *HighCreditLimit* and *LowCreditLimit* users. This suggests that granting higher credit limits does not expedite the occurrence of delinquency. Panel C reveals significant differences in almost all delinquency statuses between *HighCreditLimit* and *LowCreditLimit* users. Notably, the difference for (30, 60] is significant, but its direction contradicts the expectation of overspending.

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[18] Source: http://static.sse.com.cn/stock/information/c/202008/e731ee980f5247529ea824d20fcdb293.pdf Ant Group deems accounts overdue by more than 30 days to be in default. Additionally, it reports its default rate for accounts 90 days overdue separately.

[19] Berg et al. (2023) describe a process whereby a reminder is emailed to the BNPL consumer on the payment due date, with a grace period of 14 days before imposing a late payment fee. A second reminder with a penalty fee follows, and after another 14 days, a final reminder is sent by mail. After an additional 14-day period without payment, the account is referred to a debt collection agency and is deemed delinquent, indicating that accounts more than 42 days overdue are considered delinquent.

In summary, our findings demonstrate that consumers with access to BigTech credit do not increase their discretionary spending, do not purchase more expensive items than before, and do not exhibit a high delinquency rate. These results allow us to conclude that BigTech credit is not detrimental to consumers' financial health.

Insert Table 11 about here

5. CONCLUSIONS

Over recent years, BigTech firms renowned for their technology-related services, including e-commerce, search engines, social media, and hardware and software development, have ventured into the credit services sector. Despite this innovation, there remains a significant gap in the literature concerning the impact of BigTech credit and, more broadly, FinTech credit on consumer activities. Our paper endeavors to close this research gap by leveraging a detailed, novel dataset which integrates multiple granular dimensions of information from a leading BigTech' ecosystem.

Our research reveals that consumers who are granted BigTech credit demonstrate an approximate 20% increase in spending per month post-credit application, compared to non-credit recipients. Through a variety of cross-sectional analyses, we demonstrate that male consumers, those with lower credit ratings, or individuals exhibiting less engagement with the e-commerce platform experience a more substantial increase in spending than their counterparts. We also note that consumers presumably under greater financial constraints and those in regions with more developed logistical frameworks exhibit a more notable rise in expenditure following credit approval.

Delving into the drivers of spending increases, our data show that BigTech credit influences spending primarily by elevating the number of orders placed, rather than the amount spent per

order. This is facilitated by increased visit frequency to the platform and improved conversion efficiency for purchases.

With access to order-item-level data, we observe that credit recipients tend to purchase from a wider range of product categories and select items from a broader array of brands, shopping across numerous distinct stores. However, this enhanced inclination toward variety-seeking does not equate to reckless spending as BigTech credit does not lead to higher discretionary spending or more expensive purchases, nor does it contribute to an uplifted delinquency rate.

In conclusion, our data suggest that BigTech credit augments consumer spending and encourages financial inclusion without precipitating over-indebtedness.

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Appendix A. Variable Definitions

Main Variables	
Spending	The total out-of-pocket consumption (i.e., canceled amount and discount are excluded) for a consumer in a specific calendar month. For the application month, the consumption on the application day and the day after application is excluded.
Treatment	A dummy variable which is set to one if a consumer's credit application is rejected by the FinTech model but approved afterwards for the purpose of reject inference, and zero if the application is rejected by the FinTech model without being approved manually for reject inference.
Post	A dummy variable which is set to one if the calendar month is the month a consumer applies for credit or any of the subsequent months, and zero otherwise.
CreditLimit	The initial credit limit granted to a consumer when her application is approved.
Other Purchasing Characteristics	
Visits	The total number of visits made by a consumer in a given month. Each time a product page is browsed counts as one visit. 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 specific month scaled by 10**Visits*.
Orders	The number of orders a consumer made in a specific month. For the application month, the orders made on the application day and the day after application are excluded.
Discount	The total discount amount scaled by the total order amount by a consumer for a specific month. For the application month, the orders made on the application day and the day after application are excluded.
OrderSpending	The average spending amount of the orders for a consumer in a specific month. For the application month, the orders made on the application day and the day after application are excluded.
SKUs	The number of unique product (i.e., stock keeping unit or SKU) a consumer purchases in a specific month.
Categories	The number of product category a consumer purchases in a specific month.
Brands	The number of brands a consumer purchases in a specific month.
Shops	The number of shops a consumer purchases from in a specific month.
ItemQuantity	The number of items in an order.
ItemPrice	The average price of the items purchased in an order.
Variables used in Cross-sectional Analysis	
Male (Female)	A dummy that equals one if the consumer is a male (female) and zero otherwise.
AgeGroup1 (2/3/4)	A dummy that equals one if the consumer's age is smaller than 23 (between 23 and 29/ between 30 and 37/ greater than 37), and zero otherwise.
HighScore (LowScore)	A dummy that equals one if a user's application score for the credit granting is above (below) the median, and zero otherwise.
HighVisit (LowVisit)	A dummy that equals one if a user's monthly average visiting frequency (i.e., number of product pages browsed) in the 12-month-window prior to the 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.
HighConversion (LowConversion)	A dummy that equals one if a user's conversion rate (i.e., total consumption scaled by total visit) in the 12 month-period prior to the

	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 that equals one if a user's monthly average spending in the 12-month-window prior to the 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.
LowIncome (RestIncome)	A dummy that equals one if the consumer is (not) from a province in which the disposable income scaled by the population is among the bottom five provinces, and zero otherwise.
LowConsumption (RestConsumption)	A dummy that equals one if the consumer is (not) from a province in which the household consumer expenditure scaled by the population is among the bottom five provinces, and zero otherwise.
LowFinInclusion (RestFinInclusion)	A dummy that equals one if the consumer is (not) from a province in which the financial inclusion index is among the bottom five provinces, and zero otherwise. The financial inclusion index is from the working paper of People's Bank of China.
HighTotalRoad (RestTotalRoad)	A dummy that equals one if the consumer is (not) from a province in which total road length scaled by the population is among the top five provinces, and zero otherwise.
HighHighWay (RestHighWay)	A dummy that equals one if the consumer is (not) from a province in which highway road length scaled by the population is among the top five provinces, and zero otherwise.
HighInternetPenetration (RestInternetPenetration)	A dummy that equals one if the consumer is (not) from a province whose number of Internet users scaled by the population is among the top five provinces, and zero otherwise.
HighBroadband (RestBroadband)	A dummy that equals one if the consumer is (not) from a province where broadband access per capital is among the top five provinces, and zero otherwise.

Table 1. Summary Statistics

Panel A. Individual Variables (N=58,688)

	Mean	Std	Min	25%	50%	75%	Max
Gender	0.62	0.48	0	0	1	1	1
User Age (year)	30.73	11.04	16	22	29	37	67
Account Age (month)	27.21	24.3	0.72	5.77	21.38	42.89	110.66
Application Score	688.26	35.16	607	663	687	713	772
Credit Limit	166.36	206.37	0	0	50	450	500

Panel B. Monthly Variables

	Count	Mean	Std	Min	25%	50%	75%	Max
Visits	1,172,262	80.21	148.89	0	2	24	87	972
Orders	1,172,262	2.03	3.46	0	0	1	3	21
Spending	1,172,262	296.61	834.77	0	0	5.5	199	6084.7
OrderSpending	631,858	154.99	294.42	0	29	69	146.9	2078.5
Discount	631,478	0.19	0.19	0	0.01	0.15	0.31	0.8
Cancel	631,478	0.21	0.32	0	0	0	0.38	1

Table 2. The Effect of Credit Granting on Spending

This table presents the effect of credit granting on consumption. We perform a stacked cohort difference-in-difference analysis using the following specifications: $Spending_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$ (Panel A) and $Spending_{i,c,t} = \beta \times CreditLimit_i \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$ (Panel B), where i is the subscript for consumer, c is the subscript for cohort (note: we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month, α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. Variable definitions are in Appendix A. In both panels, column 1 uses OLS regression and column 2 uses Poisson regression. In Panel B, *CreditLimit* is scaled by 100. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Panel A

Dep. Var.= Spending	(1)	(2)
	OLS	Poisson
Treatment * Post	68.633***	0.230***
	(15.244)	(18.126)
Constant	270.343***	6.245***
	(285.623)	(2,296.364)
Individual FE	YES	YES
Month-Cohort FE	YES	YES
Observations	1,172,262	1,172,262
Adjusted/Pseudo R-squared	0.231	0.388

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Panel B

Dep. Var.= Spending	(1)	(2)
	OLS	Poisson
Treatment * Post * CreditLimit	15.036***	0.047***
	(12.862)	(15.990)
Constant	274.604***	6.261***
	(340.879)	(3,050.785)
Individual FE	YES	YES
Month-Cohort FE	YES	YES
Observations	1,172,262	1,172,262
Adjusted/Pseudo R-squared	0.231	0.388

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. The Effect of Credit Granting on Spending – Cross-sectional Analysis based on User Characteristics

This table presents the differential effect of credit granting on consumption conditional on user characteristics. We perform a stacked cohort difference-in-difference analysis with group indicators using the following specification: $Spending_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} \times UserCharacteristic_i + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (note: we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month. α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. $UserCharacteristic$ is a list of user characteristics. Variable definitions are in Appendix A. We obtain the estimates using Poisson regressions. We perform Wald Chi-squared test to compare credit granting's effects between different groups, and report the p-value of the test in the last row. All models include two-way interaction terms as control variables when applicable (estimates unreported). We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Column (1) – (3)

Dep. Var.= Spending	(1)	(2)	(3)
Treatment * Post * Male	0.270***		
	(17.570)		
Treatment * Post * Female	0.165***		
	(7.293)		
Treatment * Post * AgeGroup1		0.417***	
		(11.351)	
Treatment * Post * AgeGroup2		0.272***	
		(11.549)	
Treatment * Post * AgeGroup3		0.165***	
		(6.030)	
Treatment * Post * AgeGroup4		0.109***	
		(4.109)	
Treatment * Post * HighScore			0.179***
			(11.198)
Treatment * Post * LowScore			0.363***
			(13.653)
Constant	6.248***	6.263***	6.187***
	(803.865)	(588.358)	(435.629)
	YES	YES	YES
Applicable Two-way Interaction Terms	YES	YES	YES
	YES	YES	YES
Individual FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	1,172,262	1,172,262	1,172,262
Pseudo R-squared	0.389	0.389	0.389

p-value ($\beta_1 = \beta_2$) for Columns 1, 3-6 p-value ($\beta_1 = \beta_4$) for Column 2	**0.000**	**0.000**	**0.000**

Column (4) – (6)

Dep. Var.= Spending	(4)	(5)	(6)
Treatment * Post * HighVisit	0.188***		
	(12.632)		
Treatment * Post * LowVisit	0.332***		
	(14.294)		
Treatment * Post * HighConversion		0.157***	
		(9.309)	
Treatment * Post * LowConversion		0.350***	
		(16.858)	
Treatment * Post * HighPastSpending			0.156***
			(10.621)
Treatment * Post * LowPastSpending			0.475***
			(18.692)
Constant	6.245***	6.391***	6.469***
	(508.536)	(613.069)	(202.285)
	YES	YES	YES
Applicable Two-way Interaction Terms	YES	YES	YES
	YES	YES	YES
Individual FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	1,172,262	1,163,937	1,172,262
Pseudo R-squared	0.389	0.392	0.395
p-value ($\beta_1 = \beta_2$) for Columns 1, 3-6 p-value ($\beta_1 = \beta_4$) for Column 2	**0.000**	**0.000**	**0.000**

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4. The Effect of Credit Granting on Spending – Cross-sectional Analysis based on Financial Constraint

This table presents the differential effect of credit granting on consumption conditional on financial constraint. We perform a stacked cohort difference-in-difference analysis with group indicators using the following specification: $Spending_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} \times FinancialConstraint_i + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (note: we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month. α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. $FinancialConstraint$ is a list of variables that measure the wealth or level of financial inclusion on the province level. Variable definitions are in Appendix A. We identify each consumer's province using the delivery addresses (the most frequently chosen shipping province in their pre-credit-application orders). We obtain the estimates using Poisson regressions. We perform Wald Chi-squared test to compare credit granting's effects between different groups, and report the *p*-value in the last row. All models include two-way interaction terms as control variables when applicable (estimates unreported). We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Dep. Var.= Spending	(1)	(2)	(3)
Treatment * Post * LowIncome	0.389***		
	(5.678)		
Treatment * Post * RestIncome	0.223***		
	(17.457)		
Treatment * Post * LowConsumption		0.364***	
		(5.831)	
Treatment * Post * RestConsumption		0.223***	
		(17.269)	
Treatment * Post * LowFinInclusion			0.522***
			(5.529)
Treatment * Post * RestFinInclusion			0.223***
			(17.600)
Constant	6.249***	6.250***	6.248***
	(2,272.516)	(2,236.494)	(2,279.106)
Second-order Interaction Terms	YES	YES	YES
Individual FE	YES	YES	YES
Month-Cohort FE	YES	YES	YES
Observations	1,164,016	1,164,016	1,164,016
Pseudo R-squared	0.387	0.387	0.387
p-value ($\beta_1 = \beta_2$)	**0.017**	**0.027**	**0.002**

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. The Effect of Credit Granting on Spending – Cross-sectional Analysis based on Logistic and Internet Infrastructure

This table presents the differential effect of credit granting on consumption conditional on logistic and internet infrastructure development. We perform a stacked cohort difference-in-difference analysis with group indicators using the following specification: $Spending_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} \times Infra_i + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (note: we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month. α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. $Infra$ is a list of variables that measure the level of logistic and internet infrastructure on the province level. Variable definitions are in Appendix A. We identify each consumer's province using the delivery addresses (pre-event most frequently chosen shipping province). We obtain the estimates using Poisson regressions. We perform Wald Chi-squared test to compare credit granting's effects between different groups. We report the *p*-value in the last row. All models include two-way interaction terms as control variables when applicable (estimates unreported). We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Dep. Var.= Spending	(1)	(2)	(3)	(4)
Treatment * Post * HighTotalRoad	0.481***			
	(5.726)			
Treatment * Post * RestTotalRoad	0.221***			
	(17.267)			
Treatment * Post * HighHighWay		0.422***		
		(5.438)		
Treatment * Post * RestHighWay		0.222***		
		(17.494)		
Treatment * Post * HighInternetPenetration			0.218***	
			(11.175)	
Treatment * Post * RestInternetPenetration			0.239***	
			(12.962)	
Treatment * Post * HighBroadBand				0.238***
				(6.366)
Treatment * Post * RestBroadBand				0.227***
				(15.678)
Constant	6.249***	6.249***	6.224***	6.248***
	(2,242.393)	(2,228.239)	(1,269.380)	(1,961.248)
Second-order Interaction Terms	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES
Month-Cohort FE	YES	YES	YES	YES
Observations	1,164,016	1,164,016	1,164,016	1,164,016
Pseudo R-squared	0.387	0.387	0.387	0.387
p-value ($\beta_1 = \beta_2$)	**0.002**	**0.011**	0.445	0.803

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6. The Effect of Credit Granting on Spending – Robustness Checks

This table presents the results of the robustness checks on the effects of credit granting on spending. Column 1 uses an alternative definition of *Spending,* which is the total out-of-pocket consumption for a consumer in a specific *event month* rather than in a specific 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 uses the regular cohort approach to address the multiple treatment timing issue (Bertrand, Duflo, and Mullainathan, 2004). Column 5 employs the state-of-the-art approach proposed in Callaway and Sant'Anna (2020) to address both cohort- and time-varying treatment effects. All columns use the Poisson regression except Column 5 which uses OLS. Variable definitions are in Appendix A. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Dep. Var.= Spending	(1)	(2)	(3)	(4)	(5)
	Alternative Definition of Spending	Excluding Application Month	Staggered DID	Regular Cohort DID	C&S DID
Treatment * Post	0.245***	0.239***	0.225***	0.237***	38.081***
	(17.251)	(17.207)	(15.834)	(16.882)	(7.041)
Constant	6.252***	6.241***	6.244***	5.611***	-
	(2,176.423)	(2,253.504)	(1,994.659)	(774.541)	-
Individual FE	YES	YES	YES	NO	
Month-Cohort FE	YES	YES	NO	NO	
Cohort FE	NO	NO	NO	YES	
Month FE	NO	NO	YES	YES	
Observations	1,101,250	1,113,574	1,172,262	1,172,262	-
Adjusted/Pseudo R-squared	0.382	0.390	0.388	0.008	-

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7. The Effect of Credit Granting on Other Purchasing Characteristics

This table presents the effect of credit granting on purchasing characteristics other than spending amount. We perform a stacked cohort difference-in-difference analysis using the following specification: $Y_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (here, we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month, $Y_{i,c,t}$ is a series of consumption outcome for consumer i in calendar month t for cohort c (see Appendix A for definitions), α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

	(1)	(2)	(3)	(4)	(5)
Dep. Var.=	Visits	Conversion	Orders	OrderSpending	Discount
Treatment * Post	0.163***	0.078***	0.253***	0.020**	-0.056***
	(13.404)	(7.351)	(20.033)	(2.092)	(-6.485)
Constant	4.857***	-0.458***	0.997***	5.346***	-1.528***
	(1,799.913)	(-218.900)	(330.636)	(3,081.617)	(-906.074)
Individual FE	YES	YES	YES	YES	YES
Month-Cohort FE	YES	YES	YES	YES	YES
Observations	1,168,554	928,074	1,172,262	617,787	615,214
Pseudo R-squared	0.498	0.164	0.269	0.356	0.042

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8. The Effect of Credit Granting on Purchasing Variety Seeking - Evidence from the Detailed Sample

This table presents the effect of credit granting on purchasing characteristics based on the order-level data. We perform a stacked cohort difference-in-difference analysis using the following specification: $Y_{i,c,t} = \beta \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (here, we have six cohorts for applications made during each month of our six-month application period), t is the subscript for calendar month, $Y_{i,c,t}$ is a series of indicators of purchasing variety for consumer i in calendar month t for cohort c (see Appendix A for definitions), α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. We use a random sub-sample of consumers with detailed order-level data for the tests. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

	(1)	(2)	(3)	(4)
Variety of Purchase Measured by	SKUs	Categories	Brands	Shops
Treatment * Post	0.233***	0.220***	0.216***	0.227***
	(5.792)	(7.307)	(5.761)	(5.993)
Constant	1.175***	0.411***	0.899***	0.912***
	(129.240)	(60.826)	(105.859)	(105.804)
Individual FE	YES	YES	YES	YES
Month-Cohort FE	YES	YES	YES	YES
Observations	115,073	115,073	114,949	114,798
Pseudo R-squared	0.340	0.219	0.303	0.302

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9. The Effect of Credit Granting on Discretionary Spending- Evidence from the Order-level Data

This table presents the effect of credit granting on the likelihood of discretionary spending based on the order-level data. The unique observation is at the order-SKU level. We perform a stacked cohort difference-in-difference analysis using the following specification: $Discretionary_{i,c,t} = \beta_1 \times Treatment_i \times Post_{t,c} + \beta_2 \times Treatment_i + \beta_3 \times Post_{t,c} + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (here, we have six cohorts for applications made during each month of our sample period), t is the subscript for calendar day of the order. Variable definitions are in Appendix A. We use a random sub-sample of consumers with detailed order-level data for the tests. We obtain the estimates using a logit regression. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Dep. Var.=Discretionary	
Treatment * Post	0.011
	(0.511)
Treatment	-0.063***
	(-4.722)
Post	-0.159***
	(-10.485)
Constant	-1.313***
	0.011
Observations	239,116
Pseudo R-squared	0.001

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10. The Effect of Credit Granting on Purchasing Quantity and Purchasing Price- Evidence from the Order-level Data

This table presents the effect of credit granting on purchasing characteristics (i.e., purchasing quantity in Panel A and purchasing price in Panel B) based on the order-SKU level data. The unique observation is at the order-SKU level. We perform a stacked cohort difference-in-difference analysis using the following specification: $ItemQuantity_{i,c,t}/ItemPrice_{i,c,t} = \beta_1 \times Treatment_i \times Post_{t,c} + \alpha_i + \delta_{t,c} + \xi_j + \epsilon_{i,c,t}$, where i is the subscript for consumer, c is the subscript for cohort (here, we have six cohorts for applications made during each month of our sample period), t is the subscript for calendar day of the order, α_i represents individual fixed effects, $\delta_{t,c}$ represents date-cohort fixed effects, and ξ_j denotes the shop/brand/category fixed effects for different columns. Variable definitions are in Appendix A. We use a random sub-sample of consumers with detailed order-level data for the tests. We obtain the estimates using Poisson regressions. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

Panel A

Dep. Var.= ItemQuantity	(1)	(2)	(3)
Treatment * Post	0.014*	0.016*	0.012
	(1.674)	(1.648)	(1.056)
Constant	0.014*	0.016*	0.012
	(1.674)	(1.648)	(1.056)
Individual FE	YES	YES	YES
Date-Cohort FE	YES	YES	YES
Shop FE	YES		
Brand FE		YES	
Category FE			YES
Observations	207,467	215,005	238,781
Pseudo R-squared	0.091	0.083	0.051

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Panel B

Dep. Var.= ItemPrice	(1)	(2)	(3)
Treatment * Post	-0.000	-0.002	0.023
	(-0.018)	(-0.134)	(0.835)
Constant	-0.000	-0.002	0.023
	(-0.018)	(-0.134)	(0.835)
Individual FE	YES	YES	YES
Date-Cohort FE	YES	YES	YES
Shop FE	YES		
Brand FE		YES	
Category FE			YES
Observations	207,467	215,005	238,781
Pseudo R-squared	0.858	0.803	0.503

Robust t-statistics in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 11. Credit Usage and Delinquency

This table presents the usage of credit and delinquency rates during the credit-granting month and subsequent months.

			Proportion of accounts with days of delinquency =				
Time	Average Credit Limit	Average Credit Usage	0	(0,30]	(30,60]	(60,90]	>90
$Month_0$	377	10.71%	93.6%	6.4%			
$Month_1$	454	10.50%	93.8%	4.7%	1.4%		
$Month_2$	623	10.56%	94.4%	3.3%	1.3%	1.0%	
$Month_3$	888	10.71%	94.2%	3.4%	0.6%	0.8%	0.9%
$Month_4$	1,187	11.21%	93.9%	3.4%	0.7%	0.4%	1.5%
$Month_5$	1,430	11.30%	93.8%	3.4%	0.7%	0.4%	1.7%
$Month_6$	1,686	11.40%	93.3%	3.7%	0.7%	0.4%	1.9%
$Month_7$	1,869	11.59%	93.6%	3.5%	0.7%	0.4%	1.8%
$Month_8$	2,118	11.98%	93.1%	3.7%	1.0%	0.4%	1.8%
$Month_9$	2,281	12.40%	92.8%	3.9%	0.8%	0.7%	1.8%
$Month_{10}$	2,612	11.82%	92.9%	3.7%	0.8%	0.3%	2.2%
$Month_{11}$	2,839	10.29%	93.6%	3.0%	0.8%	0.4%	2.2%

Table 12. Credit Limit and Delinquency Rate

Panel A. Correlation between CreditLimit and Other Variables

	Male	User Age	Account Age	Application Score	Visit Pre-12M Cumulation	Spending Pre-12M Cumulation	Visit Pre-12M Average	Spending Pre-12M Average
Credit Limit	-0.05***	0.06***	0.04***	0.12***	0.03***	0.04***	0.02***	0.03***

Panel B. Delinquency Rate along Time – High CreditLimit versus Low CreditLimit

	High CreditLimit: Proportion of accounts with days of delinquency =					Low CreditLimit: Proportion of accounts with days of delinquency =				
Relative Month	0	(0,30]	(30,60]	(60,90]	>90	0	(0,30]	(30,60]	(60,90]	>90
0	93%	7%				94%	6%			
1	94%	5%	1%			94%	5%	2%		
2	94%	3%	1%	1%		94%	3%	1%	1%	
3	94%	4%	1%	1%	1%	94%	3%	1%	1%	1%
4	94%	3%	1%	0%	2%	94%	4%	1%	0%	2%
5	94%	3%	1%	0%	2%	94%	3%	1%	0%	2%
6	93%	4%	1%	0%	2%	93%	4%	1%	0%	2%
7	94%	3%	1%	0%	2%	93%	4%	1%	0%	2%
8	93%	4%	1%	0%	2%	93%	4%	1%	0%	2%
9	93%	4%	1%	1%	2%	93%	4%	1%	1%	2%
10	92%	4%	1%	0%	3%	93%	3%	1%	0%	2%
11	93%	3%	1%	0%	3%	94%	3%	1%	0%	2%

Panel C. Delinquency Rate across Status – High CreditLimit versus Low CreditLimit

	High CreditLimit			Low CreditLimit			High - Low		
Delinquency Status	# Obs	Mean	SD	# Obs	Mean	SD	Diff	t-statistic	p-value
0	124,977	0.9377	0.2417	128,284	0.9371	0.2429	0.0006	0.6386	0.5231
(0,30]	124,977	0.0393	0.1943	128,284	0.0381	0.1914	0.0012	1.5864	0.1126
(30,60]	124,977	0.0078	0.0879	128,284	0.0087	0.0931	-0.0010	-2.6461	0.0081
(60,90]	124,977	0.0043	0.0655	128,284	0.0046	0.0677	-0.0003	-1.1105	0.2668
>90	124,977	0.0109	0.1039	128,284	0.0115	0.1066	-0.0006	-1.3964	0.1626

Figure 1. Univariate Analysis – Monthly Spending before and after Credit Application

This figure plots the monthly spending (i.e., Spending) before and after credit application for both treatment and control group. The vertical axis is the monthly spending measured in RMB. The horizontal axis is the number of months before or after the application, where 0 stands for the month of application.

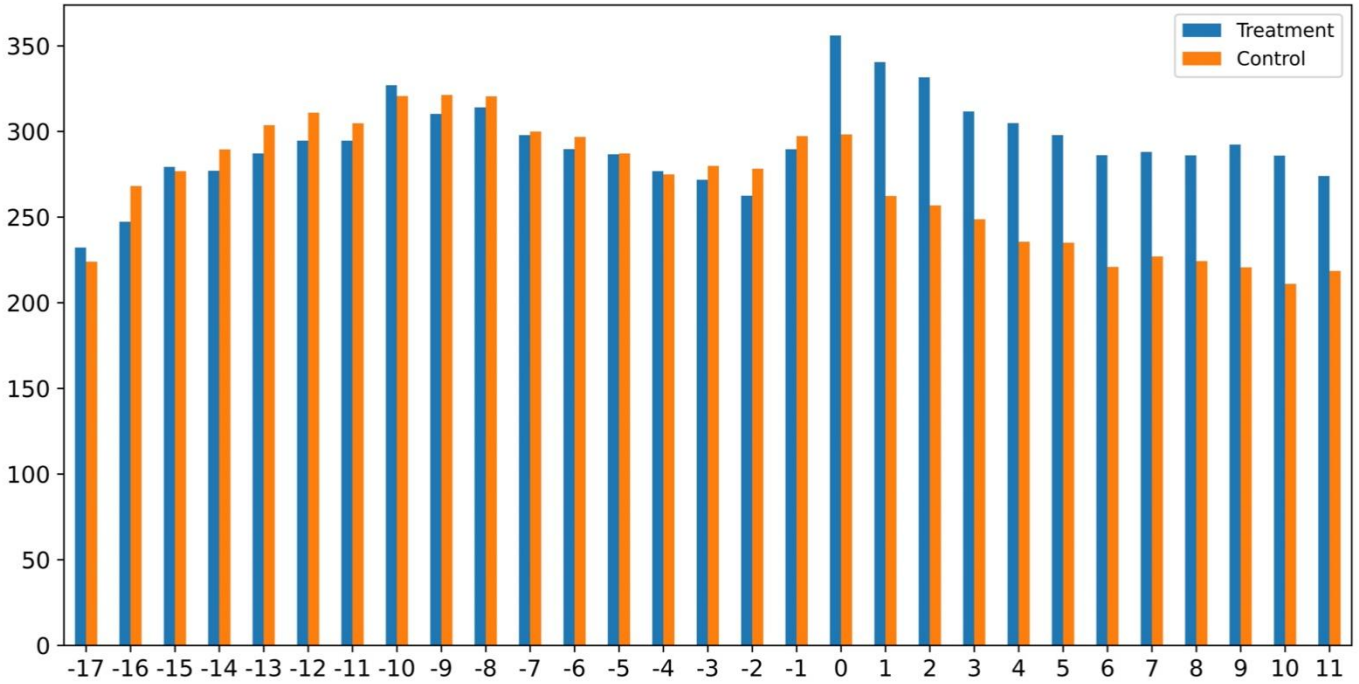


Figure 2. The Effect of Credit Granting on Spending -a Parallel Trend Test Using the Dynamic Model

These figures present the effect of credit granting on consumption. We perform a stacked cohort difference-in-difference analysis using the following specifications: $Spending_{i,c,t} = \sum_{t=-17}^{-2} \beta_t \times Treatment_i \times 1_{t,c} + \sum_{t=0}^{11} \beta_t \times Treatment_i \times 1_{t,c} + \alpha_i + \delta_{t,c} + \epsilon_{i,c,t}$, where t is the relative month where $t = 0$ demotes the month individual i applying for credit, $1_{t,c}$ is the indicator that equals one if the individual belongs to cohort c that has been treated in calendar month t and zero otherwise, and β_t captures the treatment effect of credit granting on consumption benchmarked on $t = -1$, i.e., the month prior to the application month. α_i represents individual fixed effects, $\delta_{t,c}$ represents month-cohort fixed effects. Variable definitions are in Appendix A. Panel A uses OLS regression and Panel B uses Poisson regression. We cluster standard errors by both individual and calendar month in all regressions. All continuous variables are winsorized at 1% and 99% levels.

