Credit Supply and Household Debt Decisions

By

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Abstract

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This dissertation contains two chapters. In Chapter 1, I combine randomized controlled trials with transaction-level data and survey data, I show that credit-limit extensions significantly increase consumer expectations about their future income without increasing realized income. A one-dollar higher qualified credit limit raises consumer income expectations over the next six months by 40 cents and total consumption by 34 cents. The expectation changes explain around 35% of the total spending responses to credit limit extensions. The results show that consumers infer information about their future income from credit supply, and this learning behavior impacts their economic decision-making greatly.

In the second chapter, I structurally estimate a life-cycle model incorporating the income-inference channel to study how shocks to banks' beliefs affect their credit extensions and consumers' subsequent spending behaviors. I find that, When the precision of bank signal about consumer future income increases by 25%, equilibrium credit supply and spending for the median consumer respectively increase by 14.11% and 4.11%. As suggested by the counter-factual analysis, when the advancement of information technology enables banks to extract more precise signals about household future income, credit supply, and spending tend to increase. The elasticity of spending with respect to bank-signal precision is around 0.16.

Contents

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Chapter 1

Income Inference from Credit Extension

1.1 Introduction

Credit limit is a crucial factor that affects household consumption-saving decisions, as it underpins how much consumers can borrow to smooth consumption. Understanding how consumers react to changes in credit limit is therefore important to inform the design of effective macroeconomic policies. As predicted by the workhorse economic models, variations in credit limit should have significant impacts on total spending only for those close to being liquidity-constrained. However, existing literature documents a very large average spending response to changes in credit limits. In addition, even for consumers that are far from being borrowing-constrained, credit limit extensions still induce a non-trivial amount of increases in total consumption[1]. Hence, the micro-level mechanisms of why credit limit extensions affect consumer spending remains an open question.

Standard estimation of spending responses to borrowing-limit extensions relies on random or quasi-random variations in credit limits. An implicit assumption in these settings is that consumers in the field also treat credit supply events as random. An intriguing yet unanswered question is how consumers think about banks' credit supply decisions. That is, do consumers always treat credit supply events, in the form of extended credit limits, as random shocks only to their borrowing constraints? Motivated by this question, this paper studies the effects of increases in credit limits on consumption by affecting consumer expectations.

Studying how credit supply affects consumer expectations is difficult, as one needs to identify belief changes around field credit supply events. To cope with this difficulty, I collaborate with a large commercial bank in China to explore how consumers change their expectations around credit expansion events. The analysis is comprised of two random-

[1]See Gross and Souleles (2002), Agarwal et al. (2017), D'Acunto et al. (2020), and Aydin (2022) for some examples.

ized controlled trials (RCT). The two RCTs are the same in the credit extension processes. Specifically, based on its usual internal underwriting process, the bank had planned to offer a group of customers an increase in credit card limits in both experiments. Then with the experimental design, the bank postponed the limit-change offers for a randomly-selected group (the control groups) of customers for six months. The rest of the customers (the treatment groups) received the pre-determined limit-change offers and could decide whether to accept them. Given that the credit supply offers are based on the bank's normal underwriting process, the setting gives a nice opportunity to identify the effects of qualified limit extensions in the field.

The two experiments are different in the types of information collected. For the first experiment, which served as a pilot study to shed light on the more specific research questions, all participants received a survey eliciting their beliefs about their future perspectives within one week after the treatment group received the limit-increase offers. The survey mainly asked about expectations about different components of consumer budget constraints (e.g., consumption, saving, income, delinquency probability, etc.) in the future. This study shows a significant effect of credit-limit extensions on expected future consumption, which confirms previous empirical literature documenting a large effect of limit extensions on total consumption. At the same time, consumers also update their income expectations upwards significantly. Specifically, consumers believe that their total income over the next six months would be 31 cents higher for each dollar higher qualified credit limit. However, increases in credit card limits are not associated with lower expected savings, a higher delinquency rate, or a lower unemployment rate.

The pilot study suggests that consumers associate credit extension with higher consumption financed by higher future income instead of drawing down savings, higher default choices, or increased labor supply. These findings, therefore, suggest an income-inference channel through which credit extensions affect consumption. Motivated by these findings, around the second experiment, I collect more information associated with consumer income expectations, both before and after the experiments. In addition, I also combine survey answers with consumer spending and borrowing history around the second experiment. With the combination of the RCT, survey results, and transaction history, I can dissect the effects of credit extension on total spending through a direct borrowing-constraint channel and an indirect belief channel.

Similar to the first experiment, each \$1 increase in credit limit offered by the bank increases consumers' expectations about their income over the next six months by \$0.40. Since the consumers in the treatment could choose to accept the offers or not, I continue to study how expectations respond differently to limit extension by the acceptance decision. Interestingly, the effects are significant for both those who accept and do not accept the limit changes. I find that for those who accept the limit increase, each \$1 increase in the credit limit raises six-month income expectations by \$0.48. Even for those who do not accept the offer, each \$1 increase in the credit limit increases income expectations by \$0.24.

I continue to explore whether the updates in the beliefs regarding future income are "cheap talk" or consumers indeed *act* on them. The concern is based on the recent studies documenting that large variations in individual beliefs may have a limited relationship with their actions (Ameriks et al., 2020; Giglio et al., 2021). First, I study the effects of credit extensions on consumers' total borrowing, unconditional on the changes in their income expectations. I find a \$1 higher credit limit offered by the bank increases consumers' outstanding debt by \$0.128. This estimate is quite close to the documented marginal propensity to borrow (MPB) out of credit limit in the previous literature (Gross and Souleles, 2002; Agarwal et al., 2017; Aydin, 2022), which is usually in the range of \$0.09 to \$0.20. With transaction-level data, I can also analyze the effects of relaxed credit limits on non-debt-financed spending. Similar to the findings on borrowing, I find that for a \$1 increase in the credit limit, non-debt-financed spending increases by \$0.214 in total over a six-month horizon. In sum, each dollar higher qualified credit limit increases total spending by around 34.2 cents over six months.

I continue to study the consumption responses to credit limit extension by the acceptance decisions. I find that, for those who accept the offer, a \$1 higher credit limit increases consumers' outstanding debt (non-debt-financed spending) by \$0.176 (\$0.275) over a six-month horizon. A more interesting finding is that, even for those who do not accept the offers, each dollar higher offered credit limit still increases their outstanding debt (non-debt-financed spending) by \$0.043 (\$0.104) over the six months after the experiment. This finding also suggests that realized borrowing limit might not be the only reason for which credit limit extensions affect total consumption.

Only documenting that an increase in credit limit raises consumption does not exclude other conventional channels, for example, the precautionary-saving channel. Precisely, in the buffer-stock model, credit-limit shocks affect total consumption by relaxing consumers' precautionary motives due to a lower possibility of binding borrowing constraints in the future. For comparison, in the presence of income inference, credit supply also affects spending by changing consumers' beliefs. Therefore, a natural question is how important the income-inference channel is quantitatively compared to the precautionary-saving channel. Using several decomposition strategies, I find that, controlling for the changes in income expectations, the effects of limit-extension news on borrowing decrease by around 35% for those who accept the offer. For those who do not accept the offer, the effects become insignificant. These results suggest that the income-inference channel is economically important in explaining the borrowing responses to credit-limit extensions.

A change in consumer expectations after the experiment indicates that consumers infer information from credit-limit extensions, however, this finding does not provide direct evidence on whether expectation changes are consistent with Bayesian learning. Credit-limit changes can affect consumer beliefs for two possible reasons. First, credit supply is partly forward-looking and is correlated with household earnings in the future. Consumers with uncertain income processes learn about future income perspectives from credit extensions and change expectations accordingly. Second, credit supply is uncorrelated with future income growth; nonetheless, consumers with various behavioral biases believe credit extensions signal higher future income growth anyway.

One test to distinguish between Bayesian learning and over-optimism is to compare credit supply with realized future income. Specifically, for credit-limit changes to be signals of future income, they should be correlated with consumer ex-post income changes. The intensive-margin variations of the limit changes offer an excellent opportunity to study the cross-sectional relationship between credit-limit supply and consumer future income growth, as the amounts of proposed limit increases are based on the bank's regular underwriting process.

Consistent with credit limit being a signal of consumer future income, I find a \$1 higher credit limit is associated with \$0.309 higher realized income over a six-month horizon. However, the sensitivity of income expectations to limit extension is much larger. Given that a \$1 increase in the credit limit increases income expectations by around \$ 0.40, consumers tend to become over-optimistic about their future earnings after credit-expansion shocks. I elicit a subset of the participants' subjective beliefs about the relationship between limit changes and bank beliefs about consumer future income. The results show that consumers greatly overestimate how much limit extensions signal bank beliefs about consumers' future income growth. Specifically, the average consumer believes that each dollar higher offered credit limit is associated with 0.86 dollars higher total income over the next six months from banks' perceptive. Therefore, the finding is consistent with consumers Bayesian learning about their future income from credit extension but using the wrong model to form the signals.

Over-optimistic beliefs are expected to lead to overspending and overborrowing. The resulting higher leverage increases default risks. Using 60-day delinquency as a default indicator, I find a \$1,000 higher credit limit offered by the bank increases the default rate of the consumers who accept the offers by 0.174 percentage points over the six months after the experiment. With a pre-experiment average default rate of 2.44 percentage points, this is equivalent to a 7.13% increase. After controlling for changes in income expectations, credit-limit extensions have marginally negative and insignificant effects on the default rate. Therefore, limit extensions do not seem to significantly affect default risk beyond inducing over-optimistic income expectations.

I conclude my analysis by providing some external validity based on some survey results in the US. Without an experiment and bank account data, I cannot determine the causal effects of credit expansion on consumers' expectations and spending behaviors through the income-inference channel. However, using the *reported preference* approach that is mostly used to estimate marginal propensity to consume (MPC) out of one-time wealth shocks (Shapiro and Slemrod (2003), Jappelli and Pistaferri (2014), Graziani et al. (2016), Parker and Souleles (2019), Fuster et al. (2020), and Jappelli and Pistaferri (2020), etc.), I show that, with online surveys, consumers in the US also believe that credit limit extensions are associated higher consumer spending and income in the future, but unchanged total saving and default probability. In addition, the average reported elasticity of expected income growth to credit-limit extension is around 0.29 in the US, which is very close to the estimate of around 0.30 in the Chinese data. In sum, the reported preferences are consistent with the revealed actions from the Chinese data, thereby providing supporting evidence for the external validity of the study.

Related Literature This paper mainly contributes to three strands of literature. First, it contributes to the study of borrowing limits and consumption (Zeldes, 1989; Ludvigson, 1999; Gross and Souleles, 2002; Agarwal et al., 2017; Guerrieri and Lorenzoni, 2017; Chava et al., 2020; D'Acunto et al., 2020; Gross et al., 2020; Aydin, 2022). A recent major progress is Aydin (2022), which provides a clean empirical estimation of the marginal propensity to borrow using an RCT in Turkey. Although previous literature relies on the mechanisms of credit limits affecting consumer budget constraints, the effect of credit expansions on consumer spending through changing consumer beliefs is still an open question. The lack of evidence lies in the difficulties of combining an RCT with both observational and expectations data. This paper aims to fill this gap by combining an RCT with bank-account data and high-frequency surveys, facilitating direct testing of the effects of an exogenous shock to credit constraints on consumers' beliefs and how the changes in beliefs affect households' consumption-debt decisions.

This paper also contributes to the rich literature on the MPC out of a one-time wealth transfer (Parker et al., 2013; Fuster et al., 2020; Kueng, 2018; Olafsson and Pagel, 2018; Baugh et al., 2021; Fagereng et al., 2021; Cookson et al., 2022).[2] As shown by Guerrieri and Lorenzoni (2017) and Aydin (2022), the estimates of MPB while holding beliefs fixed provide a lower bound of the MPC out of a one-time wealth transfer in the short run. The income-inference channel of credit expansion changes consumption by changing expectations about consumers' future income in the near future. Therefore, the estimation of MPC to a one-time wealth transfer in the short run based on credit-limit adjustment needs to consider the changes in consumers' beliefs. By directly controlling for the changes in consumers' expectations around a credit-expansion event, this paper identifies an exogenous shock that affects consumption and debt decisions through changing credit limits only, therefore providing a clean estimate of the lower bound of MPC to a one-time wealth shock.

Lastly, this paper contributes to a growing literature that focuses on the role of beliefs in explaining consumers' spending-saving decisions (see DellaVigna (2009) and Benjamin (2019) for a review). For example, Ameriks et al. (2016), Ameriks et al. (2020), and Ameriks et al. (2020) provide recent advances by linking survey evidence to retirement choices. Manski (2004), Ameriks et al. (2020), and Giglio et al. (2021) study the relationship between investor beliefs and stock investment. Bucks and Pence (2008), Bailey et al. (2019), and Kuchler et al. (2022) analyze how beliefs affect mortgage-leverage choices. A related study is Soman and Cheema (2002). With hypothetical scenarios from surveys, Soman and Cheema (2002) report that participants' propensity to spend out of changes in credit limit is larger if they believe credit-limit assignments accurately reflect their future earning potential. This paper builds on this literature by exploring a quantitative survey matched to transaction-level data on consumer spending and borrowing decisions. The quantitative nature of the survey answers and the employment of RCTs provide a novel identification opportunity of the effects of changes in banks' beliefs regarding consumers' financial decisions.

[2]See Attanasio and Weber (2010) and Jappelli and Pistaferri (2010) for a survey before 2010.

1.2 Methodology

Data and Institutional Environment

The data for this study comes from a large commercial bank in China. The bank operates nationally and is among the top 10 commercial banks in the country, as ranked by total assets. In 2019, the bank's total assets amounted to over $1 trillion.

The credit cards considered in this study are very similar to those in other countries. In general, each credit card is assigned a credit limit, and consumers can accumulate balances smaller than this limit every month and use the card as a payment method. Consumers earn different levels of discounts and cashback for purchasing certain types of goods or services, depending on the bank's current promotional strategy. At the end of each billing cycle, a minimum repayment is required on the credit cards (usually 10% of the current outstanding balance). Above this amount, consumers can choose to repay any proportion of the current outstanding balance. Consumers who repay all accumulated balances do not incur any interest costs and enjoy the rewards from the cashback and transaction discounts. For the unpaid amounts, the debt is carried over to the next billing cycle with a daily interest rate of five basis points[3].

Sample Restrictions

Consumers usually have multiple bank accounts. Therefore, single-provider transaction-level datasets raise concerns about the completeness of the data in covering the full extent of consumers' spending and cash savings. To alleviate these concerns, I follow recent work using single-provider transaction-level data (e.g., see Ganong and Noel (2019)) and impose two restrictions on the accounts in the empirical analysis to capture the consumers who are most likely to use the bank as their primary banking institution[4].

First, I include only those consumers in the sample whose bank accounts have at least 15 monthly outflow transactions on average during the sample period. An outflow is any debit from a checking account, including cash withdrawals, electronic payments, or debit card transactions. Imposing this criterion reduces the original sample by approximately 35%. The second restriction is that the consumers' income has to be identified by the bank by observing regular inflows to the checking accounts, which amounts to an additional drop of about 10% in the total observations. [5]

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[3]The daily interest rates on credit cards are five basis points for all the customers in the bank before 2021.

[4]Consumers often have accounts from multiple banks. However, an earlier survey from the bank shows that over 70% of the consumers use accounts only from one bank for their regular daily transactions. In addition, Nelson (2022) shows that, depending on their FICO scores, at least 80% to over 90% of the consumers in the US hold only one primary credit card account.

[5]I focus on the consumers with non-missing income information for the main analysis. In addition, the experiment also selects a small group of individuals whose income information is not observed by the bank. I use this group of individuals for robustness checks. See section IV.H for more details.

Measuring Income and Spending

The transaction-level data allow direct measurement of consumers' income inflows and spending outflows. In terms of income, I follow the steps the bank uses, which identify individual income according to a classification rule of regular inflows. The bank classifies income into three main categories: salary, business cash flows, and financial investment.

Salary is defined as the regular monthly income flow if the consumer declares that they work as an employee. The bank calculates this number in one of two ways. First, if income is paid as a direct deposit from the consumer's employer to this bank, the number is directly labeled as salary in the bank's system. Otherwise, the bank can identify monthly income if the consumer's social security insurance is paid through this bank, which is usually a fixed portion of the consumer's income.[6]

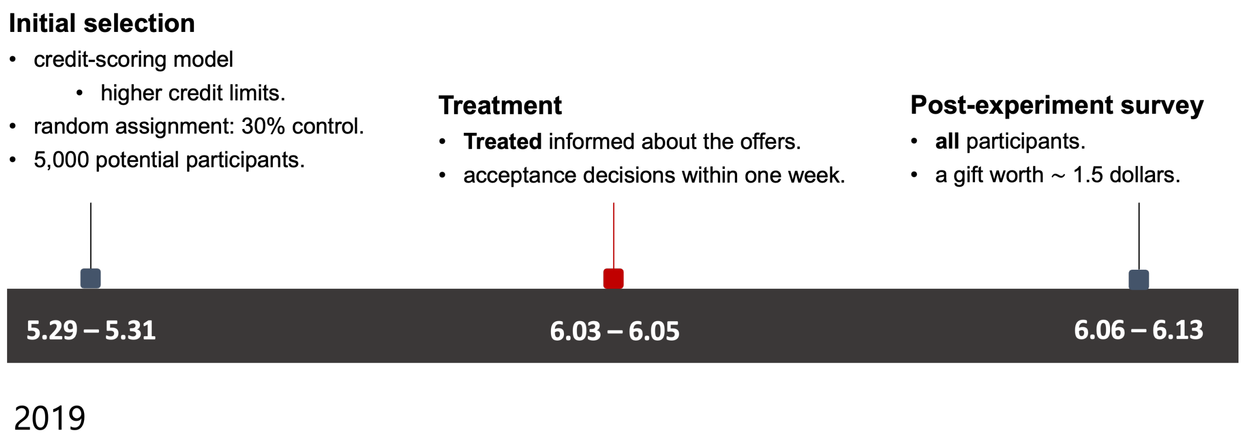
Income from business operations is the difference between total inflow and total outflow when these transactions are categorized as business operations. This category is usually the main source of income for self-employed individuals. By contrast, for income from financial investment, the bank computes it as the difference between the total inflow and the total outflow from an investment account with the financial institutions. When all the incomes in our sample are aggregated, the split of the three components comes out to be 66.74% from salary, 23.37% from business operations, and 9.89% from financial investment.

Debt is the outstanding interest-incurring balance on credit cards. For the measurement of spending, I calculate the consumer's monthly total consumption as the sum of all purchasing transactions. When consumers make purchases from credit cards in the current billing cycle, they can either repay all or a proportion of their accumulated balances. Therefore, total consumption defined in this way also consists of newly accumulated debts that are not repaid at the end of the current billing cycle. In the analyses in this study, I also define non-debt-financed spending as the difference between total consumption and newly accumulated debt. Thus, for the analysis of total spending, cumulative changes in non-debt-financed spending and those in debt are mutually exclusive.

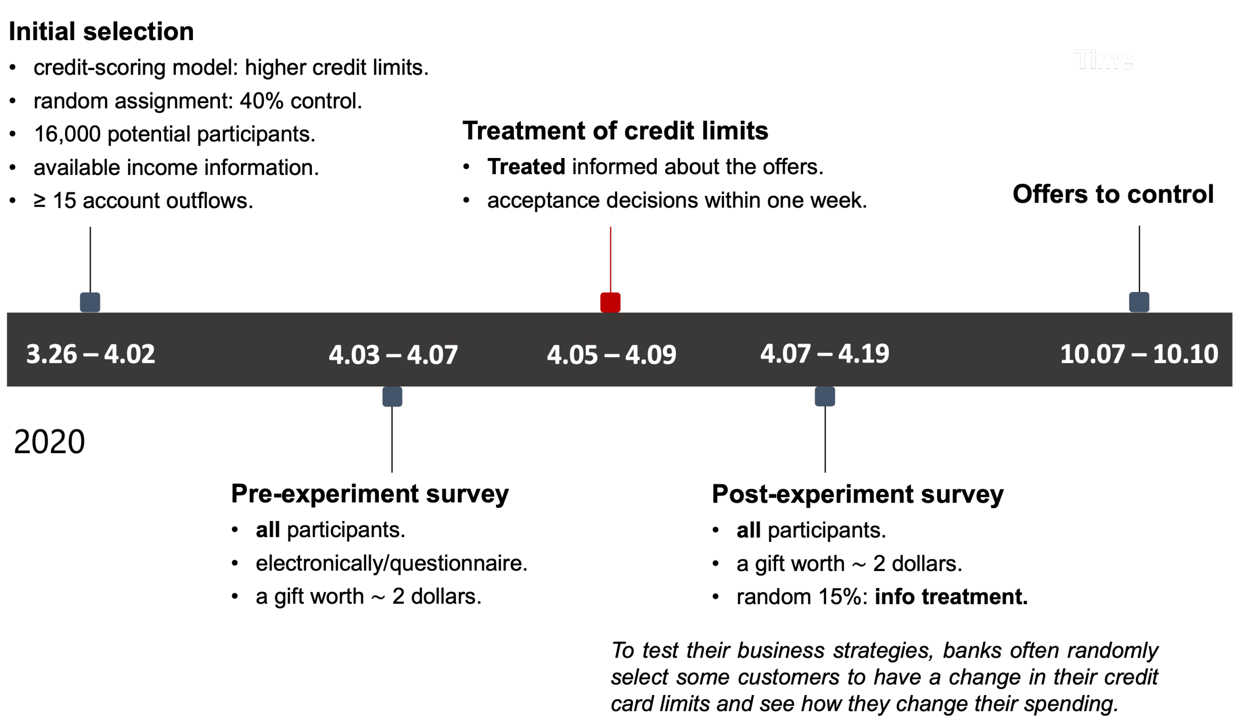
[6] In China, social security payments have six components: five types of insurance and a housing provident fund. The types of insurance are paid with a fixed proportion of workers' monthly income. One such insurance is retirement savings insurance, which is similar to the retirement savings plan in the US. For a monthly income of 5,000 CNY, the monthly contribution is 8%. However, the income base for social security is usually capped at the two tails of the income distribution. The numbers differ for different geographic areas but are usually at 30% and 300% or 40% and 400% of the previous year's average income in that area. Therefore, for those who earn more than 300% of the last year's average income in the area, the total monthly payment is equal to $8\% \times 300\% \times \bar{Y}$, where $\bar{Y}$ is the previous year's average income in the area. However, the uncapped distribution is wide enough to cover most of the workers in China. In the analysis, I remove the consumers in the capped region from the final sample. Removing these customers drops the number of participants in the sample by 9.6%.

Figure 1.1: Timeline of the Experiments

A: Pilot Study



B: Main Study



Experimental Design

Pilot Study

Panel A of Figure 1.1 depicts the experimental procedure of the pilot study. Specifically,

1. At the end of May 2019, the bank selected a group of consumers and decided to offer a new credit limit higher than their current credit card limit, based on the bank's credit-scoring rules. 30% of the consumers were put in a control group, and the rest formed the treatment group. Then a random sample of approximately 5,000 consumers was selected from those two groups of consumers as the potential subjects in this study.

2. Between June 3 to June 5, the participants in the treatment group were informed about the opportunity to increase their credit card limits to the amount offered by the bank. They could accept, reject, or ignore the offer within one week of the extension.

3. Within one week after step 2 (June 6 – June 13), all the participants were given a survey asking about their expectations[7]. At the top of the front page of the survey, the participants were informed that the survey would be used to study the expectations and preferences of representative credit card holders in China and that the information would be used only for scientific research. No one was informed about whether there was an experiment.[8]

In the end, only survey data and consumer demographics, but not financial information, are collected for the pilot study.

Main Study

At the end of March 2020, I collaborated with the bank and conducted a second study involving a larger number of participants. The main motivation for the second experiment is that the pilot study restricts access to information about consumption and borrowing. Therefore I couldn't study the experiment's effects on participants' revealed actions. In addition, based on the findings from the pilot study, I refine the questions to be more aligned with the needs of the study. Panel B of Figure 1.1 depicts the experimental procedure of the main study. Specifically,

1. At the end of March 2020, the bank selected a group of consumers and decided to offer a new credit limit higher than their current credit card limit, based on the bank's credit-scoring rules. About 40% of the consumers were put in a control group, and the rest formed the treatment group. Then a random sample of approximately 16,000

[7]See Appendix for the survey in English.

[8]The survey was designed in a Chinese survey app, and the link to the survey was sent to the participants using WeChat and text messages. After completing the survey, each participant received a gift worth approximately $1.5.

consumers was selected from those two groups of consumers as the potential subjects in this study.

2. From April 3 to April 7[9], all participants were given a survey asking about their expectations. Those completing the surveys received a gift worth approximately $2.

3. Within one week after step 2 (April 5–April 9), the participants in the treatment group were informed about the opportunity to increase their credit card limits to the amount offered by the bank. They could accept, reject, or ignore the offer within one week of the extension. The control group's offers were postponed until the beginning of October 2020.

4. Within one week after step 3 (April 7–April 19), all participants received a survey nearly the same as in step 2, but with some slight changes.[10] A randomly chosen 15% of the participants were shown the following information at the top of the survey:

   1.1 *To test their business strategies, banks often randomly select some people to have a change in their credit card limits and see how they change their spending.*

   After completing the surveys, each participant received a gift worth approximately $2.

The survey data is then merged with consumer bank-account information and transaction-level data.

The settings are similar to that in Aydin (2022). That is, the RCTs temporarily pause the internal underwriting process for a random subset of pre-selected customers for lender-initiated credit limit increases. Within the control and treatment groups, the amounts of limit change are not random and are based on the bank's risk-scoring model. Apart from the surveys, another novelty about this study is that the change in credit limit is offered but not directly extended to these customers here. Therefore, instead of *realized* increases in credit limits, I study the effects of higher *qualified* credit limits by focusing on both those who accept and do not accept the offers.

In the end, for the pilot study, 2,637 individuals completed the surveys. For the main study, for those not shown the information treatment, 4,796 participants completed both surveys. Of those, 3,014 were from the treatment group. I use this group for the main analysis. In addition, 1,793 participants completed the first survey but not the second one. The number of participants who completed the second survey but not the first was 445. The

[9] COVID induced a nationwide lockdown starting at the end of January in China. However, most areas turned to relatively normal conditions in early March. Wuhan was the latest for which the lockdown policy was removed, and the date was April 8, 2020. Therefore, COVID was expected to have a small effect on the study here.

[10] See the survey in Appendix for the changes.

Table 1.1: Summary Statistics

Age is the consumer age immediately before the experimental period. Female is a dummy variable that is 1 if the participant is female. Degree is a categorical variable taking a value from 1 to 5 that labels the participants' highest educational attainment. Hours is the number of hours the subjects usually work in a week. Income and Spending in the top panel is the survey answers to questions 1 and 5 in the pilot study. Saving, Income, Spending, and Debt in the bottom panel are the average saving, annual income, annual spending, and interest-incurring outstanding debt, respectively, over the year before the main experiment. Debt|Debt > 0 gives the summary statistics of Debt conditional on those with positive debt. Limit is the credit card limit at the bank immediately before the experiment. Limit is consumer total credit limit across all banks. p(Offer) is based on survey question 3b before the experiment. Δ Limit is the proposed change in the credit limit. All level variables are converted to dollars and are winsorized at the 1% level.

	Panel A: Control			Panel B: Treatment			Panel C: Differences	
	Pilot Study							
	Mean	SD	N	Mean	SD	N	Diff/SD	*p*-value
Age	39.12	7.80	790	38.56	8.34	1,847	-0.08	0.18
Female	0.51	0.50	790	0.51	0.50	1,847	0.01	0.96
Degree	3.36	1.06	790	3.42	1.13	1,847	0.06	0.20
Hours	44.23	8.39	790	43.65	8.77	1,847	0.07	0.21
Income	20302.11	14781.82	790	19767.23	13638.05	1,847	-0.05	0.31
Spending	13,697.92	20032.79	790	13089.11	19870.52	1,847	0.04	0.51
	Main Study							
	Mean	SD	N	Mean	SD	N	Diff/SD	*p*-value
Age	39.20	9.98	1,782	38.52	9.79	3,014	-0.07	0.24
Female	0.53	0.50	1,782	0.50	0.50	3,014	-0.06	0.27
Degree	3.47	1.21	1,782	3.38	1.26	3,014	-0.07	0.23
Hours	41.03	9.79	1,782	41.65	9.61	3,014	0.07	0.22
Saving	24105.10	47406.69	1,782	22075.96	40857.27	3,014	-0.05	0.41
Income	19405.18	17187.97	1,782	19097.23	14638.05	3,014	-0.02	0.83
Spending	12,837.72	21,810.00	1,782	13,972.80	23645.88	3,014	0.05	0.42
Debt	966.05	1202.55	1,782	979.83	1475.61	3,014	0.01	0.90
Debt|Debt>0	2339.00	1380.68	736	2389.34	1424.64	1,236	0.04	0.46
Limit	13216.16	14161.42	1,782	13131.76	17166.32	3,014	-0.05	0.53
Δ Limit	1859.35	842.33	1,782	1871.33	767.41	3,014	0.02	0.74
p(Offer)	2.29	1.98	535	2.41	2.14	905	0.05	0.48

rest did not complete either of the two surveys. Additionally, of those shown the information treatment, 898 completed both surveys. Besides, I supplement the data with 2,202 consumers who completed both surveys but whose income information is not available at the bank for robustness check.

Summary Statistics

Table 1.1 gives the summary statistics. The top panel summarizes those in the pilot study, and the bottom panel summarizes those in the main study. Panel A summarizes the participants in the control group, and Panel B summarizes those in the treatment group. All level variables are converted to US dollars for ease of comparison with existing literature. Among the given offers, about 70% are accepted. The demographics across the two studies are similar. Across both studies, the average age of the participants is around 38 years old, and around 50% are female.

In the main study, the average outstanding interest-incurring debt is about \$950 and approximately \$2,400, conditional on holding a positive amount of debt before the experiment. A simple calculation indicates around 40% of the individuals in the sample hold positive credit card debt. This proportion is at the lower bound of the range of 40% to 80% found in the previous literature using US data (Gross and Souleles, 2002; Zinman, 2009; Fulford, 2015). The average increase in credit limit is around \$1,850. This magnitude is economically significant. It is around 14.5% of the pre-experiment average total credit limit and around 9.5% of the average pre-experiment annual income.

To check the quality of the surveys, I compare the survey answers about past income with the information from the bank's database. Panel B of Figure A.1 from the Appendix presents the binned scatter plot of consumers' average monthly incomes over the six months before the experiment from the survey and that from the bank's database. The plot shows a clear linear relationship. A regression between the two measures gives an R^2 of 0.79. This finding indicates the high quality of the survey, and also suggests that consumers have a good understanding of the definition of income. Another requirement for the randomization to be effective is that the characteristics between the treatment and control groups are observationally indistinguishable before the experiment. As shown in Table 1.1, the consumers' characteristics are extremely similar between both groups. The differences are all within 10% of the sample standard deviation, with large p-values.

1.3 Results

Pilot Study

I first present some results from the pilot study. Specifically, I study the effects of qualified limit increases on consumer beliefs about their future perspective of various components of consumer budget constraints. The baseline specification is an instrumental variable (IV) regression with the following specification:

$$\begin{aligned} \Delta Limit_i &= \alpha_0 + \beta_0 Z_i + \gamma_0 X_i + e_i, \\ \mathbf{Y_i} &= \alpha_1 + \beta_1 \Delta \widehat{Limit}_i + \gamma_1 X_i + \epsilon_i. \end{aligned} \tag{1.1}$$

$\mathbf{Y_i} \in \{E_C[C_i], E_C[Y_i], E_C[W_i], E_C[D_i], E_C[U_i]\}$, where the elements are respectively the participants' expected level of total consumption and income over the six months after the

Table 1.2: Credit Extension and Expectations – Pilot Study

This table assesses the effects of credit expansion on consumer expectations. The sample is based on a pilot study in June 2019. The left-hand-side variables of the five columns are respectively the participants' expected level of total consumption and income over the six months after the experiment, the expected level of saving six months after the experiment, and the expected probability of delinquency and unemployment over the six months after the experiment. All variables are winsorized at 1% level.

	E_C[C] (1)	E_C[Y] (2)	E_C[W] (3)	E_C[D] (4)	E_C[U] (5)
$\Delta Limit$	0.366***	0.311***	0.017	0.018	-0.053
	(0.103)	(0.088)	(0.062)	(0.079)	(0.101)
First-Stage F	765.11	765.11	765.11	765.11	765.11
N	2,637	2,637	2,637	2,637	2,637

Standard Errors Clustered at City level in Parentheses
* $p<0.10$ ** $p<0.05$ *** $p<0.01$

experiment, the expected level of saving six months after the experiment, the expected probability of delinquency and unemployment over the six months after the experiment.[11]

In the first-stage regression, Z_i is the treatment status and is equal to 1 if individual i is in the treatment group. $\Delta Limit_i$ refers to the changes in the credit limit the participants see on their offers. It is positive for those in the treatment group and zero for those in the control group. Note that $\Delta Limit_i$ is equal to the realized changes in credit limit only for those who accept the offers. For those who do not accept the offer, $\Delta Limit_i$ is still positive, but the realized changes in credit limit are zero. Therefore, instead of realized credit limit, $\Delta Limit_i$ is closely tied with *news* in qualified credit supply. X_i are the province fixed effects.[12] The coefficient of interest is β_1, which measures the average causal response of consumers' expectations to the credit limits offered to the consumers.

The first-stage of (1.1) measures the average differences in the offered limit increases between the control and treatment groups. Therefore, after residualizing by X_i, β_1 is equivalent to the ratio between the difference-in-difference measure of the changes in income expectations scaled by the average offered increases in credit limits to the treatment group.

It's worth noting that the specification of (1.1) is different from estimating the local average treatment effects (LATE) of the *realized* changes in credit limit. In the setting here, I estimate the average causal response of income expectations to *qualified* increases in credit limits. Since everyone in the treatment group was informed about this news, everyone is the *complier* in the language of the LATE framework.

The results are in Table 1.2. It shows that credit extensions increase expectations about

[11]The questions eliciting these beliefs are respectively Q6, Q2, Q16, Q19, and Q13.

[12]Given the non-stratified randomization, the inclusion of the province fixed effects is not necessary. The inclusion of these fixed effects is for consistency with the analysis when decomposing the total effects of credit extensions. See section III.C for details.

future consumption and income. Specifically, each dollar higher qualified credit-limit increase is associated with 36.6 cents of higher expected spending and 31.1 cents of higher expected income over the next six months. However, there is no significant effects of news in credit extensions on the expectations of future saving, default rate, or unemployment rate. The results indicate that consumers tend to think credit extensions are associated with higher future spending as financed by higher future income but not through higher default frequency, drawing down savings, or reduced labor-income risk.

Income-Inference from Credit Extension

The pilot study suggests that consumers associate higher credit extension with higher consumption financed by higher future income but not other margins of adjustment. Motivated by this finding, around the second experiment, I collect more information associated with consumer income expectations, both before and after the experiments, and at different horizons. In addition, I also combine survey answers with consumer spending and outstanding debt around the experiment. With the combination of the RCT, survey results, and transaction history, I can dissect the effects of credit extension on consumer revealed action. In this sections, I present the results associated with the second experiment. In addition, because of the added possibility of simultaneously looking at expectations and real actions, I use the second experiment as the main study.

I first present some results about consumer income expectations before and after the second experiment. I measure consumer expectations of future income change at the six-month horizon, $E_C[\Delta Y_i]$, using the following survey question:

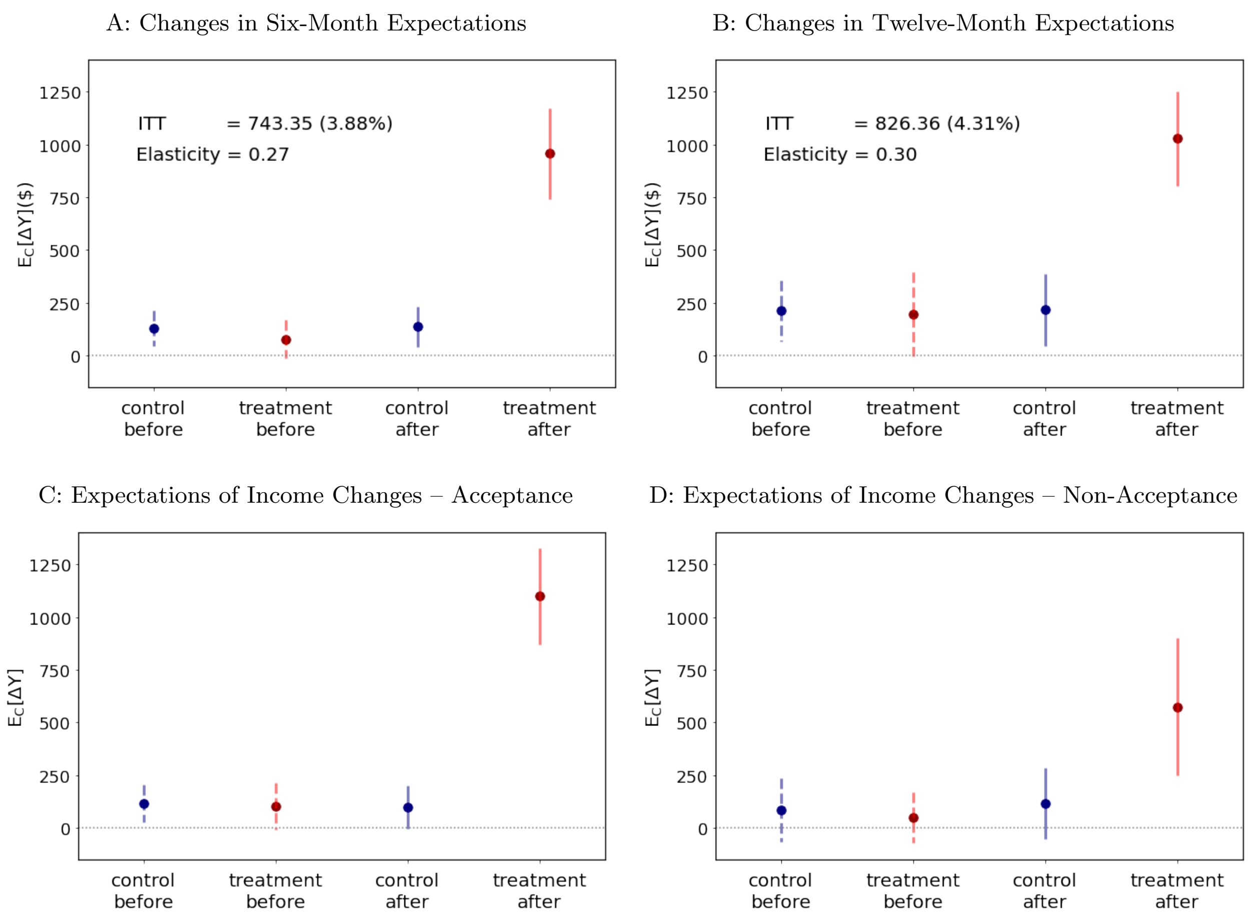
1.1 *Q2: Your expected total income over the next six months is* _____.

$E_C[\Delta Y_i]$ is the difference between the answers to Q2 and consumer average income six months before the experiment.

Panel A of Figure 1.2 gives the scatter plots of consumer income-change expectations before and after the experiment. The red segments represent the answers for the treatment group; the blue segments represent the answers for the control group; the dashed segments represent the answers from the pre-experiment surveys; and the solid segments represent the answers from the post-experiment. As shown by the figure, consumer expectations about future income changes are in general positive before the experiment. After the experiment, there is no significant changes in the expectations of the control group. However, for the treatment group, the expectation about changes in income over the next six months increases substantially. The difference-in-difference (DID) estimate gives the intent-to-treat (ITT) effect of the experiment on consumer expectations, which yields an estimate of $743.35, which is equivalent to a 3.88% increase relative to the average annual income of the participants before the experiment. With an average increase in credit limit of $1,859, the elasticity of income expectation to limit extension is 0.27.

Figure 1.2: Expectations of Income Changes

Panels A and B of this figure respectively gives the averages of participants' expected income changes over the next six and twelve months. Panels C and D give the averages of participants' expected income changes over the next six months, separately for the acceptance and non-acceptance groups. Expectation changes are defined as the differences between the answers to survey questions 2 and 4 of the main study and consumer average income over the six months before the experiment. The dots are the averages and the segments are the averages ± two times the standard errors.



While the main analysis here focuses on the six-month horizon to be consistent with the length of the experimental period, I also present the participants' expectation changes over the next 12 months. The results are shown in Panel B. Interestingly, the updates of total income over the next 12 months are very close to that over the next six months. The elasticity of 12-month income expectation to limit extension is 0.30, which is only about 10% larger than the six-month estimation. The result indicates that consumers believe that credit supply signals higher income in the very near future, or that credit supply shocks contain information about individual income that the consumers are unaware of, thereby causing an immediate update of future income expectation.

Table 1.3: Income-Inference of Credit Extension

This table assesses the effects of credit expansion on the expectation of future income growth. The specification is based on (1.1). $\Delta E_C[Y]$ is the difference between the answer of Q2 of the post-experiment survey and that of the pre-experiment survey. $\Delta Limit$ is the proposed changes in credit limit consumers see on the offers. It is positive for those in the treatment group, and zero for those in the control group. In the treatment group, Acceptance (Non-Acceptance) group contains those who accept (do not accept) the offers. In the control group, the classification of acceptance and non-acceptance is explained in section III. B. All variables are winsorized at the 1% level.

	$\Delta E_C[Y]$ All (1)	$\Delta E_C[Y]$ All (2)	$\Delta E_C[Y]$ Acceptance (3)	$\Delta E_C[Y]$ Acceptance (4)	$\Delta E_C[Y]$ Non-Acceptance (5)	$\Delta E_C[Y]$ Non-Acceptance (6)
ΔLimit	0.412***	0.394***	0.488***	0.472***	0.237***	0.239***
	(0.036)	(0.049)	(0.035)	(0.061)	(0.039)	(0.057)
Province FE	N	Y	N	Y	N	Y
First-Stage F	2398.82	2398.82	1283.55	1283.55	1017.23	1017.23
N	4,796	4,796	3,369	3,369	1,427	1,427

Standard errors clustered at city level in parentheses
* p <0.10 ** p <0.05 *** p <0.01

I continue to estimate the treatment effects of the credit-limit offers on consumers' expectations regarding future income. The specification is the same as (1.1), where the left-hand-side variable in the second stage is, $E_C[\Delta Y_i]$. The results are shown in Table 1.3. The estimated effects of credit-limit offers on consumers' income expectations are both statistically and economically significant: a \$1 higher credit limit the bank offers increases consumers' expectations regarding their future income by around \$0.412 in total over the next six months. These findings show consumers infer future income growth from changes in credit limits and thus posit an income-inference channel through which credit extensions affect consumption. In column (2), I add province fixed effects, and the results hardly change.

A natural follow-up question is how the effects of the credit-limit offers differ for those who accept and do not accept the offers. Because the choice of accepting the offers is not randomized, respectively comparing the changes in the expectations of future income of those two groups with the control group would potentially yield selection biases. To cope with this problem, I split the control group into two sub-groups: those who would and would not accept the offers if they are given the offers. I perform an out-of-sample prediction with a LASSO logistic regression using consumers' pre-experiment characteristics as the predictors. [13] Specifically, I fit the LASSO logistic regression using the treatment group with three-fold cross-validation and then predict who would accept the offers in the control group if they

[13]The predictors include gender, education, age, average income, average saving, average spending, average debt, average hours worked every week, credit score, changes in the credit score, number of credit cards owned, number of credit limit offers received before the experiment, subjective income volatility, city, short- and long-term discount rates, expectation about future income, and bank-proposed changes in the credit limits. All variables are from before the experiment.

had received them. I label those who accept the offers in the treatment group and those who are predicted to accept the offers in the control group as the *acceptance* group; those who do not accept the offers in the treatment group and those who are predicted not to accept the offers in the control group as the *non-acceptance* group.

I check the effectiveness of the model in two ways. First, I randomly split the treatment group into a training sample and a test sample. Then, I fit the model using the training sample to predict who would accept the offer given the same covariates and test the predictive power of the model by checking the error rates of the classifier using the test sample. The results show the LASSO logistic model has a strong predictive ability. Out of a total of 1,479 observations, the LASSO logistic classifier is right in 1,281 cases. This finding implies an out-of-sample error rate of only around 13%.

I continue to provide some suggestive evidence of the experiment's effects on consumer expectations separately for those in the acceptance and non-acceptance groups. The plots are in panels C and D in Figure 1.2. Regardless of being in the acceptance and non-acceptance groups, consumers in the control group have similar expectations about their future income before and after the experiment. For those in the treatment group, consumers in both the acceptance and non-acceptance groups have similar and marginally positive expectations before the experiment. This indicates that the pre-experiment income expectation is unlikely a determinant of if the consumers would accept the offers. After the experiment, both subgroups in the treatment group see a significant change in their expectations. Besides, the expectation changes for those who accept the offers are much larger than the changes for those who do not accept the offers.

To estimate the heterogeneous treatment effects of credit-limit offers on those who accept and do not accept the offers, I re-fit (1.1) separately for the acceptance and non-acceptance groups. The identification assumption is that the prediction errors of the LASSO logistic identifiers are not correlated with the changes in consumers' expectations. This concern is mediated greatly, given that the matching process uses the consumers' financial information as well as their preferences and expectations before the experiment. In addition, the small error rates, as shown in Table A.1, indicate the selection issue is at most trivial. Columns (3) to (6) in Table 1.3 give the results. Consistent with Figure 1.2, the credit-limit offers significantly influence consumers' expectations regarding their future income for both those who accept and do not accept the offers: a $1 increase in the credit limit offered by the bank increases consumers' expectations regarding their future income by around $0.48 ($0.24) over the next six months for those who accept (do not accept) the offers.

Heterogeneity in Income Inference

In this section, I explore the heterogeneity in the income-inference channel. I first study if credit-supply shocks affect consumers' expectations differentially, based on different levels of uncertainty. Presumably, if consumers infer information from the credit supply through Bayesian learning, the income-inference channel should be stronger for those with relatively more uncertain prior. Ideally, the heterogeneity analysis should be based on the signal-to-noise ratio of the learning process. However, because the survey design doesn't provide consumers' subjective uncertainty about the signals, I instead study the heterogeneity based on comparing consumers' subjective uncertainty about their future income growth before to the experiment. The measure of subjective uncertainty in income growth is based on the surveys. Specifically, each participant is asked the following question:

1.1 *Q3: With a probability of 80%, your total income over the next six months will be between _____ and _____.*

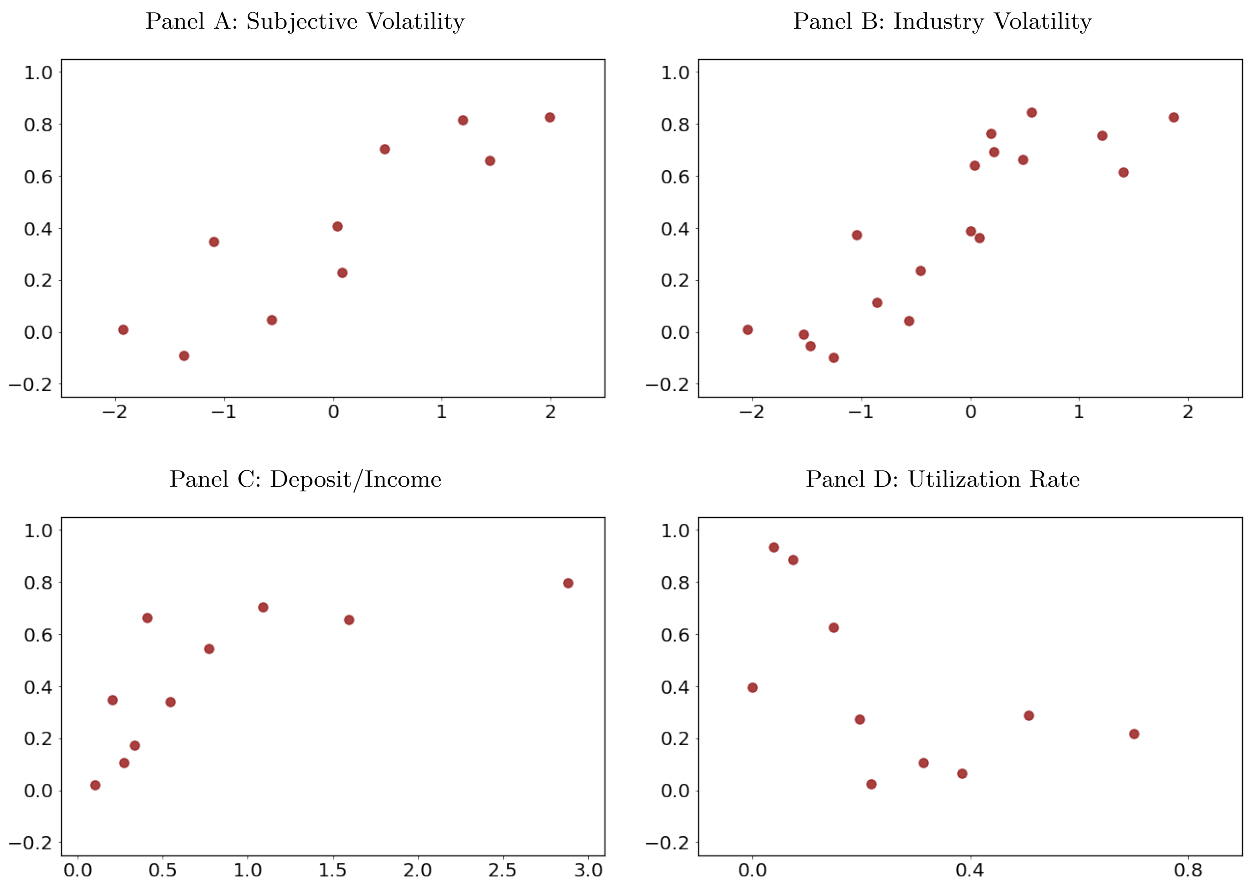
Given the answers to this question and the answers from Q2, I calculate consumers' subjective income-growth uncertainty as the standard deviation from a normal distribution, assuming consumers' log income growth is normally distributed. This measure approximates the uncertainty in consumers' prior about future income growth when learning from the bank's actions.

To explore this heterogeneity, I first split the participants into deciles based on their pre-experiment subjective uncertainty, and then fit (1.1) respectively for the 10 subjective-uncertainty groups. Panel A of Figure 1.3 plots the 10 coefficients β_1 against the standardized average subjective uncertainty in each decile. As shown by the plot, there is a clear positive relationship between the sensitivity of income change to credit supply and consumer subjective uncertainty. This is consistent with a Bayesian learning framework, in which consumers with more uncertain prior update more with respect to the signal.

Additionally, the sensitivity of income changes to credit extension is likely to be stronger when there is a larger cross-sectional variation in consumer income growth that can be observed by the bank but not the consumers. Specifically, if the bank could observe a larger amount of information on individuals who are similar to a specific consumer, the bank is more likely to extract more high-dimensional information that this consumer is lacking. Based on this logic, I construct a measure to capture the cross-sectional income-growth uncertainty that consumers face. Specifically, I select all of those consumers at the bank who have two years of income data preceding the experiment and group those consumers into 18 industries. Then, I calculate the income-growth rates at the individual level and residualize them by log age, gender, highest degree earned, and log savings. I then take the standard deviation of the residualized income growth at the industry level to form $SD(\Delta \log Income)$. $SD(\Delta \log Income)$ gives a measure of the variability of individual income growth for each industry and serves as a proxy for the cross-sectional uncertainty of consumers' income growth. When $SD(\Delta \log Income)$ is high, the bank is more likely to observe the income-growth rates of some consumers who are similar to the participants. The heterogeneity analysis of income expectation sensitivity to credit supply with respect to $SD(\Delta \log Income)$ is in Panel B of Figure 1.3. Similar to subjective uncertainty, there is a clear positive relationship between the sensitivity of income change to credit supply and industry uncertainty. This supports the conjecture that a consumer would infer more from

Figure 1.3: Sensitivity of Income Expectations to Credit Limit Extensions – Heterogeneity

This figure assesses the effects of credit expansion on the expectation of future income growth by different groups of consumers' characteristics. For each sorting variables x, I first split the participants into n groups by x, and then fit (1.1) respectively for the n groups. I then plot the coefficients β_1 from fitting the n IV regressions (1.1) with the average of x in each decile. The four panels sort the participants respectively by their subjective uncertainty, industry uncertainty, deposit-to-income ratio, and utilization ratio. $n = 10$ for deciles for panels A, C, and D, and $n = 18$ for 18 industries for panel B. Volatilities are standardized.



bank supply when the bank can observe a larger cross-sectional variation in the information that is more relevant to this consumer.

Recent studies document that the effects of credit supply on consumer spending are also large for those with high liquidity buffers (D'Acunto et al., 2020; Aydin, 2022). Explaining the finding with the standard buffer-stock model is hard. I continue to study whether consumers with different levels of liquidity buffers infer different levels of information from credit supply. To do so, I repeat the heterogeneity analysis with the consumer deposit-to-income ratio and utilization ratio. The latter is defined as the ratio of outstanding interest-incurring debt over the total credit limit. The results are in panels C and D of Figure 1.3.

Both plots show a negative relationship between the sensitivity of income change to credit supply and the degree of borrowing constraints. This helps to explain the large consumption responses to credit limit extensions for consumers that are far from their borrowing limits.

There are several potential explanations of the positive relationship between expectation changes and liquidity. The first is based on the behavioral literature focusing on individual overconfidence behaviors. Specifically, previous literature mostly documents a positive relationship between overconfidence and one's social economic class. For example, Bénabou and Tirole (2002) analyzes how overconfidence could induce higher ex-post outcomes by overcoming present bias. On the empirical side, Bhandari and Deaves (2006) and Belmi et al. (2020) document that the degree of overconfidence increases with one's social economic class, including income and education. Given that liquidity, including the wealth-to-income ratio and credit availability, in general increases with income and education, it is expected that the degree of overconfidence also increases with liquidity. Therefore, individuals with higher liquidity will overestimate how much credit extension as a positive signal tells about their income growth, consequently having a larger expectation change after seeing the limit-extension shock.

Alternatively, the positive relationship between expectation changes and liquidity could be the strategic behaviors of the banks. To be more precise, given the same belief about higher consumer future income from the banks, consumers with more liquidity are less likely to incur debt to advance assumption. Given that most of the banks' profits from credit services are from interest income net of default instead of other transaction-related income, the same signal, if accompanied by the same amount of credit extension, would create fewer profits for the banks from consumers with more liquidity. If banks have the same marginal costs of supplying credit limits to all consumers, then given the same income growth expectations, it is optimal for the banks to extend fewer credit limits to high-liquidity consumers. Consequently, consumers with rational expectations correctly infer that the same one-dollar of credit limit extension signals higher income growth expectations from the banks' perspective for consumers with higher liquidity.

Altogether, the results in Figure 1.3 suggest that people with more liquidity but less predictable future income change expectations more in response to positive shocks to borrowing limits.

Spending Responses to Credit-Supply Shocks

The previous sections show that credit-supply shocks have a considerable influence on consumers' expectations regarding their future incomes. However, recent studies have documented that consumers' expectations sometimes have limited effects on their actions (Ameriks et al., 2020; Giglio et al., 2021). To see if consumers indeed act on the changes in their beliefs, I further analyze the effects of credit expansions on consumers' spending and debt-taking behaviors after the experiment. I first show the monthly evolution of spending around the time of the experiment. Note that consumers can increase total consumption through either accumulating more unsecured debt, drawing down savings, or both. [14] For more detailed analysis, I separately study the effects of the news on credit limit extension on interest-incurring credit card debt and non-debt-financed spending. The latter is calculated by summing up all spending financed by drawing down saving, and is equal to total consumption minus total newly accumulated debt. In addition, given that most of the previous literature focus on the effects of credit limit extension on debt instead of total spending (Gross and Souleles, 2002; Agarwal et al., 2017; Gross et al., 2020; Aydin, 2022), separately studying the two ways of spending also help to compare the estimates with the previous literature.

Panels A and B of Figure 1.4 plots the evolution of the changes in debt and non-debt-financed spending around the experiment. I scale the changes by the proposed credit limit changes around the experiment for the participants in the treatment and control groups. Therefore the magnitudes give an interpretation of in terms of marginal propensity. The x-axis denotes the number of months away from the experiment. In each of the two subplots, the solid red line represents the treatment group, and the dotted blue line represents the control group. As shown, the sharp increase in borrowing and spending right after the experiment for those in the treatment group indicates the effectiveness of the experiment. Additionally, borrowing for the control group starts to increase in the seventh month after the experiment, the time when the control group receives the postponed offers. The remaining panels plot the evolution of the debt and non-debt-financed spending separately for those in the acceptance and non-acceptance groups. Similar to panels A and B, a sharp increase can be seen in borrowing and spending right after the experiment for those who are in the treatment group and accept the offer; at the same time, even for those who do not accept the offer, borrowing and spending start to increase in the experimental period. This indicates that credit limit extensions increase total consumption through channels other than relaxing current borrowing constraints.

The empirical strategy for assessing the statistical behavior of consumption after the experiment is the same as (1.1), with changes in interest-incurring outstanding debt and non-debt-financed spending as the left-hand-side variables. Table 1.4 presents the results. Panel A focuses on interest-incurring debt, and panel B focuses on non-debt-financed spending. Column (1) of Panel A gives the marginal effects of the credit limits on total interest-incurring debts using all observations in the treatment and control groups. The estimate gives the marginal propensity to borrow out of news of *qualified* limit changes (MPB^{N}). The statistic is different from the MPB estimated in the previous literature (Gross and Souleles, 2002; Agarwal et al., 2017; Gross et al., 2020; Aydin, 2022), which measures the marginal propensity to borrow out of *realized* changes in credit limit.

Column (1) of Panel A shows that for a \$1 higher qualified credit-supply extension, consumers' debts increase by \$0.128 on average six months after the experiment. Columns

[14] In a one-asset model, consumers only accumulate more debt when having zero liquid wealth. However, in the credit card markets, a sizeable amount of consumers hold both positive interest-incurring credit card debt and positive liquid saving, i.e. the co-holding puzzle (Gross and Souleles, 2002; Telyukova and Wright, 2008; Telyukova, 2013; Gathergood and Olafsson, 2022). Therefore, it is possible that consumers increase spending through both accumulating more debt and drawing down liquid savings.

Figure 1.4: Evolution of Spending and Borrowing

This figure plots the evolution of total interest-incurring debt and cumulative non-debt-financed spending on both sides of the experimental period. In each panel, the x-axis is the number of months away from the experiment. The solid red line shows the evolution of the treatment group, and the blue dotted line shows the evolution of the control group. All lines are vertically shifted so that the value for the control group at time zero is 0.

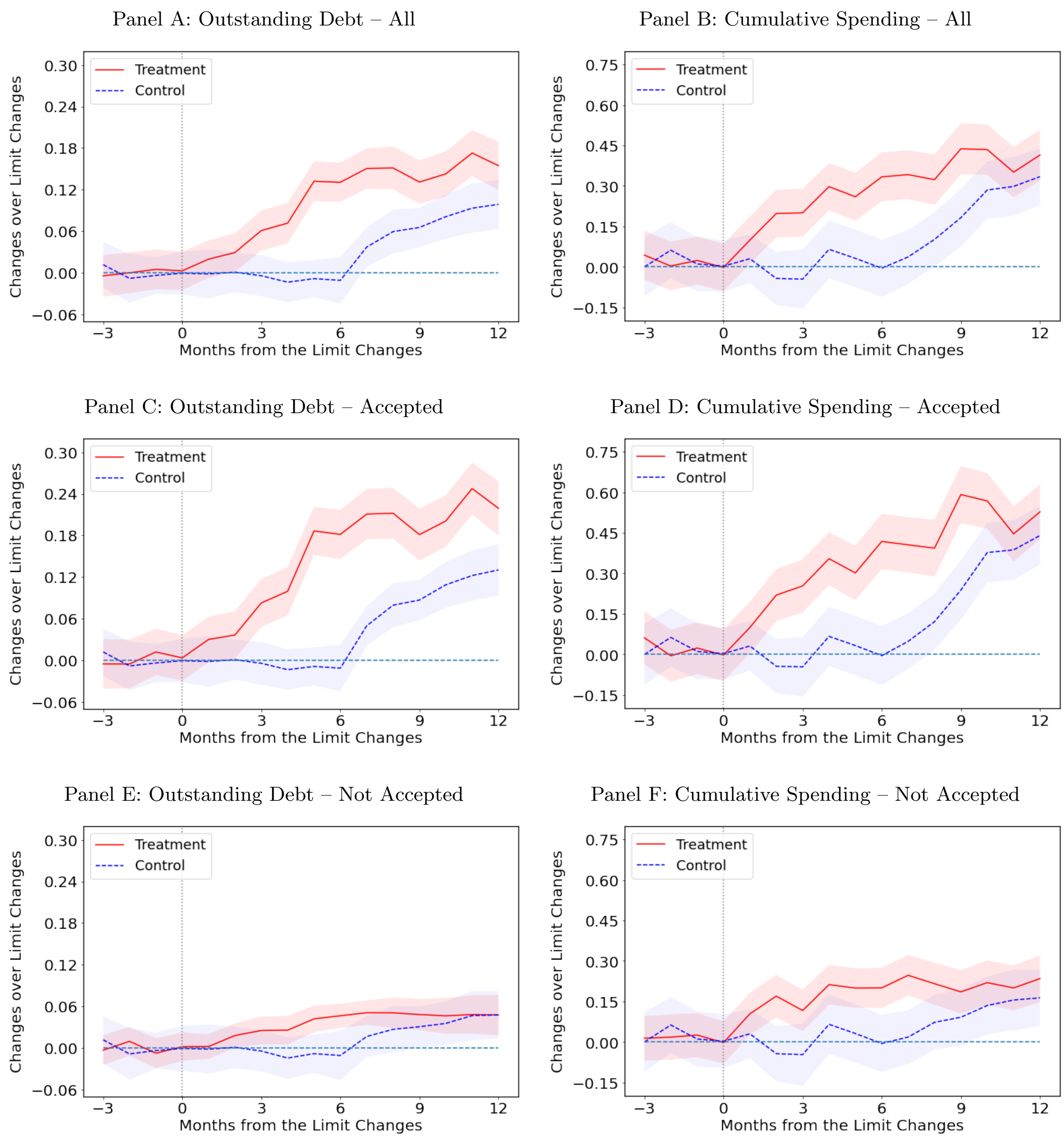


Table 1.4: Spending Responses after Credit-Extension Shocks

This table assesses the effects of credit extensions on borrowing. The specification is based on (1.1) and (1.2). ΔB is the difference between the total outstanding interest-incurring debt six months after the experiment and that at the beginning of the experiment. ΔC_s is the difference between the total spending over the six months after the experiment and that at six months before the experiment minus ΔB. $\Delta E_C[Y]$ is the difference between the answer to Q2 in the post-experiment survey and that in the pre-experiment survey. $\Delta Limit$ is the proposed changes in credit limit consumers see on the offers. It is positive for those in the treatment group, and zero for those in the control group. In the treatment group, Acceptance (Non-Acceptance) group contains those who accept (do not accept) the offers. In the control group, the classification of acceptance and non-acceptance is explained in section III. B. All variables are winsorized at the 1% level.

	(1) All	(2) Acceptance	(3) Non-Acceptance	(4) All	(5) Acceptance	(6) Non-Acceptance
	Panel A: Interest-Incurring Debt (ΔB)					
ΔLimit	0.128***	0.176***	0.043***	0.073***	0.118***	0.011
	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.010)
$\Delta E_C[Y]$				0.125***	0.122***	0.129***
				(0.013)	(0.015)	(0.023)
Province FE	Y	Y	Y	Y	Y	Y
First-Stage F	2398.82	1283.55	1017.23	104.35	72.83	18.99
Overid. p-value				0.14	0.28	0.63
N	4,796	3,369	1,427	4,796	3,369	1,427
	Panel B: Non-Debt-Financed Spending (ΔC_s)					
ΔLimit	0.214***	0.275***	0.104**	0.130***	0.173***	0.040
	(0.028)	(0.032)	(0.051)	(0.033)	(0.037)	(0.068)
$\Delta E_C[Y]$				0.243***	0.209***	0.275**
				(0.076)	(0.051)	(0.116)
Province FE	Y	Y	Y	Y	Y	Y
First-Stage F	2398.82	1283.55	1017.23	104.35	72.83	18.99
Overid. p-value				0.16	0.21	0.24
N	4,796	3,369	1,427	4,796	3,369	1,427

Standard errors clustered at city level in parentheses
* p <0.10 ** p <0.05 *** p <0.01

(2) and (3) give the results respectively for those who accept and do not accept the offers. Column (2) shows that for a \$1 increase in the credit limit, consumers who accept the offer increase their debt by \$0.176 on average six months after the experiment, whereas, as column (3) shows, even for those who do not accept the offer, a \$1 higher qualified credit limit increases their debt by \$0.043.

The estimated magnitude of $\mathrm{MPB}^{\mathrm{N}}$ in column (1) is similar to the MPB found in the previous literature. For example, the MPB is 0.11 at a 12-month horizon in Gross and Souleles (2002), between 0.08 and 0.3 in Agarwal et al. (2017), and approximately 0.16 at a nine-month horizon in Aydin (2022). However, a difference in the setting here is that I have an estimate of the effects of the credit-limit offers on borrowing for all the consumers, regardless of whether the offers are accepted. While in previous studies, analyses are usually based on the changes in credit limits that have to be accepted. In this setting, for those who accept the offer, MPB^{N} is the same as the MPB estimated in the previous literature. On the other hand, for those who do not accept the offer, suppose their borrowing would be weakly higher if they have to accept the offers, but always lower than those who choose to accept the offers. Then MPB under the scenario where credit-limit changes are directly applied to everyone should be a number between the estimate from column (1), \$0.128, and the estimate focusing on those who accept the number, \$0.176.

Panel B of Table 1.4 presents the estimates of the average effects of the proposed limited changes on non-debt-financed spending. The results are qualitatively similar to those for debt. From column (1), the consumers, on average, increase non-debt-financed spending by \$0.214 six months after the experiment. Combined with the estimates from Table 1.4, this finding is equivalent to the total spending of \$0.342 over a six-month horizon. Columns (2) and (3) show the results respectively for those who accept and do not accept the offers. As shown in the columns, for a \$1 increase in the offered credit limit, the consumers who accept (do not accept) the offer on average increase non-debt-financed spending by \$0.275 (\$0.104) six months after the experiment. Combining with the estimates from Panel A gives the MPC out of news in qualified limit change (MPC^{LN}). It measures the changes in total spending for each dollar higher credit limit qualified. From panels A and B, MPC^{LN} for the accepters and non-accepters are respectively around \$0.45 and \$0.15 over a six-month horizon. In addition, the magnitude of MPC^{LN} in columns (1) is very similar to that found in Agarwal et al. (2017). Specifically, for each dollar higher qualified credit limit, total spending increases by around 0.34 over a course of six months. In Agarwal et al. (2017), cumulative purchasing volume increases by between 20 cents to 60 cents over a year for each dollar higher credit card limit, depending on the FICO scores.

Credit-limit shocks have great effects on consumers' total spending. Conventionally, the mechanism is based on the buffer-stock model. Specifically, a relaxed credit limit reduces consumers' precautionary motives, thereby increasing their current consumption. However, this study shows credit-supply shocks also change consumers' expectations regarding their future income. Given the rosier beliefs about their earning ability, consumers increase spending even if their borrowing limit does not vary.

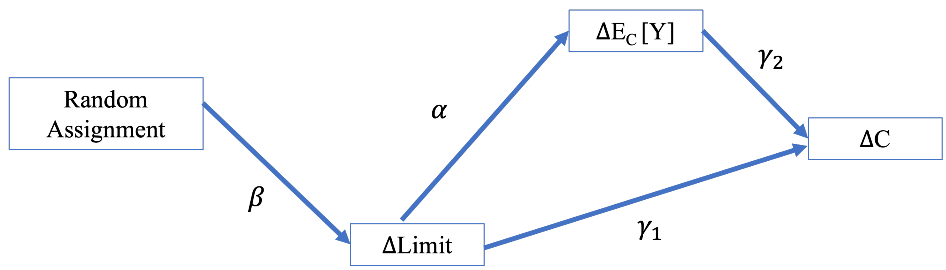
Figure 1.5 illustrate the basic logic of the experiment. That is, the random assignment increases consumers' qualified credit limits, which then affect total consumption through a direct borrowing-constraint channel, and an indirect income-inference channel. The structural equation is

$$\Delta C_i = \gamma_1 \Delta Limit_i + \gamma_2 \Delta E_C[Y_i] + u_i, \tag{1.2}$$

where γ_1 is the marginal propensity to consume out of limit increases, γ_2 is the marginal propensity to consume out of income expectation, and u_i is the structural errors.

Figure 1.5: Decomposition and Income-Inference Channel

This figure illustrates the mechanism of how the experiment is expected to affect total consumption.



A direct test of the effects of the changes in expectations on borrowing is to control for the expectation changes $\Delta E_C[Y_i]$ in (1.2). However, the changes in expectations are also a result of the experiment. It cannot be controlled as an exogenous variable in (1.1). Instead, decomposition requires treating it as a second endogenous variable, and employing more IVs to simultaneously instrument for $\Delta Limit_i$ and $\Delta E_C[Y_i]$. I use two strategies to accomplish this decomposition exercise. The first is a standard location-by-treatment strategy (Kling et al., 2007; Abdulkadiroğlu et al., 2014; Kline and Walters, 2016). Specifically, I use the interaction between the province dummies and the treatment group assignment as the IVs for $\Delta Limit_i$ and $\Delta E_C[Y_i]$, controlling for the province fixed effects.

There are several assumptions required for the validity of the location-by-treatment strategy. First, consumers' degrees of learning, α in Figure 1.5, must vary across provinces. This cross-province heterogeneity generates additional variation in the weights of the income-inference channel that affects consumer spending. This requirement is satisfied if people in different provinces have different beliefs about how well the bank could predict their future income. There are several additional necessary assumptions for exclusion restriction to hold (see Reardon and Raudenbush (2013) for details). However, a stronger sufficient assumption is that, conditional on X_i, γ_1 and γ_2 are homogeneous across the provinces (Hull, 2015; Kirkeboen et al., 2016; Kline and Walters, 2016). This assumption can be examined with the overidentification tests.

Columns (4) to (6) in Table 1.4 present the results respectively for those who accept and do not accept the offers using the province-by-treatment interactions as IVs. The first-stage partial F statistics are large and are respectively 72.83 and 18.99. This observation implies using the province-by-treatment interactions as instruments yields significant independent variations in the two channels. In both columns, a \$1 increase in the expectations of future income over the next six months increases debt by around \$0.125. Conditional on the changes in expectations, the main effects of credit-limit offers are significantly smaller. As column (5) shows, after controlling for $\Delta E_C[Y_i]$, a \$1 higher offered credit limit on average increases the debt of those who accept the offer by \$0.118 six months after the experiment. The estimate decreases by around 32.95%. Column (6) shows that for those who do not accept the offer, after controlling for $\Delta E_C[Y_i]$, the effects of a credit-limit shock on borrowing are no longer significant. This finding suggests the income-inference channel is the only significant channel through which credit-supply shocks increase the borrowing of the consumers who do not accept the offers.

The assumption that γ_1 and γ_2 do not vary at the province level can be tested with the overidentification tests. From Table 1.4, the overidentification tests all have large p-values, indicating the data are consistent with a constant-effects framework. That is, the variation in treatment effects is not coming from heterogeneous effects across provinces, in which, in some provinces, the same degree of changes in expectations triggers a larger change in total debt. Instead, the findings support a dose-response relationship. In provinces where the reaction of expectations is larger (larger dose), the effects on outcomes are larger (larger response).[15]

Similarly, Panel B of Table 1.4 focuses on the effects of a qualified limit increase on non-debt-financed spending separately through the main channel of relaxing borrowing constraints and the indirect income-inference channel. Controlling for the changes in expectations, consumption response to limit extension reduces from 0.275 (0.104) to 0.173 (0.040) for those who accept (do not accept) the offers.

The estimates in front of $\Delta E_C[Y_i]$ show a \$1 increase in the expectations of total income over the next six months raises total non-debt-financed spending by around \$0.24. Combined with debt-financed spending, a \$1 increase in the expectations of total income over the next six months increases total non-debt-financed spending by around \$0.37. If the changes in income expectations are with respect to the permanent component of consumers' lifetime income and no adjustment costs are incurred, the coefficient in front of $\Delta E_C[Y_i]$ in column (3) should be close to 1. Therefore, the estimate, which is smaller than 1 but still economically significant, indicates changes in credit supply serve as a signal of a temporary though persistent shock, to consumer beliefs about their future income[16].

[15] A concern of the running (1.2) is that credit limit extensions change income expectation at a longer horizon, of which the effects on consumption are not completely captured that of expectation changes over a six-month horizon. In Table A.4, I re-fit 1.2 while using the changes in consumer 12-month income expectations as a proxy for the changes in income expectations induced by the experiment. The estimates hardly change, indicating that credit limit extensions affect income expectation mainly by affecting the expectation over the near future.

[16] Given that the spending response seems to level off after six months, the smaller coefficient is unlikely a result of habit formation(Campbell and Cochrane, 1999; Dynan, 2000; Christiano et al., 2005; Havranek et al., 2017).

Table 1.5: Credit Extension and Consumer Behavior – Information Treatment

This table assesses the effects of credit extension on consumer income expectation, borrowing, and spending. The specification is based on (1.1). ΔB is the difference between the total outstanding interest-incurring debt six months after the experiment and that at the beginning of the experiment. ΔC_S is the difference between the total spending minus newly accumulated interest-incurring debt six months after the experiment and that at the beginning of the experiment. $\Delta E_C[Y]$ is the differences between the answer of Q1b of the post-experiment survey and that of the pre-experiment survey. $\Delta Limit$ is the proposed changes in credit limit consumers see on the offers. It is positive for those in the treatment group, and zero for those in the control group. In columns (3) to (6), $\Delta Limit$ and $\Delta E_C[Y]$ are instrumented with the random assignment indicator and the information treatment indicators. All variables are winsorized at 1% level.

	$\Delta E_C[Y]$ Accept (1)	$\Delta E_C[Y]$ Not Accept (2)	ΔB Accept (3)	ΔB Not Accept (4)	ΔC_S Accept (5)	ΔC_S Not Accept (6)
ΔLimit	0.276***	0.097	0.115***	0.017	0.156***	0.076
	(0.055)	(0.085)	(0.008)	(0.010)	(0.050)	(0.076)
$\Delta E_C[Y]$			0.106*	0.153*	0.237*	0.324*
			(0.059)	(0.081)	(0.131)	(0.183)
Controls	Y	Y	Y	Y	Y	Y
First-Stage F	592.17	264.13	17.89	9.33	11.33	7.11
N	653	245	3,989	1,672	3,989	1,672

Standard Errors Clustered at City level in Parentheses
* p <0.10 ** p <0.05 *** p <0.01

As the second strategy, I decompose the two channels with the additional sample receiving the information treatment. Specifically, after augmenting the original sample with the additional sample receiving the information treatment, I instrument the two endogenous variables (1.2) by the treatment assignment dummy and the information treatment dummy. The results are in Table 1.5. Columns (1) and (2) shed light on the first-stage results focusing only on the additional sample with the information treatment. As expected, after receiving the information treatment, expectation changes are about 20 cents smaller than those not receiving the information treatment. Columns (3) to (6) decompose the effects using the information treatment and treatment assignment as the IVs. The results are similar to those in Table 1.4, thereby providing additional supportive evidence of the relative weights of the two channels. However, the first-stage F-statistics are smaller. This indicates the information treatment has relatively weaker relevance, cautioning against the possible problems of weak instruments. Because of this concern, I use the location-by-treatment IVs as my decomposition strategy.

Weights of the Income-Inference Channel

The estimates in Table 1.4 suggest the weight of the income-inference channel in $\mathrm{MPC}^{\mathrm{LN}}$. That is, how much do expectation changes affect total spending when consumers are qualified for one dollar higher borrowing limit? In this case, the consumers, like those in this study, could choose whether to realize the one-dollar higher credit limit or not. Summing up the estimates in column (1) in Table 1.4, total spending for an average consumer increases by around 34.2 cents for each dollar higher offered credit limit. Based on column (4), total spending increases by 20.3 cents after controlling for expectation changes. Therefore the weight for the income-inference channel in $\mathrm{MPC^{LN}}$ is around 1-20.3/34.2 $\approx$ 40.64%.

Another statistic is the weight of the income-inference channel in the MPC out of realized changes in borrowing limit ($\mathrm{MPC^{L}}$). This statistic measures the spending responses when limit offers are forced to be accepted, and is usually the focus of the previous literature (Gross and Souleles, 2002; Agarwal et al., 2017; Chava et al., 2020; D'Acunto et al., 2020; Gross et al., 2020; Aydin, 2022). In this case, the weight of the income-inference channel is the same as that in $\mathrm{MPC^{LN}}$ for the accepters, as there is no difference on these consumers regardless of whether the limit change is offered or applied. Calculating the weight for $\mathrm{MPC^{L}}$ requires the hypothetical value of the total spending when the non-accepters have to accept the offers. To provide a bound of this number, first assume that total spending is always larger for the non-accepters if they had to accept the offers. Then there are two extreme cases. First, if total spending does not change after limit offers are accepted, then the weight of the income-inference channel in $\mathrm{MPC^{L}}$ is the same as that in $\mathrm{MPC^{LN}}$, which is 40.23%. The other extreme is when total spending increases to infinity for the non-accepters when they have to accept the offers. Then the weight of the income-inference channel for these people is zero. Therefore, a lower bound of the weight in $\mathrm{MPC^{L}}$ for an average consumer is the weighted average of that for the accepters and zero, which is around 24.88%. In sum, the weight of the income-inference channel in $\mathrm{MPC^{L}}$ should be a number between 28.55% and 40.64%.

A more straightforward way to calculate the weight of the income-inference channel in $\mathrm{MPC^{L}}$ that requires a much stronger assumption is directly comparing total spending for accepters and non-accepters. If one assumes that the only difference between accepters and non-accepters is the realized change in credit limit, then the weight of the income-inference channel in $\mathrm{MPC^{L}}$ is just 0.147/0.451, which is roughly 32.59%. To sum up, in general, the decomposition exercises suggest that the weight of the income-inference channel in $\mathrm{MPC^{LN}}$ is around 40%, and is between 28.55% and 40.64% for $\mathrm{MPC^{L}}$.

Heterogeneity

The identified effects of credit constraints on spending through the income-inference channel can explain several findings in the previous empirical literature. Many studies find relaxed credit limits have considerable effects on both spending and borrowing for high-liquidity consumers (Gross and Souleles, 2002; Agarwal et al., 2017; D'Acunto et al., 2020; Aydin, 2022). In addition, Gross et al. (2020) document that MPB is likely to be 20% − 30% higher during a recession in addition to the presence of liquidity constraints. These findings are inconsistent with the prediction of buffer-stock models featuring only precautionary motives. I continue to study the effects of higher borrowing limits on spending by liquidity and uncertainty while controlling for the income-inference channel. Doing so, I seek to see if the main effect of relaxed borrowing constraint is consistent with the predictions of the buffer-stock model.

Table 1.6 gives the estimates of MPC^{LN} for the consumers in different uncertainty and liquidity groups. For brevity, I only show results for total spending. In addition, considering the insignificant responses after controlling for changes in expectations for those who don't accept the offers, I focus on those who accept the offers. Panel A splits the sample by participants' within-industry volatility of income growth; Panel B splits the sample by participants' ex-ante subjective uncertainty; Panel C splits the sample by the participants' wealth-to-income ratio right before the experiment; and Panel D splits the sample by the participants' utilization rate before the experiment. The results show income expectations have similar effects on consumer borrowing; a \$1 increase in the expectations of future income over the next six months increases debt by \$0.3 after six months. Given that credit-limit shocks have considerable effects on expectations for those with higher uncertainty, the heterogeneity in MPC^{LN} is weaker across the uncertainty group after controlling for the change in expectations. For example, without conditioning on the changes in expectations, a \$1 increase in the offered credit limit on average increases the total consumption of those who accept the offer and are in the high- (low-) subjective-uncertainty group by \$0.511 (\$0.233) six months after the experiment. After controlling for the changes in expectations, this number decreases to \$0.322 (\$0.194).

As for the heterogeneity by liquidity, because the income-inference channel is stronger for those with higher liquidity buffers, the heterogeneity in MPB is larger across the wealth-to-income group after controlling for the change in expectations. As Panel C shows, unconditional on the changes in expectations, a \$1 increase in the offered credit limit on average increases the total consumption of those who accept the offer and are in the high (low) wealth-to-income group by \$0.259 (\$0.413) six months after the experiment. After controlling for the changes in uncertainty, this number decreases to \$0.088 (\$0.357). The results are similar for utilization rate.

Overreaction to Credit Supply

The previous sections show consumers make significant inferences from credit supply decisions. There are two possibilities that contribute to this finding. First, with rich cross-sectional variations, banks can extract additional information about consumers' earnings ability in the future, which then impacts banks' credit supply decisions. Consumers rationally incorporate such information from credit supply. Second, credit extensions are completely uncorrelated with future income growth. Nonetheless, over-optimistic consumers believe credit extensions signal higher future income growth anyway. In this section, I study which mechanism is more likely in the data by comparing consumer post-experiment expectations and their realized income changes.

Table 1.6: Spending Responses after Credit-Extension Shocks – Heterogeneity

This table assesses the effects of credit extensions on borrowing by consumer characteristics before the experiment, focusing on the participants in the acceptance group. The specification is based on (1.1) and (1.2). ΔC is the difference between the total spending over the six months after the experiment and that at six months before the experiment. $\Delta E_C[Y]$ is the differences between the answer of Q2 of the post-experiment survey and that of the pre-experiment survey. $\Delta Limit$ is the proposed changes in credit limit consumers see on the offers. It is positive for those in the treatment group, and zero for those in the control group. The four panels splits the consumers into high and low groups based on their pre-experiment subjective beliefs of income growth uncertainty, within-industry standard deviation of income growth, wealth-to-income, and utilization rate, respectively. In the treatment group, Acceptance group contains those who accept the offers. In the control group, the classification of acceptance is explained in section III. B. All variables are winsorized at 1% level.

	ΔC Low (1)	ΔC High (2)	ΔC Low (3)	ΔC High (4)
	Panel A: Industry-Level Volatility			
ΔLimit	0.233***	0.511***	0.194***	0.322***
	(0.043)	(0.030)	(0.048)	(0.055)
ΔE_C[Y]			0.301***	0.307***
			(0.081)	(0.053)
First-Stage F	1618.68	504.67	35.01	47.41
Overid. p-value			0.25	0.11
N	1,893	1,476	1,893	1,476
	Panel B: Subjective Volatility			
ΔLimit	0.235***	0.456***	0.180***	0.306***
	(0.036)	(0.049)	(0.042)	(0.061)
ΔE_C[Y]			0.321***	0.270***
			(0.049)	(0.089)
First-Stage F	2021.18	741.79	36.60	45.05
Overid. p-value			0.54	0.11
N	1,684	1,685	1,684	1,685
	Panel C: Wealth/Income			
ΔLimit	0.403***	0.269***	0.357***	0.088*
	(0.048)	(0.041)	(0.046)	(0.049)
ΔE_C[Y]			0.328***	0.290***
			(0.052)	(0.087)
First-Stage F	1056.36	980.63	55.16	45.84
Overid. p-value			0.83	0.07
N	1,684	1,685	1,684	1,685
	Panel D: Balance/Limit			
ΔLimit	0.446***	0.227***	0.377***	0.047
	(0.051)	(0.047)	(0.052)	(0.042)
ΔE_C[Y]			0.335***	0.306***
			(0.059)	(0.101)
First-Stage F	1213.41	643.22	32.16	39.68
Overid. p-value			0.41	0.13
N	1,684	1,685	1,684	1,685

Standard errors clustered at city level in parentheses
* p <0.10 ** p <0.05 *** p <0.01

Figure 1.6: Expectations and Realizations of Income Changes

This figure plots consumer expectations and realized income changes versus the pre-determined limit changes. The x-axis is the limit changes as proposed before the random assignment. the y-axis of the four panels is consumer pre-experiment expected income changes, realized income changes six months around the experiment, post-experiment expected income changes, and expectation errors after the experiment, respectively. expectation errors are defined as the differences between post-experiment expectation and income realization. All variables are residualized by age, degree, gender, income, saving, total spending, industry fixed effects, city fixed effects.

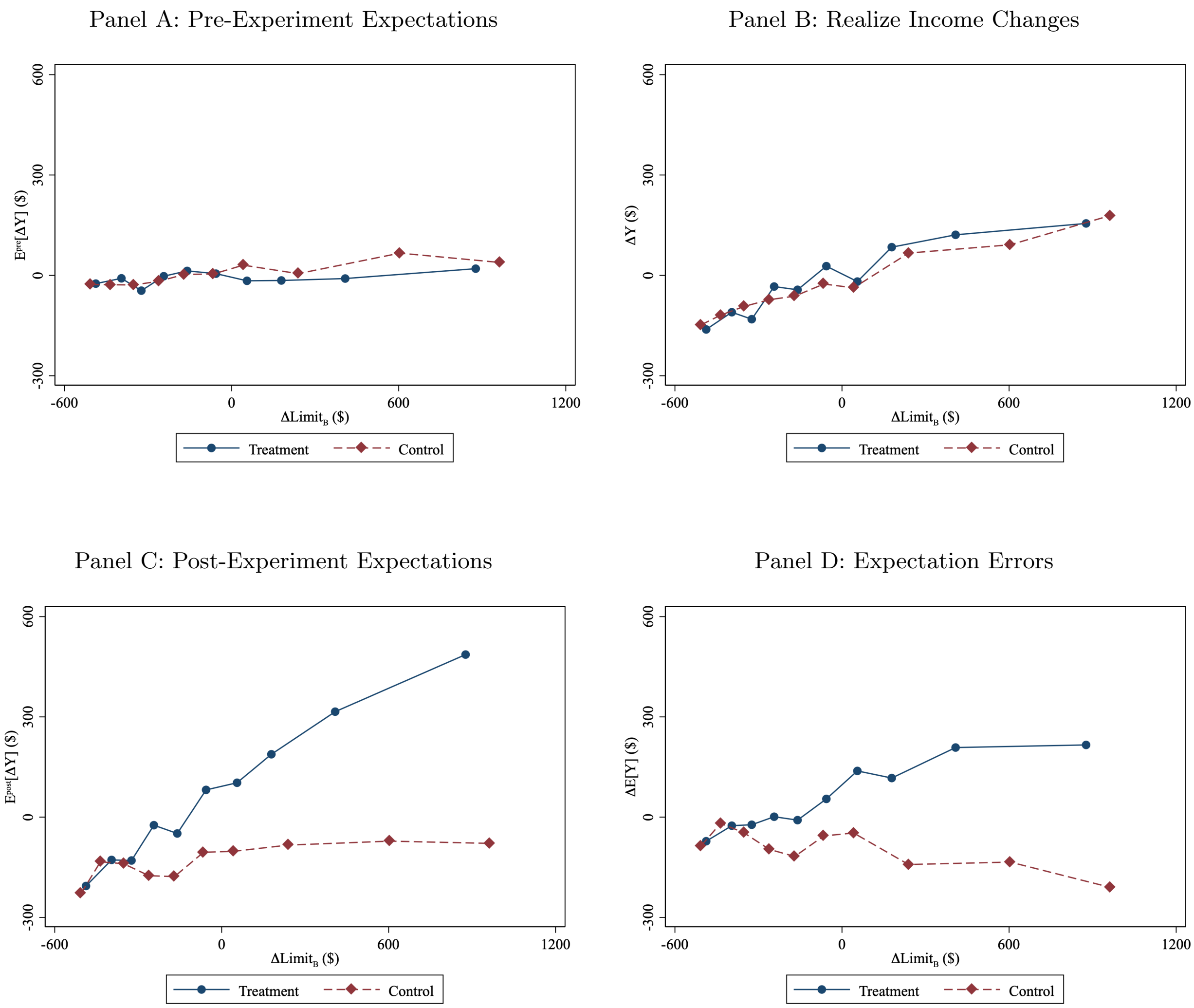


Figure 1.6 gives the binned-scatter plots of consumer income-change expectations and the realized income changes versus the pre-determined limit changes. All variables are residualized by age, degree, gender, income, saving, total spending, industry fixed effects, and city fixed effects. In all four panels, the x-axes are the limit changes as proposed before the random assignment. These numbers are positive for all participants before residualization.

In Panel A, the y-axis is consumer pre-experiment expectations about income changes over the next six months. As shown, pre-experiment expectations are not significantly correlated with proposed limit changes, and this is the case for both the control and treatment groups. From Panel B, realized income changes are positively correlated with proposed limit changes for both control and treatment groups, and the associations are similar for the two groups. Panels A and B indicate that, when banks actively offer to increase the credit limits of the consumers, banks are to some degree informed about consumer income changes in the near future.

Panel C plots consumer post-experiment expectations. Since the control group never receives the offer, there is no change in their expectations. While for the treatment group, there is a positive relationship between expectation changes and proposed limit changes. This finding confirms the previous results. Besides, comparing Panel B and Panel C, an interesting finding is that the association between post-experiment expectations and proposed limit changes is stronger than the association between realized income changes and proposed limit changes. In other words, consumers in the treatment group get over-optimistic about their earnings ability after receiving credit supply shocks. In Panel D, I plot consumer expectation errors after the experiment. Expectation errors are defined as the difference between post-experiment expectations and realized income. Confirming the results in panels B and C, expectations errors are negatively correlated with proposed limit changes for the control group and positively correlated with proposed limit changes for the treatment group.

To further explore the source of over-optimism, I study how consumers think about bank credit supply decisions as a function of consumer future income growth the bank perceives. I rely on the following questions from the survey[17]:

1.1 *Q15: Suppose your bank increases your credit card limit by 5,000 CNY this month. This would mean that the bank expects your total income to change by _____ over the next six months.*

1.1 *Q16: Suppose your bank increases your credit card limit by 10,000 CNY this month. This would mean that the bank expects your total income to be changed by _____ over the next six months.*

Suppose the answers from the two questions are respectively x_1 and x_2, I calculate the consumers' subjective beliefs about the sensitivity of credit supply to the bank-perceived income growth, λ_i, as

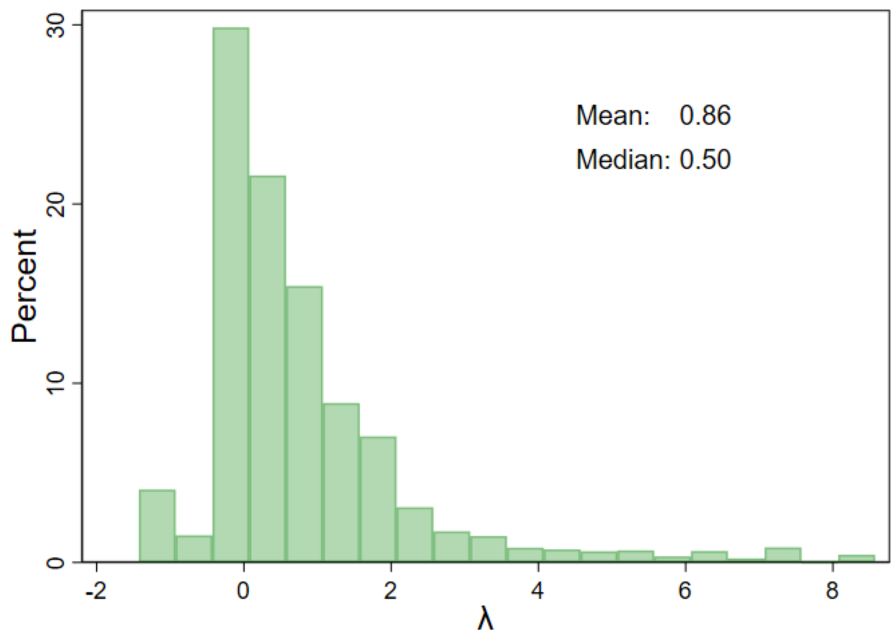
$$\lambda_i = \frac{x_2 - x_1}{10,000 - 5,000}. \tag{1.3}$$

Figure 1.7 plots the distribution of λ_i. It shows a large heterogeneity in consumers' subjective beliefs about the sensitivity of credit supply to bank-perceived income growth. Around 28.73% (6.53%) of the consumers believe $\lambda = 0$ ($\lambda < 0$). However, most of the

[17] These survey questions are sent to a random 30% of the participants.

Figure 1.7: Subjective Sensitivity of Credit Limit Extension and Bank Beliefs

This figure plots the distribution of consumer subjective beliefs about the sensitivity of income growth as perceived by the bank to credit extension, λ, which is calculated using (1.3). The variable is winsorized at the 1% level.



participants believe credit-limit extensions signal a large degree of income growth in the future. The economic significance of λ is large. Its average value is 0.86, and the median is 0.50. Thus, for a \$1 increase in the credit limit, consumers on average believe the bank expects their income to increase by \$0.86 over the next six months[18]. This number is much larger than the relationship between limit changes and realized income change, which is around 0.28 as suggested by Figure 1.6. Therefore, over-optimism is supported by consumers overestimating the importance of bank beliefs about consumer future income growth in deciding on credit supply. From a Bayesian-learning perspective, figures 1.6 and 1.7 suggest that consumers learn about their future income from credit limit extension as a signal that is around 200% larger than the true value, with a Kalman-gain of around 0.4.

Aydin (2022) explains that credit-limit increases do not have informational content for individuals with rational expectations by showing the realized income between those in the control and the treatment groups does not differ after the experiment. However, this finding is necessary as long as the experiment does not change the labor supply[19]. Otherwise, the

[18]The distribution of λ is relatively smooth, indicating the answers are not based on any rules of thumb (e.g., a one-for-one increase or always zero).

[19]There is mixed evidence on how access to higher credit limits affects labor supply. For example, Aydin

experimental design would fail due to a lack of effective randomization. Similarly, as shown in Figure 1.6, I do not find any significant difference in the realized income growth between the control and the treatment groups. But this lack of a finding does not indicate the **expectations** of future income change remain unchanged. When the bank has superior information about consumers' future income growth, credit supply is somewhat more tightly correlated with true future income growth. With effective randomization, this true future income growth, as well as the ex-ante expectation regarding future income growth, is the same for the participants in both the control and the treatment groups. However, consumers' ex-ante expectations are more *wrong* to begin with, possibly because consumers' information set is noisier with respect to the variables that the bank has better information on, at least during the period of active credit expansion. Therefore, as long as the changes in the income perspective do not change labor supply, the realized income growth will stay the same, while the expectations will be corrected to some extent, which would also change consumption and borrowing.

In addition, even if consumer expectations changes after credit extension are due to mistakes in guessing the bank's supply function, the assumption that consumers with rational expectations would learn to correct these mistakes are challengeable. Specifically, because of the many types of shocks to income throughout their lifetime and the sparsity of bank-offered credit-limit extensions, consumers with recency effects would have difficulty associating the lower ex-post income with inaccurate learning from credit-limit extensions. This is especially likely in the consumer credit market. As shown by Agarwal et al. (2013), consumers usually learn about their mistakes slowly in the consumption credit market but forget them quickly. Therefore, it is quite likely for consumers to make persistent mistakes in trying to infer information from credit supply.

Delinquency Rates

Previous literature finds mixed evidence on the effects of relaxed credit constraints on consumers' default probability. For example, in Agarwal et al. (2017), relaxed credit constraints increase the delinquency rate of all borrowers except those with the highest FICO scores. In Aydin (2022), however, a credit-supply shock doesn't have a significant effect on borrower delinquency rates. Here, I provide some additional evidence on the effects of credit-supply shocks on borrower delinquency rates, especially when accounting for the income-inference channel.

Table 1.7 presents the results. I define default as a dummy variable equal to 1 if the participants have a 60-day delinquency over the debt taken during the experimental period, and 0 otherwise. $\Delta Limit_i$ and $\Delta E_C[Y]$ are in thousands of dollars. All coefficients are multiplied by 100 for easier interpretation. As shown in column (1), for all treated participants,

(2022) shows that there are no significant changes in labor supply after being given higher credit limits. While in Herkenhoff et al. (2021), self-employment increases monotonically with increases in credit limits. Theoretically, Guerrieri and Lorenzoni (2017) shows that labor supply should decrease in response to higher credit limits, as consumers have less incentive to supply labor to repay debt.

Table 1.7: Default Probability after Credit-Extension Shocks

This table assesses the effects of credit extensions on consumers' default probability. The specification is based on (1.1) and (1.2). *Default* is equal to 100 if there is a 60-day delinquency for the debt taken over the six months after the experiment, and 0 otherwise. $\Delta E_C[Y]$ is the difference between the answer to Q2 in the post-experiment survey and that in the pre-experiment survey. $\Delta Limit$ is the proposed changes in credit limit consumers see on the offers. It is positive for those in the treatment group, and zero for those in the control group. $\Delta Limit$ and $\Delta E_C[Y]$ are multiplied by 1,000. In the treatment group, Acceptance (Non-Acceptance) group contains those who accept (do not accept) the offers. In the control group, the classification of acceptance and non-acceptance is explained in section III. B. All variables are winsorized at the 1% level.

	Default All (1)	Default Acceptance (2)	Default Non-Acceptance (3)	Default All (4)	Default Acceptance (5)	Default Non-Acceptance (6)
ΔLimit	0.106*	0.174**	0.072	-0.050	-0.071	-0.004
	(0.062)	(0.076)	(0.072)	(0.054)	(0.065)	(0.054)
$\Delta E_C[Y]$				0.209***	0.232**	0.169*
				(0.084)	(0.118)	(0.098)
Province FE	Y	Y	Y	Y	Y	Y
First-Stage F	2403.73	1293.45	1040.27	104.35	72.83	18.99
Overid. p-value				0.21	0.43	0.65
N	4,796	3,369	1,427	4,796	3,369	1,427

Standard errors clustered at city level in parentheses
* $p<0.10$ ** $p<0.05$ *** $p<0.01$

a $1,000 increase in the credit limit increases the default probability by 0.106 percentage points. With a sample average of 2.44%, this increase is equivalent to a 4.34% increase. For the participants in the treatment group who accept (do not accept) the offer, a $1,000 increase in the credit limit increases the default probability by 0.174 (0.072) percentage points. However, the estimated coefficient for those who do not accept the offer is statistically insignificant due to large standard errors. Columns (4) and (5) show updates in the expectations of future income have considerable effects on default rates: for a $1,000 higher expectation about future income, the default rate increases by around 0.2 percentage points. After controlling for the updates in the income-growth expectations, the estimated effects of credit supply on the default rate become insignificant.

The findings of the effects of credit extension on default risk shed light on the discussion of household credit cycles. Specifically, Table 1.7 shows that active credit extension could increase the household credit sector's default risks, but only by inducing over-optimistic income expectations in the future. The findings are therefore consistent with a supply-side driven story of household credit cycles featuring credit-induced sentiment.

Implications from US Data

To shed light on the external validity of the income-inference channel, I present some survey results based on US data collected through SurveyMonkey, an online survey platform.[20] Recently, Bentley et al. (2017) and Haaland et al. (2023) note data from online survey platforms such as SurveyMonkey and MTurk can have high quality when compared with traditional, larger surveys and field experiments.

Without an experiment and bank account data, I cannot determine the causal effects of credit expansion on consumers' expectations and spending behaviors through the income-inference channel in the US. However, I use two separate surveys to show consumers in the US also believe banks' credit-extension decisions signal changes in their future income growth. [21]

In the first survey, I decompose consumer beliefs about the relationships between bank credit supply and the future growth rate of various components in consumers' budget constraints. The questions are

1.1 *Q4: When a bank actively offers to increase the credit card limit of someone, how much do you think the X of this person would change in the next year?* _____.

The above survey questions are asked in four different questions respectively for $X \in \{\text{income, spending, saving, default probability}\}$. The results are plotted in Figure 1.8. It show that, when banks increase a consumer's credit limit, the common belief is that this person would see higher spending and higher income over the next year, but not lower savings or higher defaults. The elasticity of income growth expectation to credit limit growth elicited using the US survey is around 0.29, which is very close to that found in China.[22] These results indicate consumers in the US believe a higher credit limit is associated with higher future spending, and this future spending is financed by higher incomes but not lower savings or higher defaults. Therefore, the online surveys show consumers believe income growth is correlated with bank credit extensions, thus providing supporting evidence for the external validity in the US context.

1.4 Conclusion

This study provides the first set of results about the effects of credit-limit expansions on consumers' beliefs. The combination of bank account data, survey data, and an RCT provides a

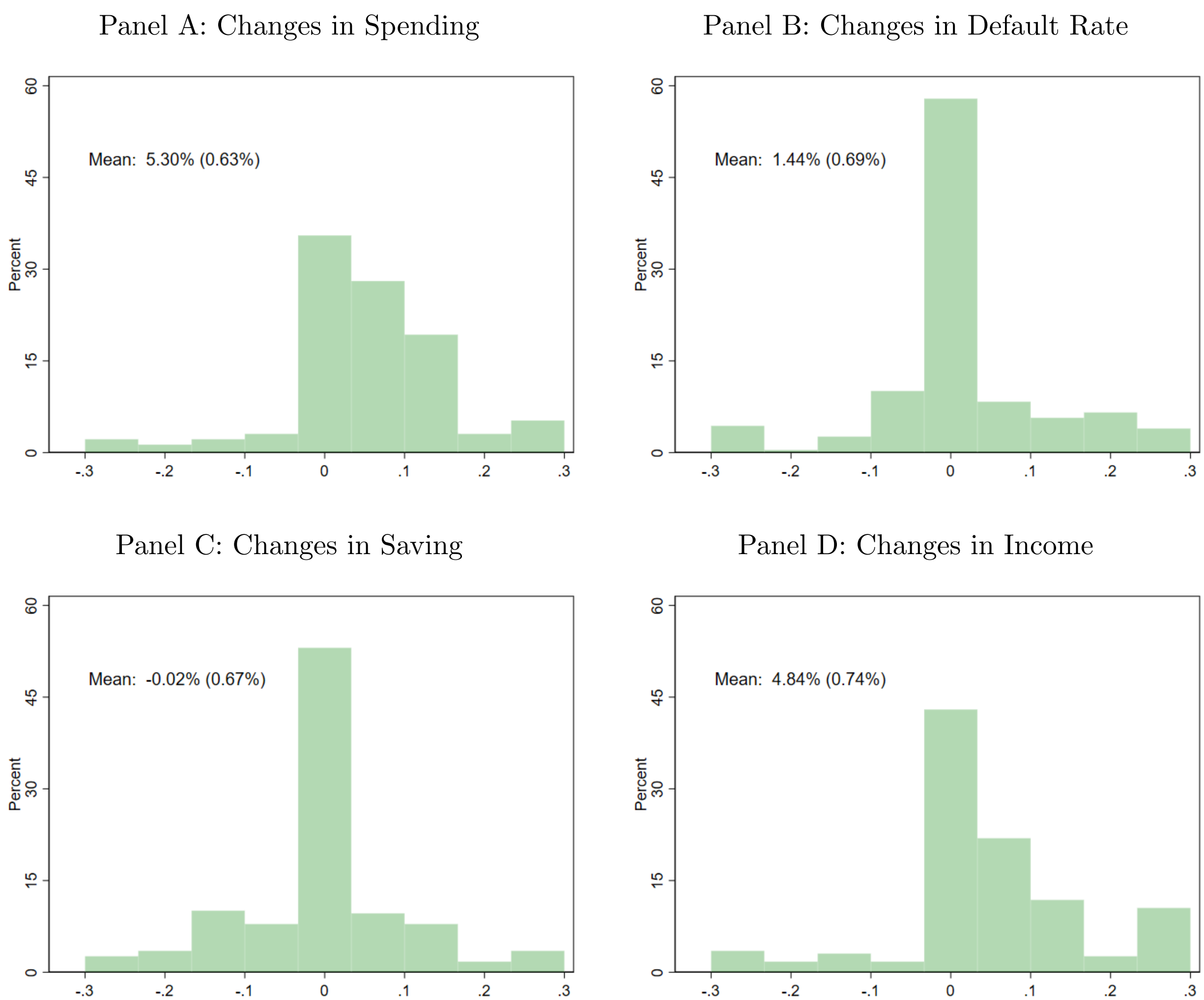
[20]The survey design, results, a brief description of SurveyMonkey, and the collection method are provided in the Online Appendix B.

[21]This strategy is similar to the *reported preference* approach in estimating MPCs. See Shapiro and Slemrod (2003), Jappelli and Pistaferri (2014), Graziani et al. (2016), Parker and Souleles (2019), Coibion et al. (2020), Fuster et al. (2020), and Jappelli and Pistaferri (2020) for some examples.

[22]The elasticity is calculated by 4.84%/(5,000/30,233), where $30,233 is the average credit card limit in 2021 across all credit card holders in Experian, as reported https://www.experian.com/blogs/ask-experian/state-of-credit-cards/here.

Figure 1.8: Reported Effects of Credit Extension

This figure plots the histogram of the answers from US Survey 1 questions 4 to 7. Panel A gives the result of changes in total spending. Panel B shows the result of changes in default probability. Panel C gives the result of changes in total saving. Panel D gives the result of changes in total income. Standard errors are in parentheses.



clean identification of the effects. The results show credit expansions strongly increase consumers' expectations regarding their future income growth. At the same time, consumers become more optimistic about their future income growth, increase their spending and borrowing, and have a higher default rate. The identified income-inference channel accounts for around 35% of $\mathrm{MPC}^{\mathrm{L}}$ and MPB. The findings have important implications for the micro-level mechanism about why consumers have large spending and debt responses to credit-limit extensions. In addition, this study provides an accurate estimation of MPB, $\mathrm{MPC}^{\mathrm{L}}$, and MPC out of one-time wealth shocks, thereby suggesting the effective design of monetary and fiscal policies.

Future analysis could investigate several avenues. First, the study here is based on the credit card market. Future research could explore such mechanism in other financial markets, including insurances, mortgage, business loan, etc. Besides, the experiment involved only credit expansions but not contractions. Consumers' learning from credit supply might involve other behavioral biases that can cause asymmetric responses. For example, suppose consumers have motivated beliefs about their earning potential (Bénabou and Tirole, 2002; Kőszegi, 2006; Zimmermann, 2020). Then they would overweight the income component in banks' credit-supply rules when facing increases in the credit limit but underweight the income component when facing decreases. Therefore, a potential direction for future research is to study the asymmetric responses of consumers' beliefs respective to both credit expansions and contractions.

This paper also sheds light on how we should think about information asymmetry in the credit market. Conventionally, the credit market is characterized by adverse selection from the borrower side. That is, lenders always suffer from an informational disadvantage. However, the findings in this paper show that lenders could extract independent signals about borrowers' future economic activities. This could potentially invert the adverse problems from the borrowers to the lenders. This is closely related to the situation of inverse selection, as recently dubbed by Brunnermeier et al. (2021). What's more, I find that consumers are over-optimistic about how positive signals from lenders could signal about their future income growth. This is to say, even if lenders may only have limited ability to extract independent information about the borrowers, over-optimistic borrowers could still believe that positive lender decisions signal good information about themselves. Therefore, featuring borrowers learning from lender decisions is expected to alter the optimal contract design in the credit market greatly.

Chapter 2

Information Sharing and Credit Conditions

2.1 Introduction

Recent decades have seen a proliferation of AI and big data. This increase is expected to significantly improve banks' statistical models. As a result, banks would expect to receive more precise signals about consumers' future economic activities. In this section, I structurally estimate a life-cycle model incorporating the income-inference channel to study how shocks to banks' beliefs affect their credit extensions and consumers' subsequent spending behaviors, especially when the signals banks receive have different levels of precision.

2.2 Setup

Income Process

The income growth of the consumers follows:

$$\begin{aligned} \log y_{i,t} &= \alpha + z_{i,t} + \epsilon_{i,t} \\ z_{i,t} &= \rho z_{i,t-1} + \nu_{i,t}, \end{aligned} \tag{2.1}$$

where $\epsilon_{i,t}$ and $\nu_{i,t}$ are i.i.d. normal shocks with $\mathbb{E}[e^{\epsilon_{i,t}}] = 1$ and $\mathbb{E}[e^{\nu_{i,t}}] = 1$. The variances of $\epsilon_{i,t}$ and $\nu_{i,t}$ are σ_ϵ^2 and σ_ν^2, respectively. α is the life-cycle component. It is assumed to be a constant and is known to everyone. The consumers do not know the true value of $z_{i,t}$ and need to make inferences based on Bayesian learning. The Kalman-filtering problem with respect to the persistent component of $\log y_{i,t}$ here is a simplified version of Guvenen (2007). At time 0, i's prior of $z_{i,t}$ follows $N\left(z_{i,0}, \sigma_{z,0}^2\right)$. In each period, consumers observe $y_{i,t}$ and update their beliefs accordingly. Therefore, the posterior of $z_{i,t}$ is

$$\begin{aligned}\hat{z}_{0,i,t+1} &= \rho\left[\hat{z}_{i,t} + \frac{\hat{\sigma}^2_{z,t}}{\hat{\sigma}^2_{z,t}+\sigma^2_{\epsilon}}(y_{i,t}-\alpha-\hat{z}_{i,t})\right]\\ \hat{\sigma}^2_{0,z,t+1} &= \sigma^2_{\nu} + \rho^2\frac{\hat{\sigma}^2_{z,t}\sigma^2_{\epsilon}}{\hat{\sigma}^2_{z,t}+\sigma^2_{\epsilon}},\end{aligned} \tag{2.2}$$

where the subscript 0 in $\hat{z}_{0,i,t+1}$ and $\hat{\sigma}^2_{0,z,t}$ captures the posterior before receiving credit shocks. The posterior distribution of income is then $\log y_{0,i,t+1} \sim N(\alpha + \hat{z}_{0,i,t+1}, \hat{\sigma}^2_{0,z,t+1} + \sigma^2_{\epsilon})$.

2.3 Consumer Preferences

Household preferences are similar to those studying consumer credit and default (e.g. Chatterjee et al. (2007) and Livshits et al. (2007)). Consumers at time 0 maximize their expected lifetime utility as

$$U_{i,0} = E_0\left[\sum_{t=0}^{T-1}\delta^t\frac{c_{i,t}^{1-\gamma}}{1-\gamma} + \delta^T\frac{w_{i,T}^{1-\gamma}}{1-\gamma}|I_{i,0}\right],$$

where for each period t, $w_{i,t}$ is the consumers' total saving. The frequency t is set to be one year to be consistent with the average frequency of changes in the credit limit. The budget constraint in each period t is

$$\begin{aligned}w_{i,t+1} &= \begin{cases}(1+r_{i,t})(w_{i,t}-c_{i,t}) + y_{i,t+1} & \text{if } d_{i,t}=0\\ (1-\chi)y_{i,t+1} & \text{if } d_{i,t}=1\end{cases}\\ w_{i,t} &\geq -(1-d_{i,t-1})l_{i,t},\end{aligned} \tag{2.3}$$

where $d_{i,t}$ is a default indicator, $l_{i,t}$ is the credit limit, and $\chi \in [0,1]$ is the marginal rate of garnishment. (2.3) states that when consumers do not default, their wealth in the next period is the sum of their income and gross saving. When the consumers default, their saving becomes zero; at the same time, they need to pay for a garnishment cost equal to χ times their income in the next period. In addition, after defaulting, their credit limit becomes zero, and they can no longer borrow. The interest rate is different for saving and borrowing and takes the value

$$r_{i,t} = \begin{cases}r_b & \text{if } w_{i,t} < 0\\ r_s & \text{if } w_{i,t} \geq 0.\end{cases}$$

2.4 Bank

This economy contains a monopolistic bank. The bank takes interest rates as given.[1] At the beginning of each period, the bank chooses the credit limit for each consumer to maximize the expected profit.

The bank doesn't observe consumers' total savings. Instead, it perceives their total saving at time t as

$$\begin{aligned}\tilde{w}_{i,t} &= \omega \cdot w_{i,t},\\ \omega &\sim N(1, \sigma_\omega^2).\end{aligned}$$

At the beginning of each period t, after the realization of $\log y_{i,t}$ and before consumers make decisions, the bank receives a noisy signal about $\log y_{i,t+1}$. The signal is

$$\log y_{B,i,t+1} = \mathbb{E}[\log y_{i,t+1}] + \xi_{i,t+1},$$

where $\xi_{i,t+1}$ is normally distributed with $\mathbb{E}[e^{\xi_{i,t+1}}] = 1$ and variance σ_ξ^2.[2]

Given the bank's signal and consumers' policy functions after observing the bank's decision, the bank's problem follows:

$$\max_{l_{i,t}} \Pi = \mathbb{E}\left[\kappa \cdot \tilde{c}_{i,t}(l_{i,t}; \tilde{\theta}_{i,t}) + (1 - d_{i,t}(l_{i,t}; \tilde{\theta}_{i,t})) \cdot r_b \cdot b_{i,t}(l_{i,t}; \tilde{\theta}_{i,t}) - \phi_0 \cdot l_{i,t}^{\phi_1} | I_B\right], \tag{2.4}$$

where κ is the income from each dollar of transaction using credit cards. $\tilde{c}$ is the total spending the bank could earn transaction income from. I assume $\tilde{c} = q \cdot c_{i,t} + (1-q) \cdot \min\{c_{i,t}, l_{i,t}\}$ so that, for a proportion q of the time, all transactions are from credit cards. But for the rest of the time, total consumption is bounded by the credit limit, and the excess proportion is transacted using other payment methods for which the bank does not earn income.[3] $\phi_0 \cdot l_{i,t}^{\phi_1}$ is the cost of supplying credit limits of $l_{i,t}$ to the consumers, $\tilde{\theta}_{i,t} = \{\tilde{w}_{i,t}, t, \hat{y}_{i,t+1}\}$ is the collection of consumer state variables from the perspective of the bank, and $\hat{y}_{i,t+1}$ is the consumers' posterior after observing the bank's credit supply.

Optimization yields the supply function

$$\log l_{i,t} = f(\log y_{B,i,t+1}; \theta_{i,t}).$$

I assume $f(\log y_{B,i,t+1}; \theta_{i,t})$ is monotonic in $\log y_{B,i,t+1}$.

[1] During the experimental period, the interest rates of the credit card debt at the bank are five basis points daily for everyone.

[2] A difference between the model and the experiments in Chapter 1 is that in the experiment, consumers could choose to accept the offer or not. However, for simplicity, I assume the agents in the model have to accept the credit-limit changes. When matching the model with the data on the relationship between credit supply and consumers' expectations, I focus on the average treatment effects on the treatment group as a whole, instead of on either those who accept or do not accept the offers.

[3] This functional form is to capture the scenario in which some large transaction is bounded by the credit limit. Consumers in this case would shift from using credit cards to other payment methods. In addition, I use this functional form to match bank total profits from transactions to total spending.

2.5 Learning from Bank Supply

The consumers treat the credit-expansion decision as a signal to $z_{i,t+1}$. From the consumers' perspective,

$$\mathbb{E}_C[\log y_{B,i,t+1}] = \eta \cdot f^{-1}(\log l_{i,t}; \theta_{i,t}) \equiv \eta \cdot g(\log l_{i,t}; \theta_{i,t}), \tag{2.5}$$

where η is a misperception parameter. When $\eta = 1$, consumers have the right perception of the bank's inverse supply function. As a result, $\mathbb{E}_C[\log y_{B,i,t+1}] = \log y_{B,i,t+1}$, and consumers have rational expectations. Then the problem is characterized by a signal-jamming equilibrium *a la* Stein (1989). (2.5) says that when receiving a credit limit of $\log l_{i,t}$, consumers do not know the true belief of the bank; rather, they believe the bank's belief about their income in the next period is η times the bank's true belief. Therefore, η captures all perception errors in the inference process.

After seeing the bank's offer, the consumers Bayesian update their knowledge and form the following belief:

$$\begin{aligned} \hat{z}_{i,t+1} &= \hat{z}_{0,i,t+1} + \frac{\hat{\sigma}^2_{0,z,t+1}}{\hat{\sigma}^2_{0,z,t+1} + \sigma^2_\xi}(\eta \log y_{B,i,t+1} - \mathbb{E}[\log y_{0,i,t+1}]), \\ \hat{\sigma}^2_{z,t+1} &= \frac{\hat{\sigma}^2_{0,z,t+1}\sigma^2_\xi}{\hat{\sigma}^2_{0,z,t+1} + \sigma^2_\xi}. \end{aligned} \tag{2.6}$$

The posterior distribution of the next-period income is then $\log y_{i,t+1} \sim N(\alpha + \hat{z}_{i,t+1}, \hat{\sigma}^2_{z,t+1} + \sigma^2_\epsilon)$.

Consumer Problems

Given an overall state $\theta_{i,t} = (w_{i,t}, t, \hat{y}_{i,t})$, the consumer value function at t is

$$V(\theta_{i,t}) = \max\left\{V_D(\theta_{i,t}), V_N(\theta_{i,t})\right\}. \tag{2.7}$$

The continuation value from defaulting is

$$V_D(\theta_{i,t}) = \max_{c_{i,t}} \frac{c_{i,t}^{1-\gamma}}{1-\gamma} + \delta\, \mathbb{E}[V((1-\chi) \times y_{i,t+1}, 0, t+1)|I_{i,t}], \tag{2.8}$$

and the continuation value from not defaulting is

$$V_N(\theta_{i,t}) = \max_{c_{i,t}} \frac{c_{i,t}^{1-\gamma}}{1-\gamma} + \delta\, \mathbb{E}[V(w_{i,t+1}, l_{i,t+1}, t+1)|I_{i,t}]. \tag{2.9}$$

2.6 Estimation

The estimation consists of two stages. In the first stage, I rely on the experiment to pin down the consumers' discount rates and the parameters associated with the learning process. In addition, I estimate parameters associated with bank profits from spending transactions based on the bank's realized income from the customers. In the second stage, I use the simulated method of moments (SMM) to get the estimates of the consumers' coefficient of risk aversion γ, rate of garnishment χ, ϕ_1, and ϕ_2 in the bank problem.

First Stage: The first-stage estimation requires the experiments and the survey data. A difference here is that the duration of the experimental period is six months, whereas the frequency in the model is nine months to be consistent with the frequency of credit-limit changes. Given that the spending and borrowing responses nearly leveled off six months after the experiments (see Figure 1.4), when estimating the parameters, I assume the spending responses and the expectation changes are the same if the experimental period are over one year.

I first estimate discount rates δ as the average values based on the following survey question:

> *Q4 f: Rather than receiving 100 CNY today, which of the following options would you choose. (select all that apply).*
>
> - X CNY in six months.

$X \in \{95, 97.5, 100, 102.5, 105, 107.5, 110, 112.5, 115\}$. For each participant j, $\delta_j = (100/x)^2$, where x is the smallest choice. I then take the sample average of δ_j as the estimated δ.

For the parameters governing the income process, α, σ_ϵ^2, and σ_ν^2, I residualize all individual income by age, year, education, industry, city, and gender, and estimate (2.1) using maximum likelihood estimation.

To get the error variances of the bank belief σ_ξ^2, I use the following survey questions:

> *Q1 b: Your expected total income over the next 6 months is _____.*
>
> *Q1 c: With a probability of 80%, your total income over the next 6 months will be between _____ and _____.*

Questions Q1 b/c from the pre-experiment and post-experiment surveys give $\mathbb{E}[\log y_{0,i,t+1}]$, $\mathbb{E}[\log \hat{y}_{i,t+1}]$, $\hat{\sigma}^2_{0,y,t+1}$, and $\hat{\sigma}^2_{y,t+1}$. Then σ_ξ^2 can be retrieved based on (6b). For the misperception parameter η, the employed estimation strategy is similar to that in section IV.F. I first use survey questions Q4a and Q4b to get a partial derivative of the consumer's subjective beliefs with respect to bank credit supply, λ_i. I then use the bank's credit model and data before 2020 to predict $\log y_{B,i,t+1}$, which gives me the partial derivative of the bank's belief with respect to the bank's supply: $\frac{\partial \log y_{B,i,t+1}}{\partial \log l_{i,t}} = \tilde{g}'$. Taking the ratio of the two numbers gives η_i, and I use the sample average of η_i as η.

Table 2.1: Estimated Parameters

This table gives the estimated parameters of the structural model. Panel A presents the parameters estimated in the first stage, Panel B gives the parameters estimated in the second stage, and Panel C gives the matched moments.

Panel A: First-Stage Parameters		Panel B: Second-Stage Prameters			Panel C: Matched Moments		
	Estimates (1)		Estimates (2)	S.E. (3)		Data (4)	Model (5)
α	-0.180	γ	2.460	(0.137)	w/c	0.866	0.866
ρ	0.940	χ	0.375	(0.006)	$p(default)$	0.023	0.024
σ_ν^2	0.002	ϕ_0	0.043	(0.001)	l/y	0.650	0.650
σ_ϵ^2	0.088	ϕ_1	0.710	(0.001)	$\partial l/\partial \log y_B$	2.106	2.106
σ_ξ^2	0.077	ϕ_2	0.005	(0.001)	c/l	0.984	0.981
η	3.500						
κ	0.025						
q	0.087						
r_b	0.197						
r_s	0.025						
σ_ω^2	0.384						

κ is directly estimated based on the bank contract. I estimate q to match the proportion of transactions using credit cards given credit limits. For errors in the bank's perception of consumers' total savings, σ_ω, I take the standard deviation of consumer total saving at the bank over the answers from question 3 (e).

Second Stage: I use SMM to estimate γ, χ, σ_ω^2, ϕ_0, and ϕ_1. The five matched moments are the average wealth-consumption ratio, average default rate, average credit limit-income ratio, the sensitivity of credit supply to shocks to the bank's beliefs, and the standard deviation of the income-limit ratio. The logic is explained ahead. The risk-aversion parameter γ captures the curvature of the utility function. Higher risk aversion increases consumer willingness to save, thereby decreasing the wealth-to-consumption ratio. The marginal garnishment cost χ directly affects consumers' willingness to default. A higher χ indicates a higher cost of default, and therefore a lower default rate. ϕ_0 and ϕ_1 together determine the costs of supplying the credit limit. A larger ϕ_0 or ϕ_1 increases the marginal costs of supplying an additional unit of credit limit. In addition, ϕ_1 captures the convexity of the costs of credit supply, and a larger ϕ_1 increases the convexity of the costs. In response to a shock to bank belief, a larger ϕ_1 gives a smaller response to the change in credit supply.

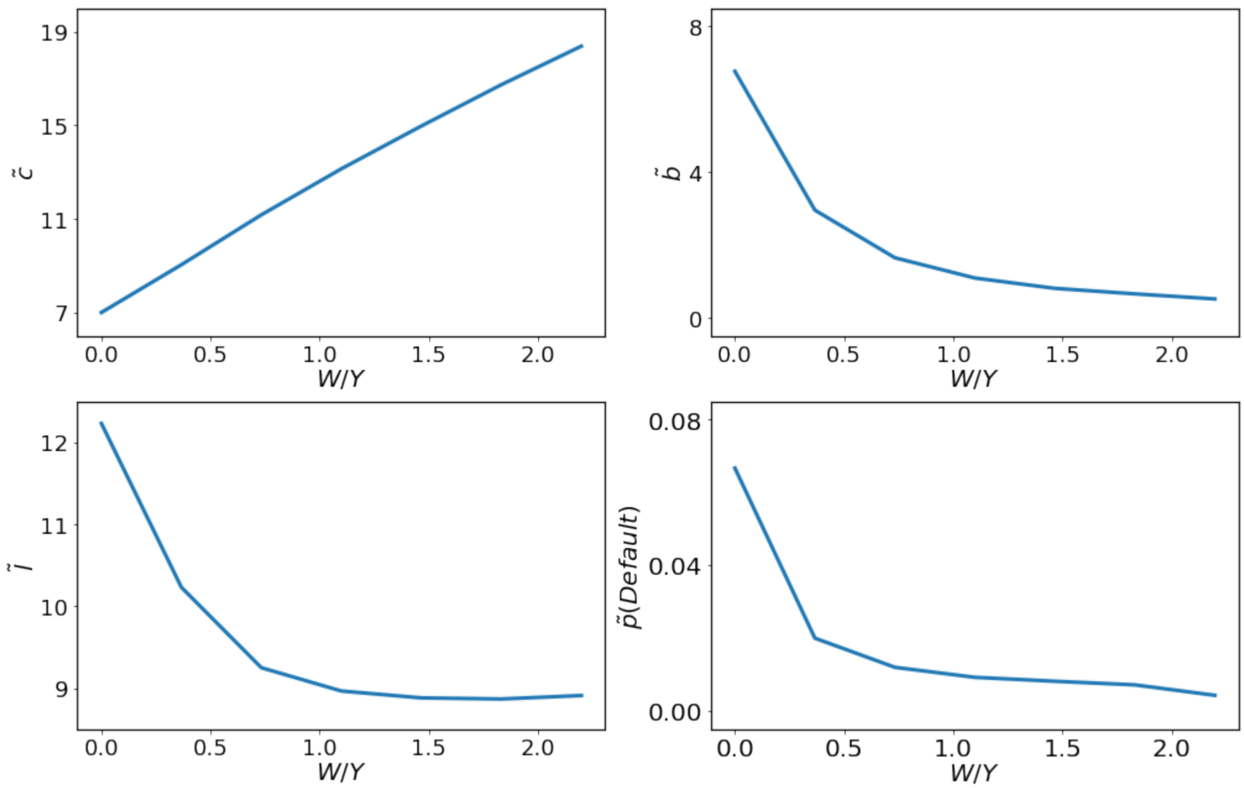
The SMM procedure searches for the set of parameters that minimize the weighted deviation between the actual and simulated moments,

$$(m - \hat{m}(\Theta))'\widehat{W}(m - \hat{m}(\Theta)),$$

where $\hat{W}$ is the variance-covariance matrix of the data moments. The calculation of the

Figure 2.1: Policy Functions

This figure plots policy functions of the simulated consumers as functions of the wealth-to-income ratio at age 38. The top-left panel plots consumption; the top-right panel plots debt; the bottom-left panel plots the credit limit; and the bottom-right panel plots the default probability.



empirical moments is straightforward and is based on the main sample of analysis. The weight matrix $\widehat{W}$ adjusts for the possibility that some moments are more precisely estimated than others. I calculate $\widehat{W}$ as the inverse of the variance-covariance matrix of the empirical moments based on 1,000 bootstrap draws with replacements. For simulated moments, I focus on the average consumer in the data and generate the moments based on the model at time $t = 0$, which is to match age 38 in the data. I fist split each variable into four groups based on the wealth-to-income ratio. The three thresholds are based on the quartiles in the data. I then take the median value in each quartile and take the average to get the simulated moments.

2.7 Results

I study consumer behavior in the model at age 38 so that the age matches that of the average participant in the data. Table 2.1 gives the estimated parameters. Panel A presents the parameters estimated in the first stage. Estimated η is on average 3.5, indicating consumers' perception of the relationship between bank belief and credit supply is around 3.5 times the true relationship. The interest rates are set to the averages in the data, which is 19.7% for borrowing and 2% for saving. $\kappa = 0.025$. Thus, for \$1 of transactions from credit cards, the bank earns \$0.025.

Panel B gives the parameters estimated in the second stage, and Panel C shows the matched moments. As shown in Panel C, SMM is successful in matching the moments. Consumers in the data have a risk aversion of 2.46. The marginal rate of garnishment is 0.375. Therefore, in case of default, consumers begin with zero savings and incur a cost that is equal to 37.5% of their income in the next period. $\phi_1 = 0.71$ implies the cost function of the credit limit is slightly concave.

Figure 2.1 plots the average policy function as a function of the consumer wealth-to-income ratio at age 38. In general, spending increases with the wealth-to-income ratio and debt and default rate decrease with the wealth-to-income ratio. The credit limit also decreases in liquidity, because high liquidity is associated with low borrowing. Even though the default probability decreases with liquidity, the low default rate in general and the high interest rate imply profitability is higher due to the high debt net of default when liquidity is low. Therefore, the credit supply is high when liquidity is low.

2.8 Counterfactual Analysis

For the counterfactual analysis, I first study the credit supply and $\mathrm{MPC}^{\mathrm{L}}$ at age 38 when incorporating the income-inference mechanism. Specifically, I induce a one-time shock of \$666 dollars to bank beliefs about consumer income at age 39 and study the credit supply and spending at age 38. I select the magnitude to match the average changes in a credit limit of around \$1,850 in the experiment. The first row of Panel A in Table 2.2 gives the results. Columns (1) and (2) give the resulting changes in credit-limit spending. A \$666 shock to bank beliefs increases the credit limit by \$1,833, increasing total spending by roughly \$647. This increase implies an $\mathrm{MPC}^{\mathrm{L}}$ of around 0.353.

The estimates in the experiment do not directly provide an empirical counterpart of $\mathrm{MPC}^{\mathrm{L}}$. However, the estimates of $\mathrm{MPC}^{\mathrm{LN}}$ for the accepters and non-accepters provide a range of $\mathrm{MPC}^{\mathrm{L}}$. Suppose that non-accepters' consumption would be larger if they have to accept the offers. In addition, assume that the accepters accept the offers because they want to consume more. Therefore, the lower bound of $\mathrm{MPC}^{\mathrm{L}}$ is the $\mathrm{MPC}^{\mathrm{LN}}$ for the whole sample, and the upper bound is that of the accepters. Therefore, a guess of the range of $\mathrm{MPC}^{\mathrm{L}}$ is 0.343 and 0.451, which contains the estimates of around 0.4 in the previous literature (Agarwal

Table 2.2: Counterfactual Analysis

This table gives the equilibrium condition under different scenarios. Panel A analyzes the weight of the income-inference channel in explaining MPC^L. Panel B analyzes the counterfactual scenario when the precision of the signals the bank receives is larger. Δl and Δc are respectively the changes in credit limit and spending at age 38. l and c are respectively the equilibrium credit limit and spending at age 38. MPC^L is the ratio of the changes in spending to changes in the credit limit. The units of Δl, Δc, l, and c are in thousand dollars.

	Panel A: MPC^L to Bank-Belief Shock		
	Δl	Δc	MPC^L
	(1)	(2)	(3)
Baseline	1.833	0.647	0.353
No Income-Inference	1.833	0.425	0.232
	Panel B: Counter-Factual σ_ξ^2		
	σ_ξ^2	l	c
	(1)	(2)	(3)
Baseline	0.077	12.431	9.876
Hypothetical	0.096	10.677	9.470
Diff. %	25.00%	-14.11%	-4.11%

et al., 2017; Aydin, 2022), and also the 0.353 from the structural estimation. Therefore, the model is successful in generating the average spending responses to limit extensions.

I then consider the case in which consumers' action of learning from the bank is shut down. To do so, I increase the credit limit by the same amount as when there is a shock of \$666 to bank beliefs at age 39. However, I assume the consumers do not update their expectations based on bank action but treat limit changes as exogenous changes. The second row of Panel A in Table 2.2 gives the results. When the income-inference channel is shut down, the same increase in the credit limit increases total spending by roughly \$425. This finding implies an MPC^L of around 0.232. Therefore, when the income-inference channel is shut down, MPC^L decreases by around 34%, roughly matching the range of weights from the empirical estimates.

For the second counterfactual, I study the scenario when the variance of the signals the bank receives decreases by 25% from 0.077 to 0.096. The latter value is similar to the estimated value based on the consumers whose income data are not observed by the bank. This analysis seeks to provide insights into the equilibrium credit supply and consumption when banks have more accurate information-processing technology, and also shed light on a policy that shares consumer income data from the administrative agency to the banks. Panel B of Table 2.2 presents the results. When σ_ξ^2 increases by 25% from 0.077 to 0.096, equilibrium credit supply and spending at age 38 respectively decrease by 14.11% and 4.11%. Therefore, when the advancement of information technology enables banks to extract more precise signals about household future income, credit supply, and spending tend to increase. The elasticity of spending with respect to bank-signal precision is around 0.16.

2.9 Appendix

Households Consumption and Preferences Survey – Pilot Study

Credit cards are an important method for daily consumption. To better understand the impact of credit cards on people's lives, we randomly selected a certain number of active credit card users from our bank to complete this survey. We hope to use this survey to study the consumption behaviors and preferences of the residents generally. Therefore, we will focus only on highly summarized information for scientific research purposes, such as average values. We will not disclose the personal information of the participants in any respect. We will not, in any way, change the types of financial products we provide, including those regarding credit scores, credit limits, deposit rates, etc., based on the participants' personal answers.

1. Your total income over the last six months is ______.
2. Your expected total income over the next six months is ______.
3. With a probability of 80%, your total income in six months will be between ______ and ______.
4. Do you have any form of debt?
   a) no debt
   b) housing
   c) car
   d) business loan
   e) other
5. Your total spending over the last six months is ______.
6. Your expected total spending over the next six months is ______.
7. With a probability of 80%, your total spending over the next months will be between ______ and ______.
8. Why do you use credit cards? (please rank the following options
   a) Convenience
   b) Promotion and Cash Back
   c) Building up Credit Score
   d) Not Enough Income
   e) Other reasons
9. How many hours do you usually work every week?
   a) 3 days or less
   b) 4 or 5 days
   c) 6 or 7 days
10. What's the level of your pressure from work? (1 the lowest, 5 the highest)
    a) 1

b) 2
c) 3
d) 4
e) 5

11. Relative to someone who is similar to you, you think you are working
    a) less
    b) about the same
    c) more
12. Relative to someone who is similar you, you think your level of pressure from working is
    a) lower
    b) about the same
    c) higher
13. Please guess the probability that you will lose your job during the next six months.
14. How often do you calculate your total wealth
    a) 1 week and less
    b) 1 week to 1 month
    c) 1 to 3 months
    d) 3 to 6 months
    e) more than 6 months
15. Banks give everyone a number from 1 to 10 to label their financial risk (the highest the safest). What's the safety score you believe you currently have?
16. Your expected total saving in six months is ______.
17. With a probability of 80%, your total saving in six months will be between ______ and ______.
18. From how many banks do you use their bank accounts for daily transactions?
    a) 1 or less.
    b) 2
    c) 3
    d) 4 or more
19. What's the probability that you would not to be able to repay your credit card debt on time over the credit card debt you take over the next six months?

Households Consumption and Preferences Survey – Main Study[4]

Credit cards are an important method for daily consumption. **To test their business strategies, banks often randomly select some people to have a change in their credit card limits and see how they change their spending**. To better understand the impact of credit cards on people's lives, we randomly selected a certain number of active credit card users from our bank to complete this survey. We hope to use this survey to study the consumption behaviors and preferences of the residents generally. Therefore, we will focus only on highly summarized information for scientific research purposes, such as average values. We will not disclose the personal information of the participants in any respect. We will not, in any way, change the types of financial products we provide, including those regarding credit scores, credit limits, deposit rates, etc., based on the participants' personal answers.

1. Your total income over the past six months is ______.
2. Your expected total income over the next six months is ______.
3. With a probability of 80%, your total income over the next six months will be between ______ and ______.
4. Your expected total income over the next twelve months is ______.
5. With a probability of 80%, your total income over the next twelve months will be between______ and ______.
6. How many hours do you usually work every day? ______ hours
7. How many days do you usually work every week? ______ days
8. How many hours on average will you work every week over the next six months? ______ hours
9. How many days on average will you work every day over the next six months? ______ days
10. How many hours on average will you work every day over the next twelve months? ______ hours
11. How many days on average will you work every week over the next twelve months? ______ days
12. * Why didn't you accept the credit card limit increase offered to you last week? (Ignore if you accepted) [5]
    a) Too busy to accept
    b) Afraid of overspending
    c) Other
13. Your total saving across all banks is ______.
14. Your total saving across all banks in six months will be ______.

The questions below were sent to a random 30% of the participants in the pre-experiment surveys.

[4] The order of the choices in questions labeled with * was randomized. The sentence in bold was randomly sent to 15% of the subjects.

[5] Only included in the post-experiment surveys.

15. Suppose your bank increases your credit card limit by 5,000 Yuan this month. This would mean that the bank expects your total income to be changed by ______ in the next six months.
16. Suppose your bank increases your credit card limit by 10,000 Yuan this month. This would mean that the bank expects your total income to be changed by ______ in the next six months.
17. Suppose your bank increases your credit card limit by 5,000 Yuan this month. This would mean that the bank expects your total income to be changed by ______ in the next 12 months.
18. Suppose your bank increases your credit card limit by 10,000 Yuan this month. This would mean that the bank expects your total income to be changed by ______ in the next 12 months.
19. The bank assigns each customer a credit score to label the relative safeness for granting a loan. What would be the credit score you believe you have at the bank? (Please give a number between 0 and 10, 10 being the safest).
20. Rather than receiving 100 Yuan today, which options would you choose? (select all that apply)
    a) 100 Yuan in six months.
    b) 102.5 Yuan in six months.
    c) 105 Yuan in six months.
    d) 107.5 Yuan in six months.
    e) 110 Yuan in six months.
    f) 112.5 Yuan in six months.
    g) 115 Yuan and more in six months.
21. Rather than receiving 150 Yuan in six months, which of the following options would you choose? (select all that apply)
    a) 150 Yuan in twelve months.
    b) 153 Yuan in twelve months.
    c) 156 Yuan in twelve months.
    d) 159 Yuan in twelve months.
    e) 162 Yuan in twelve months.
    f) 165 Yuan in twelve months.
    g) 168 Yuan and more in twelve months.
22. Suppose there is a game. With a 60% probability, you will win 150 Yuan; with a 40% probability, you will receive 50 Yuan. Which of the following option would you choose over playing this game? (select all that apply)
    a) 70 Yuan for sure.
    b) 75 Yuan for sure.
    c) 80 Yuan for sure.
    d) 85 Yuan for sure.
    e) 90 Yuan for sure.
    f) 95 Yuan for sure.
    g) 100 Yuan for sure.

h) 105 Yuan for sure.
i) 110 Yuan for sure.
j) 115 Yuan for sure.
k) 120 Yuan for sure.

23. * How likely is it for you to get a credit card limit increase from the bank over the next 6 months?

a) Very unlikely
b) Unlikely
c) Somewhat likely
d) Likely
e) Very likely

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