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by

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June 2017 Discussion Paper No. 60

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Fiscal Stimulus and Debt Burden: Evidence from Thailand's First-Car-Buyer Tax Rebate Program^{*}

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This version: April 10, 2019

Abstract: This paper examines the extent to which a fiscal stimulus involving durable goods induces financial distress and causes debt burden. Using account-level loan data to study the impact of Thailand's first-car-buyer tax rebate scheme at both individual and postcode levels, we find that the program led to higher loan delinquency and crowded out other loan originations, which are symptoms implied by excessive debt burden. The adverse impacts were more pronounced for passenger car buyers than for truck buyers and there were local negative spillovers to non-participants. Our findings raise questions about the merit of promoting economic growth by inducing debt-fueled spending and suggest that the design of durable-goods stimulus policy should focus more on productive business durables than on consumer durables.

Keywords: Fiscal Stimulus, Debt Burden, Tax Rebate, Durable Goods, Household Debt, Delinquency, Credit Bureau Data

JEL Classification: D12, E62, E65, H31

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1. Introduction

Durable goods stimulus programs have been used by various governments as a fiscal policy to boost domestic demand boost during economic slowdown. This paper provides empirical evidence of an adverse effect from such program, namely, debt burden of households. The rationale is that the purchase of durable goods is often debt-financed so the stimulus programs may promote debtfueled spending among its participants, potentially resulting in higher delinquency and excessive debt burden. This debt burden could further spread to non-participants as well. Our findings contribute to the literature that questions the merit of durable goods stimulus programs. However, while other studies focus on the subsequent decline in consumption due to the intertemporal substitution effects, our paper provide evidence on an additional mechanism in which the programs could adversely impact the economy.

This paper uses unique account-level loan data from Thailand's National Credit Bureau (NCB) to study the impacts of the country's first-car-buyer tax rebate scheme that was implemented during 2011-2012. In order to understand the impacts on debt at the individual level, we first study how the program affects financial health of its participants. Specifically, we explore how it affects debt-servicing capacity as well as propensity to acquire new loans of its participants. Next we investigate the general equilibrium impacts of the program at a postcode level. Specifically, we study the possible spillovers into nonparticipants who live in the same local economy and may suffer adverse consequences as participants have financial distress. Our findings can shed light on the importance of the effects on household debt which are crucial for policymakers who consider introducing debt-fueled fiscal stimulus in the future.

The first-car-buyer tax rebate program in Thailand was a stimulus program that handed out an excise tax rebate to individuals who were first-time buyers and purchased their first car or truck that fit the specified qualifications during 2011-2012. The size of the program was relatively large in terms of both the number or participants and the rebate size. This makes the program a good case study since it is likely to produce noticeable impacts on both participants and local economies.

We use a difference-in-differences strategy to study the impacts of the first-car-buyer tax rebate program. Based on the loan-level data from the National Credit Bureau, the treatment group consists of first-time car buyers who originated loans for the qualified models during the program period. The control group comprises first-time buyers who also originated loans for the qualified models but did it outside the program period and thus were not eligible for the rebate. The major difference between the two groups is that those in the treatment group benefited from the tax incentive by purchasing the car during the program period, while the control individuals did not. We study the impacts of the program at both individual and postcode levels. At the individual level, we focus our analysis on two sets of outcome variables: delinquency probability and new loan origination propensity. At the postcode level, we construct a variable that measures the postcode's exposure to the first-car-buyer tax rebate program by computing the ratio of individuals in the treatment group to all borrowers for each postcode. In parallel to the analysis at the individual level, we examine two sets of outcome variables: the share of loan outstanding with delinquency status and the quarterly growth of total loan outstanding in each postcode.

Our analysis yields three striking findings. First, the first-car-buyer tax rebate program produced significant effects on the participants' financial health. There were adverse impacts on loan portfolios of the participants—resulting in significantly higher delinquency on both first-car loans and other unsecured loans. One possible explanation of such strong effects on delinquency is that the program encouraged premature vehicle purchases. It induced individuals who would otherwise not purchase a vehicle during the specified period to participate in the program. Many of the purchases were financed by loans and the borrowers might not yet be ready to service the loan without financial hardship. We also provide empirical evidence that these effects are unlikely to be due to other explanations such as the credit supply channel where higher banking competition results in lower loan-underwriting standard and the possible delay of tax rebate.

Second, we find general equilibrium impacts of the first-car-buyer tax rebate scheme: postcodes with higher exposure to the program experienced higher delinquency and lower loan growth. Our further investigation reveals that these general equilibrium impacts were not only from the aggregation of the impacts on the participants but also the spillovers to non-participants living in the same local economy.

Finally, there is a clear distinction between consumer and business durables. While we observed a significant negative impact on new loan originations for buyers of passenger cars, we find no significant effect for buyers of trucks which were likely to be used for business purposes.

This paper has an important policy implication: fiscal policies aimed to stimulate the economy in the immediate term may lead to excessive debt burden and create an adverse consequence in the longer term as higher delinquency, fewer new loan originations, and slower loan growth can cause a decline in future consumption. This raises questions about the merits of promoting economic growth by inducing debt-fueled spending. An insight from our study also offers an implication on the design of durable-goods stimulus policy: if implemented, such policies should focus on productive business durables rather than on consumer durables.

The rest of this paper is as follows. Section 2 discusses related studies and contributions of this paper to the literature. Section 3 describes background on the first-car-buyer tax rebate scheme. Section 4 focuses on the analysis of the program impacts at the individual level. Section 5 turns to the analysis of the program at the local economy level. Finally, Section 6 concludes.

2. Related Literature

Our study is related to four strands of literature. First, the first-car-buyer tax rebate program shared key features with other durable goods stimulus that has been implemented in other countries. For example, Adda and Cooper (2000) study the effects of subsidies on durable goods markets, focusing on scrapping subsidies in France during the 1980s and 1990s. The motivations for the programs were to promote employment and output in the automobile industry as well as to substitute high-pollution cars with less polluting ones. They find that these policies stimulated the automobile industry in the short run but led to subsequent low activities. Following the global financial crisis, several countries implemented similar programs to revive automobile demand. Although the details of these programs varied, most had the aim to remove inefficient vehicles from the road. Examples include the Cash-for-Clunkers program in the U.S. (2009) as well as similar programs in France (2009), Germany (2009) and Japan (2009-10). These programs

involved offering an incentive for individuals to purchase durable goods in a given time frame, effectively moving future demand to the present. Mian and Sufi (2012) find that the U.S. Cashfor-Clunkers program indeed led to additional purchases under the program period but the effect was almost completely reversed only seven months after the program ended. In line with Mian and Sufi (2012), Hoektra, Puller, and West (2017) use a regression discontinuity design and find that the program had no impact on the number of vehicles sold over the 10-month period. They also show that the incentives on fuel-efficient vehicles result in lower total household spending on new vehicles and may undermined the program's short-term stimulus objective. Focusing on the incidence of subsidies associated with the 2009 German car scrappage program, Kaul, Pfeifer, and Witte (2016) find that buyers of expensive cars received relatively large subsidy due to their large price elasticity. In this paper, we extend this literature to study the impacts of the durable goods stimulus on household debt and its potential consequence on debt burden of households in a developing country

Another strand of related literature consists of a number of studies on the effects of income shocks on household debt. For example, Agarwal, Liu, and Souleles (2007) use consumer credit data in the U.S. to study the reaction of consumer spending and debt to tax rebates in 2001. They find that consumers initially saved more by paying down credit card debt but soon afterward increased their spending. Spending increased most for liquidity-constrained consumers while savings rose more for unconstrained consumers. More recently, Agarwal and Qian (2014) study the impact of fiscal policy on consumption, using panel data of consumer financial transactions to study how consumers responded to a positive exogenous unanticipated income shock from the Growth Dividend Program in Singapore in 2011. They find the impacts of fiscal stimulus on household's spending. They also find that spending on credit card increased following the announcement of the program, then decreased after disbursement before increasing again. Finally, they find that the impacts on consumption spending and debt were from liquidity-constrained rather than creditconstrained consumers. Relatedly, Aaronson, Agarwal, and French (2012) study the impact of a minimum wage hike on household debt. They find that households who experienced the hike increased their spending, and the majority of the additional spending was on durables, especially automobiles. Spending on durables and debt were higher and more persistent than what predicted by the permanent income hypothesis, consistent with models with collateral constraints. Our paper

contributes to this literature by studying the impact of first-car-buyer tax rebates on household debt. Not only do we analyze the impact of the program on car loans, but we also explore possible spillovers to other loans in the buyers' portfolio as well as spillovers to loans of non-participants.

Our study is also related to literature on credit expansion and defaults. In an influential paper, Mian and Sufi (2009) document that housing credit led to excessive household debt and mortgage defaults at the onset of the subprime crisis in the U.S. They outline three hypotheses behind this observation: an income-based hypothesis (i.e., income prospect of subprime borrowers may improve in the early 2000s), a supply-based hypothesis (i.e., expansion of credit driven by lenders, probably due to better diversification, subsidized loans or guarantees from government, greater moral hazard from securitization), and an expectation-based hypothesis (i.e., lenders' increased expectations of future house price growth). Using zip code-level data, they argue that the default crisis was consistent to the supply-based hypothesis. Recently, Agarwal and Song (2017) study the impact of housing credit on personal bankruptcy in Singapore. They identify two mechanisms that housing credit expansion would lead to higher defaults: the composition effect (i.e., household's selection into buying more expensive home) and the debt burden effect (i.e., households borrowed more but did not change in housing choice). Linking housing transaction and personal bankruptcy datasets, they argue that an increase in housing credits led to an increase in personal bankruptcy, and the effect was driven by debt burden rather than selection. Our study contributes to this literature by analyzing credit expansion on another type of secured loans, namely, automobile loans. Our findings share similar concerns with the studies above-credit expansion could lead to excessive borrowing and subsequent defaults. We also distinguish between loans for consumer versus business durables.

Finally, this paper joins the growing number of studies that use loan-level or transaction-level data to analyze behaviors and financial health of households. In addition to the studies mentioned earlier, other examples include Mian and Sufi (2015) who use individual-level data from credit bureau and study household debt in the U.S. during 2000-2007. They find that default was substantially larger for individuals with the lowest initial credit scores. Coibion et al. (2016) use household-level debt data over 2000-2012 and show that low-income households in high-inequality areas accumulated less debt relative to their income than low-income households in

lower-inequality regions, and the price of credit is higher and access to credit is harder for lowincome households in high-inequality. Dynan (2012) uses household-level data to examine the impact of debt overhang on consumption and finds that highly leveraged homeowners had larger declines in spending between 2007 and 2009 than other homeowners, despite having smaller changes in net worth. Bernstein (2015) analyzes a transaction-level dataset with comprehensive information on assets, liabilities, and deposits for all customers of a major U.S. financial institution from 2010-2014 and finds that negative equity causes a reduction in household labor supply. Recently, Agarwal, Koo, and Qian (2017) uses a panel dataset of consumer financial transactions in Singapore and shows that, in response to price discount, households not only cut future spending (intertemporal substitution) but also reduce their spending in other categories (inter-temporal substitution).

3. Background on Thailand's First-Car-Buyer Tax Rebate Program

The first-car-buyer tax rebate program in Thailand is a scheme that gives qualified buyers of passenger cars or trucks an excise tax rebate. The program was announced in late August 2011 and went into effect in September 2011. The main objectives of the program as specified by the government were to encourage car ownership among low- and middle-income households and stimulate the economic growth through automobile and related industries. The government also expected the value added tax revenue to partially compensate its cost associated with the excise tax rebate.

The program consisted of an excise tax rebate of 50,000-100,000 baht depending on the car models.¹ The rebate was given as cash and did not require the program participants to have existing tax liabilities. There were several qualifications that each car buyer must satisfy in order to be eligible to participate in the program. First, a participant must be a first-time car buyer. Second, the buyer must purchase one of the qualified models of passenger cars or trucks, as declared by the government. For passenger cars, the qualified models were those under the price limit of one

¹ During the period of our sample, the value of the Thai baht appreciated from approximately 36 baht per U.S. dollar in the early 2009 to below 29 baht per U.S. dollar in April 2013. The currency then steadily depreciated to around 35 baht per U.S. dollar by the end of 2016.

million baht and with engines not larger than 1,500 CC. For trucks, the price limit was also one million baht but there was no restriction on the engine size. Third, the car must be purchased or the order must be placed between September 2011 and December 2012. Finally, the buyer must possess the car for at least five years. It is important to note that the amount of the tax rebate was significant. Specifically, at the time that the program was implemented in 2012, a 100,000-baht rebate was approximately 3,500 U.S. dollars, which was equivalent to 63% of Thailand's per capita GDP. It is therefore possible that the program created significant impacts on financial situations of the participants.

The qualified participants were scheduled to receive their excise tax rebate about one year after possessing the car, although the actual timing varied considerably. It is important to note that the government allowed participants to submit just the order receipt before the end of 2012. This flexibility allowed the program participants to make the actual purchase, and thus originate the car loan, after 2012. In addition, the severe flood that hit the majority of Thailand in the second half of 2011 caused a major interruption in domestic car production that did not resume to the normal level until much later, resulting in a large number of participants receiving their cars in 2013. More specifically, according to the Fiscal Policy Office (FPO), approximately 1.26 million people participated in the program; of these participants, 54.7 percent participated in 2011-2012, while the remaining 45.3 percent participated in 2013. In the end, according to the Excise Department, more than 90 percent of the participants received their cars by the end of 2013. In this study, we therefore consider that the program period was from September 2011 to December 2013.² In total, about 1.25 million individuals participated in the program. To get a sense of the size, the number of total car purchases under the Cash-for-Clunker program in the US in 2009 was just about 680,000. This suggests that the first-car-buyer rebate program likely produced noticeable impacts on the Thai economy.

[Figure 1]

 $^{^{2}}$ Given that we consider the program period that could be longer than what actually took place, if there is any bias in our estimates it should be towards finding no difference between the treatment and the control groups.

Figure 1 illustrates the aggregate sales of passenger cars and commercial vehicles from 2010 to 2014. There is a large jump in the sales during the first-car-buyer tax rebate program. During the peak of the program (April 2012 – March 2013), the monthly average sale of both vehicle types was 65,411 cars. That figure is in sharp contrast to the monthly average sales during the 12-month periods before (October 2010 – Sep 2011) and after (January 2014 – December 2014) the program was implemented, which were 38,124 and 36,713 cars, respectively. The fact that the average sales before and after the program were not very different also suggests that a significant number of the purchases during the program were also pulled forward from well into the future.

[Figure 2]

Figure 2 shows the development of the overall automobile loan market from 2009 to 2015. It gives a consistent picture with the aggregate sales illustrated in Figure 1. There is a large increase in the number of automobile loan accounts during the program period. Interestingly, the share of non-performing loans (NPL share, defined as the amount of loans that were delinquent for at least three months divided by the amount of total outstanding loans) rose significantly starting from when the program took off in 2012. The number of auto loans also noticeably increased at a slower rate after the program ended in 2013.

4. Empirical Analysis at the Individual Level

4.1 Data

We begin our study of the effects of the first-car-buyer tax rebate scheme by analyzing indebtedness at the individual level. The data in this analysis are account-level loan data from Thailand's National Credit Bureau (NCB).³ We start with a stratified random sample that oversamples individuals with car loans and construct a quarterly panel of individuals from

³ For further background on the NCB data and general findings on debt and delinquency from the data, see Chantarat, Lamsam, Samphantharak, and Tangsawasdirat (2017).

December 2009 to March 2016. The sample contains about 100,000 individuals, each with information on loan composition, loan history, and basic demographics.

Although the credit bureau data are rich in various aspects, they do not have explicit information on the participation in the first-car-buyer tax rebate program. To deal with this challenge, we first identify a car loan for each individual in our sample that did not have any car loan prior to that loan origination and assume that this loan was from the first-time car purchase.⁴ Next, given that the program applied to certain car models, we further limit our sample to include only first-time car buyers who purchased car models that satisfied the program's criteria, as described earlier. We also include only individuals who presented in the credit bureau database for at least 5 quarters prior the purchase of their first car. In the end, our sample contains 8,358 individuals who were considered as first-time car buyers (58,134 observations). About 55 percent of the final sample were buyers of passenger cars while the rest purchased trucks.

We then divide our sample into treatment and control groups. The treatment group is defined as those who originated first-car loans between September 2011 and December 2013 since they were likely to participate in the first-car-buyer tax rebate program. In contrast, the control group consists of first-time buyers who originated loans outside the above-mentioned period and were not eligible for the program. Specifically, due to the above requirement that individuals need to be in the credit bureau database for at least 5 quarters prior the purchase of their first car, we define the control group as those who purchased their first cars during April-August 2011 and January-December 2014. There are 5,830 individuals in our treatment group, accounting for approximately 70 percent of the sample.

[Table 1]

Table 1 provides summary statistics of demographic and portfolio characteristics of individuals in the treatment and the control groups in our sample at the time of their first car purchase. Individuals in the treatment groups, on average, were about 41 years old—only two years younger than those

⁴ Given our strategy, our sample misses first-time buyers who purchased car in cash (i.e. no loan origination) and includes non-first car buyers whose earlier purchases were either in cash or with loans originated before December 2009.

in the control group. The degree of urbanization was about 50 percent for both groups. Individuals in the two groups also had quite similar portfolio characteristics. The differences between these two groups in some dimensions were statistically significant but the magnitudes were all very small. This lessens the concern that the distribution of demographic characteristics of new buyers might change once the tax rebate became available

4.2 Empirical Strategy

We use a difference-in-differences strategy to estimate the impacts from the first-car-buyer tax rebate program. However, given that individuals in our treatment and control groups purchased their cars in different periods (quarters), we first identify the quarter when the purchase took place. We then define the quarters after the purchase as post-purchase periods and the quarters before the purchase (as well as the quarter when the purchase took place) as the pre-purchase periods. Given that the impacts of the program could fade away over time, we compare the outcome variables of the 12 quarters around the time of each purchase, six quarters from the pre-purchase and six quarters from the post-purchase periods. Our sample therefore includes individuals who bought qualified passenger car or truck models as their first vehicle between April 2011 and December 2014.⁵

Specifically, for the basic specification we estimate the following regression:

$$y_{it} = \alpha + \beta \cdot treatment_i + \gamma \cdot treatment_i \cdot post_{it} + time FE + individual FE + province_year FE + \varepsilon_{it}$$

where y_{ii} is an outcome variable associated with each period t for each individual i; $post_{it}$ is an indicator variable that equals one for post-purchase periods of individual i and zero otherwise; and $treatment_i$ is an indicator variable that equals one if individual i is in the treatment group and zero otherwise. Again, the treatment group includes first-time buyers who bought qualified

⁵ In a previous version of this paper, we estimated regression coefficients by month (rather than grouping them into pre- and post-periods). The overall conclusions are similar.

passenger car or truck models during the program period (September 2011 to December 2013). The control group includes those who bought the same qualified models but outside the program period (January 2010 to August 2011 and January 2014 to December 2014). We also include time fixed effects and province-year fixed effects to control for macroeconomic conditions that could impact our outcome variables, and individual fixed effects to capture unobserved time-invariant characteristics of each individual.⁶ Robust standard errors are clustered at the cohort group level defined using decades of birth.

4.2.2 Outcome Variables

We examine two sets of outcome variables in this study: (1) probability of loan delinquency for each individual; and (2) probability that each individual originated new loans besides the first car loan.

Delinquency: The first set of outcome variables includes loan delinquency for each individual. This is an indicator variable that is equal to 1 for individuals whose loans were overdue on a payment by at least one month. The first-car-buyer tax rebate program likely incentivized some individuals who would otherwise not purchase cars during the specified period to participate in the program. These individuals would only be qualified for car loans later in the future but were tempted by the tax incentives to purchase the car prematurely. We expect this group of buyers to face higher delinquencies, relative to the control group whose decisions to purchase their first car were not distorted by the tax rebate. Thus, through this premature-purchase effect, the tax rebate program would produce an adverse impact on delinquency.

However, participants in the first car scheme also received tax rebates from the government. In this case, one may interpret that the rebates from the program lowered the effective amount of the

⁶ Although it would be better to include province-quarter fixed effects in the regressions to capture the heterogeneity in local macroeconomic conditions in each period, this was not operationally possible as auto loans are concentrated in large provinces while many small provinces do not have a lot of participants. We perform a robustness check using province-quarter fixed effects but limit our sample to only the top ten largest provinces based on the number of participants:

 $y_{it} = \alpha + \beta \cdot treatment_i + \gamma \cdot treatment_i \cdot post_{it} + time FE + individual FE + province_time FE + \varepsilon_{it}$ The results are shown in Tables A.3 and A.4 in the Appendix.

new auto loan, and hence debt burden for the treatment group, while the amount for the control group was unchanged. Through this tax-rebate effect, we should expect individuals in the treatment group to have lower delinquency than those in the control group. The net effect of the program therefore depends on the relative importance of the premature-purchase and tax-rebate effects. If the premature-purchase effect dominates the tax-rebate effect, it will produce higher delinquency for the program participants.

Finally, the effects on delinquency may not only limit to the buyers' first-car loans, but there could be spillovers to other loans in their loan portfolio as well. We analyze these spillovers by separately estimating the impacts of the program on the delinquency of (1) first-car loans only, (2) unsecured loans, i.e., credit card, personal loan, and overdraft, and (3) other secured loans, i.e., mortgage and hire purchase loan, excluding the first-car loan.

New Loan Origination: The second set of outcome variables includes the probability that each individual originated other new loans besides the first car loan itself. The rationale behind our interest in these variables is that the new auto loans from the first car purchases could substitute or complement other loan originations. A new auto loan undoubtedly results in higher debt burden of the borrower and could crowd out or substitute other loan origination. On the other hand, if the new cars were used in productive activities it is possible that they could help individuals get other loans. This could be either from borrowers' higher income generated by new production activities that justifies lenders to supply more loans to them or from their demand for additional loans necessary for the operation of new businesses. For example, if a car was used by an individual in a new enterprise, this business may also need an overdraft account or take personal loans to further finance its operation. This complementarity effect would lead to an increase in other loan origination probability of the borrowers.

Due to the tax rebate, debt burden from the new car loans would be lower for the treatment than the control, implying less substitution effect for the borrowers participating in the program. In addition, given the magnitude of the rebates, it is also possible that the stimulus helped facilitate entrepreneurship which in turn resulted in more new loan originations, reinforcing the complementarity effect. However, the tax rebates could induce certain individuals to purchase their first car prematurely. The program could push these individuals closer to their already-low debt limit—leading to a larger drop in the origination of other loans. The net effect of the first-car-buyer tax rebate program on new loan origination therefore depends on the degree of substitution and complementarity between the new car loan and other loans.

Finally, to shed light on the adjustment of individual's portfolio choices in response to the program, we estimate the impacts of the program on all new loan origination as well as on new unsecured loans and new secured loans separately. Also, given that passenger cars are likely to be used mainly for consumption purpose while trucks are more likely to be used in business operations, we will analyze the impacts of the first car program on the outcome variables for passenger cars and trucks separately.

[Figure 3]

Figure 3 illustrates the pattern of the outcome variables. Panel 3a shows higher delinquency probability of the first car auto loan for the treatment group as compared to the control groups. Other panels show the variables both before and after the car-purchase decision, which allow us to examine the common trend assumption for the treatment and the control groups. For the delinquency variables (Panels 3b and 3c), the propensity to delinquent for the two groups moves almost parallel during the pre-purchase period. This is consistent with the common trend assumption before the program participation. The patterns for new loan originations (Panels 3d-3f) are similar, although less parallel. Tables A.1 and A.2 in the Appendix show the means of the outcome variables and their differences as a further illustration of the identification strategy. We examine these impacts using a more formal framework later in this section.

4.3 Empirical Results

Delinquency Probability: We begin our analysis by estimating the overall impacts of the first-carbuyer tax rebate program on delinquency probability. Table 2 presents selected estimated coefficients for delinquency regression. For delinquency on the first car loans, we find higher delinquency probability for individuals in the treatment group relative to the control group for both passenger cars and trucks, consistent to the argument that the program encouraged individuals to purchase their first cars prematurely. Furthermore, our results show that the increase in delinquency probability was not limited to the first car only, but there was also evidence of higher delinquency on unsecured loans for both passenger car and (less significantly) truck buyers, implying spillover effects from first-car loans to other loans in the buyers' portfolio. In addition, we find no impact on delinquency of other secured loans, which is intuitive because defaulting on secured loans would involve confiscation of collateral so we expect borrowers to choose to default on unsecured loans first.

[Table 2]

New Loan Origination: Next, we study the impact of the first-car-buyer tax rebate scheme on the probability that individuals would originate other new loans. The results in Table 3 show that overall the probability that individuals in the treatment group would originate new loans following the first car purchase was significantly lower than that of the control group. We further find that the effects were evidently different between the first-time buyers of passenger cars and trucks— the overall impact was driven by passenger car purchases and there was no impact of the program among truck buyers. Given that passenger cars and trucks were likely used in different activities (i.e., mainly as household consumption for passenger cars versus in business production for trucks), our findings imply that the tax rebate program tended to crowd out new loans when the stimulus was on consumer durables and there was no impact on new loans when subsidies were for productive business durable goods. Alternatively speaking, this finding suggests that loans for business durables that have potential to generate additional future cash flows to the borrowers are less subject to debt ceiling than loans for consumer durables.

[Table 3]

In summary, the results in this section show that: (1) the first-car-buyer tax rebate scheme led to higher delinquency propensity on the first-car loans, both for passenger car and truck purchases; (2) there were spillovers of the program on other loans in the portfolio of first-time car buyers, causing higher delinquency probability on unsecured loans, particularly for the passenger car

purchases; and (3) there is a distinction between loans for consumer and business durables—the program's negative impact on new loan originations was driven by passenger car purchases and there was no impact among truck buyers. These findings altogether support the importance of the premature-purchase effect from Thailand's first-car-buyer tax rebate, and suggest that this effect was more pronounced for consumer durable loans.

Discussion: There are concerns with respect to our identification strategy that deserve further discussion. First, it is possible that we may misidentify individuals in the treatment groups—some of them may fit all of our criteria discussed above but did not actually participate in the program while some participants were excluded from the treatment group if they did not take loans for car purchase. However, this potential misidentification is likely to make it less likely to find statistically significant differences in the responses between the treatment and the control groups.

Second, given that the program puts an upper limit on the price of the qualified models, the tax rebate might induce some individuals to switch down from the more expensive models to the qualified ones. However, this shift would likely make the debt burden less severe and dampen the debt-shifting effect, and consequently make it more difficult to generate the adverse effects of the program that we find in our empirical study.

Third, the stimulus program may induce a change of financing method among the would-be car buyers. More specifically, the cash rebate may allow some potential buyers to use all cash without debt financing; these buyers could have better credit quality, suggesting the deteriorating of the loan performance documented in this paper being attributable to a change in borrower composition (rather than a causal impact on debt overhang). However, given that buyers only received the cash rebate at least 12 months after the purchase, this change towards cash payment was thus unlikely, especially for those with credit constraints.⁷

⁷ We also obtain the number of new registrations of auto vehicles excluding motorcycles (from the Department of Land Transport) and compare it with the number of new car loans (from the National Credit Bureau) during the period of this study. We find that registration growth and loan growth were almost identical (if any, loan growth was slightly higher), suggesting that the switch from loan to cash purchase was unlikely.

Fourth, there are alternative candidates for the control group. One possibility could be individuals who purchased their first car during the program period, but the car models were not qualified for the tax rebate. Another candidate could be individuals who purchased qualified car models during the program period, but were not qualified for the rebate because the purchase was not their first car. None of these candidates, including the one we use in this paper, is a perfect control group because the decision to purchase a car was not random, implying that individuals in the treatment and the control groups possessed some different characteristics. The rationale behind our decision to choose individuals who were first-car-buyers that purchased qualified car models outside the program period as our control group is that in this study we aim to focus on the demand-shifting effect of the program. Our control individuals were first-time car buyers that purchased the same car models as their treatment counterparts. The major difference from the treatment individuals is that they were not incentivized by the tax rebate to purchase the car during the program period.

Fifth, given that the qualified participants were scheduled to receive their excise tax rebate about one year after possessing the car and the actual timing varied considerably. It might be possible that the increased delinquency was due to the delay in rebate payment. Individuals may anticipate the rebate in one year and plan their cash flow accordingly; nevertheless, the delay in the payout may have pushed them to delinquency. To explore the extent of this concern, we further perform a regression analysis by dividing the post-program period (6 quarters) into two sub-periods that last 3 quarters each. Tables A.5 and A.6 in the Appendix shows that the results are generally consistent with our baseline analysis. There is no strong evidence that the cash rebate helps mitigate the deterioration of borrowers' loan performance.⁸

Sixth, given that there was a massive flood in 2011 that could confound the effects of the first-carbuyer tax rebate scheme, we perform a placebo experiment. In the placebo test, we replicate the baseline analysis but using non-qualifying vehicles. The treatment group is the purchase of nonqualifying models inside the program period (Sep 2011 – Dec 2013) while the control group includes the purchase of non-qualifying models outside the program period (Jan 2010 – Aug 2011 and Jan 2014 – Dec 2014). For passenger cars, non-qualifying models are all car models not

⁸ We find that the delinquency outcome for unsecured loan is not statistically significant for the second post period. However, we do not find such evidence for other loan types.

eligible for the program; for trucks, they are all truck models with loan value greater than 1 million baht. The results are shown in Tables A.7 and A.8. We do not find any significant effect on both delinquency and new loan probabilities. This null result helps mitigate the concern that the 2011 mass flood was driving our baseline result.

Finally, it is worth noting the background of Thailand's auto loan practice. The auto loan market is dominated by private commercial banks and leasing companies. The loans are generally offered in a fixed interest rate plan. Typically, car dealers partner with financial institutions and offer financing plans for their customers. These plans vary by the number of months serving and the amount of down payment but are independent of the individual characteristics of the borrowers.⁹ The facts that the auto loan markets are dominated by private agents and that the loans are offered irrespective of individual's characteristics lessen the concerns that the bank might ease up their lending standards to assist the government in supporting the auto sales. In addition, we examine the differential effect in areas with more or less bank competition by interacting the treatment variable with postcode-level competition. Specifically, we divide postcodes into two groups (Low and High competition) as proxied by the Herfindahl index for all loans at the year of vehicle purchase: a postcode is considered to be having high banking competition if its Herfindahl index is greater than the sample median level. The results are shown in Tables A.9 and A.10 in the appendix. The finding does not lend strong support to the credit supply channel where higher banking competition leads to lower loan-underwriting standard and adverse financial outcome.

5. Empirical Analysis at the Postcode Level

So far our analyses have focused on the impacts of the first-time car buyer tax rebate program on individuals. However, the program could also generate impacts at a larger geographic level. In this section, we explore possible general equilibrium impacts of the program by studying debt at a

⁹ Financial institutions may choose to decline applications for some borrowers with unfavorable credit history. Also note that, our findings on higher delinquency do not imply that financial institutions behave irrationally as we do not observe their interest rates and total profits generated from the program participants.

more aggregate level, namely, the postcode level. Our goal is to examine whether there were spillovers of the program into non-first-car borrowers.

5.1 Data

For the postcode-level analysis, we first construct a variable that measures the postcode's exposure to the first-car-buyer tax rebate program at the end of 2013 by computing the ratio of individuals in the treatment group (as defined in the previous section) to all borrowers for each postcode.¹⁰ Figure 4 presents a map of the share of program exposure across postcodes, which illustrates the heterogeneity in the program exposure throughout the country. Next, we generate outcome variables from the dataset that contains one million borrowers randomly sampled from the credit bureau database from January 2010 to December 2015. We then collapse the individual sample into a quarterly-postcode panel. All loan outstanding variables are collapsed by summing over all individuals in each postcode. Finally, we define post-program period as the quarters after the end of 2013, i.e., the first quarter of 2014 onwards.

[Figure 4]

Parallel to the analysis at the individual level, we examine two sets of outcome variables in this postcode-level analysis: (1) the average share of loan outstanding with delinquency status, being overdue on a payment by at least one month, and (2) the quarterly growth of total loan outstanding in each postcode. We further consider the outcomes on two groups of loans in each postcode: (a) all loans of all borrowers, and (b) all loans of non-first-car borrowers, where the non-first-car borrowers were those who did not originate any qualified auto loan between September 2011 and December 2013 or had existing car loans before September 2011.

[Table 4]

¹⁰ We construct this exposure variable from the dataset that contains all borrowers in Thailand's banking system.

Table 4 presents summary statistics of the outcome and exposure variables at the postcode level. Panel A shows that all measures of loan growth were relatively lower in the post-program period, while all measures of delinquent loan shares were relatively higher. There was also a wide variation in how the postcodes were exposed to the first-car-buyer tax rebate program. The ratio of program participants to all borrowers ranges from 5.2 percent in the 10th percentile to 12.4 percent in the 90th percentile, with its mean at 8.8 percent.

5.2 Empirical Strategy

We use quarterly postcode-level data to estimate the following regression:

$$y_{it} = \alpha_1 exposure_i + \alpha_2 post_t + \beta_1 exposure_i \cdot post_t + province_year FE + time FE + postcode FE + \varepsilon_{it}$$

where y_{it} is an outcome variable and $post_t$ is an indicator variable that equals one for years 2014-2015 and zero otherwise. The exposure variable, $exposure_i$, is the ratio of the number of individuals in the treatment group to the total number of borrows in each postcode, ranging from zero to one. We use robust standard errors and cluster them at the province level.

5.3 Empirical Results

Table 5 shows the regression results for the general equilibrium effects of the first-car-buyer tax rebate scheme at the postcode level. Column (1) examines the overall impacts of the first-car-buyer tax rebate program on the local economy at the postcode level. It shows that postcodes with higher exposure to the program had higher shares of loan outstanding with delinquency status (Panel A) and experienced lower loan growth (Panel B). These findings are consistent with what we find earlier in the individual-level analysis.

[Table 5]

It is important to note that the general equilibrium effects could come from two sources. First, they could be the aggregation of the program's impacts on loan delinquency and new loan origination at the individual level presented in the previous section. Alternatively, the effects could come from the spillovers from program participants to non-participants living in the same local economy, i.e., in the same postcode. The channels of these spillovers could include possible lower spending of program participants due to their debt overhang caused by the first-car loans, which was a result from shifting future demand forward to the present while not being financially ready for the debt service. To isolate the spillover effect of the program on non-participants, we examine the program impacts on loans of non-participants only. Column (2) of Table 5 illustrates that postcodes with higher exposure to the program experience higher delinquency of non-participants' loans, implying the existence of negative spillovers.

Finally, one may infer from Figure 4 that exposure to the first-car-buyer tax rebate tax rebate was different between rural and urban areas, and our measure of exposure might just capture the difference in urbanization rather than the program itself. We provide in Table A.11 in the Appendix that our overall results hold when we perform the analysis separately for urban postcodes and rural postcodes.

6. Concluding Remarks

This paper uses account-level loan data from Thailand's National Credit Bureau to study debt overhang from the durable stimulus programs. Specifically, we analyze the impacts of the first-car-buyer tax rebate scheme that was implemented in Thailand during 2011-13 on delinquency and new loan origination at the individual level as well as delinquency ratio and loan growth at the local economy (postcode) level. At the individual level, we find that the delinquency probability for auto loans as well as unsecured loans and other secured loans was significantly higher for the treatment group (individuals likely participating in the program), relative to that of the control group (those not eligible for the program). The overall new loan probability was also significantly lower for the treatment group. At a more aggregate level, we find that the local areas with higher exposure to the program experienced higher delinquency ratios of loan outstanding and lower loan growth rates, even among non-participants of the tax rebate program.

We analyze possible mechanisms that led to higher debt burden. First, our empirical evidence does not seem to suggest that the effects of the first-car-buyer tax rebate scheme are driven by the credit supply channel where higher banking competition results in lower loan-underwriting standard, or the possible delay of tax rebate. Second, given that the numbers of vehicle sales before and after the program were comparable, the additional purchases during the program period likely came from those who would not purchase the vehicle without the rebate. Third, our adverse results are also pronounced for passenger cars, but not trucks. Altogether, our findings suggest that a likely mechanism that led to the excessive debt burden was an increase in new demand for unproductive consumer durables that was financed by lower-quality loans.

The findings in this paper highlight the effects on debt burden from fiscal stimulus and have an important policy implication: fiscal policies aimed to stimulate the economy in the immediate may lead to excessive debt burden and create an adverse consequence in the longer term as higher delinquency, fewer new loan originations, and slower loan growth can cause a decline in future consumption. This raises questions about the merits of promoting economic growth by inducing debt-fueled spending.¹¹

An insight from our study also offers an implication on the design of durable-goods stimulus policy. We find that the effects were different between first-time passenger car buyers and first-time truck buyers, with the fall in new loan probability being concentrated among passenger car purchases. In other words, debt-fueled fiscal stimulus seems to produce less adverse impact when it is implemented on productive business durables rather than on consumer durables.

Finally, we emphasize that this study focuses on the impact of debt-fueled fiscal stimulus on household indebtedness. This paper neither considers the costs and benefits of the program on other aspects nor the welfare impacts on the economy. We leave those issues to future studies.

¹¹ It is worth noting that if the majority of program participants choose to purchase via cash, then there could still be positive aggregate impact of the fiscal stimulus—cash buyers may stimulate the economy by increasing other spending. However, the Excise Department released the information about financing methods of program participants in September 2012. Of 35% of all participants, roughly 16% used cash while 84% financed with credit. The information about the remaining participants were never released publicly.

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Figure 1: Sales of Passenger Cars and Commercial Vehicles (Units: Cars)

Notes: This figure shows monthly sales of passenger cars and commercial vehicles in Thailand during 2010-2014.

Source: Bank of Thailand

Figure 2: Thailand's Automobile Loans (2009-2015)



Notes: This figure shows the number of automobile loan accounts (in million) and its share of non-performing loans (in percent). The non-performing loan is defined as a loan that has been delinquent for at least 90 days. The period is from 2009-2015.

Source: Authors' estimate from National Credit Bureau data





Notes: This figure compares the means of outcome variables during the pre- and post-program periods. Panels 3a-3c show the probability of delinquency, while Panels 3d-3f show the probability of new loan origination. *Source:* Authors' estimate from National Credit Bureau data

Figure 4: Share of the First-Car-Buyer Tax Rebate Program Exposure across Postcodes



Notes: This figure maps the share of exposure to the first-car-buyer tax rebate program across postcodes at the end of 2013. The exposure is defined as the ratio of individuals in the treatment group to all borrowers in each postcode. Darker shade indicates higher exposure. *Source*: Authors' estimate from National Credit Bureau data

Variables		Contro	1	Treatment			Treatment-	
	Mean	SD	n	Mean	SD	n	- Control	
A: Demographics								
Age	42.8	9.6	2,528	41.4	9.3	5,830	-1.33***	
							(0.22)	
Urban	0.5	0.5	2,528	0.5	0.5	5,830	0.01	
							(0.01)	
<u>B: Portfolio</u>								
Number of loan accounts	8.7	9.8	2,528	8.6	9.5	5,830	-0.15	
							(0.23)	
Having a mortgage	0.2	0.4	2,528	0.2	0.4	5,830	-0.02*	
							(0.01)	
Having a personal								
loan	0.6	0.5	2,528	0.6	0.5	5,830	-0.01	
							(0.01)	
Having credit card(s)	0.4	0.5	2.528	0.4	0.5	5.830	-0.07***	
		010	_,	011	0.0	0,000	(0.01)	
Having commercial							(****)	
bank loans	0.6	0.5	2,528	0.5	0.5	5,830	-0.06***	
							(0.01)	

Table 1: Summary Statistics: Individual Level

Notes: This table presents summary statistics of demographic and portfolio characteristics for the control and the treatment groups in our sample at the time of their first car purchase. Numbers in parentheses indicate standard error. ***, **, ** denotes significance at the 1%, 5%, and 10% levels, respectively.

Table 2: Estimated Impacts on Delinquency Probability

		All		Р	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First car	Unsecured	Secure ex.	First car	Unsecured	Secured	First car	Unsecured	Secured
			First car			ex. First			ex. First
						car			car
Post	-0.018***	0.000	-0.001	-0.016**	-0.000	-0.003	-0.021***	-0.001	0.001
	(0.002)	(0.002)	(0.010)	(0.005)	(0.003)	(0.013)	(0.003)	(0.008)	(0.007)
Treatment x Post	0.031***	0.024***	0.016	0.026***	0.025**	0.011	0.041***	0.021*	0.027
	(0.003)	(0.004)	(0.017)	(0.004)	(0.008)	(0.015)	(0.007)	(0.010)	(0.021)
Observations	58,134	80,547	24,866	32,381	49,139	15,130	25,753	31,408	9,736
Number of borrowers	8,358	7,267	2,922	4,656	4,345	1,738	3,702	2,922	1,184

Notes: This table presents the estimated impacts of the first-car-buyer tax rebate program on delinquency probability. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		F	Passenger Cars			Trucks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
VARIABLES	All	Unsecured	Secured	All	Unsecured	Secured	All	Unsecured	Secured	
Post	0.010*	-0.007**	0.018***	0.006	-0.008	0.013***	0.015***	-0.006***	0.024***	
	(0.004)	(0.002)	(0.002)	(0.006)	(0.004)	(0.002)	(0.002)	(0.001)	(0.003)	
Treatment x Post	-0.014**	-0.009*	-0.005*	-0.024**	-0.016*	-0.008***	0.000	0.000	-0.000	
	(0.004)	(0.004)	(0.002)	(0.009)	(0.007)	(0.002)	(0.010)	(0.010)	(0.006)	
Observations	98,985	98,985	98,985	55,273	55,273	55,273	43,712	43,712	43,712	
Number of observations	8,358	8,358	8,358	4,656	4,656	4,656	3,702	3,702	3,702	

Notes: This table presents the estimated impacts of the first-car-buyer tax rebate program on probability of new loan origination. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Summary Statistics: Postcode-Level Analysis

	Pre			Post	Post-Pre		
	Mean	SD	n	Mean	SD	n	
Loan growth (All)	0.070	0.092	10632	0.033	0.054	10632	-0.036***
Loan growth (Non-First-car)	0.062	0.086	10632	0.036	0.055	10632	-0.026*** (0.001)
Delinquent loan Share (All)	0.067	0.043	11518	0.095	0.043	10632	0.028*** (0.001)
Delinquent loan Share (Non-First- car)	0.071	0.047	11518	0.093	0.045	10632	0.023*** (0.000)
		Pan	el B: Expo	osure varie	able		
		Mean	SD	n	10 th	50 th	90 th
Ratio of program	orrowars	0.088	0.037	1109	0.052	0.088	0.124

Panel A: Outcome Variables

participants to all borrowers *Notes*: This table presents summary statistics of outcome and exposure variables at the postcode level. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)		
	All	Non-participants		
Panel A: Share of loan o	utstanding wit	th delinquent status		
Exposure*Post	0.198***	0.153***		
	(0.047)	(0.049)		
Observations	22,150	22,150		
Number of postcodes	886	886		
Fixed Effects	Post	code, Time		
Panel B: Growth of loan	outstanding			
Exposure*Post	-0.182***	-0.038		
-	(0.035)	(0.040)		
Observations	21,264	21,264		
Number of postcodes	886	886		
Fixed Effects Postcode, Time				

Table 5 Effects on Delinquency and Loan Growth: Postcode Regression Analysis

Notes: This table presents the estimated general equilibrium effects of the first-car-buyer tax rebate program at the postcode level. Post is a dummy variable that equals to one for the post-purchase period. Exposure is the ratio of the number of individuals in the treatment group to the number of borrowers in each postcode. Exposure x Post is the interaction variable between Exposure and Post. All specifications include postcode-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on provinces. Numbers in parentheses indicate standard error. ***, **, ** denotes significance at the 1%, 5%, and 10% levels, respectively.

Appendix

Table A.1: Basic difference-in-difference framework (Delinquency propensity)

	A: Delinquency (First car)			
	Pre	Post	Difference (Post-Pre)	
Control	N/A	0.020***	N/A	
		(0.001)		
Treatment	N/A	0.056***	N/A	
		(0.001)		
Difference (Treatment-Control)	N/A	0.036***	N/A	
		(0.002)		

Panel A: Delinquency (First car)

Panel B: Delinquency (Unsecured loans)

	B: Delinquency (All unsecured)			
	Pre	Post	Difference (Post-Pre)	
Control	0.076***	0.120***	0.044***	
	(0.003)	(0.003)	(0.004)	
Treatment	0.087***	0.121***	0.034***	
	(0.002)	(0.002)	(0.003)	
Difference (Treatment-Control)	0.011***	0.001	-0.010**	
	(0.003)	(0.003)	(0.005)	

Panel C: Delinquency	(Secured loans	excluding first o	car)
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	C: Delinquency (All secured)			
	Pre	Post	Difference (Post-Pre)	
Control	0.044***	0.065***	0.021***	
	(0.004)	(0.005)	(0.006)	
Treatment	0.056***	0.079***	0.023***	
	(0.003)	(0.003)	(0.004)	
Difference (Treatment-Control)	0.012**	0.014***	0.002	
	(0.005)	(0.005)	0.007	

Notes: This table presents basic difference-in-difference framework for delinquency probability of first car loans (Panel A), all unsecured loans (Panel B) and all secured loans excluding the first car loans (Panel C). Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

Table A.2: Basic Difference-in-Difference Framework (New Loan Propensity)

	Panel A: All new loans excluding first car			
	Pre	Post	Difference (Post-Pre)	
Control	0.125***	0.136***	0.011**	
	(0.003)	(0.003)	(0.004)	
Treatment	0.149***	0.133***	-0.017***	
	(0.002)	(0.002)	(0.003)	
Difference (Treatment-	0.025***	-0.003	-0.027***	
Control)	(0.004)	(0.003)	(0.005)	

Panel B: All new unsecured loans

	Pre	Post	Difference (Post-Pre)
Control	0.112***	0.112***	-0.001
	(0.003)	(0.003)	(0.004)
Treatment	0.139***	0.114***	-0.025***
	(0.002)	(0.002)	(0.003)
Difference (Treatment-	0.027***	0.002	-0.025***
Control)	(0.004)	(0.003)	(0.005)

Panel C: All new secured loans excluding first car

	Pre	Post	Difference (Post-Pre)
Control	0.016***	0.029***	0.013***
	(0.001)	(0.001)	(0.002)
Treatment	0.013***	0.024***	0.011***
	(0.001)	(0.001)	(0.001)
Difference (Treatment-	-0.003**	-0.006***	-0.002
Control)	(0.001)	(0.002)	(0.002)

Notes: This table presents basic difference-in-difference framework for new loan origination probability of all new loans excluding the first car loans (Panel A), all new unsecured loans (Panel B) and all new secured loans excluding the first car loans (Panel C). Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		P	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First car	Unsecured	Secure ex.	First car	Unsecured	Secured	First car	Unsecured	Secured
			First car			ex. First			ex. First
						car			car
Post	-0.018***	0.002	0.003	-0.019**	0.005	0.001	-0.021***	-0.003	0.013
	(0.003)	(0.002)	(0.013)	(0.005)	(0.007)	(0.024)	(0.002)	(0.010)	(0.009)
Treatment x Post	0.033***	0.025***	0.016	0.030***	0.023	0.005	0.042***	0.031*	0.034
	(0.004)	(0.007)	(0.018)	(0.007)	(0.012)	(0.018)	(0.003)	(0.013)	(0.022)
Observations	29,321	42,535	14,967	18,262	28,093	9,756	11,059	14,442	5,211
Number of borrowers	4,216	3,810	1,695	2,625	2,472	1,079	1,591	1,338	616

Table A.3: Estimated Impacts on Delinquency Probability (Only Major Provinces)

Notes: This table illustrates a robustness test where we include only ten provinces with the largest number of program participants and replace the province-year fixed effects with the province-quarter fixed effects. It presents the estimated impacts of the first-car-buyer tax rebate program on delinquency probability. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		F	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	All	Unsecured	Secured	All	Unsecured	Secured	All	Unsecured	Secured
Post	0.004	-0.010**	0.014*	-0.000	-0.008*	0.006	0.013	-0.012	0.027***
	(0.008)	(0.003)	(0.006)	(0.008)	(0.004)	(0.006)	(0.008)	(0.007)	(0.005)
Treatment x Post	-0.012	-0.010	-0.002*	-0.022***	-0.021**	-0.001	0.001	0.007	-0.005
	(0.007)	(0.007)	(0.001)	(0.004)	(0.006)	(0.003)	(0.017)	(0.019)	(0.007)
Observations	49,950	49,950	49,950	31,173	31,173	31,173	18,777	18,777	18,777
Number of borrowers	4,216	4,216	4,216	2,625	2,625	2,625	1,591	1,591	1,591

Table A.4: Estimated Impacts on New Loan Probability (Only Major Provinces)

Notes: This table illustrates a robustness test where we include only ten provinces with the largest number of program participants and replace the province-year fixed effects with the province-quarter fixed effects. It presents the estimated impacts of the first-car-buyer tax rebate program on probability of new loan origination. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		Р	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First car	Unsecured	Secure ex.	First car	Unsecured	Secured	First car	Unsecured	Secured
			First car			ex. First			ex. First
						car			car
Post1	-0.017***	0.009	0.002	-0.017***	0.011**	0.000	-0.020***	0.002	0.003
	(0.002)	(0.007)	(0.009)	(0.004)	(0.004)	(0.009)	(0.002)	(0.013)	(0.011)
Treatment x Post1	0.030***	0.022***	0.016	0.025***	0.021***	0.012	0.039***	0.025*	0.027
	(0.002)	(0.003)	(0.016)	(0.004)	(0.004)	(0.013)	(0.006)	(0.012)	(0.022)
Post2	-0.033***	0.041***	0.017**	-0.026**	0.044***	0.019	-0.042**	0.032**	0.011
	(0.004)	(0.004)	(0.006)	(0.008)	(0.005)	(0.013)	(0.012)	(0.011)	(0.006)
Treatment x Post2	0.049***	0.018	0.013	0.036***	0.021	0.007	0.068**	0.011	0.027
	(0.006)	(0.012)	(0.019)	(0.007)	(0.016)	(0.021)	(0.020)	(0.011)	(0.025)
Observations	58,134	80,547	24,866	32,381	49,139	15,130	25,753	31,408	9,736
Number of borrowers	8,358	7,267	2,922	4,656	4,345	1,738	3,702	2,922	1,184

Table A.5: Estimated Impacts on Delinquency Probability (Splitting Post-Program Period)

Notes: This table illustrates a robustness test where we split the post-purchase period into two sub-periods. It presents the estimated impacts of the first-car-buyer tax rebate program on delinquency probability. Post1 is a dummy variable that equals to one for the first to third quarter after first-car purchase. Post2 is a dummy variable that equals to one for the forth to sixth quarter after first-car purchase. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		F	Passenger Cars	6		Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	All	Unsecured	Secured	All	Unsecured	Secured	All	Unsecured	Secured
Post1	0.007	-0.013**	0.020***	-0.003	-0.019**	0.015***	0.018**	-0.006*	0.026***
	(0.006)	(0.005)	(0.002)	(0.007)	(0.006)	(0.002)	(0.005)	(0.003)	(0.003)
Treatment x Post1	-0.011	-0.005	-0.006**	-0.015	-0.007	-0.008**	-0.003	-0.002	0.000
	(0.006)	(0.006)	(0.002)	(0.011)	(0.008)	(0.003)	(0.009)	(0.010)	(0.006)
Post2	0.010	-0.019**	0.031***	0.008	-0.017	0.026***	0.014	-0.019*	0.038***
	(0.009)	(0.007)	(0.004)	(0.013)	(0.010)	(0.006)	(0.009)	(0.008)	(0.004)
Treatment x Post2	-0.019*	-0.013*	-0.008	-0.039***	-0.030***	-0.011***	0.006	0.007	-0.003
	(0.008)	(0.006)	(0.004)	(0.005)	(0.006)	(0.003)	(0.016)	(0.015)	(0.007)
Observations	98,985	98,985	98,985	55,273	55,273	55,273	43,712	43,712	43,712
Number of borrowers	8,358	8,358	8,358	4,656	4,656	4,656	3,702	3,702	3,702

Table A.6: Estimated Impacts on New Loan Probability (Splitting Post-Program Period)

Notes: This table illustrates a robustness test where we split the post-purchase period into two sub-periods. It presents the estimated impacts of the first-car-buyer tax rebate program on probability of new loan origination. Post1 is a dummy variable that equals to one for the first to third quarter after first-car purchase. Post2 is a dummy variable that equals to one for the forth to sixth quarter after first-car purchase. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		Р	assenger Cars			Trucks	
-	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First car	Unsecured	Secure ex.	First car	Unsecured	Secured	First car	Unsecured	Secured
			First car			ex. First			ex. First
						car			car
Post	-0.040***	0.017	-0.072	-0.046***	-0.028	-0.058	0.001	0.035	-0.090
	(0.006)	(0.015)	(0.042)	(0.004)	(0.034)	(0.061)	(0.005)	(0.054)	(0.112)
Treatment x Post	0.005	-0.004	0.083	0.006	0.040	0.068	-0.001	0.019	0.012
	(0.009)	(0.015)	(0.042)	(0.007)	(0.033)	(0.058)	(0.009)	(0.055)	(0.036)
Observations	45,878	64.084	19,800	28,295	42,384	13,009	17,583	21,701	6,790
Number of borrowers	6,605	5,754	2,280	4,044	3,733	1,461	2,561	2,021	819

Table A.7: Estimated Impacts on Delinquency Probability (Placebo Experiment)

Notes: This table replicates the baseline analysis but using non-qualifying vehicles (Placebo experiment). It presents the estimated impacts of the first-car-buyer tax rebate program on delinquency probability. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All]	Passenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	All	Unsecured	Secured	All	Unsecured	Secured	All	Unsecured	Secured
Post	0.056*	0.012	0.048^{***}	0.088	0.029	0.075***	0.044***	0.008	0.018
	(0.024)	(0.020)	(0.006)	(0.053)	(0.041)	(0.010)	(0.008)	(0.015)	(0.011)
Treatment x Post	-0.062	-0.029	-0.004	-0.093	-0.046	-0.006	-0.012	-0.021	0.021
	(0.049)	(0.020)	(0.004)	(0.052)	(0.042)	(0.008)	(0.044)	(0.043)	(0.012)
Observations	78,247	78,247	78,247	48,007	48,007	48,007	30,240	30,240	30,240
Number of borrowers	6,605	6,605	6,605	4,044	4,044	4,044	2,561	2,561	2,561

Table A.8: Estimated Impacts on New Loan Probability (Placebo Experiment)

Notes: This table replicates the baseline analysis but using non-qualifying vehicles (Placebo experiment). It presents the estimated impacts of the first-car-buyer tax rebate program on probability of new loan origination. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. Treatment x Post is the interaction variable between Treatment and Post. All specifications include individual-, quarter-and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, ** denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		Р	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First car	Unsecured	Secure ex.	First car	Unsecured	Secured	First car	Unsecured	Secured
			First car			ex. First			ex. First
						car			car
_									
Post	-0.022***	-0.014	-0.018	-0.025***	-0.008	-0.025	-0.017**	-0.025	-0.005
	(0.003)	(0.011)	(0.014)	(0.005)	(0.010)	(0.017)	(0.004)	(0.014)	(0.013)
Treatment x Post	0.030***	0.042***	0.036**	0.029***	0.037***	0.035**	0.033***	0.052***	0.041
	(0.004)	(0.007)	(0.013)	(0.006)	(0.009)	(0.013)	(0.004)	(0.012)	(0.024)
Post x High	0.006	0.023	0.029	0.014**	0.014	0.041	-0.005	0.036	0.010
	(0.005)	(0.016)	(0.016)	(0.005)	(0.018)	(0.024)	(0.006)	(0.019)	(0.014)
Treatment x Post x High	0.003	-0.029*	-0.034	-0.005	-0.019	-0.042	0.012	-0.046**	-0.022
-	(0.010)	(0.013)	(0.017)	(0.011)	(0.019)	(0.031)	(0.010)	(0.015)	(0.013)
Observations	58,121	80,540	24,861	32,376	49,134	15,125	25,745	31,406	9,736
Number of borrowers	8,355	7,265	2,921	4,655	4,344	1,737	3,700	2,921	1,184

Table A.9: Estimated Impacts on Delinquency Probability (Banking competition)

Notes: This table illustrates a robustness test where we examine the differential effect in areas with different banking competition levels. It presents the estimated impacts of the first-car-buyer tax rebate program on delinquency probability. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. High is a dummy variable equals to one if a borrower is in a postcode with high banking competition. A postcode is considered having high banking competition if its Herfindahl index for all loans at the year of vehicle purchase is greater than the sample median level. Treatment x Post is the interaction variable between Treatment and Post. Post x High is the interaction variable between Post and High. Treatment x Post x High is the interaction variable among Treatment, Post and High. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

		All		P	assenger Cars			Trucks	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	All	Unsecured	Secured	All	Unsecured	Secured	All	Unsecured	Secured
Post	0.005*	-0.010*	0.015*	0.003	-0.008	0.010	0.006	-0.015	0.022***
	(0.002)	(0.005)	(0.006)	(0.007)	(0.005)	(0.007)	(0.007)	(0.013)	(0.005)
Treatment x Post	-0.011	-0.007	-0.004	-0.024***	-0.019**	-0.007	0.011	0.012	0.003
	(0.008)	(0.009)	(0.004)	(0.006)	(0.006)	(0.006)	(0.021)	(0.024)	(0.008)
Post x High	0.009	0.005	0.005	0.005	0.000	0.005	0.013	0.012	0.003
C C	(0.006)	(0.010)	(0.007)	(0.007)	(0.009)	(0.007)	(0.013)	(0.016)	(0.007)
Treatment x Post x High	-0.004	-0.002	-0.003	0.001	0.005	-0.002	-0.016	-0.017	-0.004
C C	(0.009)	(0.011)	(0.006)	(0.008)	(0.010)	(0.006)	(0.016)	(0.020)	(0.007)
Observations	98,964	98,964	98,964	55,263	55,263	55,263	43,701	43,701	43,701
Number of borrowers	8,355	8,355	8,355	4,655	4,655	4,655	3,700	3,700	3,700

Table A.10: Estimated Impacts on New Loan Probability (Banking competition)

Notes: This table illustrates a robustness test where we examine the differential effect in areas with different banking competition levels. It presents the estimated impacts of the first-car-buyer tax rebate program on probability of new loan origination. Post is a dummy variable that equals to one for the post-purchase period. Treatment is a dummy variable that equals to one for the treatment group. High is a dummy variable equals to one if a borrower is in a postcode with high banking competition. A postcode is considered having high banking competition if its Herfindahl index for all loans at the year of vehicle purchase is greater than the sample median level. Treatment x Post is the interaction variable between Treatment and Post. Post x High is the interaction variable between Post and High. All specifications include individual-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on decades of birth. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	All	Non-Auto-Loans
Panel A1: Share of	loan outstanding	with delinquent status
exposure*post	0.256***	0.210***
	(0.070)	(0.070)
Observations	10,775	10,775
Number of zip	431	431
Fixed Effects	Pos	tcode, Time
Panel A2: Growth o	of loan outstandi	ng
exposure*post	-0.247***	-0.149***
	(0.051)	(0.048)
Observations	10,344	10,344
Number of zip	431	431
Fixed Effects	Pos	stcode, Time

Table A.11: Postcode regression analysis (Urban and Rural areas)

Panel A: Urban postcodes (Top 50% in term of urbanization degree)

Panel B: Rural postcodes (Bottom 50% in term of urbanization degree)

	(1) All	(2) Non-Auto-Loans
Panel B1: Share of l	oan outstanding	g with delinquent status
exposure*post	0.143**	0.123*
	(0.069)	(0.071)
Observations	11,375	11,375
Number of zip	455	455

Fixed Effects	Postc	Postcode, Time		
Panel B2: Growth o	f loan outstanding			
exposure*post	-0.195***	-0.145**		
	(0.055)	(0.059)		
Observations	10,920	10,920		
Number of zip	455	455		
Fixed Effects	Postc	ode, Time		

Notes: This table illustrates a robustness test where we split all postcodes into two groups based on their urbanization degree. It presents the estimated general equilibrium effects of the first-car-buyer tax rebate program. The urbanization degree is based on percent of population living in municipality in each postcode. Post is a dummy variable that equals to one for the post-purchase period. Exposure is the ratio of the number of individuals in the treatment group to the number of borrowers in each postcode. Exposure x Post is the interaction variable between Exposure and Post. All specifications include postcode-, quarter- and province-year-fixed effects. Standard errors are heteroscedasticity-robust and are clustered on provinces. Numbers in parentheses indicate standard error. ***, **, * denotes significance at the 1%, 5%, and 10% levels, respectively.