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Are Fast Court Proceedings Good or Bad?: Evidence from Japanese Household Panel Data*

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Abstract

We analyze the effect of the degree of judicial enforcement on the probability of credit constraints, the amount of loan and the probability of default. Contrary to the traditional view on judicial efficiency of credit market, our estimation results show that better judicial enforcement increases the probability of being rationed and decreases credit granted by banks, consistent with laziness effects. In order to confirm the laziness effect more directly, we analyzed the effect of the degree of judicial enforcement on the probability of default and found that better judicial enforcement increases the probability of default, as expected.

Key words: Judicial enforcement; Credit allocation; Bankruptcy

JEL classification: D12; G21; G33; K12; K41; K42

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Abstract

We analyze the effect of the degree of judicial enforcement on the probability of credit constraints, the amount of loan and the probability of default. Contrary to the traditional view on judicial efficiency of credit market, our estimation results show that better judicial enforcement increases the probability of being rationed and decreases credit granted by banks, consistent with laziness effects. In order to confirm the laziness effect more directly, we analyzed the effect of the degree of judicial enforcement on the probability of default and found that better judicial enforcement increases the probability of default, as expected.

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Introduction

The length of trials has become shorter continuously in Japan. For instance, the average length of proceedings of civil trials in district courts was 17.3 months in 1973, 12.9 months in 1990 and 7.8 months in 2006. “Act on the Expediting of Trials¹” was promulgated in 2003, with the object of concluding trials as fast as possible and of realizing the rights under fair, appropriate and adequate proceedings. Court in Japan has done research on how to improve the management of the proceedings, and it has made efforts to accomplish its object by, for example, organizing the issue more adequately and by investigating intensively the most appropriate evidence to cases. How does the speed of trials affect economic behavior?

The economic research on the effect of different legal systems has been the center of theoretical and empirical investigations since mid-1990s. Many papers originated by La Porta, Lopez-De-Silanes, Shleifer, and Vishny (1997) analyzed the linkage between legal system and financial market. There are two channels: the degree of judicial enforcement and the content of the law.

Since Japanese laws are same nationwide, the contents of law are uniform in judicial districts, but the degrees of judicial enforcement are not. This feature makes it possible to disentangle the enforcement effect from the legislative effect, unlike the countries that have different laws in states.

There are many papers in Japan that analyzed the determinants of the probability of borrowing constraint and those of loan size, but there is no paper in Japan that analyzes the effect of the degree of judicial enforcement on credit allocation. Great attention should be paid to the impact of the degree of judicial enforcement especially in Japan, where the trials have sped up and it is expected that the downward trend in the length of trials is maintained. Using data on Japanese households drawn from “Japanese Panel Survey of Consumers² (hereafter the JPSC),” we could analyze the effect of the degree of judicial enforcement on credit allocation by combining the JPSC data with the data on judicial districts because the JPSC has a detailed information on the respondent’s living place.

Moreover, this paper controls more explanatory variables that capture local economic situation and local credit market activity such as the gross domestic product, bad loan ratio, market concentration and the depth of credit market at prefectural level. Since the speed of court performance by judicial districts is affected by those local factors, it is crucial to control these variables in order to capture the pure effect of the degree of judicial enforcement.

In addition, our data set allows us to investigate the impact of the degree of enforcement on the flow of debt, not on the stock. Current quality of judicial enforcement is expected to affect the credit amount recently granted by banks. All related papers except for Fabbri (2002) used the stock of debt, and many papers found the insignificant effect of the degree of judicial enforcement on loan size. One reason for the insignificance might be that the stock of debt reflects past choices of lenders and borrowers as well as current choices.

¹ In Japanese, Saiban no Jinsokuka ni kansuru Houritsu.

² In Japanese, Shouhi Seikatsu ni kansuru Paneru Chousa.

The estimation results we found are striking. Contrary to the traditional view on judicial efficiency of credit market, better judicial enforcement increases the probability of being rationed and decreases credit granted by banks, other things being equal. These findings are consistent with “laziness effect:” if judicial enforcement is stricter, an access to credit might be harder for good-type borrowers. In order to confirm the laziness effect, we analyzed the effect of the degree of judicial enforcement on the probability of default and found that better judicial enforcement increases the probability of default, as expected.

The paper is organized as follows. In section I, we present the results of previous theoretical and empirical studies. In section II, we describe the data and the variables used in our estimation. In section III, we present the estimation method and results, and section IV concludes.

I. Previous Studies

Countries have different legal systems. There are many literatures that investigated the relationship between the content of law and credit market. For example, La Porta, Lopez-De-Silanes, Shleifer, and Vishny (1997) found cross-country evidence that better quality of law enforcement, measured by an assessment by investors in different countries of the law and order environment they operate, has a significantly positive effect on the breadth of credit markets. Gropp, Scholz and White (1997) used the data from 1983 Survey of Consumer Finances and found that state-level personal bankruptcy exemptions have a significant positive effect on the probability of credit rationing. They also found that generous state bankruptcy exemptions increase the amount of credit held by high-asset households and reduce the availability of credit to low-asset households. Berkowitz and White (2004) also investigated how personal bankruptcy law affects an access to credit. They found that small businesses located in states with high homestead exemptions are more likely to be credit rationed and have less amount of loan, even though the effect of exemption level is non-monotonic.

Even if countries have same legal framework like Japan, the efficiency of judicial enforcement differs depending on the court performance in each judicial districts. This is the topic in which we are interested.

Fabrizio and Padula (2004) and Jappelli, Pagano and Bianco (2005) formalized the economic mechanism through which the performance of courts affect the credit allocation. For example, Fabrizio and Padula (2004) assume that the loan contract is securitized with collateral and that if the borrower fails to repay, the title to the collateral is transferred to the bank. The key assumption is that the judicial system determines when the collateral is transferred in case of default. If enforcement procedure is slow, the transfer of collateral is later. Borrowers can get positive utility from keeping the collateral longer, and hence the borrower’s incentive to repay the loan becomes lower. Responding to it, lenders increase the minimum collateral requirement. As a result, the probability that borrowers are credit constrained might increase when judicial enforcement is weak.

Slow court procedure is a cost for lenders because the liquidation value of the collateral is low due to depreciation or legal expenses might be high. Thus, a weak judicial enforcement tightens the lender’s participation constraint or both the lender’s participation constraint and the borrower’s incentive compatibility constraint. Both

effects make lenders shrink the availability of credit. Thus, their another prediction is that the amount of loan received by borrowers might decrease when judicial enforcement is weak.

Many economists argue that it is crucial to protect creditor rights for the optimal performance of credit markets. However, Zazzaro (2005) takes account of information asymmetries concerning borrower's entrepreneurial talent and theoretically shows that improvements in the judicial enforcement reduce the bank's incentive to screen borrowers adequately and banks might evaluate borrowers erroneously. As a result, an access to credit might be harder (easier) for good-type (bad-type) borrowers, and thus better judicial enforcement worsen the credit allocation and increases default rate (See Manove, Padilla and Pagano (2001) for similar results).

In summary, the theory shown by Fabbri and Padula (2004) and Jappelli, Pagano and Bianco (2005) predicts that better judicial enforcement reduces the probability of rationing and raises loan size, and it seems that their theory is regarded as a traditional view on judicial efficiency of credit markets. However, the effect of the degree of judicial enforcement might be contaminated due to the existence of laziness effect as shown in Zazzaro (2005) and Manove, Padilla and Pagano (2001). If laziness effect exists, the average effect of judicial enforcement on credit allocation is ambiguous. Namely, if better judicial enforcement reduces the probability of rationing and raises loan size, one cannot infer that laziness effect exists. However, if better judicial enforcement has opposite effect in sign to the traditional view, one can say that laziness effect prevails.

Next, we show you related empirical evidences. Jappelli, Pagano and Bianco (2005) used Italian provincial data in 1984-95 and the Italian National Institute of Statistics (ISTAT) data for the indicator of judicial efficiency and found that the stock of pending trials per thousand inhabitants, which is an indicator of poor judicial enforcement, is significantly associated with more overdraft loans (an indicator of credit constraints) and with less lending/GDP, consistent with their theoretical prediction. Moreover, Fabbri (2002) used firm data in Spain for 1998 and the Spanish National Institute of Statistics (INE) for the judicial enforcement indicators and found that both poor enforcement indicators (length of trials and the number of proceedings which last more than one year over the total number of concluded proceedings) have negative effects on the logarithm of the total credit granted during the interview's year and on the stock of financial debt. She also found, using firm level data in Italy for 1991 and the ISTAT data, that the good judicial enforcement indicator (ratio of completed judicial proceedings to total number of pending) has a positive effect on stock of total debt and poor judicial enforcement indicator (length of trials) has a negative effect on stock of total debt³.

On the other hand, not a few papers do not necessarily support a traditional view on judicial efficiency. Fabbri and Padula (2004) used the 1989, 1995 and 1998 waves of "Survey of Household Income and Wealth (SHIW)" and the ISTAT data. Their indicator for the quality of judicial enforcement is the backlog of trials pending divided by the number of incoming trials. They found that the backlog of trials pending has a significant positive effect on the probability of being credit constrained, consistent with

³ Only length of trials at second and third degree of jurisdiction has no significant effect on stock of total debt.

their theory, but no significant effect on the amount of debt. Similarly, Brown, Jappelli and Pagano (2008) found that if time to enforce payment is longer, perceived credit constraints are significantly lower, but the ratio of total debt to total assets is unchanged, using the Business Environment and Enterprise Performance Survey (BEEPS) 2002, which collects firm-level data in all countries except for Turkmenistan in which the EBRD is operational. In addition, Jappelli, Pagano and Bianco (2005) found that when using the length of trials, it has no significant effect on both credit constraints and loan size. Magri (2007) used the 1989, 1995 and 1998 waves of SHIW, same as in Fabbri and Padula (2004), but she used a different measure of judicial efficiency: the average time for recovery, which is obtained from a questionnaire sent by the Bank of Italy to Italian Banks. She found that recovery time has no significant effect on the probability of credit rationing and the amount of loan. Finally, Alessandrini, Presbitero and Zazzaro (2008) used the last three waves of Italian firm-specific data that covers the period 1995-2003 and the ISTAT data. Most importantly, they found that the efficiency of courts in recovering bad loans increases the probability of rationing, which implies the existence of laziness effect which Zazzaro (2005) and Manove, Padilla and Pagano (2001) showed.

This paper, first, examines the effect of the degree of judicial enforcement on the probability of being rationed and the amount of loan. Our estimation results are opposite in sign to the traditional view and implies the existence of laziness effect: better judicial enforcement increases the probability of being rationed and decreases the amount of loan. Next, to confirm the existence of laziness effect, we examine the effect of the degree of judicial enforcement on the probability of bankruptcy and found that better judicial enforcement increases the probability of bankruptcy, as expected.

II. Data and Variables

The JPSC survey has been conducted by the Institute for Research on Household Economics⁴ once a year since 1993. This paper uses the 2003-07 waves because the survey in those waves asks whether the respondent (or their spouse) was credit constrained during the year⁵. The respondents are women, but the survey asks not only about the respondents but also about their family members such as their spouses, children and parents. The number of observation was 2136 in 2003, 1977 in 2004, 1863 in 2005, 1770 in 2006, and 1694 in 2007 in the full sample, which means that the sample we used is an unbalanced panel. After excluding observations with missing values of the variables included in our regression, the number of remained observations was between 1200 and 1500 in each year. In A and B of section III, we kept an observation if households applied for loan during the year in order to identify household who were rationed by banks. The number of remained observations was 166 in 2003,

⁴ In Japanese, Kakei Keizai Kenkyuusho.

⁵ The question about credit constraints was also asked in the 1993 wave and the 1998 and later waves, but until 2002 the survey asked whether the respondent (or spouse) has ever been credit constrained or not, which means that we cannot distinguish exactly when they were credit constrained. Thus, we do not use the 1993-2002 waves. In addition, unfortunately, the respondents whose ages are between 24-29 in 2003 were also asked about the experience of credit constraint. Thus, we have no way but to regard the respondents whose ages are between 24-29 in 2003 and who have been credit constrained as those who were credit constrained during the year.

157 in 2004, 125 in 2005, 150 in 2006 and 112 in 2007.

An advantage to use the JPSC is that the respondents were asked whether they (or their spouse) were credit constrained during the year. The JPSC also collects data on the amount of loan that was granted by financial institutions during the year. Data on the flow of debt was not available in many previous studies regarding the degree of judicial enforcement, and they used the stock debt which reflects the past choices of lenders and borrowers as well as current choices. However, current quality of judicial enforcement is expected to affect the credit amount quite recently granted by banks. Thus, we rely on this flow data on debt as a loan size affected by the degree of judicial enforcement. Moreover, although the JPSC does not include the data on whether households apply for the loan, it is possible to identify loan-applicants by matching the question on the flow of debt with that on credit constraints (see A of section II for the detail). In addition, the JPSC survey collects the information on whether households had difficulties with loan repayment. We use this information to construct the proxy of the probability of default. Finally, the JPSC gives us the data on financial and real assets regarded as good proxies for the collateral, and it has much important information used when banks evaluate the quality of loan applicants, such as whether households are self-employed and the proxy for tenure.

A. The Variables on Household Characteristics

To identify credit-constrained borrowers, first we have to identify loan-applicants because households who did not apply for loan cannot be rationed by banks. The JPSC survey has not asked if households apply for a loan, but it collects data on the amount of loan granted by financial institutions during the year. We defined households who were granted credit during the year or those who were not granted credit and who were rejected their loan application during the year as loan-applicants.

Concerning whether households are credit constrained, we used the following questions: “Did you (or your spouse) have a loan application turned down?” and “Did you (or your spouse) have the loan amount reduced when you applied for a loan?”⁶ We regarded the respondents who answered “yes” to one or more of the above questions as being credit constrained. As for loan size, we used the logarithm of loan that was granted during the year.

Due to lack of data on direct information of default, we had to construct the proxy variable that indicates early but imperfect default. Specifically, we used the following question “Did you have difficulties with loan repayment during the year?” and regarded the respondents who answered “yes” to this question as default borrowers.

As explanatory variables that drive credit rationing and loan size, we used age and the square of the respondents (*AGE*, *AGED*), the logarithm of monthly disposable income of the respondents (*LINC*)⁷, the logarithm of the sum of the amount of financial

⁶ Many previous studies about borrowing constraints such as Cox and Jappelli (1993) and Duca and Rosenthal (1993) included households who discouraged to apply for loan. Our data also has the question to identify discouraged borrowers, but we do not include them in rationed households because it seems that households who do not apply for loan are not affected by the degree of judicial enforcement.

⁷ The reason why we do not use the annual disposable income is that the survey asks about annual income received in the previous year and taxes paid in the previous year. Using annual disposable

assets (bank and postal deposits and investment securities) and the value of real assets (land and housing) (*LWEALTH*), a dummy variable that equals to one if the respondents own their house (*HOME*), the logarithm of outstanding loan (*LLOAN*), a dummy variable that equals to one if the respondents are self-employed (*SELF-EMPLOYED*), a dummy variable that equals to one if the respondents are unemployed (*UNEMPLOYED*), a ratio of monthly amount of loan repayment over monthly income (*REPAY*), a dummy variable that equals to one if the respondents works in the same company as that they worked in the previous year (*TENURE*), a dummy variable that equals to one if the respondents graduated college (*COLLEGE*), a dummy variable that equals to one if the respondents are married (*MARRIED*), the number of children (*CHILD*), a dummy variable that equals to one if the respondents lives in metropolitan areas (*METRO*), a dummy variable for medium city (*MEDIUM CITY*), seven area dummies (*AREA1-7*), and year dummies (*YEAR2004-07*).⁸

In addition, in order to take account of regional economic situation and credit market activity, we also included the gross prefectural domestic product divided by population in each prefecture (*PREFECTURAL GDP*), the ratio of non-performing loan over total lending in regional banks by prefectures (*BAD LOANS*), the Herfindahl-Hirschman Index (the sum of the squared individual share of lending in regional banks by prefectures) (*HERFINDAHL*) and the total lending in each prefecture divided by prefectural GDP (*DEPTH*).

B. The Variables on Judicial Enforcement

Courts in Japan consist of the Supreme court, High courts, District courts, Summary courts, and Family courts. When borrower fails to repay their loan and lenders are willing to attach the borrower's property and sell it through court order, lenders resort to District courts⁹. In principle, when plaintiff (lender) resorts to a court, the competent court is that of the defendant (borrower)'s district of residence or collateral. Our data set includes the detailed information of the place where the respondents live. Thus, we can completely match the observation from the JPSC with the judicial data of the relevant court. We use data on 50 District courts from the public relations division in Supreme Court and the Annual Report of Judicial Statistics published by general secretariat, Supreme Court of Japan. All prefectures other than Hokkaido have one jurisdiction for district courts, and Hokkaido has four jurisdictions.

We use two indicators for the degree of judicial enforcement. The first indicator

income requires us to waste the last year (2007). In order to avoid a further reduction in the number of observations, we used monthly disposable income.

⁸ Our data set includes data on married and unmarried respondents. Concerning *AGE*, *AGES*, *LINC*, *SELF-EMPLOYED*, *UNEMPLOYED*, *REPAY*, *TENURE*, and *COLLEGE*, if the respondents are married, we used the data on the respondents' husbands because they seem to be household heads or main income earners.

⁹ In fact, the court performance in high courts and summary courts also seems to affect credit allocation because high courts have jurisdiction over appeals lodged against judgments of district courts and because, in order to attach the borrower's property, lenders might have to obtain the official document through the procedure of summary courts that shows the existence of the right to claim loan repayment. Thus, in section III, we also use the judicial data on high courts and summary courts, as robustness checks.

is the length of trials in district court from 2003 to 2007¹⁰. The data on the length of trials include all civil trials at the first degree of jurisdiction in district courts. It measures the time elapsing between the date of initial recording of a trial and that of the court sentence in each year¹¹. In regression, we use three dummy variables (*1st Enforcement Quartile1*, *2nd Enforcement Quartile1* and *3rd Enforcement Quartile1*) to represent quartiles of the distribution of length of trials, with the highest quartile being the excluded category. *1st Enforcement Quartile1* is a dummy variable that equals to one if the length of trials is less than 7.4 months and zero otherwise, *2nd Enforcement Quartile1* is a dummy variable for the length of trials which is between 7.4 and 8.1 months, and *3rd Enforcement Quartile1* is a dummy variable for the length of trials which is between 8.1 and 8.8 months. Thus, these dummy variables indicate better judicial enforcement, compared to the excluded category.

The second indicator is the number of pending civil trials over the number of incoming civil trials in district courts from 2003 to 2007. The data include all civil trials except for work or family related cases in district courts. The number of pending civil trials over the number of incoming civil trials reflects the duration of future trials, while the length of trials (the first indicator) reflects the duration of current and past trials. Many previous studies used the number of pending trials but used different normalization such as population and the number of judges and administrative staffs. In our analysis, we normalize by the number of incoming trials, following Fabbri and Padula (2004), because it responds to the business cycle and reflects in the degree of litigation. As well as the case of length of trials, we use three dummy variables (*1st Enforcement Quartile2*, *2nd Enforcement Quartile2* and *3rd Enforcement Quartile2*) to represent quartiles, with the highest quartile being the excluded category. *1st Enforcement Quartile2* is a dummy variable that equals to one if the pending rate is less than 39.9, *2nd Enforcement Quartile2* is a dummy variable for the pending rate which is between 39.9 and 43, and *3rd Enforcement Quartile2* is a dummy variable for the pending rate which is between 43 and 45.5. Thus, these dummy variables indicate better judicial enforcement, compared to the excluded category.

In introduction, we presented how court proceedings have sped up in Japan. Is the quickness maintained even if compared to other countries? According to “The Second Report on the Acceleration of Trials¹²,” the length of civil trials at the first degree of jurisdiction in 2004 was 8.3 months in Japan, 22.4 months in England, 9.6 months in France, and 8.5 months in the U.S. Only Germany has shorter length of trials (7.2 months) than that in Japan among those five countries. In addition,

¹⁰ We thank the public relations division in Supreme Court for providing the data on the length of trials by judicial districts. Since the division gave us the data for the years 1989-2006, we constructed the length of trials for 2007 by doing the linear, log, exponential, quadratic, and power approximation and by choosing one approximation that has the highest R-squared for each judicial district. The equations calculated by Excel are $Y=a+b*X$, $Y =a+b*\log(X)$, $Y =a*\exp(b*X)$, $Y =a+b*X+c*X^2$, and $Y =a*X^b$, respectively. Y is the length of trials, and X is year.

¹¹ In order to avoid the measurement error, previous studies used the indicators of judicial enforcement that exclude the cases that have no relation to loan contract. Unfortunately, we could not obtain the data on the length of trials by the type of cases. Thus, we had no choice but to use the length of trials in all civil cases. Instead, the second indicator we used excludes work or family-related cases in district courts.

¹² In Japanese, Saiban no Jinsokuka ni kakaru Kensyou ni kansuru Houkokusho (Dai Ni-kai).

<http://www.courts.go.jp/about/siryu/jinsoku/hokoku/02/index.html>

Djankov, La Porta and Lopez-de-Silanes and Shleifer (2003) compared durations of the procedure for the collection of a check returned for non-repayment in 109 countries. Duration in Japan is 60 months and is ranked 7th from the bottom¹³. Thus, the duration of trials in Japan is short even by international standards.

(Insert Figure1 and 2 here)

Next, let me show you the speed of trials geographically in Figure1 and Figure2. Figure1 displays the length of trials in 50 district courts of Japan. The upper figure is for 2000 and the lower is for 2006. As you can see, the length of trials became shorter in 2006 than that in 2000, which means that the quality of judicial enforcement improved across the country. The median was 9 months in 2000, but 7.7 months in 2006. Looking at the graded bars which indicate the length of trials is longer than the median, we can see that the poor judicial enforcement persists in some areas. For example, dividing Japan by 8 areas (Hokkaido, Tohoku, Kanto, Chubu, Kinki, Chugoku, Shikoku, and Kyusyu), the judicial enforcement is worse in Chubu, Kanto and Shikoku. On the other hand, the length of trials in Hokkaido is particularly short both in 2000 and 2006. Figure2 displays the number of pending trials over that of incoming trials, and it has similar pattern with the length of trials. The ratio of pending trials became shorter in 2006 than that in 2000, and it is high especially in Chubu and Kanto. In addition, all the ratios of pending trials are smaller than median in Hokkaido, which means that Hokkaido has a better quality of judicial performance compared to the other areas.

C. Descriptive Statistics

In this section, we present the mean and standard deviation of variables used in our regression. Table1 shows characteristics of loan applicants, but classifies them depending on whether households are rationed or not.¹⁴ The share of all types of rationed households is 21 percent (=149/710), rejected households are 19 percent (=135/710) and reduced households are 2 percent (=17/710).¹⁵ Comparing rationed

¹³ Djankov, La Porta and Lopez-de-Silanes and Shleifer (2003) also calculate the duration of the procedure for the eviction of a residential tenant for nonpayment of rent. The duration in Japan is 363 months and is ranked 87 in 109 countries.

¹⁴ Total observation which includes both loan applicants and non-loan applicants is 6862, and the number of households who were rejected their loan application and/or reduced their loan amount is 161. Thus, only 2 percent (=161/6862) of households are credit constrained on average during the 2003-07. However, if we also include discouraged borrowers in rationed households as well as rejected and reduced households, the share of rationed households is 4 percent (=292/6862). Kohara and Horioka (2006), which used the different waves of the same survey as ours, presented that the share of households that are rejected, reduced and/or discouraged was 7.61 percent in 1993, 9.29 percent in 1998 and 15.40 percent in 2003. Although households we used look less rationed, this difference seems to mainly come from the difference in definition of credit constraints because Kohara and Horioka (2006) defined households that have been credit constrained in the past as credit-constrained households while we defined households that were credit-constrained only in the previous year as credit-constrained households. Under the same definition as that of Kohara and Horioka (2006), the share of credit-constrained households was 10 percent (=128/1226) in 2007, relatively similar to the share of rationed households calculated by Kohara and Horioka (2006).

¹⁵ Some households answered “yes” to both two questions on credit constraints. Thus, the total shares of each type of rationed households (21.4 percent (= (135+17)/710)) are not equal to the share

households with non-rationed households¹⁶, rationed households have lower income, wealth, homeownership, loan, tenure, educational attainment and the gross prefectural domestic product per capita than non-rationed households. In addition, rationed households are more likely to be self-employed and unemployed and less likely to live in metropolitan area. Moreover, rationed households have more children and more repayment of loan over income than non-rationed households. Focusing on legal variables of interest, all means of *Enforcement Quartile1* and *Enforcement Quartile2* are not significantly different between rationed and non-rationed households. However, comparing rejected households with non-rejected households, 2nd *Enforcement Quartile1* is statistically higher in rejected households than in non-rejected households. This means that better judicial enforcement might have a positive effect on the probability of being rationed, consistent with laziness effect. On the other hand, none of *Enforcement Quartile2* is significantly different between rejected and non-rejected households. This might indicate that future duration of trials does not matter in determining credit constraints while past or current duration of trials does in Japan.

(Insert Table1 here)

III. Estimation Method and Results

A. The Probability of Credit Constraints and the Degree of Judicial Enforcement

In this section, we analyze the relation between the degree of judicial enforcement and the probability of being rationed. We focus only on households who applied for their loan during the year because we cannot define households that were rationed by banks for households who did not apply for loan. However, there is a possibility that using the sub-sample causes the sample selection problem because there might be correlation between the unobservable factors that decide the probability of applying for loan and those that decide the probability of credit constraints. Although we conducted the maximum-likelihood probit model with sample selection,¹⁷ the correlation between the error terms of the two equations was not statistically different from zero in any specifications¹⁸. Thus, we regarded the sub-sample of loan-applicants as that does not cause the sample selection problem. Furthermore, we utilized the panel structure of the data and estimated a random effects logit model¹⁹ as well as pooling analysis.

(Insert Table2 here)

of rationed households (20.9 percent ((= (149/710))).

¹⁶ We performed t test on the equality of means of selected variables (*ttest* in STATA 9) and present ed only the results that have statistically significant difference.

¹⁷ We used *heckprob* command in STATA 9.

¹⁸ Magri (2007) also found that they cannot reject the hypothesis that the errors in the two equations are not correlated.

¹⁹ We did the likelihood-ratio test and found that the null hypothesis that the proportion of the total variance contributed by the panel-level variance component equals zero was rejected. This result indicates that the panel-level variance component is important, and thus a random effects logit model is preferable to pooling logit model.

Table 2 shows the estimation results for the probability of credit constraints. The results (1) and (2) are estimated by pooling probit model, where the dependent variable is a dummy variable which equals to one if households are “Rejected” and/or “Reduced” and zero otherwise. The results (3) and (4) are estimated by random effects logit model, using the same dependent variable as in (1) and (2). The results (5) and (9) are estimated by ordered probit model, where the dependent variable takes two if households are “Rejected,” one if households are “Reduced” and zero otherwise²⁰. The marginal effects in (5) are for the probability of outcome two and those in (9) are for the probability of outcome one. The results (6) and (10) are estimated as in (5) and (9), using different legal variables. Lastly, the results (7) and (8) are estimated by random effects logit model, where the dependent variable is a dummy variable which equals to one if households are “Rejected.”

Let us first look at the impact of household characteristics. From the view point of supply side adverse selection, the debt ceiling is lower for young borrowers, and thus an increase in *AGE* relaxes credit constraints. In most cases, *AGE* has a significantly negative effect on the probability of being rationed, as expected. For example, the marginal effects of *AGE* and *AGED* in (3) mean that the probability of being rationed decreases until borrowers are about 41 years old ($=0.0826 / (2 \times 0.0010)$), and then approaches zero. *LINC* has no significant effect in any cases. The insignificance of the marginal effect of *LINC* is contrary to expectation because an increase in income expected to relax credit constraints. Kohara and Horioka (2006) also found that income is not so much important in Japan as a factor determining the probability of being rationed, contrary to empirical evidence in the U.S. *LWEALTH* have a significant negative effect in most cases, as expected because if household assets are higher, the demand for credit decreases and the debt ceiling increase. The marginal effects of *HOME* are never significant, contrary to expectation because homeownership is thought to be the proxy for collateral and for previous good credit records. *LLOAN* has a significant and negative effect on the probability of being rationed. We expected that the higher outstanding loans are, the higher is the probability of being rationed, but this negative effect might reflect that the large amount of outstanding loans functions as an indicator of good credit records in the past. *SELF-EMPLOYED* has a significant and positive effect on the probability of being rationed in some cases, as expected while the marginal effects of *UNEMPLOYED* are never significant. *REPAY*, in most cases, has a significant and positive effect on the probability of being rationed, as expected because the higher *REPAY* is, the lower is the ability of repay. The estimation result for *TENURE* indicates that if tenure is longer, the probability of being rationed decreases, as expected. Kohara and Horioka (2006) found that college graduates are significantly less likely to be borrowing constrained, but in our results, the marginal effect of *COLLEGE* is not significant, which is the same result as that in Jappelli (1990). The interpretation of the marginal effect of *MARRIED* is not straightforward because the default of *MARRIED* is female. Female has an ambiguous effect on the probability of being rationed because their desired borrowing might be lower due to a lower level of expected future income, while their debt ceiling might be lower due to discrimination. Married couples are expected to be less rationed because their desired borrowing might be lower due to economies of scale and their debt ceiling might be higher due to, for

²⁰ If households are “Rejected” and “Reduced,” the dependent variable takes two.

example, less mobility. We found that *MARRIED* have a significant positive effect on the probability of credit constraints, which implies that female has a negative effect on the probability of being rationed and the absolute value of the negative effect is larger than that of married couples. *CHILD* has a significant positive effect, as expected because it is expected that if households have more children, the desired borrowing increases and the credit constraints becomes tight. It seems that city size (*METRO* and *MEDIUM CITY*) does not matter in determining credit constraints. Prefectural variables such as *BAD LOANS* and *HERFINDAHL* have no significant effect on the probability of being rationed, while *PREFECTURAL GDP (DEPTH)* has a significant and negative (positive) effect on the probability of being rationed in two cases.

Turning to legal variables of interest, all *Enforcement Quartile2* have no significant effect, consistent with descriptive evidence presented in Table1. On the other hand, *2nd Enforcement Quartile1* have significant positive effects on the probability of being rationed in (1), (5), (7) and (9). These results imply that better judicial enforcement increases the probability of being rationed, consistent with laziness effects, where under better judicial enforcement, an access to credit is harder for good-type borrowers. On the other hand, the marginal effects of *1st Enforcement Quartile1* are never significant. Note that laziness effects imply that better judicial enforcement makes an access to credit harder for good-type borrowers, while it makes an access to credit easier for bad-type borrowers. The mixture of laziness effects might cause the insignificance of the marginal effects of *1st Enforcement Quartile1*. Nevertheless, the point estimates of *1st Enforcement Quartile* still show a positive difference.

Looking at the magnitude of the marginal effects of *2nd Enforcement Quartile1* in (1), the probability of being rationed is 11 percent points higher in judicial districts with *2nd Enforcement Quartile1* (*2nd best* judicial enforcement) than in judicial districts with the worst judicial enforcement. As shown in C of section II, the share of rationed households is 21 percent, which means that moving from the worst judicial enforcement to *2nd best* judicial enforcement increases the probability of being rationed by 52 percent $(=(11/21)*100)$. The effect on the probability of being rejected loan application is more pronounced. The estimation results (5) and (7) indicate that moving from the worst judicial enforcement to *2nd best* judicial enforcement increases the probability of being rejected loan application by nearly 63 percent $(=(12/19)*100)$ and 137 percent $(=(26/19)*100)$, respectively. Moreover, the estimation results (9) indicate that moving from the worst judicial enforcement to *2nd best* judicial enforcement increases the probability of being reduced credit by roughly 50 percent $(=(1/2)*100)$.

As robustness checks, first, we used the dummy variables of length of trials and those of pending rates in high courts, instead of those in district courts. High courts are located in 8 major cities in Japan, and some high courts have different branches, that are 6 branches throughout Japan. Thus, the judicial data on high courts consist of only 14 categories each year, which mean that the variation in the jurisdiction data on high courts across individuals is very small. As expected, the dummy variables of length of trials and those of pending rates in high courts have no significant effect on the probability of being rationed, rejected and reduced, or the dummy variables are dropped during the estimation due to colinearity. Second, we used the dummy variables of

pending rates in summary courts,²¹ instead of those in district courts. The judicial data on summary courts consist of 50 categories each year, same as that on district courts. The dummy variables of pending rates in summary courts have no significant effect on credit allocation. This is the same results as in Table2 using those in district courts, indicating that the future duration of trials does not matter in determining the probability of being rationed.²² Finally, we previously left discouraged borrowers out of rationed borrowers because they did not apply for a loan, but here we include discouraged borrowers in rationed households. Surprisingly, we found more significant effect of judicial enforcement on the probability of being rationed. More specifically, all *Enforcement Quartile1* have significant and positive effects on the probability of being rationed (rejected, reduced, and/or discouraged) in pooling probit model, and 2nd and 3rd *Enforcement Quartile1* have significant and positive effects on the probability of being rationed in random effects logit model.

B. The Loan Size and the Degree of Judicial Enforcement

The estimation results in A of section III showed that better judicial enforcement increases the probability of being rationed on average. In this section, we examine the effect of the degree of judicial enforcement on the amount of credit granted during the year. 12 percent of loan applicants are not granted any credit. In order to take into account the wide-spread presence of zero, we conducted pooling tobit²³ and random effects tobit estimation.²⁴ The dependent variable is the logarithm of loan size and the explanatory variables are same as in A of section III.

(Insert Table3 here)

Let us first look at the impact of household characteristics in Table3. *LWEALTH* have a significant positive effect in every case, as expected because if households have higher wealth, they can pledge more collateral to lenders. On the other hand, *HOME* has a significant and negative effect on loan size. Contrary to the effect of the amount of wealth, *HOME* might capture less demand of new loan. *LLOAN* has a significant and positive effect on loan size. This positive effect might be caused by simultaneity problem because *LLOAN* includes not only loan granted in the past but also that granted during the year. If we exclude loan granted during the year from *LLOAN*, the marginal effects of *LLOAN* became insignificant and positive in most cases. We also found that *TENURE* is positively related to loan size, consistent with expectation. The negative

²¹ For summary courts, there is no data on length of trials.

²² We also used the level of length of trials and that of the number of pending trials over the number of incoming trials, instead of the dummy variables of them. We found that the marginal effects of the level of length of trials and pending rates are never significant on the probability of being rationed, rejected or reduced. However, this does not necessarily mean that the degree of judicial enforcement has no effect on credit allocation because the variation in the level of length of trials and pending rates is small and the small variation might cause those insignificances.

²³ We utilized the interval regression with left-censoring of observations to estimate our tobit model containing robust standard errors adjusted for clusters in id.

²⁴ The null hypothesis that panel-level variance component equals zero was rejected. This result indicates that the panel-level variance component is important, and thus a random effects tobit model is preferable to pooling tobit model.

effect of *MARRIED* might reflect less demand of loan for married couple because of economies of scale in the consumption of durables. *CHILD* also has a significant and negative effect on loan size. When households have more children, the desired borrowing becomes higher. Our estimation result might indicate that the debt ceiling banks allow might decrease when household have more children.

Turning to legal variables of interest, all *Enforcement Quartile2* have no significant effect. On the other hand, all marginal effects of *Enforcement Quartile 1* are significantly negative. These results indicate again the existence of laziness effect: an improvement of judicial enforcement makes an access to credit harder for good-type borrowers. The magnitude of the effect of change in judicial enforcement is very pronounced. The estimation results in random effects tobit model show that moving from the judicial districts with the worst judicial enforcement to those with the 1st, 2nd and 3rd best judicial enforcement reduces the loan size by 78 percent $((\exp(-1.5215)-1)*100)$, 76 percent $((\exp(-1.4181)-1)*100)$, and 56 percent $((\exp(-0.8137)-1)*100)$, respectively.

As robustness checks, first, we used the level of length of trials and that of the number of pending trials over the number of incoming trials, instead of the dummy variables of them. Although the level of length of trials and that of the number of pending trials has no significant effect on the probability of being rationed, we found that the marginal effects of the level of length of trials are significantly positive on loan size both in pooling tobit model and random effects tobit model, consistent with the results in Table3. Second, we used the dummy variables of length of trials and those of pending rates in high courts, instead of those in district courts. As mentioned in A of section III, these dummy variables have a very small variation. As expected, they have no significant effect on loan size. Finally, we used the dummy variables of pending rates in summary courts²⁵, instead of those in district courts. We found that the dummy variables of pending rates in summary courts have no significant effect on loan size, consistent with the results in Table3.

C. The Proxy of the Probability of Default and the Degree of Judicial Enforcement

The estimation results in A and B of section III imply that better judicial enforcement increases the probability of being rationed and decreases loan size. These results are expected to occur by rationing more good-type borrowers because better judicial enforcement decreases the incentive to screen and evaluate borrowers erroneously. As a result, credit allocation is expected to be worse than when judicial enforcement is worse. If it is correct, the probability of default will increase on average. In order to confirm it, in this section, we analyze the relation between the degree of judicial enforcement and the proxy of the probability of default.

Since we do not have direct information on default, we used the information on whether or not households had difficulties with loan repayment during the year as a proxy variable because the difficulties with loan repayment might imply future default. As explanatory variables that drive household's default, we used three dummy variables that indicate adverse events²⁶ as well as all explanatory variables other than

²⁵ For summary courts, there is no data on length of trials.

²⁶ See, for example, Fay, Hurst and White (2002) for an empirical study that investigated the effect of adverse events on personal bankruptcy.

UNEMPLOYED used in A and B of section III. The three dummy variables are *DIVORCE* (whether the respondents divorced during the year), *LOST JOB* (the respondents or family members lost their jobs during the year)²⁷, and *SICK* (the respondents or their family members got sick during the year). We would have liked to take advantage of the panel data, but the panel logit estimation did not work well because few households had difficulties with loan repayment. Therefore, we chose to conduct the pooling probit estimation.

(Insert Table4 here)

First, looking at the estimation results for household characteristics in Table4, although the level of income does not matter in determining the probability of default, all adverse events (*DIVORCE*, *LOST JOB* and *SICK*), which reduce the ability to repay the loan, have significant and positive effects on the probability of default. The magnitude is also pronounced. *DIVORCE*, *LOST JOB* and *SICK* increase the probability of default by 1.18, 0.38, and 0.12 percent point. Since the share of default households is 0.37 percent, to divorce, to lose job and to get sick increase the probability of default by 319, 103 and 32 percent, respectively. Moreover, *LWEALTH*, *REPAY*, *TENURE*, *MARRIED* and *HERFINDAHL* also have significant effects on the probability of default, but the magnitude of their marginal effects is not so remarkable.

Turning to the effect of legal variables, as expected, *Enforcement Quartile1* has a positive effect on the probability of default. Moving the judicial districts with the worst judicial enforcement to those with 1st and 2nd best judicial enforcement increases the probability of default by 0.08 and 0.07 percent point, respectively. Since the share of default households is 0.37 percent, the probability of default is 22 and 19 percent higher in judicial districts with 1st and 2nd best judicial enforcement than in judicial districts with the worst judicial enforcement, respectively.

As robustness checks, we used the dummy variables of length of trials and those of pending rates in high courts, instead of those in district courts. This time, all *Enforcement Quartile1* have no significant effect on the probability of default, but 1st *Enforcement Quartile2* has a significant and positive effect on the probability of default, which implies that better enforcement increases the probability of default, consistent with the results in Table4. We also used the dummy variables of pending rates in summary courts²⁸, instead of those in district courts. The dummy variables of pending rates in summary courts have no significant effect on the probability of default, consistent with the results in Table4.²⁹

²⁷ In order to use *LOST JOB*, we did not control *UNEMPLOYED* at this time because of their high correlation.

²⁸ For summary courts, there is no data on length of trials.

²⁹ We used the level of length of trials and that of the number of pending trials over the number of incoming trials, instead of the dummy variables of them. We found that the marginal effects of the level of length of trials and pending rates are never significant on the probability of default. However, this does not necessarily mean that the degree of judicial enforcement has no effect on the probability of default due to the same reason in footnote 22.

IV. Conclusion

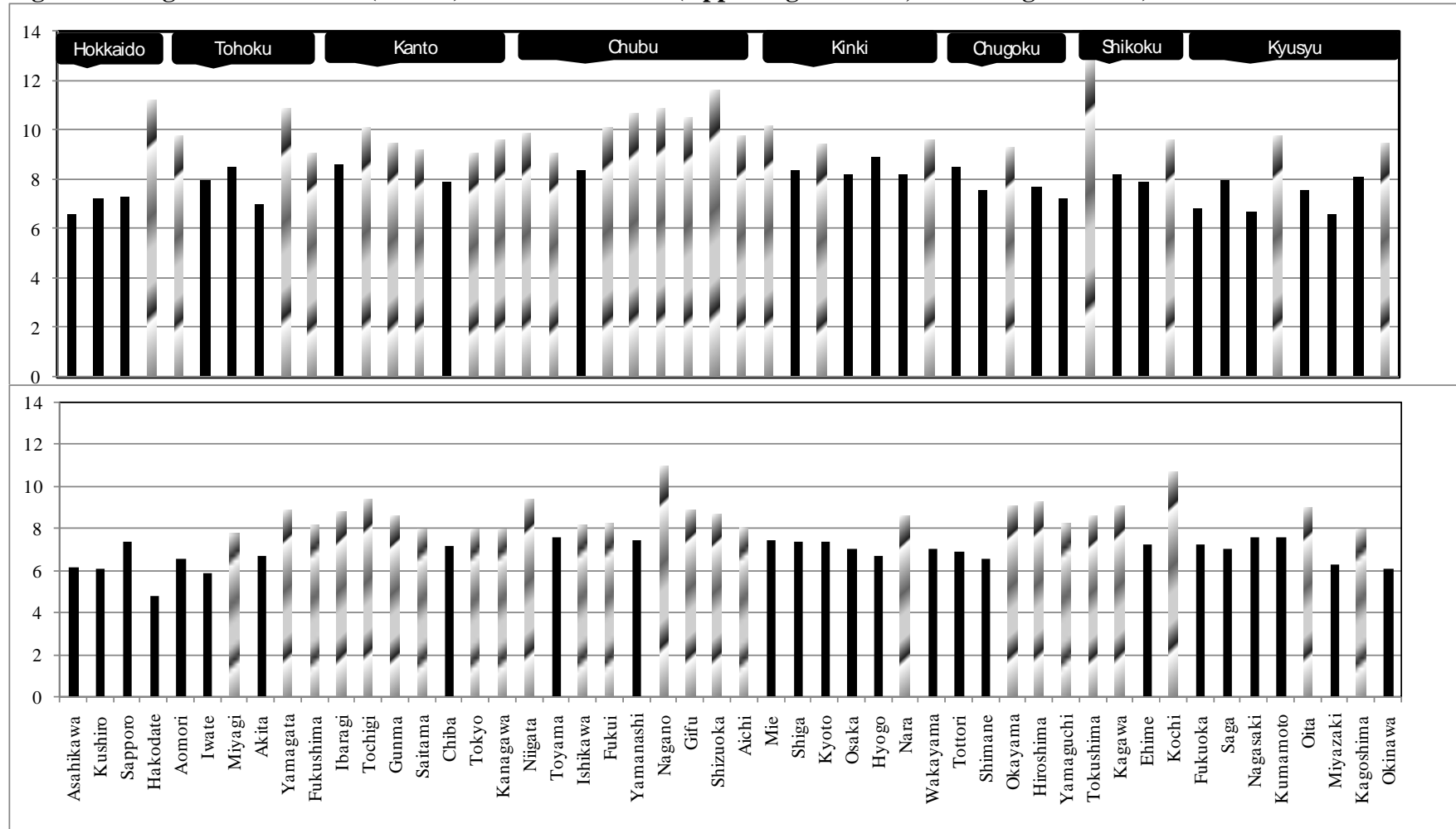
Is better judicial enforcement better for economy and credit market? We tried to answer this question empirically using Japanese household micro data. A traditional view on judicial efficiency of credit markets shows that better judicial enforcement reduces the probability of rationing and raises loan size. On the other hand, laziness effect considerations show that improvements in the judicial enforcement reduce the bank's incentive to screen borrowers adequately and banks might evaluate borrowers erroneously. As a result, an access to credit might be harder (easier) for good-type (bad-type) borrowers. In this case, the effect of the degree of judicial enforcement on credit allocation is ambiguous, but there is a possibility that better judicial enforcement has an opposite effect to traditional view on judicial efficiency. Moreover, worsening credit allocation might lead to higher default rate.

To investigate the effect of the degree of judicial enforcement on the probability of credit constraints, loan size and the probability of default in Japan, we used data on Japanese households drawn from the "Japanese Panel Survey of Consumers" and the data on the court performance by judicial districts. Our estimation results are consistent with laziness effect considerations. Controlling for household characteristics and regional economic situation and credit market activity at prefecture level, we found that better judicial enforcement increases the probability of credit constraints and decreases loan size, which implies that an improvement in judicial enforcement makes an access to credit harder for good-type borrowers. Thus, our results show that better judicial enforcement decreases the availability of credit to households in Japan, contrary to the traditional view. We also found that if the degree of judicial enforcement improves, the probability of default increases. Therefore, our estimation results suggest that better judicial enforcement makes it easier to recover the loan but may be socially harmful through decreasing the credit households can receive and increasing default rate.

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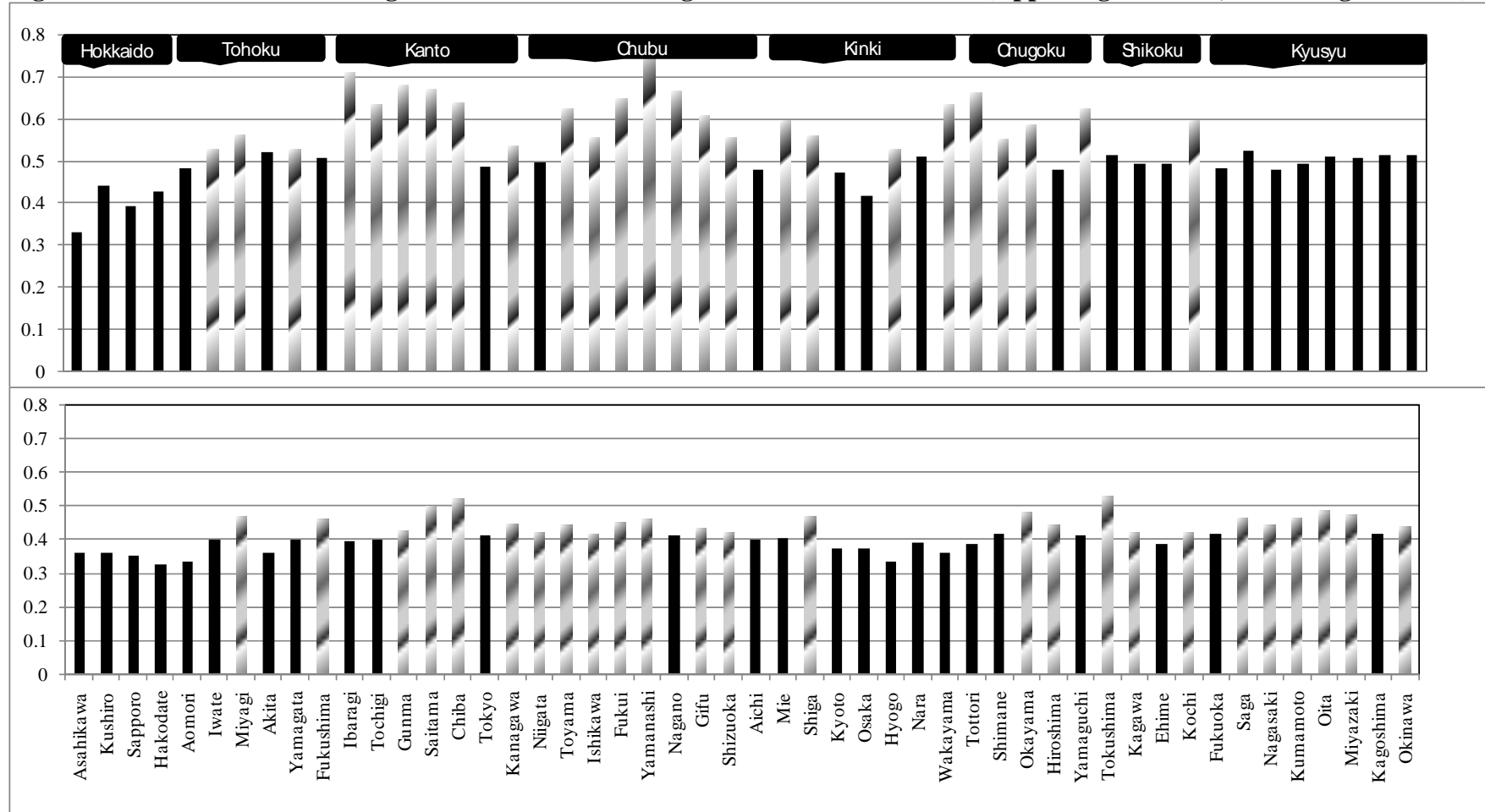
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Figure 1: Length of Civil Trials (Month) in District Courts (Upper Figure: 2000, Lower Figure: 2006)



Source: The public relations division in Supreme Court; Note: Length of trial measures the time elapsing between the date of initial recording of a trial and that of the court sentence. The trials include all civil trials at the first degree of jurisdiction in district courts.

Figure 2: The Number of Pending Trials / That of Incoming Trials in District Courts (Upper Figure: 2000, Lower Figure: 2006)



Source: Annual Report of Judicial Statistics; Note: The trials include all civil trials except for work or family related cases in district courts.

Table1: Characteristics of Households Who Applied for Loan

	Full		Non-Rationed		Rationed		Rejected		Reduced	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
1st Enforcement Quartile1 (dummy variable)	0.25	0.44	0.25	0.44	0.25	0.43	0.23	0.42	0.41	0.51
2nd Enforcement Quartile1 (dummy variable)	0.19	0.39	0.18	0.39	0.23	0.42	0.24	0.43	0.18	0.39
3rd Enforcement Quartile1 (dummy variable)	0.30	0.46	0.30	0.46	0.30	0.46	0.30	0.46	0.24	0.44
1st Enforcement Quartile2 (dummy variable)	0.24	0.43	0.23	0.42	0.29	0.45	0.27	0.44	0.53	0.51
2nd Enforcement Quartile2 (dummy variable)	0.29	0.45	0.30	0.46	0.26	0.44	0.27	0.44	0.12	0.33
3rd Enforcement Quartile2 (dummy variable)	0.22	0.41	0.23	0.42	0.17	0.38	0.18	0.38	0.18	0.39
AGE	37.33	8.00	37.53	7.71	36.58	9.00	36.48	8.82	38.06	10.44
INC (in ten thousands of yen)	26.27	15.16	27.21	14.38	22.73	17.39	22.81	18.05	22.76	8.93
WEALTH (in ten thousands of yen)	1610.16	2063.73	1812.85	2172.03	846.98	1345.35	873.33	1370.04	724.12	1100.11
HOME (dummy variable)	0.60	0.49	0.65	0.48	0.44	0.50	0.44	0.50	0.53	0.51
LOAN (in ten thousands of yen)	956.40	1185.51	1006.01	1195.18	769.62	1132.88	737.46	1101.52	1239.41	1429.65
SELF-EMPLOYED (dummy variable)	0.13	0.33	0.11	0.32	0.19	0.39	0.19	0.39	0.18	0.39
UNEMPLOYED (dummy variable)	0.04	0.20	0.03	0.17	0.09	0.28	0.10	0.30	0.00	0.00
REPAY (in decimal form)	0.35	0.42	0.32	0.38	0.43	0.57	0.39	0.52	0.71	0.80
TENURE (dummy variable)	0.77	0.42	0.82	0.39	0.60	0.49	0.60	0.49	0.59	0.51
COLLEGE (dummy variable)	0.23	0.42	0.25	0.43	0.14	0.35	0.14	0.35	0.18	0.39
MARRIED (dummy variable)	0.77	0.42	0.77	0.42	0.75	0.43	0.76	0.43	0.65	0.49
CHILD	1.62	1.15	1.57	1.13	1.79	1.20	1.81	1.20	1.65	1.11
METRO (dummy variable)	0.23	0.42	0.24	0.43	0.17	0.38	0.19	0.39	0.06	0.24
MEDIUM CITY (dummy variable)	0.65	0.48	0.64	0.48	0.71	0.45	0.70	0.46	0.82	0.39
PREFECTURAL GDP (in billions of yen)	4.37	1.29	4.43	1.32	4.15	1.16	4.18	1.20	3.94	0.51
BAD LOANS (in percent)	5.14	1.92	5.10	1.90	5.29	1.98	5.33	2.01	4.73	1.66
HERFINDAHL (in percent)	54.58	23.82	54.02	23.92	56.70	23.36	57.55	23.82	49.51	17.53
DEPTH (in decimal form)	0.68	0.38	0.69	0.39	0.65	0.34	0.66	0.35	0.61	0.24
Number of Observation	710		561		149		135		17	

Note: 1) The data are from the Japanese Panel Survey of Consumers during 2003-07. 2) “Full” are households that applied for loan during the year. 3) “Non-Rationed” are households that applied for loan and that were not rationed during the year. 4) “Rationed” are households that applied for loan and that were rejected their loan application and/or reduced their loan amount during the year. 5) “Rejected” are household that applied for loan and that were rejected their loan application during the year. 6) “Reduced” are household that applied for loan and that were reduced their loan amount during the year.

Table2: Judicial Enforcement and the Probability of Being Rationed

	Rationed				Rejected				Reduced	
	Pooling Probit		Random Effects Logit		Pooling Ordered Probit		Random Effects Logit		Pooling Ordered Probit	
	Marginal Effect (Standard Error)		Marginal Effect (Standard Error)		Marginal Effect (Standard Error)		Marginal Effect (Standard Error)		Marginal Effect (Standard Error)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1st Enforcement Quartile1	0.0357 (0.0635)		0.0514 (0.1415)		0.0431 (0.0591)		0.0978 (0.1643)		0.0040 (0.0053)	
2nd Enforcement Quartile1	0.1112 ** (0.0630)		0.1514 (0.1383)		0.1241 ** (0.0603)		0.2567 * (0.1526)		0.0101 ** (0.0049)	
3rd Enforcement Quartile1	0.0579 (0.0481)		0.1081 (0.1127)		0.0544 (0.0438)		0.1039 (0.1137)		0.0050 (0.0042)	
1st Enforcement Quartile2		-0.0175 (0.0600)		-0.0519 (0.1436)		-0.0106 (0.0568)		-0.0033 (0.1530)		-0.0010 (0.0056)
2nd Enforcement Quartile2		-0.0389 (0.0424)		-0.0785 (0.1066)		-0.0291 (0.0390)		-0.0369 (0.1130)		-0.0029 (0.0040)
3rd Enforcement Quartile2		-0.0450 (0.0438)		-0.0740 (0.1003)		-0.0386 (0.0394)		-0.0836 (0.1023)		-0.0039 (0.0042)
AGE	-0.0459 *** (0.0183)	-0.0461 *** (0.0183)	-0.0826 * (0.0451)	-0.0816 * (0.0448)	-0.0365 ** (0.0161)	-0.0360 ** (0.0164)	-0.0724 (0.0495)	-0.0688 (0.0488)	-0.0036 ** (0.0018)	-0.0035 ** (0.0017)
AGED	0.0006 *** (0.0002)	0.0006 *** (0.0002)	0.0010 * (0.0006)	0.0010 * (0.0006)	0.0004 ** (0.0002)	0.0004 ** (0.0002)	0.0009 (0.0006)	0.0008 (0.0006)	0.0000 ** (0.0000)	0.0000 ** (0.0000)
LINC	0.0030 (0.0067)	0.0035 (0.0069)	0.0051 (0.0174)	0.0054 (0.0177)	0.0008 (0.0061)	0.0014 (0.0063)	-0.0051 (0.0170)	-0.0031 (0.0170)	0.0001 (0.0006)	0.0001 (0.0006)
LWEALTH	-0.0098 *** (0.0039)	-0.0095 *** (0.0039)	-0.0156 * (0.0094)	-0.0152 (0.0095)	-0.0093 *** (0.0035)	-0.0090 *** (0.0035)	-0.0181 ** (0.0094)	-0.0175 * (0.0096)	-0.0009 ** (0.0004)	-0.0009 ** (0.0004)
HOME	0.0205 (0.0528)	0.0189 (0.0532)	-0.0135 (0.1333)	-0.0139 (0.1337)	0.0249 (0.0464)	0.0235 (0.0471)	0.0529 (0.1302)	0.0539 (0.1263)	0.0025 (0.0046)	0.0023 (0.0046)
LLOAN	-0.0364 *** (0.0053)	-0.0356 *** (0.0053)	-0.0742 *** (0.0218)	-0.0726 *** (0.0224)	-0.0339 *** (0.0048)	-0.0330 *** (0.0048)	-0.0810 *** (0.0216)	-0.0776 *** (0.0229)	-0.0033 *** (0.0010)	-0.0032 *** (0.0009)
SELF-EMPLOYED	0.0749 (0.0537)	0.0929 * (0.0547)	0.1505 (0.1236)	0.1751 (0.1183)	0.0687 (0.0473)	0.0870 * (0.0491)	0.1122 (0.1136)	0.1458 (0.1116)	0.0060 (0.0043)	0.0072 * (0.0044)

UNEMPLOYED	0.0019 (0.0907)	0.0042 (0.0921)	0.0730 (0.2364)	0.0744 (0.2360)	0.0114 (0.0874)	0.0167 (0.0906)	0.1220 (0.2393)	0.1395 (0.2367)	0.0011 (0.0081)	0.0016 (0.0082)
REPAY	0.1316 *** (0.0413)	0.1272 *** (0.0408)	0.2284 ** (0.1085)	0.2151 ** (0.1112)	0.1000 *** (0.0343)	0.0959 *** (0.0340)	0.1457 (0.1023)	0.1347 (0.1037)	0.0098 ** (0.0042)	0.0093 ** (0.0041)
TENURE	-0.1397 *** (0.0553)	-0.1489 *** (0.0562)	-0.2774 ** (0.1158)	-0.2888 ** (0.1206)	-0.1121 ** (0.0500)	-0.1212 ** (0.0513)	-0.1969 * (0.1200)	-0.2134 * (0.1200)	-0.0095 ** (0.0045)	-0.0100 ** (0.0045)
COLLEGE	-0.0066 (0.0475)	-0.0111 (0.0475)	-0.0425 (0.1168)	-0.0516 (0.1160)	-0.0067 (0.0433)	-0.0095 (0.0434)	-0.0588 (0.1224)	-0.0621 (0.1192)	-0.0007 (0.0043)	-0.0009 (0.0043)
MARRIED	0.0873 ** (0.0412)	0.0859 * (0.0420)	0.1915 * (0.1063)	0.1880 * (0.1088)	0.0832 ** (0.0366)	0.0818 ** (0.0375)	0.2384 ** (0.1078)	0.2288 ** (0.1112)	0.0089 ** (0.0046)	0.0087 * (0.0047)
CHILD	0.0428 ** (0.0187)	0.0410 ** (0.0187)	0.0895 ** (0.0435)	0.0857 ** (0.0436)	0.0393 ** (0.0172)	0.0379 ** (0.0174)	0.1000 ** (0.0472)	0.0966 ** (0.0472)	0.0038 ** (0.0020)	0.0037 ** (0.0019)
METRO	-0.0221 (0.0565)	-0.0187 (0.0588)	-0.0496 (0.1482)	-0.0436 (0.1537)	-0.0133 (0.0527)	-0.0122 (0.0548)	0.0359 (0.1652)	0.0312 (0.1670)	-0.0013 (0.0053)	-0.0012 (0.0054)
MEDIUM CITY	0.0177 (0.0444)	0.0142 (0.0451)	0.0267 (0.1106)	0.0159 (0.1093)	0.0202 (0.0398)	0.0162 (0.0407)	0.0593 (0.1210)	0.0468 (0.1172)	0.0020 (0.0041)	0.0016 (0.0041)
PREFECTURAL GDP	-0.0685 * (0.0368)	-0.0625 * (0.0353)	-0.1360 (0.0908)	-0.1209 (0.0898)	-0.0529 (0.0337)	-0.0504 (0.0326)	-0.0847 (0.0988)	-0.0835 (0.0960)	-0.0052 (0.0036)	-0.0049 (0.0034)
BAD LOANS	-0.0058 (0.0111)	-0.0076 (0.0108)	-0.0136 (0.0265)	-0.0162 (0.0257)	-0.0009 (0.0096)	-0.0026 (0.0094)	0.0128 (0.0269)	0.0089 (0.0254)	-0.0001 (0.0009)	-0.0002 (0.0009)
HERFINDAHL	-0.0002 (0.0011)	0.0000 (0.0011)	-0.0002 (0.0023)	0.0001 (0.0022)	-0.0000 (0.0009)	0.0001 (0.0010)	0.0007 (0.0023)	0.0012 (0.0023)	-0.0000 (0.0001)	0.0000 (0.0001)
DEPTH	0.2122 * (0.1318)	0.2302 * (0.1281)	0.4116 (0.3539)	0.4339 (0.3457)	0.1571 (0.1210)	0.1815 (0.1188)	0.2465 (0.3743)	0.3084 (0.3579)	0.0154 (0.0121)	0.0175 (0.0120)
No. of Observation	710	710	710	710	710	710	710	710	710	710
Wald Chi2 (33)	153.20	141.96	85.55	87.66	160.96	146.65	72.23	75.32	160.96	146.65
Prob>Chi2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Log Likelihood	-281.14703	-282.57185	-272.80040	-273.48984	-327.88186	-330.08624	-259.39454	-261.58756	-327.88186	-330.08624
Pseudo R-squared	0.2293	0.2254			0.2026	0.1973			0.2026	0.1973
LR test of rho=0:chibar2(01)			16.52	17.93			13.73	15.59		
Prob>=Chibar2			0.000	0.000			0.000	0.000		

Note: 1) The dependent variables in (1)-(4) are dummy variables that equal to one if households were rejected their loan application and/or reduced their loan amount during the year and zero otherwise. 2) The dependent variables in (5)-(8) are dummy variables that equal to one if households were rejected their loan application during the year and zero otherwise. 3) The dependent variables in (9)-(10) are dummy variables that equal to one if households were reduced their loan amount during the year and zero otherwise. 4) We also controlled seven area dummies and four year dummies. 5) Marginal effects are evaluated at the means. 6) Standard errors in pooling ordered probit estimation are adjusted for clustering on id. 7) Standard errors in random effects logit estimation are bootstrapped. 8) *, ** and *** indicate significant at the 10%, 5%, and 1% level, respectively.

Table3: Judicial Enforcement and Loan Size

	Pooling Tobit		Random Effects Tobit	
	Marginal Effect (Standard Error)		Marginal Effect (Standard Error)	
1st Enforcement Quartile1	-1.6918 *** (0.6617)		-1.5215 *** (0.5158)	
2nd Enforcement Quartile1	-1.7145 *** (0.5832)		-1.4181 *** (0.4798)	
3rd Enforcement Quartile1	-0.9435 ** (0.4146)		-0.8137 ** (0.3559)	
1st Enforcement Quartile2		0.2255 (0.7102)		0.1571 (0.6544)
2nd Enforcement Quartile2		0.2340 (0.5237)		0.2044 (0.5327)
3rd Enforcement Quartile2		0.6300 (0.4669)		0.4304 (0.4411)
AGE	0.2169 (0.2128)	0.2039 (0.2122)	0.1905 (0.1795)	0.1741 (0.1782)
AGED	-0.0024 (0.0026)	-0.0022 (0.0026)	-0.0021 (0.0022)	-0.0019 (0.0022)
LINC	0.0361 (0.0820)	0.0266 (0.0821)	0.0398 (0.0726)	0.0323 (0.0732)
LWEALTH	0.1381 *** (0.0394)	0.1354 *** (0.0397)	0.1026 *** (0.0364)	0.0995 *** (0.0361)
HOME	-1.6883 *** (0.4209)	-1.6963 *** (0.4296)	-1.3240 *** (0.4367)	-1.3273 *** (0.4290)
LLOAN	0.9339 *** (0.0693)	0.9297 *** (0.0699)	0.8399 *** (0.0451)	0.8379 *** (0.0463)
SELF-EMPLOYED	0.5594 (0.5836)	0.4693 (0.5861)	0.4663 (0.5616)	0.4020 (0.5468)
UNEMPLOYED	-0.1760 (1.4252)	-0.2481 (1.4455)	-0.2498 (1.2855)	-0.2945 (1.2659)
REPAY	-0.1075 (0.2663)	-0.1267 (0.2718)	0.0790 (0.3213)	0.0502 (0.3362)
TENURE	0.7798 (0.5270)	0.8574 * (0.5342)	0.6576 (0.4487)	0.7454 * (0.4509)
COLLEGE	0.3982 (0.3555)	0.4334 (0.3514)	0.4371 (0.3532)	0.4611 (0.3656)
MARRIED	-0.9992 * (0.5393)	-1.0121 * (0.5523)	-0.8612 * (0.4689)	-0.8681 * (0.4839)
CHILD	-0.4378 ** (0.2234)	-0.4257 ** (0.2223)	-0.4080 ** (0.1811)	-0.4074 ** (0.1815)
METRO	0.4304 (0.6164)	0.3543 (0.6354)	0.3787 (0.5592)	0.3095 (0.5592)
MEDIUM CITY	0.2369 (0.5154)	0.2411 (0.5252)	0.2153 (0.4249)	0.2257 (0.4200)
PREFECTURAL GDP	-0.1740 (0.3917)	0.0145 (0.3574)	-0.2106 (0.4045)	-0.0332 (0.3341)
BAD LOANS	0.0218 (0.1091)	0.0741 (0.1054)	0.0126 (0.1088)	0.0586 (0.1077)

HERFINDAHL	-0.0029 (0.0120)	0.0007 (0.0111)	-0.0041 (0.0119)	-0.0004 (0.0107)
DEPTH	0.7346 (1.3432)	-0.1304 (1.2800)	0.7855 (1.3922)	0.0267 (1.2463)
No. of Observation	710	710	710	710
Wald Chi2 (33)	337.08	326.05	343.99	359.68
Prob>Chi2	0.0000	0.0000	0.0000	0.0000
Log Likelihood	-1868.8176	-1872.3801	-1860.7007	-1864.3334
LR test of sigma_u=0: chibar2(01)			16.23	16.09
Prob>=Chibar2			0.000	0.000

Note: 1) The dependent variable is the logarithm of loan granted during the year. 2) We also controlled seven area dummies and four year dummies. 3) Marginal effects are evaluated at the means. 4) Standard errors in pooling tobit estimation are adjusted for clustering on id. 5) Standard errors in random effects tobit estimation are bootstrapped. 6) * , ** and *** indicate significant at the 10%, 5%, and 1% level, respectively.

Table4: Judicial Enforcement and the Probability of Default

	Pooling Probit	
	Marginal Effect (Standard Error)	Marginal Effect (Standard Error)
1st Enforcement Quartile1	0.00080 *	
	(0.00087)	
2nd Enforcement Quartile1	0.00073 **	
	(0.00064)	
3rd Enforcement Quartile1	-0.00001	
	(0.00011)	
1st Enforcement Quartile2		0.00033
		(0.00039)
2nd Enforcement Quartile2		0.00005
		(0.00017)
3rd Enforcement Quartile2		-0.00006
		(0.00015)
DIVORCE	0.01175 ***	0.01155 ***
	(0.00953)	(0.00893)
LOST JOB	0.00378 ***	0.00379 ***
	(0.00352)	(0.00351)
SICK	0.00123 ***	0.00150 ***
	(0.00076)	(0.00089)
AGE	-0.00006	-0.00008
	(0.0007)	(0.00009)
AGED	0.00001	0.00001
	(0.00001)	(0.00001)
LINC	-0.00000	-0.00000
	(0.00001)	(0.00002)
LWEALTH	-0.00004 ***	-0.00005 ***
	(0.00003)	(0.00003)
HOME	0.00010	0.00011
	(0.00022)	(0.00026)
LLOAN	0.00001	0.00002
	(0.00001)	(0.00001)
SELF-EMPLOYED	0.00038	0.00048
	(0.00048)	(0.00058)
REPAY	0.00008 **	0.00010 **
	(0.00007)	(0.00008)
TENURE	-0.00095 ***	-0.00117 ***
	(0.00077)	(0.00095)
COLLEGE	-0.00008	-0.00117
	(0.00013)	(0.00095)
MARRIED	0.00032 ***	0.00037 ***
	(0.00024)	(0.00026)
CHILD	0.00004	0.00005
	(0.00005)	(0.00006)
METRO	0.00023	0.00020
	(0.00041)	(0.00042)

MEDIUM CITY	0.00002 (0.00018)	-0.00000 (0.00023)
PREFECTURAL GDP	-0.00002 (0.00013)	-0.00010 (0.00014)
BAD LOANS	0.00002 (0.00004)	0.00000 (0.00004)
HERFINDAHL	-0.00001 *** (0.00001)	-0.00001 *** (0.00001)
DEPTH	-0.00024 (0.00040)	-0.00006 (0.00001)
No. of Observation	6222	6222
Wald Chi2 (35)	258.34	316.34
Prob>Chi2	0.0000	0.0000
Log Pseudolikelihood	-97.012608	-98.269727
Pseudo R-squared	0.3608	0.3525

Note: 1) The dependent variable is a dummy variable that equals to one if households had difficulties with loan repayment during the year and zero otherwise. 2) We also controlled seven area dummies and four year dummies. 2) Marginal effects are evaluated at the means. 3) Standard errors are adjusted for clustering on id. 4) * , ** and *** indicate significant at the 10%, 5%, and 1% level, respectively.