Lending Pro-Cyclicality and Macro-Prudential Policy: Evidence from Japanese LTV Ratios†

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This version: December 2013

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† This study is a product of the project "Design of Interfirm Networks to Achieve Sustainable Economic Growth" under the program for Promoting Social Science Research Aimed at Solutions of Near-Future Problems conducted by the Japan Society for the Promotion of Science (JSPS) and the Research project *Research on Efficient Corporate Financing and Inter-firm Networks* at the Research Institute of Economy, Trade, and Industry (RIETI). Earlier versions of this paper have been presented at the HIT-TDB-RIETI international workshop, Hitotsubashi University, Bank of Japan, Kobe University, DBJ-RICF, the Monetary Economics Workshop, 2013 FMA European meeting in Luxembourg, the Federal Reserve Bank of San Francisco and the Stockholm School of Economics' House of Finance. The authors thank Eijiro Fukuda, Tetsuro Furuta, Mariassunta Giannetti, Masaharu Hanazaki, Takashi Hatakeda, Masahiro Hori, Tomohiko Inui, Keiichiro Kobayashi, Toshitaka Komoda, Daisuke Miyakawa, Jun-Ichi Nakamura, Koji Nakamura, Makoto Saito, Toshiyuki Soma, Basak Tanyeri, Yoshiro Tsutsui, Mitsuhiro Usui, Yukihiro Yasuda, Koichi Yoshimura and Yuki Teranishi for useful comments. The authors are also grateful to Chihiro Shimizu and Yumi Saita for providing land price data, Makoto Hazama, Takaya Kimiwada, Wakana Toyama, Sohei Mizohata, Kana Matsuda, and Charee Kwak for superb research assistance, and Teikoku Databank, Ltd. for data provision. H. Uchida thanks the Grant-in-Aid for Scientific Research (B, #24330103) from JSPS for financial support. The views expressed in this paper are ours and do not necessarily reflect those of any of the institutions with which we are affiliated.

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Abstract

Using a large and unique micro dataset compiled from the official real estate registry in Japan, we examine the loan-to-value (LTV) ratios for business loans from 1975 to 2009 to draw some implications for the ongoing debate on the use of LTV ratio caps as a macro-prudential policy measure. We find that the LTV ratio exhibits counter-cyclicality, implying that the increase (decrease) in loan volume is smaller than the increase (decrease) in land values during booms (busts). Most importantly, LTV ratios are at their lowest during the bubble period in the late 1980s and early 1990s. The counter-cyclicality of LTV ratios is robust to controlling for various characteristics of loans, borrowers, and lenders. We also find that borrowers that exhibited high-LTV loans performed no worse ex-post than those with lower LTV loans, and sometimes performed better during the bubble period. Our findings imply that a simple fixed cap on LTV ratios might not only be ineffective in curbing loan volume in boom periods but also inhibit well-performing firms from borrowing. This casts doubt on the efficacy of employing a simple LTV cap as an effective macro-prudential policy measure.

Keywords: loan-to-value (LTV) ratios, pro-cyclicality, macro-prudential policy, bubble

JEL classification codes: G28, R33, G21, G32

1. Introduction

The recent financial crisis with its epicenter in the U.S. followed a disastrous financial crisis in Japan more than a decade before. It is probably not an exaggeration to argue that these crises shattered the illusion that the Basel framework – specifically Basel I and Basel II – had ushered in a new era of financial stability. These two crises centered on bubbles that affected both the business sector (business loans) and the household sector (residential mortgages). In Japan banks mostly suffered from the damage in the business sector, while in the U.S. banks mostly suffered from damage in the household sector. Following the first of these crises, the Japanese crisis, a search began for policy tools that would reduce the probability of future crises and minimize the damage when they occur. Consensus began to build in favor of countercyclical macro-prudential policy levers (e.g., Kashyap and Stein 2004). For example, there was great interest and optimism associated with the introduction by the Bank of Spain of dynamic loan loss provisioning in 2000. Also, Basel III adopted a countercyclical capital buffer to be implemented when regulators sensed that credit growth has become excessive.[1]

More recently, however, doubt has emerged about these tools. Not only did dynamic loan loss provisioning fail to prevent the Spanish banking crisis, new evidence suggests that it may even have promoted risk-taking (Illueca, Norden and Udell 2012). Likewise, doubts about capital requirements in general as a macro-prudential tool to smooth credit fluctuations have also been raised in light of "leaks" in the banking system including the existence of shadow banking (Aiyar, Calomiris and Wieladek 2012, Kim and Mangla 2013).

Our focus is on a macro-prudential policy lever of another kind that has received a great deal of attention recently – caps on LTV (loan-to-value) ratios. As a macro-prudential policy tool LTV caps are designed to accomplish two objectives: dampen the acceleration of asset prices during a bubble period (the pricing channel) and limit the build-up of systemic risk in the financial system

[1] This abstracts from an ongoing debate over the interaction of monetary policy and macro-prudential policy in achieving financial stability (e.g., Woodford 2012, Svensson 2012, Crowe et al. 2013, Maddaloni and Peydró 2013, Suh 2013).

due to highly leveraged loans (the risk channel). In addition, LTV caps have a micro-prudential effect on the build-up risk of risk at the individual bank level. Specifically in this paper we look retrospectively at real estate-based lending in the business sector in Japan during the bubble period (i.e., the run-up in real estate pricing prior to the Japanese financial crisis) and the bust period that followed. Our goal is to analyze the efficacy of an LTV cap in terms of the risk channel and business lending in Japan. Specifically we assess whether a simple LTV cap would have worked in Japan in a big part of the sector where most of the systemic damage occurred during the crisis – the business sector.[2]

We focus on the business sector for three reasons. First, as we mentioned above, unlike in the U.S., losses in real estate-based business lending was a dominant driver in the Japanese financial crisis. Thus, the relevant counterfactual in terms of macro-prudential policy tools in Japan is whether LTV caps would have worked in *business* lending. In particular, we look at business lending to nonfinancial firms that are not in the real estate business.[3] Second, despite the nearly exclusive policy focus so far on residential mortgage LTVs, LTV caps can be applied to many other types of loans including business lending. This includes, in particular, business lending secured by real estate – the focus of our paper. During the Japanese bubble period this was the most common form of SME (small and midsized enterprise) financing – so common, in fact, that it was referred to as the "collateral principle" in Japan. And, third, an analysis of the efficacy of LTV caps in business lending may also shed some light on the efficacy of residential and commercial mortgage LTV caps. However, on this score we will be careful to qualify the applicability of our findings in the business sector to the efficacy of LTV caps in the residential and commercial mortgage markets.

By way of preview our results suggest that a simple (i.e., unconditional) LTV cap would not

[2] While we focus on a simple (i.e., unconditional) LTV cap, not all LTV cap proposals are of this sform. Some proposals advocate implementing LTV caps that change in a countercyclical fashion by linking them, for example, to housing prices (e.g., Crowe et al. 2013). Our analysis could be viewed as an investigation into whether simple LTV caps should be rejected in favor of conditional LTV caps.

[3] Our data exclude real estate firms, the other component of the "business" sector that suffered significant losses during the Japanese financial crisis.

have been effective if it had been (counterfactually) implemented in Japan during the bubble period. In our univariate tests that reach back to the beginning of the real estate bubble we find, surprisingly, that the LTV ratio was countercyclical, not pro-cyclical. This finding of pro-cyclicality holds even in a multivariate analysis in which we control for loan characteristics, firm characteristics, lender characteristics, and key policy variables (although data limitations do not permit a multivariate analysis that spans the entire pre-bubble/post-bubble business cyclical as we conducted in our univariate analysis). Taken together our univariate and multivariate tests on the cyclicality of LTV ratios indicate that the imposition of an unconditional cap at the beginning of the bubble period would not have been binding, and thus not effective as a counter-cyclical macro-prudential policy tool. We also examine the relationship between LTV ratios and borrower ex post performance. Here we find another surprising result: the performance of high LTV loans was no worse than that of low LTV loans – and, some cases better. This finding suggests that an unconditional LTV cap may not target the intended borrowers and could even a negative effect by discriminating against higher quality firms.

The remainder of our paper is composed as follows. The next section provides some context for our analysis of LTV ratios and business lending during the real estate bubble in Japan. Section 3 provides details on our data. Section 4 analyzes the cyclicality of LTV ratios. Section 5 investigates the ex post performance of high versus low LTV loans. Section 6 concludes with a discussion of the policy implications of our findings.

2. The Context: LTV Caps, Business Lending and Japanese Financial Crisis

The Japanese experience shares a common feature with the U.S. financial crisis in that they both reflect the historical pattern that credit booms and busts are often accompanied by surges in real estate prices. In both cases the bubble resulted in a significant build-up of non-performing loans. In Japan banks mostly suffered from the damage that occurred in the business sector, while in the U.S. banks suffered from the damage in the household sector (i.e., residential mortgages). It is often claimed that these surges invite excessive risk-taking based on lax bank lending standards including in real estate lending (e.g., Borio et al., 2001; Horvath, 2002; Borio and Lowe; 2002: Berger and Udell 2004). Loan-to-value (LTV) ratios - the ratio of the amount of a loan to the value of assets pledged as collateral - are a key measure of this behavior, because these ratios capture a major component of lenders' risk exposure. LTV ratios also play an important role in the mechanism of the amplification of shocks to borrowers within an economy, and might amplify the effect of income shocks on the housing market (Stein 1995).[5] For example, using different country-level or US city-level panel data, multiple studies consistently find that the effects of income shocks on house prices and/or mortgage borrowing are larger in countries and in periods where LTV ratios are higher (Almeida, Campello, and Liu 2006, IMF 2011, Lamont and Stein 1995, Lim et al. 2011, Stein 1995). These studies indicate that the strength of a "financial accelerator" mechanism is positively associated with LTV ratios.

Our analysis of LTV caps in Japan, however, will be primarily focused on the risk channel and not the pricing channel. That is, our analysis primarily focuses on whether simple LTV caps in business lending would have dampened the build-up of systemic risk in the banking system. While our data are not well-suited to analyze the pricing channel, they offer a unique opportunity to analyze the risk channel. This is because the purpose of most of the loans in our sample is not to finance the purchase of the real estate that secures the loan. As we will discuss later, the bulk of the loans in our sample are likely used to finance working capital (i.e., accounts receivable and inventory) -- even though they are secured by real estate. For these loans any price channel effect is, at most, indirect. However, in one test we use a subset of our loans to offer some results that may be suggestive in terms of the pricing channel.

In the policy arena, efforts are underway to construct an effective framework to deal with excessively risky secured loans, and/or to block the banking sector's amplification of shocks to the market and/or the economy. Imposing restrictions (caps) on LTV ratios is one of the most

[5] Some studies, including Stein (1995), focus on down-payment ratios, which are the inverse of LTV ratios.

prominent macro-prudential policy proposals designed to achieve these goals (see, for instance, FSB 2012).[7] In fact, restrictions on LTV ratios have already been applied in a number of countries in an anticipation of future real estate booms and busts. According to a survey conducted by the IMF in 2010, 20 out of 49 countries, especially those in Asia (Hong Kong, Korea, Singapore, etc.) and Europe (Norway, Sweden, etc.), use caps on LTV ratios as a macro-prudential instrument (Crowe, et al. 2013, Lim, et al. 2011), and among these 20 countries, 11 countries set fixed caps while 9 countries adopt time-varying caps (Lim et al. 2011). Some countries do not directly impose hard limits on LTV ratios, but try instead to incentivize low LTV loans by setting lower capital charges on loans with lower LTV ratios (FSB 2011).

While the current debate on LTV caps, as we mentioned above, is centered on residential mortgages, there is much to be learned by studying the LTV ratios in business lending.[9] In most countries (Berger and Udell 2006, Beck, Demirguc-Kunt, and Martinez Peria, 2008) real estate is very often pledged as collateral in general business lending. This is common even when the purpose of the loan is not related to the purchase of the real estate (Berger and Udell 2006). In fact, excessive bank risk-taking in business lending secured by real estate is considered one of the primary causes of the credit bubble and the bad loan problems in Japan during the late 1980s through the 1990s (e.g., Ueda 2000).[10] During the bubble period, banks were thought to have underwritten business loans with lax lending standards, in anticipation of further surges in real estate prices. In our exploration of the efficacy of LTV caps as a macro-prudential tool, we exploit this feature of business lending by asking whether a cap on the LTV ratio in business lending could

[7] FSB (2012) states that "(f)rom an historical perspective, high-LTV ratio loans consistently perform worse than those with a high proportion of initial equity. While it is common for individual lenders to apply a cap on LTV ratios, it is not necessary for regulators and supervisors to mandate such a cap if they satisfy themselves that the underwriting standards are sufficiently prudent and are unlikely to be eroded under competitive pressure. However, jurisdictions may consider imposing or incentivising limits on LTV ratios according to specific national circumstances."

[9] Policy consideration of LTV ratios has been virtually entirely focused on real estate lending. However, it is interesting to note that LTV ratios – and by extension LTV caps –could be applied to other types of lending. This includes consumer lending to finance, for example, automobile purchases. It could also be applied to other types of business where commercial lenders typical set policies on LTV ratios in lending against accounts receivable, inventory and equipment (Udell 2004).

[10] Several studies find evidence suggesting that land pledged as collateral plays a critical role in removing borrowing constraint for firms in Japan (e.g., Ogawa et al. 1996, Kwon 1998, Ogawa and Suzuki 2000).

have curbed banks' excessive risk-taking during the bubble period in Japan.

To the best of our knowledge, empirical evidence on LTV ratios, especially for business loans, is sparse. We are not aware of any studies using disaggregated data that examine how business LTV ratios evolve throughout the business or credit cycle. As a result, it is unclear even whether this LTV ratio is pro-cyclical.[11] Moreover, no studies have directly examined the anecdotes that banks in Japan actually set higher LTV ratios and took excessive risk during the bubble period (see, for instance, Yoshida 1994). It is therefore impossible to infer whether a cap on LTV ratios could have constrained banks' risk taking and thus weakened the link between real estate prices and bank lending.

In analyzing the evolution of LTV ratios, we also investigate for the first time the characteristics of business borrowers who obtain high LTV loans. This allows us to analyze the possibility that LTV caps might counterproductively restrict the availability of credit for creditworthy borrowers.

Specifically, this paper examines the evolution of LTV ratios and the ex post performance of borrowers, using unique loan-level data from Japan's official real estate registry. The data include detailed information on over 400,000 business loans secured by real estate established from 1975 to 2009. Of particular importance is the information on loan amounts and the identity of land pledged as collateral. Following a widely used approach in the field of real estate economics, we measure land values by estimating an hedonic model, and use them together with the amount of loans secured by the land pledged as collateral to calculate LTV ratios. Using these ratios, we investigate their cyclicality and the relationship between these ratios and the ex-post performance of borrowers. Despite the richness of this data set, there are some shortcomings in our data. The most important of these is that our data are synthetic in nature and the real estate collateral that we observe in the registry is limited to collateral that still appears in the registry in 2008 or afterwards.

[11] There are some studies that have examined the relationship between *aggregate* lending and property prices and the implications of imposing an LTV cap (e.g., Gerlach and Peng 2005, Iacoviello 2005, Igan and Kang 2011).

This includes, however, collateral that was initially registered from 1975 to 2009. This might create survivorship bias in our analysis in that it excludes any collateral that was removed from the registry prior to 2008. However, our data are rich in information about loan, firm, and lender characteristics, which allow us to control for this bias.

Our finding that the LTV ratio exhibits counter-cyclicality is especially intriguing because both the numerator (the amount of loans) and the denominator (the value of the land pledged as collateral) exhibit *pro*-cyclicality. As we noted above the counter-cyclicality of the LTV ratio is also robust to controlling for various loan-, borrower-, and lender characteristics, and thus to controlling for survivorship bias.

On the secondary focus of our paper, the relationship between LTV ratios and borrower ex post performance, our findings are equally intriguing. Using several versions of a DID (difference-in-differences) approach, we compare the ex post performance of borrowing firms that obtained high LTV loans (treatment group) with those that obtained low LTV loans (control group). Our findings consistently show that the performance of high LTV borrowers is not poorer on average. Rather, we find that their performance is sometimes better than that of low LTV borrowers at the end of the bubble period.

To put our results into context, conventional wisdom on the Japanese bubble period argues that lax loan underwriting standards by Japanese banks ultimately led to massive bad loan problems. This "imprudence view" of bank lending implies that LTV ratios should have been higher during the bubble period, and that high LTV borrowers should have performed more poorly. Our findings are inconsistent with the predictions inherent in this conventional wisdom and cast serious doubt on such a simplistic description of Japan's bubble period. The findings also suggest that LTV ratios are not a good indicator of excessive risk-taking by lenders. Taken together, our findings call for a more nuanced view of bank behavior during the bubble period in Japan, and in credit booms in general.

More generally, our findings have important policy implications. Proponents of LTV caps argue that curbing high LTV ratio loans will enable us to reduce bank risk and dampen the financial amplification of economic shocks. Our findings do not support this view. First, a simple cap on the LTV ratio is unlikely to impose a binding constraint on bank lending during the boom period, because the LTV ratio exhibits counter-cyclicality. This finding is consistent with one of the conclusions in Goodhart et al. (2012)'s theoretical study, which indicates that LTV ratio caps might be ineffective in boom periods because of large increases in asset prices.[12,13] Second, rather than poor, the ex post performance of firms with high LTV loans was, in fact, better during the bubble period. Had there been a cap on the LTV ratio during the bubble period in Japan, such firms would not have been able to obtain financing. Thus, our findings cast doubt on LTV caps as an effective macro-prudential policy measure.

3. Data and the definition of LTV ratios

3.1 Data

Our dataset is constructed from a very large database on Japanese firms compiled by the Teikoku Databank (TDB), the largest credit information provider in Japan. For its sample of firms, the TDB database contains extremely detailed information on collateral registered during the period from 1975 to 2009. TDB extracts its information from the official real estate registry in Japan. This registry is based on the Real Property Registration Act, and compiles for public notification information on each piece of real property regarding its description (e.g., specifications of the property's and related buildings) and associated property rights (e.g., the ownership, security interests). Any transfer and/or termination of rights are also recorded in the official real estate

[12] Goodhart et al. (2012) construct a general equilibrium model and calibrate the effects of different macro-prudential policy measures on credit expansion and house prices. Regarding the cap on the LTV ratio, they conclude that "it is difficult to impose higher loan to value requirements [...] enough to slow down credit expansion (and house price appreciation)" because "the boom brings large increases in asset prices," "[t]he high prices deliver capital gains to all the existing owners of the assets," and "[t]he gains to current mortgage holders [...] lower the loan to value ratio on their mortgages" (all citations are from p.42 of Goodhart et al. 2012).

[13] More generally, our findings are consistent with research that indicates that changes in credit standards did not drive the boom period (Justiniano, Primiceri, and Tambalotti 2013) and inconsistent with studies that find that LTV caps would be effective (e.g., Suh 2013).

registry.

For a particular piece of real property owned by a firm or its CEO, TDB acquires from the official registry information (at the time of TDB's research) about its address, acreage, type of land (e.g., building site or paddy field), type of building (e.g., office, residential, commercial, and industrial), its ownership, and most importantly for our analysis, whether it is pledged as collateral. Collateral information collected by TDB includes the claim holder(s), the debtor(s), the amount of loans against which the collateral is pledged, and the date it was registered.

Unfortunately, however, the TDB database does not collect all of the information contained in the official real estate registry. This includes two deficiencies relevant for our analysis. First, the TDB database does not contain information on the seniority among multiple claim holders (i.e., first lien, second lien, etc.). We assume in our analysis that a claim holder is senior to other claim holders if the date of its registry predates those of the others. If there are multiple loans with the same registration date, we assume that they have the same priority. Second, TDB records information on property rights that are effective at the time it conducts credit research on the firm. Because information on rights that have been terminated is not stored in the TDB database, we cannot trace the history of property rights for every piece of property.

Another issue with our data is related to the fact that in Japan, collateral takes one of two types: ordinary collateral and *ne-tanpo*. The former is collateral pledged in a manner common in other countries. The latter, also frequently used in Japan, is different from the common form. Specifically, *ne-tanpo* is a type of collateral usage related to repeated lending such as loans for working capital. The direct translation of the word "ne" in Japanese is "root," and the word "tanpo" is "collateral." As the label implies, once *ne-tanpo* is pledged, it stays pledged to the lender and will therefore secure any future loans extended to the borrower up to a specified ceiling (i.e., maximum outstanding loan amount). In other words, even if a loan that is secured by a *ne-tanpo* is repaid, the relevant real property will automatically be reserved as collateral for any new future loans from the same lender (up to the ceiling amount) unless the *ne-tanpo* is registered as "released" (i.e., terminated)[14] Thus, the loan balance secured by *ne-tanpo* fluctuates (or revolves), although the property that is pledged stays the same. Unlike many lines of credit, *ne-tanpo* is not associated with a specific commitment to lend in the future. The main motivation to use *ne-tanpo* is to avoid the collateral-related transactions cost when firms need to borrow serially in the spot market. We are able to identify whether a piece of collateral is *ne-tanpo*, and are able to identify the maximum outstanding amount of loans (i.e., the ceiling) that can be made in the future against the particular *ne-tanpo*.

As noted above, we focus on the LTV ratios of business loans. Because the TDB database does not specify whether a piece of real estate that is pledged as collateral is associated with a business loans or a loan to the CEO (i.e., a loan to the firm's owner to finance a residence), we distinguish them based on the following criteria. First, we classify all of the loans secured by *ne-tanpo* as business loans, because *ne-tanpo* is usually not used for residential loans. Second, loans are also classified as business loans if their debtors are firms (not their CEOs). Third, if the debtor(s) are the firm's CEOs or board members, we then check whether the firm uses the related personal property as collateral. If this is the case, we classify them as business loans. Finally, if information on the identity of debtors is not available, we exclude the observation from the sample because it is difficult to determine whether the relevant loan is a business loan or a residential one. Using these criteria to identify business loans, the number of observations on the LTV ratio for business loans is 420,889, and the number for residential loans is 37,352.[15]

Although the richness of the information on real estate registrations in the TDB database is unprecedented in the literature, there are several caveats to using these data that stem from sample selection. First, our sample firms are mostly small and medium-sized enterprises (SMEs), because SMEs are the target for TDB's credit research on real estate registrations.[16]

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[14] There is no expiration date for *Ne-tanpo*.

[15] Ono et al. (2013) also discuss the evolution of LTV ratios for residential loans.

[16] Although TDB's research on the real estate registry is mandatory for SMEs, for listed and/or large firms (those with the amount of capital larger than 100 million yen (roughly $1.25 million) and with the number of employees larger than 100), the research is conducted based upon requests of customers (i.e., those who need

Second, TDB's database does not cover all of the real estate that a firm (and its CEO) owns. In principle, TDB always obtains registrations on a firm's headquarters and its CEO's residential real estate. However, TDB's data on the other real estate that the firm or its CEO possesses is generated on demand only. Note that a CEO's personal real property is often pledged as collateral for business loans to the CEO's SME. This highlights the fact that SME loans in Japan (and elsewhere in the world) can be – and routinely are – collateralized by both business assets ("inside collateral") and personal assets ("outside collateral").

Third, and most importantly, although we have data on collateral that was registered from 1975 to 2009, we only have pre-2008 data if they appear in the most recent credit report that TDB compiled during the period from 2008 to 2010.[17] To put it differently, all of the registrations in our sample consist of those that existed in the registry from 2008 to 2010, and so those registered before 2007 are included only when they *remained* registered until at least 2008.[18] Thus, our data are *synthetic* in the same sense that Petersen and Rajan's (2002) data are synthetic.[19] There are some cases where TDB conducted credit searches on a firm several times during the period from 2008 to 2010. In such cases, we only use the most recent data because changes in the names of the addresses (e.g., street and city names), which most likely occur because of municipal mergers, make it difficult to track the same land in constructing our panel data set.

The cross-sectional like nature of our data also creates two shortcomings for our analysis. First, we cannot exploit data variation in time series dimensions to control for loan, borrower, or lender fixed effects. Second, we might suffer from a survivorship bias problem in the sense that "bad" firms that went bankrupt and were liquidated before 2008 are not included in our dataset. [20]

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such information).

[17] We do have some observations for collateral that was registered before 1975 and after 2009, but we do not use them because of the small number of observations.

[18] A collateral registered in 1999, for example, would be removed from the TDB database if the loan was paid off and the security interest in the property was terminated as a result. Likewise a bankrupt firm would be removed.

[19] Petersen and Rajan (2002) use data on the year a firm began a relationship with a given lender, but the data set is conditioned on the firm existing in a specific later year (year 1993) where the information is obtained. Thus, firms that did not survive until 1993 are not included in their sample.

[20] In Appendix Table A-2 shows the number of observations per year used in our univariate and regression

In the regression analysis below, we try to address this problem by controlling for as many firm- and loan-characteristics as possible. A bias might also arise in our sample of firms, depending on when their registrations occurred. Because older firms have survived longer, they are likely to be more creditworthy than those with younger registration dates. In order to circumvent this problem, we control for firm age at the time of registration.

As noted above, we have information on LTV ratios for 420,889 total observations on collateral registrations made during the period from 1975 to 2009 used for the univariate analysis in section 4.1. For 297,692 observations on firms that were registered from 1990 through 2009 we have information about the basic characteristics of the borrowing firms, e.g., the number of employees, their industry, location, and the identity of lenders with a security interest. For a subset of 59,125 of these 297,692 firms we also have information from firm financial statements. We use this subset of 59,125 observations in our regression analyses in section 4.2.

3.2 Definition of LTV ratios

LTV ratios are defined as the ratio of the amount of a loan, either being extended or committed, to the current value of real estate being pledged as collateral. It represents the exposure of each lender, because if the value (V) decreases by 1-LTV percent, then the lender may suffer a loss given default if the debtor has a negative equity position.

Information about the numerator (L) is available from the TDB database as already explained above. To calculate V, the denominator, we note that land values are the product of a pricing vector of unit prices and a vector of land attributes, the latter of which is obtained from the TDB database. To estimate land prices, we follow an approach that is widely used in the field of real estate economics and estimate a hedonic model. This approach assumes that the price of a parcel of land is the sum of the values of its attributes such as size, floor area ratio, physical distance to a

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analysis. This provides a good indication of the size of our missing observations from which a survivorship might arise. For example, the table shows that the number of observations at the beginning or sample period (i.e., 1975-1977) is roughly one-third the size of our sample at the end (i.e.,2006-2008).

metropolis in the region, etc. We start with the dataset *Public notice of land prices* (PNLP) compiled by the Land Appraisal Committee of the Ministry of Land, Infrastructure, Transport and Tourism of the Government of Japan, and estimate a hedonic model in which the log price of land (taken from the PNLP) is a function of different explanatory variables.[21] Using the parameter estimates from this estimation, we then project (predict) the current price of each piece of land in our dataset based on its characteristics from the TDB database.[22],[23]

The calculation of the LTV ratio becomes more complicated when there are multiple loans and multiple lenders with different levels of priority. For example, even in a simpler case where there are multiple loans secured with the same land, the LTV ratios of junior loans need to take into account the amount of senior loans. We provide an illustrative explanation on how we calculate the LTV ratio in these and other cases in the Appendix.

Note that we calculate an *origination LTV ratio,* i.e., the LTV ratios are based on the L and V *at the time of loan origination.* We calculate the LTV ratio at origination for two reasons. First, from a bank management point of view this is the LTV ratio that is relevant to the loan underwriting decision. Second, the policy debate principally relates to LTV caps imposed at the time of origination.

It is worth mentioning that buildings are commonly pledged as collateral in Japan together with the land on which they are built. However, we have no information on the value of buildings. Therefore, our analysis is confined to land value only. To some extent, this is not likely to be a serious problem, because in practice bankers in Japan have historically put smaller emphasis on the

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[21] The explanatory variables in this estimation are the log size of the land, the regulatory upper limit of the floor area ratio, the Euclidean distance of the land to the highest price piece of land in the same prefecture, the square term of the Euclidean distance, the Euclidean distance of the land to the highest price piece of land in the same city, the square term of the distance, the latitude of the land and its square term, the longitude of the land and its square term, and dummy variables representing the type of land district (i.e., whether the land is located in a residential, commercial, or industrialized district). We run a regression for each combination of land district type (3 types: residential, commercial, or industrialized), year (35 years: from1975 to 2009), and region (either 47 prefectures or 15 regional units), which resulted in the total of 3,813 estimated regressions.

[22] For more details for the estimation of V, see Ono et al. (2013).

[23] We cannot directly use the PNLP because its scope is limited and the PNLP does not provide us with the prices for the particular pieces of land that our sample firms pledge as collateral.

value of buildings than land when taking collateral. This is because in Japan, the value of buildings depreciate relatively rapidly, presumably because the market for used buildings is not very liquid, and their durable years are much less than in Europe or the U.S.[24] However, to control for this potential bias stemming from a lack of information on building market values, by including a variable for the book values of the buildings in the regression analysis below.

4. Cyclicality of LTV ratios

In this section, we address the primary focus of our paper – the efficacy of an unconditional LTV cap as a macro-prudential policy tool. Specifically, we examine changes in LTV ratios over time and their determinants, with particular emphasis on whether LTV ratios exhibit pro-cyclicality. Recall that a necessary condition for an unconditional LTV cap to be effective is the existence of pro-cyclical behavior in the LTV ratio. We also note that a finding of pro-cyclicality in LTV ratios would be consistent with the existing evidence on the pro-cyclicality in lending (e.g., Borio et al., 2001; Horvath, 2002; Borio and Lowe, 2002; Berger and Udell 2004). After providing some background information on Japanese aggregate business activity and Japan's land price bubble, we explore the evolution of LTV ratios over the Japanese business cycle in section 4.1. In section 4.2 we report the results from our multivariate analysis that controls for, among other things, survivorship bias.

[24] For instance, Tokumitsu (2006), part of a series of practitioner manuals for bankers, suggests that banks should use 10 to 20 years as the lifespan of wooden houses. The 2009 National Survey of Family Income and Expenditure (Ministry of Internal Affairs and Communications, the Government of Japan) assumes that the lifespan (depreciation rates) of houses is 22 and 47 years (11.4% and 5.3%) respectively in the case of wooden and steel-frame or reinforced concrete houses. The Council for Social Infrastructure (2005) reports that in Japan, residential houses lose their physical integrity within 31 years on average, which is far shorter than 44 years in the U.S. and 75 years in the U.K. Regarding commercial property (such as office buildings), we don't have any specific evidence justifying this practice (of devaluing buildings) by bankers. However, it is likely that the depreciation of commercial property in Japan relative to the rest of the world maps the relatively rapid depreciation of residential property in Japan. Of course, this necessarily implies a much more rapid depreciation of commercial property in Japan than residential property as it does in other countries.

4.1 LTV ratios over the business cycle: Univariate analysis

4.1.1 Background information: The business cycle and the bubble

In order to provide some context for our analysis of the evolution of LTV ratios in Japan, we first take a brief look at the Japanese business cycle and the land price bubble in Japan during the late 1980s to the early 1990s using macro statistics. Figure 1 shows the time-series path of real GDP in Japan, the average land price, and the stock of bank loans outstanding. Real GDP growth rate during the so-called “bubble” period from late 1980s to early 1990s (shaded in the figure), was about 5 percent on average, while the growth rate of land prices was more than 10 percent. Bank loans also exhibited double-digit growth. The surge in land prices was especially remarkable during the last few years of the bubble period.

After the bubble burst, Japan encountered several economic expansions and recessions and the real GDP growth rate never exceeded its level during the bubble period. The growth rate of bank loans exhibited a similar cyclical pattern after the bubble burst, but the rate was smaller on average than the GDP growth rate. Land prices showed a steady decline over these twenty years, finishing with a price level comparable to that in the early 1980s.

4.1.2 Cyclicality of loans, land values, and LTV ratios

We begin our analysis by first examining separately the evolution of the numerator and the denominator of the LTV ratio, i.e., the amount of loans originated (L) and the estimated value of the collateralized land (V). We then turn to the evolution of the LTV ratio (L/V) itself.[25]

Figure 2 shows the changes in the 25, 50, and 75 percentiles of our L and V through the business cycle. The respective patterns of the evolution of L and V are not particularly surprising in and of themselves – both are pro-cyclical. They each have an increasing trend until 1991 when the asset price bubble burst in Japan, and a decreasing one until the mid-2000s. They go up

[25] Note that our L that is at the loan level is inevitably in flow terms, while the amount of loans outstanding that is at the aggregate level in Figure 1 is in stock terms.

afterwards, with the increase in the loan amount larger than the increase in the land value. Also, these changes in land value are consistent with our findings using macro statistics in Figure 1.

Now we turn to the LTV ratio, the key focus of our analysis. Figure 3 shows the LTV ratio by percentile (25th, 50th, and 75th percentile). Our finding here is striking in the sense that it is inconsistent with conventional wisdom on lax lending standards in Japan. Notwithstanding that its numerator and denominator fluctuate in a pro-cyclical manner, the LTV ratio clearly exhibits counter-cyclicality, at least until early the 2000s after which it disappears.

Although our focus is not on the absolute level of the LTV ratio, the observed median LTV greater than one might seem surprising. However, as we noted above, we do not (and cannot) include the market value of buildings which are also often pledged as collateral as well. We address this problem in our multivariate analysis below by including the book value of buildings (from firm balance sheets) as a control in our regressions.

Note that our finding of a counter-cyclical LTV ratio until the early 2000s is not driven by the stickiness of the land prices. As shown above, V indeed exhibits pro-cyclicality. The fact that loans and land values are both pro-cyclical diminishes a concern that the counter-cyclicality of the LTV ratio is just an artifact of data problems.

4.1.3 Discussion

A concern in our analysis might be that the counter-cyclicality of the LTV ratio is driven by a survivorship bias inherent in our data. As noted above, our sample firms are those that survived until 2008 or afterwards, and so the LTV ratios in earlier years reflect those of longer-lived firms that are likely to be more creditworthy. However, if such a survivorship bias existed in our data, the LTV ratio should have a monotonically decreasing trend reflecting the change in the mix of firm quality over time: that is, for better-quality firms that dominate the earlier periods, banks would be willing to lend more for the same amount of collateral, ceteris paribus[26]. This is not the case in Figure 3. However, there might still be other forms of survivorship bias that might affect our findings in the other direction. For example, high quality firm might demand less credit because they have more internal resources, and thus tend to have low LTV loans. To account for any bias, below we will include a variety of controls for loan characteristics, borrower characteristics and lender characteristics – and then see if the counter-cyclicality observed in our univariate analysis survives in our multivariate analysis.

Our finding is not entirely inconsistent with findings elsewhere. In fact, our finding of counter-cyclical LTV ratios is consistent with one of the conclusions in Goodhart et al. (2012)'s theoretical study. They construct a general equilibrium model and calibrate the effects of different policy measures on credit expansion and house prices. As for the cap on the LTV ratio, they conclude that because the boom brings large increases in asset prices and lowers the LTV ratio, it is difficult to "lean against the wind to reduce the credit expansion and house prices in the boom via regulation" (Goodhart et al. 2012, p.42).

There is also other empirical evidence that is consistent with our finding. The Bank of Japan (2012, Chart IV-3-10) shows that the evolution of the LTV ratios for housing loans in Japan during the period 1994 to 2009 clearly exhibits an increasing trend in the transition of the LTV ratio in the residential mortgage market. Justiniano, Primiceri, and Tambalotti (2013, Figure 1.2.) find that residential mortgages LTVs in the U.S. remained unchanged during the housing boom until 2006 and then the ratio spiked after the collapse of housing prices. In a similar vein, the FSA (2009, Exhibit 4.1) reports that average LTV ratios for home purchases in the U.K. have been generally falling from 1997 to the late 2000s, especially during the credit boom period. Our results are not directly comparable because in these studies the markets (residential vs. business loans) and the variable definition (post-origination versus at origination) of the LTV ratios are different. Nevertheless, we do note that our findings are consistent with those in these studies.

[26] Assuming comparable credit demand between high and low quality firms.

The counter-cyclicality of the LTV ratio means that banks' real time exposure was decreasing during the bubble period in terms of current pricing (i.e., bank exposure was not increasing conditional on lenders lacking contemporaneous knowledge of being in a bubble period). This suggests that a simple cap on the LTV ratio as a macro-prudential measure may not work as a binding constraint on bank lending during the boom period.

4.1.4 LTV ratios with different definitions of V

One possible criticism of our methodology is that lenders might be taking into account the expectation of future land values when underwriting loans, which makes it inappropriate to use the current value of land in calculating the LTV ratio as we did above. To address this concern, we calculate and compare the LTV ratios under different definitions of V.

First, we calculate the LTV ratios with the value of land evaluated one year later, V(t+1). This is a benchmark case where lenders could perfectly foresee and underwrite their loans based on the value of land realized one year later. Using this forward value of V might also be appropriate for a different reason: there might be a lag in reporting the land price in the data that we used to predict land values (i.e., PNLP). Second, lenders might alternatively underwrite loans based on the value of collateral that is somewhat naively predicted based on its past values. To consider such a case, we calculate V that is interpolated from its previous year's growth rate, i.e., $V(t-1)\cdot\{V(t-1)/V(t-2)\}$.

Figure 4 compares the medians of LTV ratios calculated under these alternative definitions with the one used above. It is evident that the counter-cyclicality of the LTV ratios remains even if we employ these different Vs. Thus, our finding of counter-cyclical LTV ratios is robust to different assumptions about V.

4.2 Cyclicality of LTV ratios: Multivariate analyses

4.2.1 Methodology

In this section we conduct multivariate regressions to analyze the determinants of LTV ratios.

That is, we investigate whether the counter-cyclicality of the LTV ratios that we found in section 4.1 still holds after controlling for a variety of factors including importantly those that might address a potential survivorship bias inherent in our data. In particular, because our data are synthetic in nature, older data are associated with longer-lived borrowers, and thus our findings in section 4.1 might suffer from survivorship bias. Unless we have data for non-survivors, it is impossible to completely control for this bias. However, we have rich information on the characteristics of the loans, borrowers, and lenders in our sample, and so we can examine whether the counter-cyclicality of the LTV ratios that we found in the univariate analysis is preserved after controlling for these characteristics. Because the LTV ratios are one of the key contract terms set by lenders, this regression also indicates how lenders determine the ratios. To the extent that the counter-cyclicality disappears by controlling for these factors, the counter-cyclicality of the LTV ratio that we found in the previous section is an artifact of the differences in the loan-, borrower-, and/or lender-characteristics in different years, part of which might stem from the survivorship bias. However, to the extent that it does not disappear, we can confirm that the evolution of LTV ratios is indeed counter-cyclical.

The dependent variable in this analysis is the LTV ratio. The main independent variables of interest are the registration year dummies (*L_YEAR*, year 1990 is the default). In addition to these registration year dummies, we add our controls for loan characteristics, borrower (firm) characteristics, and lender characteristics. A list of the variables and their definitions are provided in Table 1. Table 2 reports their summary statistics.

Table 2 shows that the mean LTV ratio is 7.7 while the median is 1.4, suggesting that there are outliers with large LTV ratios. To exclude possible outlier effects, we run quantile regressions rather than ordinary least squares.[27] Also, to deal with the simultaneity bias, we take the borrower and lender characteristics variables as of one year prior to the origination/registration of the loans.

[27] We also run ordinary least squares regressions after dropping observations that fall in 1% tails of the distribution of LTV ratios. The results (not reported) are qualitatively the same as those of the median regression below.

Due to limitations in the availability of many of our independent variables, we cannot run the regression from 1975, the initial year for which we can calculate LTV ratios. All of our variables are available beginning in 1989. But in order to take one year lags, our sample period for the regression analyses begins in 1990 and ends in 2009.

4.2.2 Control variables

Loan characteristics

We control for a number of different loan characteristics beginning with the use of a dummy variable for *ne-tanpo* (*L_netanpo*). As explained in section 3.1, we have two types of collateral in our data set: "ordinary" for ordinary term loans, and *ne-tanpo* which specifies a ceiling loan amount that a debtor can borrow up to in the future. *Ne-tanpo* allows banks to take collateral in anticipation of loans that might be committed to in the future. It's not obvious whether we should expect a positive vs. negative coefficient on this variable. To the extent that *ne-tanpo* proxies for shorter maturity (i.e., working capital) financing and that forward commitments might on average be extended to safer borrowers, the coefficient might be higher. However, it might also be the case that forward nature of the commitment itself increases risk driving a lower LTV ratio.

We also use four dummy variables to capture loan priority (*L_PR1-4*, the default case is fifth or lower priority). Because the payoff sensitivity of junior loans (like second mortgage home equity loans in the U.S) to changes in the value of the underlying real estate is greater than the sensitivity of senior loans, LTV ratios may be different for these loans controlling for risk and assuming comparable demand.

Firm characteristics

The variables for firm characteristics are the natural logarithm of sales (*F_lnSALES*), return on Assets (*ROA*) defined as the ratio of operating profit to total asset, the capital asset ratio (*F_LEV*), and firm age (*F_AGE*). These ratios proxy for firm risk, performance and transparency. We also expect that these variables control for the potential survivorship bias in our data.

We also use the ratio of buildings to total assets (*F_BUILD*). This variable is constructed from information on the balance sheets of our sample firms. This variable is to address a possible bias stemming from the non-availability of the market value of buildings in the denominator of the LTV ratio. In the presence of such a bias, *F_BUILD* is expected to have a positive coefficient.

Finally, to control for region- and industry-specific factors that might affect LTV ratios, we use nine regional dummies (*F_REG*1-9, Hokkaido/Tohoku is the default), and nine industry dummies (*F_IND1-9*, other industries is the default).

Lender characteristics

As for lender characteristics, we use a dummy variable indicating whether a loan is extended by the main bank (as opposed to non-main banks) (*BK_MAIN*), which is defined as the lender listed at the top of TDB's list of lenders that the firm transacts with.[28] Because main banks are generally considered to take more credit risk than non-main lenders, we expect a positive coefficient on *BK_MAIN*.

We also use six dummy variables representing the type of lender (*BK_TYPE1-6*).[29] All dummies take the value of zero if the main bank is a city bank, the largest banks in Japan by size and all of whom operate nationwide. Regional banks (including second-tier regional bank), indicated by *BK_TYPE1*, are middle-sized regional lenders that operate in a specific region. Shinkin banks (*BK_TYPE2*) and Credit cooperatives (*BK_TYPE3*) are small cooperative financial institutions and operate in yet smaller regions. For some sample firms their main bank is a government-affiliated financial institution, which is indicated by *BK_TYPE4*. *BK_TYPE5* indicate other banks, security companies, or insurance companies, and *BK_TYPE6* indicate that the lender is other than these financial institutions (i.e., non-banks, credit guarantee corporations, non-financial firms, etc.).

[28] The banks on the list are ordered based on their importance as subjectively determined by TDB.
[29] See Uchida and Udell (2010) for more on bank types in Japan.

These variables help control for any risk appetite that might vary by bank type.

Policy variables

In addition to loan-, borrower-, and lender-characteristics, we add dummy variables to control for two policy initiatives that might affect the level of LTV ratios. We use a dummy variable *PL_CEILING* to indicate a policy measure that placed a ceiling on the aggregate amount of loans to real estate firms for each bank. This ceiling was introduced by the Ministry of Finance in 1990 and removed in 1991 to curb the booming lending to real estate firms. This dummy takes a value of one if the registration year is either 1990 or 1991 and the borrower is a real estate firm. We expect that this variable to have a negative coefficient.

We also use a dummy variable *PL_ACTION* that indicates that the lender was subject to the *Action Program on Relationship Banking* in 2003 implemented by the Financial Services Agency (FSA). We add this variable because the Action Program required that the FSA request that regional banks, Shinkin banks, and credit cooperatives avoid an “excessive” reliance on collateral and personal guarantees when extending loans to SMEs. We thus create a dummy variable that takes a value of one if the registration year is 2004 or later, and if the lender type is one of these three. To the extent that banks responded positively to the request and became more willing to lend to firms having less real estate, we expect the *PL_ACTION* to have a positive effect on the LTV ratio.

4.2.3 Results

Table 3 shows the regression results. Column (A) of this table reports our baseline results using the median (50 percentile) regression. At first glance, we can confirm that most of the variables are significant with the expected signs.

The key finding for our analysis is that the year dummies exhibit an increasing trend in the LTV ratios from 1994 to 2009 (*L_YEAR1994-2009*) compared to 1990. This means that the LTV ratios in the midst of, or just after, the bubble period were low compared with those afterwards. This finding is consistent with the counter-cyclical LTV ratio that we found in our univariate analysis (Figure 3). We note that our result here, unlike in our univariate analysis, now controls for a variety of factors that might also affect the LTV and that also control for potential survivorship bias. Thus, irrespective of observable loan characteristics and borrower characteristics, banks in Japan during the bubble period did not lend more aggressively (in terms of their risk exposure as measured by LTV ratios). Rather, the increase in the value of collateral during the boom was more than offset by the increase in the loan amount. An increase in the LTV ratio would have, counterfactually, implied that lending standards decreased. Our finding (of a decreasing LTV), however, does not provide any evidence of this (Figure 2). Again, this finding casts doubt on the effectiveness of a simple LTV cap as a macro-prudential tool.

We acknowledge that we cannot completely rule out the possibility that unobservable time-varying factors contribute to the increasing trend of the LTV ratio in the median regression, and that the impact of the time dummies might vary if we could control for such factors. However, we have controlled for many loan-, borrower-, and lender characteristics. Thus, we argue that, for the most part, these controls cover the most likely co-determinants.

4.2.4 Robustness

To check whether the counter-cyclicality of LTV ratios found in the median regression is also preserved at the margins, we run quantile regressions at the 10 percentile (for lower LTV ratios) and 90 percentile (for higher LTV ratios). The results are respectively reported in columns (B) and (C) of Table 3.

We find, as in the baseline median regression (column (A)), that the coefficients of the year dummies in both columns reflect an increasing trend from 1994 to 2009 (*L_YEAR1994-2009*). Note, however, that the coefficients for the year dummies are smaller in the 10 percentile regression while they are larger in the 90 percentile one, as compared to those in the median regression.

These findings suggest that the magnitude of the counter-cyclicality of LTV ratios is modest for lower LTV ratio loans, while it is amplified for higher LTV ratio loans. Because regulatory caps on LTV ratios are targeted for the higher range of LTV ratios, our finding that 90 percentiles of LTV ratios are more counter-cyclical reinforces the doubt our analysis sheds on the effectiveness of simple LTV caps to dampen credit bubbles.

We also ran quantile regressions on just "ordinary" loans. These regressions (not shown) provide information on two dimensions. First, they indicate whether our main multivariate results are specific to the type of loan. And, second, they shed a bit light on the issue of LTV caps on loans that are used to purchase real estate. Unfortunately, our data does not include information on the *purpose* of the loan. However, it is highly unlikely that *ne-tanpo* loans are used for anything other than financing working capital (i.e., accounts receivable and inventory). Thus, to the extent that business loans secured by real estate are used to purchase the underlying (associated) real estate, this would be confined to "ordinary" loans. These tests are the only tests that touch on the pricing channel in that a binding LTV cap (i.e., constraint) on a business loan used to purchase real estate could operate through the pricing channel as well as through risk channel. Our results in these regressions do not qualitatively differ from our reported regressions (except with regard to some large coefficients in the 90 percentile regressions).

5 LTV ratios and the ex post performance of borrowers

5.1 Methodology

In this section, we examine the relationship between the level of the LTV ratio and the ex post performance of the borrowers (after the relevant loans are extended). The purpose of this analysis is to examine the ex post effect that a cap on the LTV ratio might have. One of the main arguments made by proponents of a cap on LTV ratios is that high-LTV ratio loans perform worse than low-LTV loans (see, for example, FSB 2012). If this is the case, imposing an LTV cap would inflict little or no harm, or even minimize bank losses by constraining loans to poorly performing borrowers. However, to the best of our knowledge, there is little evidence on the ex-post performance of high LTV business borrowers that justifies this assertion, especially for business loans. Their ex-post performance might not be worse, or even better, and so the cap as a macro-prudential policy may counter-productively constrain creditworthy borrowers.

We take a two-stage approach to examine the ex post performance of high-LTV borrowers: We first construct a sample of *treatment* observations, and *control* observations in order to compare with them. Then we compare the ex post performance between these two groups of observations using a difference-in-differences (DID) approach.

In the first stage, we define the treatment observations as the firms that exhibit high-LTV loans, where high LTV ratios are defined as those in the fourth quartile of the entire sample of LTV ratios.[30] We then define the *control* observations using two alternative procedures. One is to simply consider non-treatment firms, i.e., firms that do not exhibit high-LTV loans, as control observations. A comparison between the treatment and non-treatment firms allows us to directly test the claim that high-LTV loans perform worse.

In the alternative procedure for selecting control observations we choose those non-treatment firms that have similar ex-ante characteristics with each treatment firm by employing a propensity score matching approach. By matching firms with similar characteristics in this manner we can control for the differences in ex-post performance between high- versus low-LTV firms that might stem from the differences in their ex-ante characteristics. Also, by focusing on a subset of non-treated firms with similar characteristics, the matched control observations also allow us to eliminate, at least partially, the survivorship bias that a simple unmatched control group might suffer from. To calculate the propensity scores, we run a probit regression that models the

[30] In the case where a firm obtained multiple secured loans in a year, we use the one with the highest LTV ratio. Note also that the unit of observations for the analyses below is a firm that obtained loan(s) in a particular year, while in the analyses in the previous section is based on loan-level dataset. As a result, the number of observations is reduced from 59,125 loans to 48,334 firms.

probability that a borrower obtains a high-LTV loan conditional on the covariates that are used as the independent variables in the quantile regressions in the previous section.[31] Then, for each treatment observation (i.e., high-LTV borrower), the matched observation is selected from the non-treatment firms based on having the closest propensity score.[32]

In the second stage, we analyze the DID between the treatment and control firms in terms of several performance variables. For each treatment or control firm, we take differences in its performance variables from year t (when the loan was originated) to year $t+k$ (k = 1 to 5). This is to eliminate time-invariant firm-fixed effects. We then calculate the average difference in these differences within the treatment firms and their control firms (either unmatched or matched). The performance variables that we employ are (1) the number of employees and (2) the log amount of sales to represent firm growth (in terms of size), (3) ROA to represent changes in firm profitability, and (4) the capital-asset ratio to represent changes in credit risk. Due to data availability, the sample period begins in 1990 as in the quantile regression in the previous section. The sample period ends in 2004, because we take five year differences in the performance variables.

5.2 Results

Table 4 shows the results of the ex-post performance analysis. Panel (1) of this table reports the results using the unmatched DID estimator, while Panel (2) reports the propensity score matching DID (PSM-DID) estimator. In each table, column (A) reports the estimation results using the whole sample, while columns (B), (C), and (D) respectively report those using subsamples of 1990-94, 95-99, and 2000-04. In each column, we show the average ex-post performance of treatment groups (firms that obtained high LTV loans in year t) and control groups (firms that obtained lower (i.e., non-high) LTV loans in year t) in terms of their differences between year t and t

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[31] The results of the probit estimation are qualitatively the same as those of the quantile regressions in Table 3, so we do not report them. The results are available from the authors.

[32] There are several matching algorithm to find the closest control observations. We employ 5-nearest matching, in which 5 observations whose propensity scores are the closest to each treatment observation are chosen.

+ k (k = 1, 2, ..., 5). The columns also show the difference-in-differences of the performance variables between these two groups and corresponding standard errors, together with the results of hypothesis testing, where the null hypothesis is that the average performance of the treatment groups and the control groups are the same.

Looking first at the unmatched DID estimator using the whole sample (panel (1), column (A)), we see that treatment firms exhibiting high LTV loans perform better than control firms exhibiting lower LTV loans in terms of employment growth (*d_F_EMP* in years t+1 and t+2) and of changes in profitability (*d_F_ROA* in years t+3, t+4, and t+5). We find no significant differences between these two groups for sales growth (*d_F_lnSALES*) and changes in the capital-asset ratios (*d_F_LEV*). As indicated by significant and positive DID estimators for *d_F_EMP*, *d_F_lnSALES*, and *d_F_ROA* in column (B), the high LTV borrowers perform better especially in years 1990-94 (during and after the bubble burst). However, columns (C) and (D) show that, after the bubble burst, we no longer find that treatment firms performed better, and sometimes we find worse performance (e.g., negative DID estimators for *d_F_lnSALES*).

Panel (2) that shows the matched-DID estimators, from column (A) using the whole sample, we find almost no significant differences in the average performance between borrowers that exhibited high LTV loans and those that exhibited lower LTV loans. These findings suggest that the performances of high LTV borrowers and the performance of low LTV borrowers with similar ex-ante characteristics are comparable. Looking at columns (B), (C) and (D), we observe that the average performance of treatment firms was better during 1990-94, but the differences almost disappeared afterwards.

To summarize, we find that the ex-post performance of the firms that obtained loans with higher LTV ratios was actually better than those with lower LTV ratios during or just after the bubble period, and for the most part not significantly different afterwards. These findings suggest that a high LTV ratio during the bubble period (as well as during the other period) does not reflect by itself lax lending standards in the sense that firms that obtained these loans did not perform poorly afterwards. Rather, at least in hindsight, firms with high LTV loans during the bubble period performed better in terms of their growth – even controlling for firm characteristics that likely reflect (at least to some extent) loan underwriting standards. This finding has an important policy implication. In the previous sections, we find evidence suggesting that a simple cap on the LTV ratio on business loans would have been ineffective in dampening lending booms. In addition to this ineffectiveness, the finding in this section implies that imposing a simple LTV cap might curb lending to growing firms.

6 Conclusion

Using unique data from the official real estate registry in Japan, this paper took a close look at the LTV ratios of business loans in order to draw some implications for the ongoing debate on the use of LTV ratio caps as a macro-prudential policy measure. We find that, although the amount of loans and the value of land pledged as collateral are individually pro-cyclical, their ratio, i.e., the LTV ratio, exhibits counter-cyclicality. This finding is robust to controlling for various loan-, borrower-, and lender-characteristics, and to controlling for survivorship bias. We also find that, ex post, borrowers that were granted loans with high LTV ratios did not perform poorly compared with those granted low LTV loans.

Our findings also have important policy implications. Caps on LTV ratios are a pressing topic of debate among policymakers. Proponents argue that curbing high LTV loans would enable us to reduce bank risk. Our findings do not support this view. First, a simple cap on LTV ratios may not work well in practice as a macro-prudential policy measure because the LTV ratio exhibits counter-cyclicality. Second, we do not find that the ex post performance of firms with high LTV loans was worse than that of firms with low LTV loans. Moreover, based on some measures it tended to be better. Thus had there been an LTV cap during the bubble, it might have prevented higher quality firms from borrowing. These findings imply that a simple, or unconditional cap on LTV ratios might not only be ineffective in curbing loan volume in boom periods but may also counter-productively constrain well-performing borrowers. Thus, our findings cast doubt on simple LTV caps as an effective macro-prudential policy measure and suggest that the efficacy of an LTV cap may depend crucially on how it is conditioned.

While our analysis focuses on business lending, our results may provide some insight into the efficacy of LTV caps on residential mortgage lending. That is, our findings that a simple LTV cap on business lending would not have been binding suggests that imposing a cap in Japan would neither have dampened the growth of real estate prices nor the build-up of risk in the banking system. Further, our results on the ex post performance of LTV loans also castes additional doubt on the effectiveness of LTV ratios in preventing the build-up of risk in bank loan portfolios. Both of these could apply to residential mortgage lending as well. However, two caveats are worth mentioning. First, we have evidence that high LTV lending in the U.S. residential mortgage market in the form of subprime mortgages generated in the years just before (and just after) real estate prices began to fall, resulted in higher losses. Second, first mortgages in the residential real estate market are virtually always used to purchase the real estate itself. This is not likely the case for most of the loans in our sample, particularly the *ne-tanpo* loans. Thus, for these loans there may not be a direct link between LTV ratios and asset pricing during a bubble. But, it should be noted, that this is also true for home equity lines of credit (HELOCs) in the U.S., many of which were used for purposes other than purchasing or improving existing real estate.

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Appendix Calculation of LTV ratios: an illustration

Suppose that a firm owns four pieces of real estate (numbered from 1 to 4), and borrows using six loans, two from Bank Alpha, two from Bank Beta, and two from Bank Gamma (see Figure A-1). The firm pledges its properties as collateral to these banks: Land 1 is pledged to loan A extended by Bank Alpha in year 1985; land 2 is pledged to loan B extended by Beta in 1990 and is also pledged to loan F extended by Gamma in 1995; land 3 is pledged to loan C extended by Beta in 2000 and is also pledged to loan F by Gamma in 1995; and land 4 is pledged to loan D extended by Alpha and is also pledged to loan E extended by Gamma, and both pledged are registered on the same date in 2005.

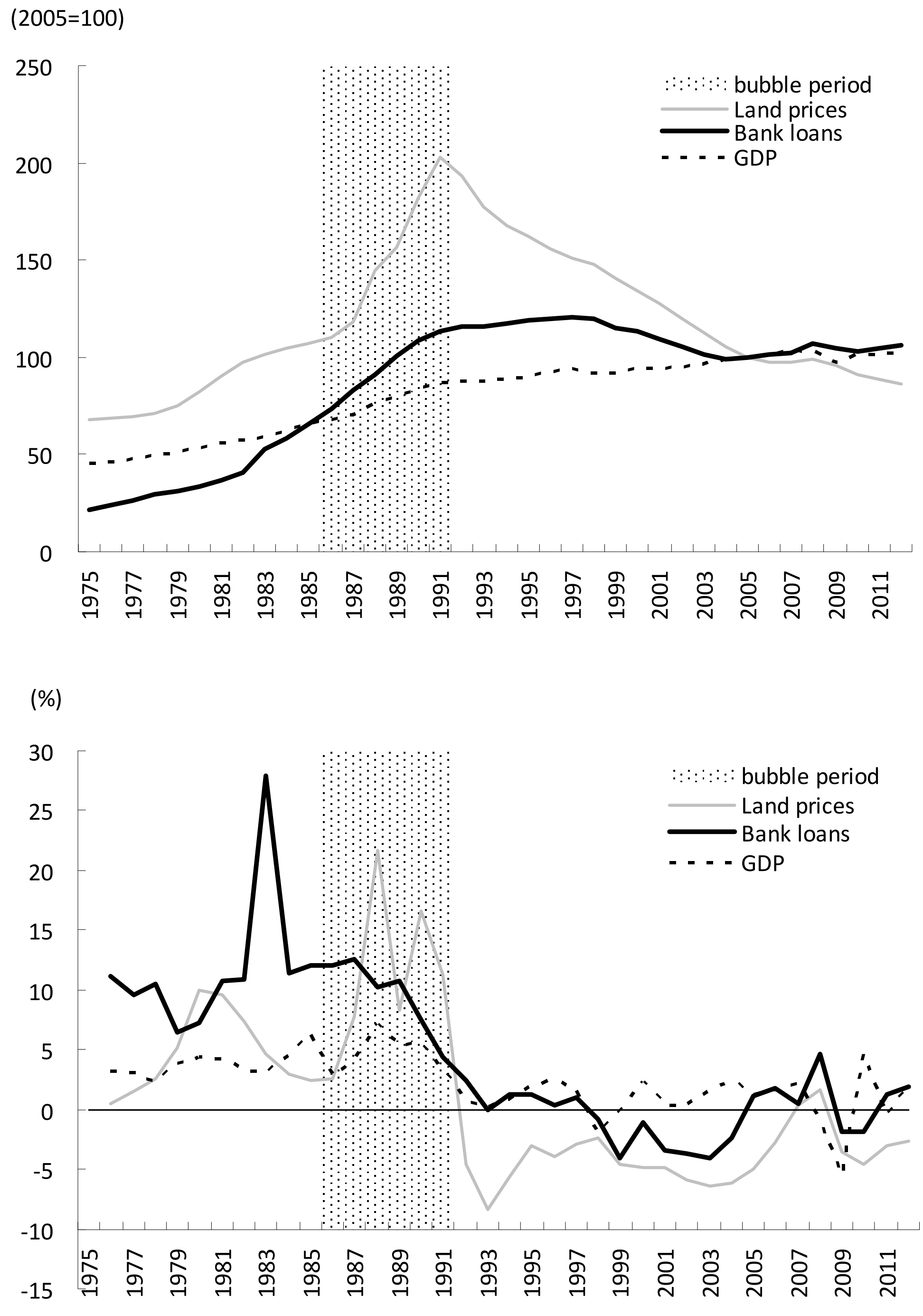
Calculation is fairly simple if a land is pledged to only one claim holder. In the example above, this is the case for loan A. Information about the amount of loan A, represented by LA, is provided by TDB database. The value of land A in year 1985, V1(1985), is estimated by the hedonic approach described in Ono, Uchida, Udell, and Uesugi (2013). The LTV ratio for loan A (LTV_A(1985)) is simply obtained by dividing LA by V1(1985).

If a piece of land is pledged to multiple claim holders (and loans) and/or if multiple pieces of land are pledged to one claim holder, the calculation of the LTV ratio becomes complicated. The calculation differs depending on the seniority among different loans. As noted above, we assume that a claim holder is senior to other claim holders if the date of its registration predates those of the others. In the example above, land 2 is pledged to loan B as well as to loan F. Because loan B (originated in year 1990) was extended prior to loan F (in year 1995), we assume that loan B is senior to loan F. The LTV ratio of loan B is calculated in the same manner as in the case with one claim holder: LTV_B(1990)=LB/V2(1990).

The calculation also differs for junior loans. In this example, land 3 is pledged to loan C as well as to loan F, and the former (underwritten in year 2000 by Beta) is subordinated to the latter (underwritten in year 1995 by Gamma). In this case, the amount of the senior loan (loan F) should be taken into account when calculating the LTV ratio for loan C. That is, the LTV ratio that properly expresses the exposure defined above for Bank Beta is LTV_C(2000)=(LF+LC)/V2(1995). The calculation is similar if there are several loans with the same registration date, in which case we assume that they have the same rank of priority. In the example above, land 4 is pledged to loan D and loan E that are extended respectively by Alpha and Gamma on the same date. In this case, LTV_D(2005)=LTV_E(2005)=(LD+LE)/V4(2005).

The most complicated is the LTV ratio for a loan to which multiple properties are pledged as collateral. In our example, Loan F extended by Gamma is backed by two properties, land 2 and land 3. As for land 2, Gamma is junior to Beta, whereas for land 3, it is the most senior lender. In this case, we cannot define the LTV ratio in a suitable manner, because the ratio cannot be conceptualized in terms of bank exposure in this a situation. Thus, we decided to eliminate such observations from the sample of our empirical analysis. The number of observations eliminated in this manner is, however, small. Also note that the LTV ratio of a loan secured by multiple properties can be well defined as long as the rank of seniority is the same among all properties. For example, if loan F were a senior loan for both land 2 and land 3, then LTV_F(1995) = LF/(V2(1995)+V3(1995)). In a similar vein, if instead loan F were junior, then LTV_F(1995) = (LB+LC+LF)/ (V2(1995)+V3(1995)).

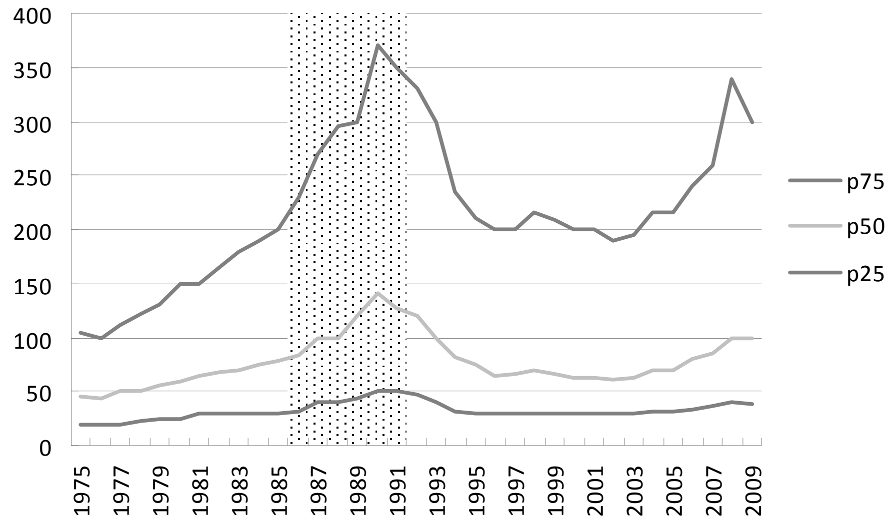
Figure 1 GDP, land price, and bank loans (level and growth rate)



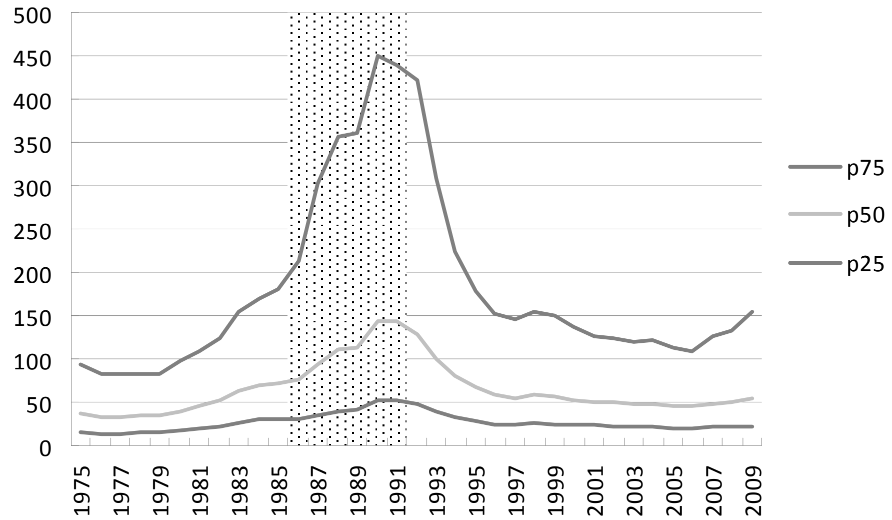
Source: Cabinet Office, "National Accounts," Land Appraisal Committee of the Ministry of Land, Infrastructure, Transport and Tourism, "Land Market Value Publication," Bank of Japan, "Financial and Economic Statistics"

Figure 2 Loans and values over the business cycle

(A) Amount of Loans (L)



(B) Values of Land (V)



(Unit: in million Japanese yen)

Figure 3 LTV ratios over the business cycle

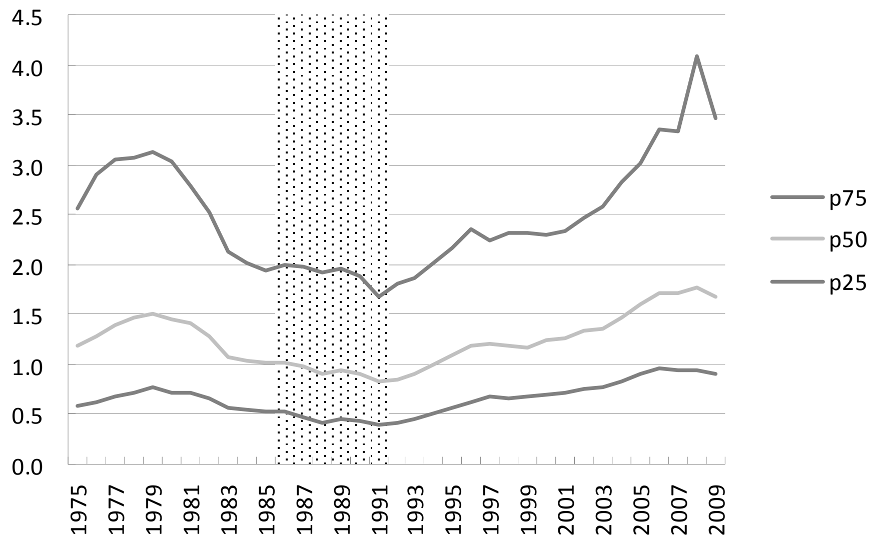


Figure 4 Medians of LTV ratio with different definitions

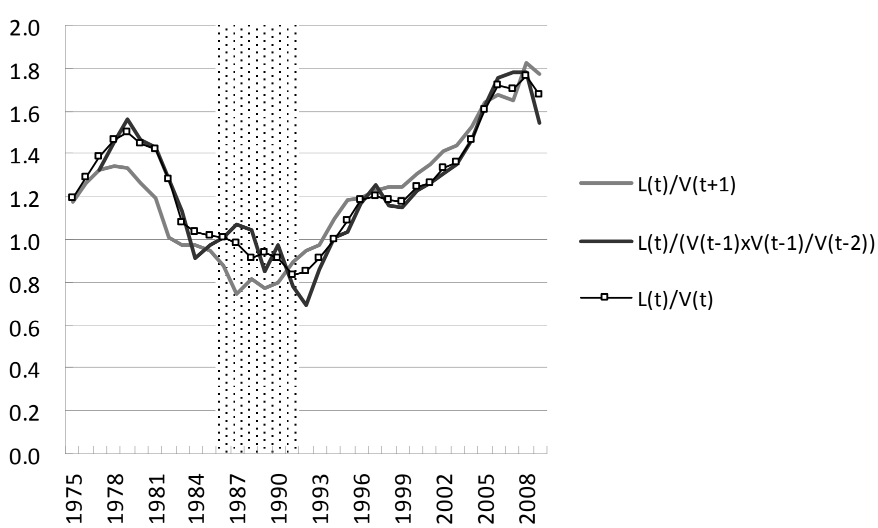


Table 1 Variable definitions

This table summarizes the definitions of the variables used in the main analysis.

Dependent variable	
LTV	Loan-to-value ratio
Registration year	
YEAR1991-2009	Registration year dummies: *L_YEARX* = 1 if the year is *X* (=1991, ..., 2009). The default is 1990.
Loan characteristics	
L_netanpo	*Ne-tampo* dummy: = 1 if the collateral is *ne-tanpo*
L_PR1-4	Loan priority dummies: *L_PRX* = 1 if the priority rank is *X*th (*X* = 1, ..., 4). The default (*L_PR0*) is fifth or lower priority.
Borrower characteristics	
F_lnSALES	Log of gross annual sales
F_ROA	Return on Asset: = operating profit / total asset
F_LEV	Capital-asset ratio: = net worth / total asset
F_BUILD	Building-asset ratio: = building / total asset
F_AGE	Firm age
F_IND1-7	Borrower industry dummies: *F_INDX* = 1 if the industry is that indicated by *X* (= 1, ... , 7), where *X* = 1 for Construction, = 2 for Manufacturing, = 3 for Wholesale, = 4 for Retail and restaurant, = 5 for Real estate, = 6 for Transportation and communication, and = 7 for Services. The default (*F_IND0*) is the others.
F_REG1-9	Borrower regional dummy: *F_REGX* = 1 if the headquarters of the firm is located in a region indicated by *X* (= 1, ... , 9), where *X* = 1 for North Kanto, = 2 for South Kanto (Metropolitan), = 3 for Koshin-etsu, = 4 for Tokai, = 5 for Keihanshin, = 6 for Other Kinki, = 7 for Chugoku, = 8 for Shikoku, and = 9 for Kyushu and Okinawa. The default (*F_REG0*) is Hokkaido and Tohoku.
Lender characteristics	
BK_MAIN	Main bank dummy: = 1 if the lender is a main bank (top-listed bank) of a borrower firm.
BK_TYPE1-6	Lender type dummies: *BK_TYPEX* (*X* = 1, ..., 6) = 1 if the type of lender is that indicated by *X* (= 1, ..., 6), where *X* = 1 for a regional or second-tier regional bank, = 2 for a Shinkin bank, = 3 for a credit cooperative, = 4 for a government-affiliated financial institution, = 5 for other bank, security company, or insurance company, etc., and = 6 for others (non-banks, credit guarantee corporations, non-financial firms, etc.). The default (*BK_TYPE0*) is city banks.
Policy measures	
PL_ACTION	FSA's action program dummy: = 1 if a lender is subject to the FSA's Action Program on Relationship Banking (*YEAR* is 2004 or afterwards and the lender type is either 1, 2, or 3 (i.e., a regional, second-tier regional, or Shinkin bank, or a credit cooperative).
PL_CEILING	Dummy representing the MOF's ceiling policy to real estate firms: =1 if the registration year is either 1990 or 1991 and the borrower is a real estate firm.

Table 2 Summary statistics

This table shows summary statistics of the variables used in the main analysis. See Table 1 for their definitions.

	NOB	mean	sd	min	p50	max
Dependent variable						
LTV	59,125	7.718	434.321	0.000	1.385	99681.800
Registration year						
YEAR1990	59,125	0.025	0.156	0	0	1
YEAR1991	59,125	0.038	0.191	0	0	1
YEAR1992	59,125	0.038	0.191	0	0	1
YEAR1993	59,125	0.035	0.183	0	0	1
YEAR1994	59,125	0.032	0.176	0	0	1
YEAR1995	59,125	0.033	0.178	0	0	1
YEAR1996	59,125	0.035	0.183	0	0	1
YEAR1997	59,125	0.039	0.194	0	0	1
YEAR1998	59,125	0.043	0.203	0	0	1
YEAR1999	59,125	0.041	0.198	0	0	1
YEAR2000	59,125	0.045	0.208	0	0	1
YEAR2001	59,125	0.051	0.220	0	0	1
YEAR2002	59,125	0.054	0.225	0	0	1
YEAR2003	59,125	0.061	0.240	0	0	1
YEAR2004	59,125	0.066	0.248	0	0	1
YEAR2005	59,125	0.068	0.252	0	0	1
YEAR2006	59,125	0.075	0.263	0	0	1
YEAR2007	59,125	0.081	0.273	0	0	1
YEAR2008	59,125	0.076	0.265	0	0	1
YEAR2009	59,125	0.065	0.246	0	0	1
Loan characteristics						
L_netanpo	59,125	0.660	0.474	0	1	1
L_PR0	59,125	0.070	0.255	0	0	1
L_PR1	59,125	0.586	0.492	0	1	1
L_PR2	59,125	0.219	0.413	0	0	1
L_PR3	59,125	0.085	0.278	0	0	1
L_PR4	59,125	0.040	0.197	0	0	1
Borrower characteristics						
F_lnSALES	59,125	13.924	1.296	0	13.904	21.915
F_ROA	59,125	0.032	0.084	-6.457	0.027	2.429
F_LEV	59,125	0.181	0.257	-13.801	0.155	0.999
F_BUILD	59,125	0.288	0.268	0	0.246	9.942
F_AGE	59,125	29.769	15.753	1	29	119
F_IND0	59,125	0.003	0.057	0	0	1
F_IND1	59,125	0.317	0.465	0	0	1
F_IND2	59,125	0.212	0.409	0	0	1
F_IND3	59,125	0.252	0.434	0	0	1
F_IND4	59,125	0.052	0.222	0	0	1
F_IND5	59,125	0.051	0.220	0	0	1
F_IND6	59,125	0.032	0.176	0	0	1
F_IND7	59,125	0.080	0.272	0	0	1
F_REG0	59,125	0.133	0.340	0	0	1
F_REG1	59,125	0.030	0.170	0	0	1
F_REG2	59,125	0.298	0.458	0	0	1
F_REG3	59,125	0.070	0.255	0	0	1
F_REG4	59,125	0.106	0.307	0	0	1
F_REG5	59,125	0.164	0.371	0	0	1
F_REG6	59,125	0.015	0.120	0	0	1
F_REG7	59,125	0.067	0.250	0	0	1
F_REG8	59,125	0.026	0.158	0	0	1
F_REG9	59,125	0.092	0.289	0	0	1
Lender characteristics						
BK_MAIN	59,125	0.269	0.443	0	0	1
BK_TYPE0	59,125	0.146	0.353	0	0	1
BK_TYPE1	59,125	0.296	0.456	0	0	1
BK_TYPE2	59,125	0.153	0.360	0	0	1
BK_TYPE3	59,125	0.016	0.126	0	0	1
BK_TYPE4	59,125	0.174	0.379	0	0	1
BK_TYPE5	59,125	0.013	0.112	0	0	1
BK_TYPE6	59,125	0.202	0.402	0	0	1
Policy measures						
PL_ACTION	59,125	0.222	0.415	0	0	1
PL_CEILING	59,125	0.001	0.034	0	0	1

Table 3 Estimation results - Quantile regressions

Estimation method: Quantile regression	(A) Median (p50)			(B) p10			(C) p90		
Devendent variable: *LTV*	Coef.		(Std. Err.)	Coef.		(Std. Err.)	Coef.		(Std. Err.)
Registration year									
YEAR1991	-0.019		(0.045)	-0.054	**	(0.023)	-0.015		(0.224)
YEAR1992	0.003		(0.045)	-0.032		(0.023)	-0.157		(0.224)
YEAR1993	0.055		(0.046)	0.001		(0.023)	0.000		(0.228)
YEAR1994	0.212	***	(0.047)	0.070	***	(0.024)	0.632	***	(0.233)
YEAR1995	0.403	***	(0.046)	0.151	***	(0.024)	0.870	***	(0.233)
YEAR1996	0.531	***	(0.046)	0.207	***	(0.023)	0.959	***	(0.231)
YEAR1997	0.451	***	(0.045)	0.213	***	(0.023)	0.959	***	(0.226)
YEAR1998	0.465	***	(0.044)	0.219	***	(0.022)	0.890	***	(0.222)
YEAR1999	0.506	***	(0.045)	0.262	***	(0.023)	0.896	***	(0.225)
YEAR2000	0.606	***	(0.044)	0.283	***	(0.022)	1.031	***	(0.222)
YEAR2001	0.617	***	(0.043)	0.297	***	(0.022)	1.275	***	(0.218)
YEAR2002	0.690	***	(0.043)	0.353	***	(0.022)	1.152	***	(0.217)
YEAR2003	0.791	***	(0.042)	0.362	***	(0.021)	1.380	***	(0.214)
YEAR2004	0.884	***	(0.043)	0.414	***	(0.022)	1.947	***	(0.217)
YEAR2005	1.030	***	(0.043)	0.457	***	(0.022)	1.772	***	(0.217)
YEAR2006	1.079	***	(0.042)	0.490	***	(0.021)	2.152	***	(0.215)
YEAR2007	1.048	***	(0.042)	0.476	***	(0.021)	2.253	***	(0.213)
YEAR2008	0.995	***	(0.042)	0.439	***	(0.021)	2.282	***	(0.214)
YEAR2009	0.985	***	(0.043)	0.434	***	(0.022)	2.227	***	(0.216)
Loan characteristics									
L_netanpo	-0.066	***	(0.013)	0.017	**	(0.007)	-0.189	***	(0.066)
L_PR1	-0.842	***	(0.023)	-0.284	***	(0.011)	-7.792	***	(0.124)
L_PR2	-0.196	***	(0.025)	-0.054	***	(0.012)	-4.871	***	(0.129)
L_PR3	0.091	***	(0.028)	0.010		(0.014)	-3.027	***	(0.144)
L_PR4	0.092	***	(0.034)	0.041	**	(0.017)	-2.087	***	(0.172)
Borrower characteristics									
F_lnSALES	0.187	***	(0.005)	0.054	***	(0.003)	0.915	***	(0.031)
F_ROA	0.252	***	(0.069)	0.232	***	(0.035)	0.007		(0.342)
F_LEV	-0.138	***	(0.022)	-0.077	***	(0.014)	-0.430	***	(0.126)
F_BUILD	0.107	***	(0.021)	-0.008		(0.011)	0.231	**	(0.099)
F_AGE	-0.008	***	(0.000)	-0.004	***	(0.000)	-0.006	***	(0.002)
Lender characteristics									
BK_MAIN	-0.008		(0.014)	-0.016	**	(0.007)	-0.101		(0.071)
BK_TYPE1	0.173	***	(0.021)	0.053	***	(0.011)	0.902	***	(0.106)
BK_TYPE2	0.109	***	(0.023)	0.042	***	(0.012)	0.355	***	(0.120)
BK_TYPE3	0.181	***	(0.048)	0.042	*	(0.024)	0.779	***	(0.239)
BK_TYPE4	-0.018		(0.021)	-0.021	*	(0.011)	-0.010		(0.108)
BK_TYPE5	0.111	**	(0.053)	-0.049	*	(0.027)	3.389	***	(0.266)
BK_TYPE6	-0.021		(0.022)	-0.021	*	(0.011)	0.016		(0.111)
Policy measures									
PL_ACTION	-0.041	*	(0.022)	-0.015		(0.011)	0.322	***	(0.112)
PL_CEILING	-0.148		(0.162)	-0.059		(0.078)	-1.090		(0.765)
constant	-0.897	***	(0.084)	-0.269	***	(0.043)	-0.524		(0.481)
Industry dummies	Yes			Yes			Yes		
Regional dummies	Yes			Yes			Yes		
NOB	59,125			59,125			59,125		
Pseudo R2	0.020			0.013			0.034		

d_F_LEV	t+1	-0.003	-0.002	-0.001	(0.001)	-0.003	-0.001	-0.002 *	(0.001)	0.001	0.000	0.002	(0.001)	0.000	0.000	0.000	(0.001)
	t+2	0.001	0.002	-0.001	(0.001)	0.001	0.004	-0.003	(0.002)	0.009	0.005	0.004 **	(0.002)	0.004	0.004	0.000	(0.002)
	t+3	0.006	0.007	-0.001	(0.001)	0.007	0.008	-0.001	(0.002)	0.015	0.012	0.003	(0.002)	0.008	0.008	0.000	(0.002)
	t+4	0.013	0.013	-0.001	(0.001)	0.012	0.014	-0.002	(0.002)	0.023	0.020	0.003	(0.002)	0.013	0.012	0.000	(0.002)
	t+5	0.020	0.019	0.001	(0.001)	0.019	0.019	0.000	(0.003)	0.032	0.026	0.006 **	(0.003)	0.018	0.017	0.001	(0.002)

(2) Matched control

		(A) Entire sample				(B) 1990-1994				(C) 1995-1999				(D) 2000-2004			
		Treatment	Control	DID	(S.E)	Treatment	Control	DID	(S.E)	Treatment	Control	DID	(S.E)	Treatment	Control	DID	(S.E)
d_F_EMP	t+1	0.417	0.257	0.160 *	(0.085)	1.463	0.821	0.642 **	(0.254)	-0.022	-0.283	0.261	(0.207)	0.165	0.117	0.048	(0.150)
	t+2	0.487	0.302	0.185	(0.134)	2.070	1.200	0.870 **	(0.395)	-0.477	-0.936	0.458	(0.320)	0.387	0.297	0.091	(0.244)
	t+3	0.278	0.165	0.113	(0.174)	2.128	1.026	1.103 **	(0.501)	-1.497	-1.884	0.386	(0.406)	0.459	0.468	-0.009	(0.308)
	t+4	0.194	-0.010	0.203	(0.217)	2.074	0.378	1.697 ***	(0.586)	-2.472	-2.883	0.411	(0.479)	0.640	0.790	-0.150	(0.363)
	t+5	0.108	-0.316	0.423	(0.259)	1.427	-0.806	2.233 ***	(0.664)	-3.009	-3.420	0.411	(0.551)	0.816	0.989	-0.173	(0.411)
d_F_lnSALES	t+1	0.008	0.004	0.005	(0.003)	0.027	0.005	0.023 ***	(0.006)	0.001	-0.003	0.003	(0.006)	0.014	0.015	-0.001	(0.005)
	t+2	0.010	0.005	0.005	(0.004)	0.031	0.001	0.030 ***	(0.009)	-0.018	-0.013	-0.004	(0.008)	0.036	0.031	0.005	(0.007)
	t+3	0.008	0.003	0.005	(0.005)	0.048	0.009	0.038 ***	(0.010)	-0.043	-0.039	-0.004	(0.010)	0.049	0.047	0.001	(0.008)
	t+4	0.005	0.006	-0.001	(0.006)	0.047	0.003	0.043 ***	(0.011)	-0.074	-0.065	-0.009	(0.011)	0.059	0.073	-0.014	(0.010)
	t+5	-0.003	0.000	-0.003	(0.007)	0.029	-0.006	0.035 ***	(0.012)	-0.085	-0.076	-0.009	(0.013)	0.042	0.052	-0.010	(0.011)
d_F_ROA	t+1	-0.005	-0.005	0.001	(0.001)	-0.007	-0.006	0.000	(0.001)	-0.002	-0.003	0.001	(0.001)	-0.003	-0.003	0.000	(0.001)
	t+2	-0.005	-0.005	0.000	(0.001)	-0.010	-0.012	0.002	(0.002)	-0.001	-0.001	0.000	(0.002)	-0.001	0.000	-0.001	(0.001)
	t+3	-0.006	-0.007	0.001	(0.001)	-0.012	-0.015	0.003	(0.002)	0.000	-0.001	0.001	(0.002)	-0.003	-0.001	-0.002	(0.001)
	t+4	-0.006	-0.007	0.000	(0.001)	-0.014	-0.017	0.003 *	(0.002)	0.000	-0.001	0.001	(0.002)	-0.003	0.000	-0.003 *	(0.001)
	t+5	-0.007	-0.008	0.001	(0.001)	-0.018	-0.020	0.002	(0.002)	0.001	0.000	0.001	(0.002)	-0.006	-0.007	0.000	(0.002)
d_F_LEV	t+1	-0.003	-0.002	-0.001	(0.001)	-0.003	-0.001	-0.002	(0.001)	0.001	0.000	0.001	(0.001)	0.000	-0.001	0.000	(0.001)
	t+2	0.001	0.002	-0.001	(0.001)	0.001	0.003	-0.002	(0.002)	0.009	0.006	0.004 *	(0.002)	0.004	0.004	0.000	(0.002)
	t+3	0.006	0.007	0.000	(0.001)	0.007	0.007	-0.001	(0.002)	0.015	0.012	0.002	(0.002)	0.008	0.006	0.002	(0.002)
	t+4	0.013	0.013	0.000	(0.002)	0.012	0.012	0.000	(0.003)	0.023	0.022	0.001	(0.003)	0.013	0.011	0.002	(0.002)
	t+5	0.020	0.017	0.003	(0.002)	0.019	0.017	0.002	(0.003)	0.032	0.025	0.007 **	(0.003)	0.018	0.014	0.004	(0.003)

This table presents th… on-high LTV borrower groups, where high-LT… as extended, and spans from 1990 to 2004. V… ployee (*d_F_EMP*), in sales in logarithm (*d_*… s the difference in the average ex-post perfor… TV loans). ***, **, * respectively indicate th… bservations are simple unmatched non-treatm… cores to each treatment observation.

Figure A-1 Illustrative setting for LTV calculation

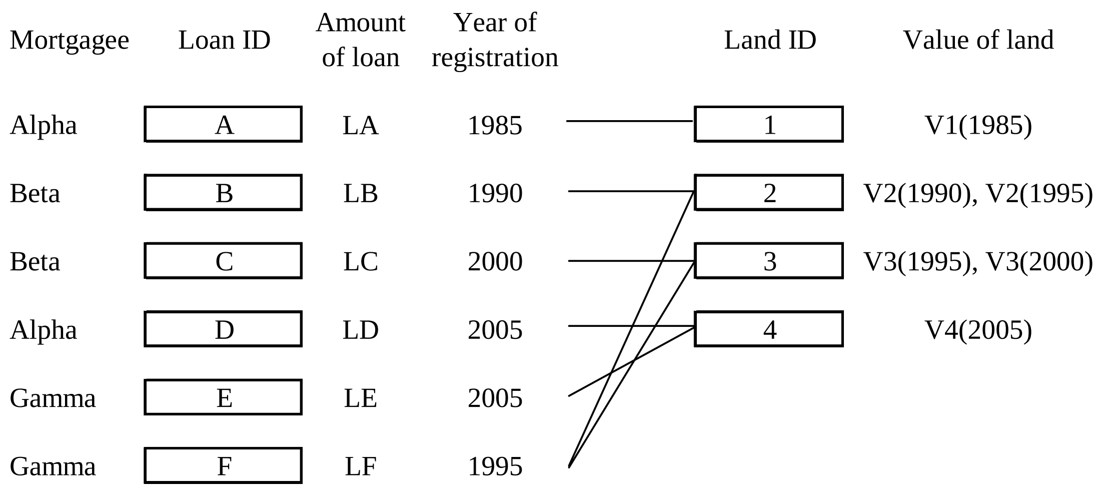


Table A-2

Observations by year (NOB) for both univariate and regression analysis

Univariate analysis (for Figure 3)		Regression analysis (For Tables 2 and 3)	
year	NOB	year	NOB
1975	5575		
1976	5354		
1977	5946		
1978	6386		
1979	6697		
1980	6977		
1981	7927		
1982	8124		
1983	8041		
1984	8521		
1985	9328		
1986	10714		
1987	14083		
1988	14599		
1989	17341		
1990	18604	1990	1473
1991	17488	1991	2245
1992	16017	1992	2232
1993	14910	1993	2055
1994	14863	1994	1896
1995	15572	1995	1936
1996	14977	1996	2050
1997	15333	1997	2311
1998	14300	1998	2535
1999	13228	1999	2413
2000	14403	2000	2684
2001	15380	2001	3025
2002	15400	2002	3172
2003	16171	2003	3631
2004	16714	2004	3884
2005	16933	2005	4044
2006	16484	2006	4409
2007	15661	2007	4803
2008	15099	2008	4501
2009	10229	2009	3826
total	443379		59125