Center for Research on Startup Finance

Working Paper Series No.008

Lending Pro-Cyclicality and Macroprudential Policy: Evidence from Japanese LTV Ratios

Arito Ono

Hirofumi Uchida

Gregory F. Udell Iichiro

Uesugi

October 25, 2017

Center for Research on Startup Finance,
Graduate School of Business Administration, Kobe University
2-1, Rokkodai-cho, Nada-ku, Kobe, 657-8501, JAPAN
http://www.b.kobe-u.ac.jp/~uchida/CRSF/CRSF_toppage.html

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

Arito Ono□

Hirofumi Uchida□

Gregory F. Udell□Iichiro

Uesugi□

† Earlier versions of this paper have been presented at HIT-TDB-RIETI workshop, Hitotsubashi, Bank of Japan, Kobe, DBJ-RICF, MEW, 2013 FMA Europe, FRB San Francisco, Stockholm School of Econ., 2013 Macroeconomics Conf., NUS, Bangor, FDIC, Bank of Portugal, FRB Kansas City, 50th Bank Structure Conf., Concluding Conf. of the Macroprudential Research Network of the European System of Central Banks, 3rd MoFiR Workshop, ABFER 2014 Annual Conf., DePaul, Villanova, BOJ-IMES, and 2015 AFA. The authors thank G. Barlevy, A. Berger, M. Berka, L. Black, C. Brown, T. Duprey, E. Ergungor, E. Fukuda, T. Furuta, M. Giannetti, M. Hanazaki, T. Hatakeda, M. Hori, T. Hoshi, K. Hosono, D. Ikeda, T. Inui, R. Jain, C. Kahn, S.B. Kim, K. Kobayashi, T. Komoda, T. Kurozumi, M. Kowalik, S. Lin, D. Miyakawa, J.I. Nakamura, K. Nakamura, E. Ors, S. Otani, A. Rose, M. Saito, K. Schaeck, T. Soma, A. Srinivasan, M. Summer, B. Tanyeri, Y. Teranishi, Y. Tsutsui, Y. Uchida, M. Usui, L. Wall, K. Watanabe, Y. Yasuda, and K. Yoshimura, and seminar participants for useful comments, C. Shimizu and Y. Saita for providing land price data, M. Hazama, T. Kimiwada, W. Toyama, S. Mizohata, K. Matsuda, and C. Kwak for superb research assistance, Teikoku Databank, Ltd. for data provision, and JSPS KAKENHI Grant Numbers JP25220502, JP24330103, and JP16H02027, the "Designing Industrial and Financial Networks to Achieve Sustainable Economic Growth" project under the Ministry of Education, Culture, Sports, Science and Technology's program "Promoting Social Science Research Aimed at Solutions of Near-Future Problems," and RIETI. The views expressed in this paper are ours and do not necessarily reflect those of any of the institutions with which we are affiliated.

□ Faculty of Commerce, Chuo University, 742-1 Higashinakano, Hachioji, Tokyo 192-0393, Japan. Email: aono@tamacc.chuo-u.ac.jp. Tel.: +81-42-674-3595.

□ Corresponding Author: Walter H. Shorenstein Asia-Pacific Research Center, Stanford University, 616 Serra St., Encina Hall, Stanford University, Stanford, CA 94305-6055, and Graduate School of Business Administration, Kobe University, 2-1 Rokkodai, Nada, Kobe 657-8510, Japan. Email: uchida@b.kobe-u.ac.jp. Tel.: +81-78-803-6949.

□ Kelley School of Business, Indiana University, East 10th Street, Bloomington, 47405, IN, USA. Email: gudell@indiana.edu. Tel.: +1- 812-855-3304.

□Institute of Economic Research, Hitotsubashi University, 2-1 Naka, Kunitachi, Tokyo 186-8603, Japan. Email: iuesugi@ier.hit-u.ac.jp. Tel.: +81-42-580-8357.

Lending Pro-Cyclicality and Macroprudential Policy: Evidence from Japanese LTV Ratios

Abstract

Using unique micro data compiled from the real estate registry in Japan, we examine more than 400,000 loan-to-value (LTV) ratios for business loans to draw implications for caps on LTV ratios as a macroprudential policy measure. We find that the LTV ratio exhibits counter-cyclicality, behavior that would have severely impeded the efficacy of a simple LTV cap had it been imposed. We also find that borrowers obtaining high-LTV loans are more risky but grew faster than those with lower LTV loans, which implies that a simple fixed cap on LTV ratios might inhibit growing (albeit risky) firms from borrowing.

Keywords: loan-to-value (LTV) ratios, pro-cyclicality, macroprudential policy, bubble
JEL classification codes: G28, G21

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. These two crises centered on bubbles in real estate prices that affected business loans secured by real estate and mortgages. In Japan banks mostly suffered from damage in the business sector, while in the U.S. larger banks mostly suffered from damage in the household sector and smaller banks were significantly affected by damage in commercial real estate lending. 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, and consensus began to build in favor of countercyclical macroprudential policy tools (e.g., Kashyap and Stein 2004).

The most prominent tool in the macroprudential policy toolbag is caps on LTV (loan-to-value) ratios (see e.g., FSB 2012), which have already been implemented in a number of countries.[1] The LTV ratio (the loan amount (L) divided by the value of collateral (V)), has long been used in loan underwriting as a measure of risk exposure. Caps on LTV ratios potentially work through two channels (CGFS 2012): (1) "strengthen[ing] the resilience of the financial system" by decreasing

[1] According to a survey conducted by the IMF in 2010, 20 out of 49 countries, especially those in Asia (Hong Kong, Korea, etc.) and Europe (Norway, Sweden, etc.), use caps on LTV ratios as a macroprudential instrument (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).

the probability of default (PD) and loss-given-default (LGD), which we call the *risk channel,* and (2) "restrict[ing] the quantity of credit by limiting the funding available to certain borrowers" in order to dampen growth in real estate prices, which we call the *pricing channel*. Behind these channels, there are two implicit assumptions: (i) LTV ratios are pro-cyclical, so that the caps can curb more loans in boom periods than in bust periods; and (ii) loans with higher LTV loans are riskier, so that the caps can curb risky loans and decrease the risk exposure of the lenders.

The aim of this paper is to examine these implicit assumptions, by looking retrospectively at real estate-based lending in the business sector in Japan during the bubble and bust periods.[2] Using an unusually comprehensive dataset that includes detailed information on over 400,000 business loans secured by real estate extended from 1975 to 2009, we first examine whether LTV ratios evolved in a pro-cyclical manner – a necessary condition for a simple LTV cap to work. We then compare the ex post performance of business borrowers with high versus low LTV loans in order to analyze whether a simple LTV cap would have limited the availability of credit to risky borrowers – a necessary condition for a simple LTV cap to dampen the risk channel.

By way of preview, we first find that from the beginning of the real estate bubble, the LTV ratio

[2] While we focus on a simple (i.e., unconditional) LTV cap, not all LTV cap regulations and proposals are of this form. Lim et al. (2011) show that among 20 countries that impose caps on LTV ratios, 11 countries set fixed caps while 9 countries adopt time-varying caps. 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.

was countercyclical, not pro-cyclical, at least until the early 2000s, although its numerator (L) and the denominator (V) were both pro-cyclical.[3] This finding of pro-cyclicality holds even in a multivariate framework in an analysis over a shorter period from 1990 to 2009.[4] We also find little evidence that our results are driven by possible biases due to data limitations. This finding is inconsistent with the first of the two implicit assumptions behind LTV caps as a macroprudential policy instrument, pro-cyclicality of LTV ratios.

Second, we find in tests on the ex post performance that, while firms that obtain high LTV loans are more risky, they also exhibit greater growth. This finding is robust to controlling for ex ante firm characteristics that might be correlated to the choice of high versus low LTV ratios. The finding of the higher risk is consistent with the second implicit assumption behind LTV caps, but the finding of the greater growth is inconsistent with an implicit assumption that firms obtaining high-LTV loans are of lower quality.

Our findings have important policy implications. The first finding, the counter-cyclicality of LTV ratios, suggests that a simple (i.e., unconditional) LTV cap might be non-binding and ineffective, at least if it had been (counterfactually) implemented in Japan for business loans during

[3] The finding of pro-cyclical lending is consistent with the existing evidence (e.g., Borio et al. 2001, Berger and Udell 2004).

[4] Data limitations do not permit a multivariate analysis that spans the entire pre-bubble/post-bubble business cycle as we conducted in our univariate analysis (see section 4.2).

the bubble period. The second finding, higher risk and greater growth for firms that obtain highLTV loans, suggests that the simple (counterfactual) LTV cap might have produced an important unintended consequence: inhibiting firms with higher growth (albeit more risky) from borrowing. Although we cannot directly generalize these implications for LTV caps for mortgages, our findings at least suggest a need to examine the two implicit assumptions behind LTV caps.

While we view our findings on efficacy and unintended consequences as the paper's most important contribution, we also note that our findings are unique in the literature because they are based on a massively large loan-level database on business loans. Despite a growing body of research on the efficacy of macroprudential policy tools, to the best of our knowledge, our paper is the first empirical study that examines LTV caps for business loans using a micro dataset.[5,6] Also, most existing studies of LTV ratios use aggregate data and so inevitably confine their analysis on ex post performance to macro-performance variables such as aggregate credit growth (e.g., Cerutti, Claessens and Laeven 2015). Unlike these studies, we can evaluate the performance at the borrower level.[7]

---

[5] Note that the current debate on LTV caps is centered on residential mortgages. Section 2.2 discusses the similarities and differences between LTV caps on business loans and on mortgages.

[6] For a recent comprehensive review of the literature on the efficacy of these tools, see IMF-FSB-BIS (2016), particularly Annex 2.

[7] Igan and Kang (2011) (for Korea) and Laufer (2014) (for U.S.) use micro data to study the effect of LTV caps on residential mortgages on housing demand and pricing. Basten and Koch (2014) analyze the Basel III countercyclical capital buffer and its interaction with LTV caps on Swiss residential mortgage pricing. These analyses of the LTV cap mainly focus on the pricing channel while we focus on the risk channel (see subsection 2.2).

The remainder of our paper is composed as follows. The next section provides some context for our analysis by discussing the objectives of LTV caps and how they relate to business loans. Section 3 provides details on our data. Section 4 analyzes the cyclicality of LTV ratios. Section 5 investigates the ex post performance of high LTV loans. Section 6 concludes the paper with some policy implications.

2. The context: LTV caps and our analysis

2.1. The objectives of LTV caps

The main goal of macroprudential policies is "to reduce systemic risk, defined as the risk of widespread disruptions to the provision of financial services that have serious negative consequences for the real economy" (CGFS 2012). This translates into two (not mutually exclusive) objectives: (i) strengthening the resilience of the financial system to economic downturns and other aggregate shocks, and (ii) limiting the build-up of financial risks (by "*leaning against the financial cycle*") (CGFS 2010).

To meet objective (i), LTV caps are expected to operate through the *risk channel* through which they directly decrease the probability of default (PD) and the loss-given-default (LGD) of the banking industry's loan portfolio, and thereby lower systemic risk. LTV caps may also address objective (ii) via the *pricing channel* through which they restrict the quantity of credit in order to reduce real estate demand and suppress increases in real estate prices.[8] In other words, LTV caps address objective (ii) through their "impact on the credit cycle" (CGFS 2012).

In both channels, there are two implicit assumptions. First, for caps on LTV ratios to be effective during the bubble period, LTV ratios must be pro-cyclical, so that a fixed cap can curb more loans in boom periods than in bust periods. Second, for the caps to purge risky loans, loans with higher LTV ratios must be riskier. However, whether or not these assumptions actually hold, and LTV caps are effective through either channel, is ultimately an empirical question – and, in the case of business loans, one for which there is an acute paucity of work. We address this gap.

2.2. LTV caps on residential mortgages vs. LTV caps on business loans

While the current debate on LTV caps is centered on residential or commercial mortgages, we focus on LTV ratios in business lending in Japan. We believe that this focus is interesting for two important reasons.

First, LTV caps can be applied (theoretically) to many other types of loans secured by real estate. In most countries real estate is very often pledged as collateral in general business lending, especially for small and midsized enterprises (SMEs), even when the purpose of the loan is not to

[8] Theoretical work by Stein (1995) shows that LTV ratios play an important role in amplifying shocks to borrowers and to the housing market. Consistent with this prediction, empirical studies find that the effects of income shocks on housing prices and/or mortgage borrowing are larger when LTV ratios are higher, suggesting that the strength of a "financial accelerator" mechanism is positively associated with LTV ratios (Lamont and Stein 1999, Almeida et al. 2006, Lim et al. 2011). Imposing caps on LTV ratios might constrain this accelerator mechanism.

purchase the real estate itself (Berger and Udell 2006, Beck et al. 2008).[9] This is likewise true in Japan.[10] Moreover, some countries (e.g., Singapore) now impose LTV caps irrespective of the types of loans (CGFS 2012, Lim et al. 2011).

Second, and more importantly, we focus on LTV ratios in business lending in Japan because whether LTV caps would have worked in business lending is the relevant counterfactual in the context of the Japanese financial crisis in terms of macroprudential policy tools. Real estate-based *business* lending is considered one of the primary causes of the credit bubble in Japan (e.g., Ueda 2000). During the bubble period, banks were thought to have underwritten high-LTV business loans with lax lending standards anticipating surging real estate prices (e.g., Yoshida 1994). Thus, by analyzing the (counterfactual) efficacy of an LTV cap in business lending in Japan, we focus on the *primary* cause of the Japanese financial crisis and investigate whether this macroprudential policy tool might have worked in preventing it.[11] We will return to the connection between LTV caps on business lending and mortgages in the conclusion.

[9] LTV caps could further be applied to other types of lending secured by assets other than real estate, e.g., consumer lending to finance automobile purchases, and business loans lent against accounts receivable, inventory and equipment (Berger and Udell 2006). For these types of loans lenders typically set policies on LTV ratios as part of their underwriting standards.

[10] Although we do not have precise figures on the fraction of SME loans that are secured by real estate, the fraction of SMEs that pledged real estate collateral to any lender was 51.9% during 2007-2010 based on the database used in this paper (see Ono et al. 2015.). The figure might have been even higher during the bubble period, which was before the Japanese government had urged in 2003 that banks to avoid an "excessive" reliance on collateral and personal guarantees when extending loans to SMEs (see subsection 4.2.5). For a nice discussion of the particular importance of real estate as collateral in Japan (and, as well, an aversion to unsecured lending), see Gan (2007).

[11] The Japanese government did consider introducing LTV caps for loans secured by real estate in the early 1990s to deal with the real estate bubble (Council of Land Policy 1990). In hindsight, however, only a ceiling on the amount of loans to real estate firms was implemented (see subsection 4.2.5).

Through its focus on business lending, our analysis will necessarily be concentrated on the risk channel and not the pricing channel. This is because for most loans in our sample, the purpose was not to finance the purchase of the real estate that secures the loan – even though the loan was secured by real estate – but to finance the firm's operation as is commonly the practice in business lending worldwide. Thus, any effect on demand for, or prices of, real estate is, at most, indirect.[12]

3. Data and the definition of LTV ratios

3.1. Data

Our data contain 420,889 total observations on collateral registrations during the period from 1975 to 2009. Our dataset is constructed from a huge database on Japanese firms compiled by the Teikoku Databank (TDB), the largest credit information provider in Japan. The sample firms in this database are mostly SMEs, because SMEs are the target for TDB's credit research. The TDB database covers almost one third of the entire universe of firms in Japan (see Ono et al. 2015) and SMEs dominate the business sector in Japan – as in virtually every other economy. Thus, our sample is likely to represent the population of the Japanese business sector well.

The database contains very detailed information on the collateral registrations which TDB

[12] On the pricing channel, some studies on residential mortgage examine the relation between lending and property prices, and examine the implications of imposing an LTV cap, although they rely on aggregated data and/or only check bivariate correlations (e.g., Gerlach and Peng 2005, Iacoviello 2005, Igan and Kang 2011, Barlevy and Fisher 2012, Vandenbussche et al. 2013, Kuttner and Shim 2013).

extracts from the official real estate registry. This registry is based on the Real Property Registration Act, and compiles information on each piece of real property regarding its description (e.g., specifications on property and related buildings), associated property rights (e.g., ownership and security interests), and any transfer and/or termination of rights.

For any real property owned by a firm or its CEO, TDB acquires from the official registry its address, acreage, type of land (e.g., building site or paddy field), type of building (e.g., office, residential or industrial), its ownership, and most importantly, 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, TDB does not collect some of the information contained in the official real estate registry. It does not collect information on seniority when there are multiple claim holders (i.e., first, second, or lower liens), so we assume that a claim holder is senior if the date of its registry predates those of the others.[13] Also, TDB only records registration information that is effective when it conducts credit research on the firm. Terminated registration information is erased from the TDB database, so we cannot trace the history of registration information for a piece of property. Finally, the TDB database does not specify whether a pledged piece of real estate is associated with a business loan or a loan to the CEO/owner to finance a residence. However, other information

[13] If there are multiple registrations at the same date, we assume that they have the same priority. [14] See Appendix A for our identification of business loans.

allows us to make this distinction so that we can focus on business loans exclusively.[14]

In Japan, collateral takes one of two types: ordinary collateral and *ne-tanpo*. The former is like collateral pledged in other countries, but the latter, also frequently used in Japan, is associated with repeated lending such as loans for working capital. As the label implies ("ne" means *root* and "tanpo" means *collateral*), once *ne-tanpo* is pledged, it remains pledged to the lender and will automatically secure any future loans extended by the same lender to the borrower up to a specified maximum, until its registration is "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. The main motivation to use *ne-tanpo* is to avoid the collateral-related transactions cost for serial borrowings in the spot market. We can identify whether a piece of collateral is *ne-tanpo*.

Although the richness of the information on real estate registrations in the TDB database is unprecedented in the literature in business lending, there are several caveats to using these data that stem from sample selection. First, TDB's database neither covers all of the real estate that a firm (and its CEO) owns, nor covers registration of all sample firms. For firms in its database TDB always collects registration information on a firm's headquarters and its CEO's residence, but only collects data on its other real estate in response to a customer request. While TDB collects

[14] There is no automatic expiration date for *Ne-tanpo*, and unlike lines of credit, ne-tanpo is not associated with a specific commitment to lend in the future.

registration information on all sample SMEs, it only collects the information on listed and/or large firms (equity capital exceeding 100 million yen and more than 100 employees), upon request.

Second, and most importantly, although we have data on collateral 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.[15] 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.[16] Thus, our data are *synthetic* in nature.[18] If TDB conducted credit searches on a firm several times during the 2008-2010 period, as occurred occasionally, we only use the most recent data. 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.

This *cross-sectional*-like nature of our data has two shortcomings. 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 our dataset, "bad" firms that went bankrupt and were liquidated before 2008 are not included. Registration information on repaid "good loans"

---

[15] 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.
[16] 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. [18] Our data are synthetic in the same sense as Petersen and Rajan (2002) who 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.

that were removed from the registry are not included as well. In our regressions, we try to address these shortcomings by controlling for as many firm- and loan-characteristics as possible.

We use information on LTV ratios for our 420,889 total observations on collateral registrations from 1975 to 2009 in our univariate analysis (section 4.1). For our regression analysis we use a 59,125 subset of these observations for which we also have financial statement information (section 4.2).[17] In Figure 1 we report the number of observations per year used in both our univariate and regression analysis. This provides an indication of the magnitude of our missing observations that might drive a survivorship bias. The figure shows that the number of observations for our univariate analysis at the beginning of the sample period is roughly one-third the size of our sample at the end, but even for the first years, we have more than 5,000 observations. The sample size is smaller for our multivariate analysis, but we still have more than 1,000 observations for its first year.

3.2. Definition of LTV ratios

LTV ratios are defined as the ratio of the amount of a loan, either being extended or committed (maximum), to the current value of real estate being pledged as collateral. It reflects lender

[17] We have additional variables for lender characteristics from lenders' financial statements, but the statements are available for a smaller number of observations. However, even when we add these variables to the baseline specifications, the results (available upon request to the authors) are qualitatively unchanged from what we will report in later sections.

exposure, because a decrease in the value (V) by 1-LTV exposes the lender to loss given default.

We obtain the numerator (L) of the LTV ratios from the TDB database as explained above. V, the value of the property, is not available in the TDB database, but information on acreage is available. So we multiply land acreage from TDB by a per-acreage land price estimated with a hedonic model, an approach widely used in real estate economics. 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 metropolis in the region, etc. In this estimation we use 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, which reports land prices for a limited number of places in Japan. Using all data available in this dataset, we estimate more than 3,000 hedonic model regressions by land district type, year, and region, where the log price of land is a function of many different explanatory variables for the attributes of the land. Using the parameter estimates from this estimation, we predict the current price of each piece of land in our dataset based on its characteristics from the TDB database.[18] For more details on the estimation of V, see Appendix B.

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

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 Appendix C.

Our LTV ratios are *origination LTV ratios,* i.e., those based on the L and V *at the time of loan origination.* Using the origination LTV ratio is appropriate, because, from a bank management point of view, this is the relevant ratio in loan underwriting. Also, the policy debate principally relates to LTV caps imposed at the time of origination.

It is worth mentioning that although buildings are commonly pledged as collateral in Japan together with the land on which they are built, we have no information on the value of buildings, and so 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 less emphasis on the value of buildings than land as collateral. However, we cannot rule out the possibility that the omission of the value of buildings may affect our results. We will return to this issue when we discuss our findings in subsection 4.4.2.

4. Cyclicality of LTV ratios

In this section, we address the primary focus of our paper – cyclical changes in LTV ratios. Recall that a necessary condition for an unconditional LTV cap to be effective is the existence of pro-cyclical behavior in the LTV ratio. 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 a univariate setting. In section 4.2 we report the results from our multivariate analysis that controls for a variety of factors in order to determine whether our univariate results are simply an artifact of differences in the loan-, borrower-, and/or lender-characteristics. Section 4.3 provides a discussion of our main findings, and section 4.4 considers whether our main findings are robust to possible biases due to limitations that are inherent in our data.

4.1. Cyclicality of LTV ratios: Univariate analysis

4.1.1 Background information: The business cycle and the bubble

In order to provide some context, we first take a brief look at the Japanese economic conditions and the land prices using aggregate statistics. Panel (A) of Figure 2 shows the time-series path of the real GDP, the market value of real estate (sum of the market values of land and buildings), and the stock of bank loans outstanding at the aggregate level.[19] The "bubble" period from late 1980s to early 1990s is shaded. The spike in land prices at the end of the bubble period is especially remarkable.

Panel (B) of Figure 2 shows the aggregate LTV ratio based on L and V in Panel (A), which is the ratio of L, aggregate bank loans outstanding from Panel (A), over V, the aggregate market value of real estate from Panel (A). We find that this ratio is decreasing during the bubble period, although it has an increasing trend after the burst of the bubble. Thus, this "LTV" ratio exhibits counter-cyclicality at least in the period around the bubble.[20] However, this is a crude indicator and may not capture the evolution of the actual LTV ratio for business loans.[23] Below, we will check whether we observe a similar cyclicality using detailed micro loan level data.

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

[19] Note that our L to calculate origination LTV ratios is in flow terms, while the amount of loans outstanding in Figure 2 is in stock terms.

[20] Note that we focus on cyclicality with respect to real estate prices and not with respect to general business conditions (as reflected in GDP growth), because the primary concern for policy makers when they impose a cap on the LTV ratio is to curb excessive lending when the value of collateral increases due to surging real estate prices. [23] This ratio differs from the LTV ratios at the loan level we use below in many respects. First, figures for loans and real estate are in stock terms, while they are in flows terms for our loan level LTV ratio. Second, the loans for the aggregate ratio include both commercial and residential loans and the value includes real estate that does not secure loans, while the loan level LTV ratio we use below focuses on business loans and land used for securing those loans. Third, the aggregate LTV ratio includes unsecured loans, while our loan level LTV ratio uses only secured loans.

the collateralized land (V). Figure 3 shows the changes in the 25, 50, and 75 percentiles of L and V through the business cycle. The pro-cyclical patterns of the evolution of L and V are not particularly surprising. They each have an increasing trend until around 1991 when the bubble burst, and a decreasing one until the mid-2000s. They go up afterwards, with the increase in the loan amount larger than the increase in the land value. These changes using micro loan level data are on balance consistent with the findings using aggregate statistics in Figure 2, and the finding of pro-cyclical lending is consistent with the existing evidence (e.g., Borio et al. 2001, Berger and Udell 2004).

Now we turn to the LTV ratio, the key focus of our analysis. Figure 4 shows the LTV ratio by quartile (25th, 50th, and 75th percentiles). Notwithstanding that its numerator and denominator fluctuate pro-cyclically, the LTV ratio clearly exhibits counter-cyclicality, at least until early in the 2000s when it disappears. Note that the counter-cyclicality until the early 2000s is not driven by the stickiness of the land prices because as shown above, V indeed exhibits pro-cyclicality. The fact that loans and land values are both pro-cyclical diminishes concern that the counter-cyclicality of the LTV ratio is just an artifact of simple data problems.

Although our finding of a counter-cyclical LTV ratio is consistent with the finding in Panel (B) of Figure 2 (at least during and after the bubble period), it is striking in the sense that it is inconsistent with conventional wisdom in Japan on lax lending standards during the bubble period. We next turn to our multivariate analysis where we examine whether our finding of counter-cyclicality holds after controlling for a number of factors.

4.2. Cyclicality of LTV ratios: Multivariate analyses

4.2.1 Methodology and main variables

In this section, we investigate whether the counter-cyclicality of LTV ratios found in section 4.1 still holds after controlling for a variety of factors. These factors include many of the same variables that could be used by lenders when setting the LTV ratios in the loan underwriting process. To the extent that counter-cyclicality disappears after employing these controls, then our previous finding is just an artifact of differences in the loan-, borrower-, and/or lender-characteristics in different years. However, to the extent that our finding does not disappear, it confirms that the LTV ratios are indeed counter-cyclical. Because the LTV ratios are one of the key contract terms set by lenders, this regression also indicates how lenders determine the ratios. Our multivariate analysis will also allow us to address a number of concerns with our univariate analysis.[21]

Table 1 shows variable definitions and summary statistics except for the registration year dummies that are summarized in Figure 1. Our dependent variable is the LTV ratio. The main

[21] We will further address these concerns from various dimensions in section 4.4.

independent variables are the registration year dummies (*YEAR1991-2009*, with 1990 as the default). We also use our controls for loan, borrower (firm), and lender characteristics, some of which will be explained in subsection 4.2.2. We focus on whether the year dummies exhibit the same counter-cyclicality after controlling for all of these factors.

Because LTV ratios measure risk exposure, it is interesting to examine their determinants not only for average LTVs but also for relatively high and low LTV ratios. We thus run three quantile regressions rather than OLS regressions: median (50 percentile (p50)), 10 percentile (p10), and 90 percentile (p90) regressions. Focusing on median is better than focusing on mean because as Table 1 shows, the mean LTV ratio (7.7) is relatively higher than the median (1.4), suggesting that there are outliers with large LTV ratios.[22]

To deal with the simultaneity bias, we use the borrower and lender characteristics variables as of one year prior to the origination/registration of the loans. Data limitations regarding many of our variables preclude us from running the regression from 1975 as in our univariate analysis. All of our variables are available beginning in 1989. In order to take one year lags, our sample period begins in 1990 and ends in 2009.

[22] When we run OLS regressions after dropping observations that fall in 1% tails of the LTV distribution, the results (not reported) are qualitatively the same as those of the median regression below.

4.2.2 Control variables

Loan characteristics: We have two types of collateral in our data set: ordinary and *ne-tanpo* (see section 3.1). The dummy, *L_netanpo*, captures the case where banks take *ne-tanpo* in anticipation of loans that might be committed to in the future. Table 1 shows that 66% of our sample loans are *ne-tanpo* loans. Because loans secured by *ne-tanpo* are usually used to raise working capital, *L_netanpo* is a proxy for short maturity. This *ne-tampo* dummy thus controls for both the term structure of interest rates and the possible lower risk of working capital loans.

We also use four dummy variables to capture loan priority (*L_PR1-4*, the default case is the fifth or lower priority, labelled as *L_PR0*). 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. Not surprisingly there are more senior loans than junior loans (see Table 1).

Firm characteristics: Our firm controls are the natural logarithm of sales (*F_lnSALES*), profitability (*ROA*: the ratio of operating profit to total assets), the capital-asset ratio (*F_CAP*), and firm age (*F_AGE*), which proxy for firm risk, performance and transparency. We also expect that these variables to some extent control for the potential survivorship bias. We also include the ratio of buildings to total assets (*F_BUILD*) based on the balance sheet information, to address the possible bias stemming from the non-availability of the market value of buildings in the denominator of the LTV ratio. Finally, to control for region- and industry-specific factors that might affect LTV ratios, we use nine regional dummies (*F_REG1-9*, Hokkaido/Tohoku is the default (= *F_REG0)*), and seven industry dummies (*F_IND1-7*, other industries is the default (=*F_IND*0)).

Lender characteristics: Lender controls include a dummy variable for whether the lender is the main bank (*BK_MAIN*), defined as the lender listed at the top of TDB's list of the borrower's lenders.[23] This controls for the likelihood that the main banks assumes more credit risk than other banks. We also use six lender type indicators (*BK_TYPE1 – BK_TYPE6*) that capture the different types of commercial lenders in Japan (see Table 1 for more detail).

Policy variables: We add dummy variables to control for two policy initiatives that might affect the level of LTV ratios. The first is a policy measure that placed a ceiling for all banks on the aggregate amount of loans to real estate firms. The Ministry of Finance introduced the ceiling in 1990 to curb the booming lending to real estate firms and removed it in 1991 (see Uemura 2012). *PL_CEILING* is a dummy that takes a value of one if the registration year is either 1990 or 1991 and the borrower is a real estate firm.

---

[23] The banks on the list are ordered based on their importance as subjectively determined by TDB.

The second initiative is the 2003 *Action Program on Relationship Banking* imposed by the Financial Services Agency (FSA) in Japan. The FSA requested that regional banks, Shinkin banks, and credit cooperatives avoid an "excessive" reliance on collateral and personal guarantees when extending loans to SMEs. The dummy variable *PL_ACTION* takes a value of one if the registration year is 2004 or later, and if the lender type is one of the above three. This controls for the possible change in the willingness of banks to lend without taking collateral.

4.2.3 Results

Table 2 shows the regression results. Column (A) reports our baseline results using the median (50 percentile) regression, and columns (B) and (C) report the results for the quantile regressions at the 10 (for lower LTV ratios) and 90 (for higher LTV ratios) percentiles. At first glance, we can confirm that in each column, most of the variables are significant and reflect their expected signs.

The key finding here is that the year dummies in each column consistently exhibit an increasing trend in the LTV ratios from 1993 or 1994 to 2009 (as 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 4). We note that our multivariate results now control for a variety of factors that might affect the LTV ratio and that also, to some extent, control for potential survivorship bias. Our finding suggests that irrespective of observable loan, firm, and lender characteristics, Japanese banks 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 more than offset the increase in the loan amount.

If we compare the results for different percentiles, we find that the year dummy coefficients are smaller in the smaller percentile regressions. This suggests that the magnitude of LTV counter-cyclicality is modest for lower LTV ratio loans, but amplified for higher LTV ratio loans.

We also ran quantile regressions on just "ordinary" loans as opposed to *ne-tanpo* loans for two reasons. First, this will indicate whether our main multivariate results are specific to the type of loan. Second, it will shed a bit of light on the issue of LTV caps on loans that are used to purchase real estate. Although our data do not include information on the purpose of the loan, it is highly likely (as explained earlier) that *ne-tanpo* loans are used for financing working capital, and so secured business loans used to purchase the underlying (associated) real estate would be confined to "ordinary" loans. This regression that focuses only on ordinary loans can produce purer, although indirect, information on loans where the pricing channel (section 2.1) might also be relevant. The results in these regressions (not reported) did not exhibit a qualitative difference from the above regressions, except for some large coefficients in the 90 percentile regressions.

4.3. Discussion

Our finding of the counter-cyclical LTV ratio means that bank risk exposure was decreasing, not increasing, during the boom period in terms of current (real time) pricing, at least conditional on lenders lacking contemporaneous knowledge of being in a bubble period. This finding implies that banks in Japan did not take excessive risk during the bubble period, which is inconsistent with the conventional wisdom blaming banks that fueled funds to form the bubble.

Our finding has an important implication on the efficacy of implementing a cap on the LTV ratio. The implicit assumption behind the unconditional cap on the LTV ratio as a macroprudential measure is that the ratio is pro-cyclical. Our finding implies that this may not be the case. Because of the counter-cyclicality of the LTV ratio, a simple cap on the LTV ratio may not have worked in Japan as a binding constraint to dampen the build-up of risk in the banking system during the boom period. If we want to curb the volume of credit during the boom, we might need a very low LTV cap, and/or vary the level of the cap in a counter-cyclical manner.

One might wonder this issue of efficacy is an unnecessary concern for loans of other types in other countries, especially residential mortgages, because such loans are significantly different from business loans, and may not necessarily exhibit counter-cyclicality. While our results on LTV cyclicality in the business loan market are not consistent with recent experience with residential mortgages in other countries (e.g., mortgages in Ireland, Sweden and subprime mortgages in the U.S.), they are consistent with residential mortgages under other conditions (e.g., Japan from 1994 to 2009 (Bank of Japan 2012) and the U.K. from 1997 to the late 2000s (FSA 2009)).[24] They are also consistent with Goodhart et al. (2012) who calibrated the effects of different macroprudential policy measures on credit expansion and house prices in a general equilibrium model.[25] Thus, irrespective of the markets (residential vs. business loans), counter-cyclicality of LTV ratios is not necessarily an uncommon phenomenon, and thus the ineffectiveness of a simple LTV cap can be a concern for residential mortgages or other types of loans beyond the real estate-based business loans we assess in this paper.

4.4. Some methodological issues

In the previous subsections, we find that the LTV ratio is counter-cyclical. Although we find robustness of the finding to controlling for observable loan-, borrower-, and lender-characteristics, our findings may still suffer from biases due to limitations that are inherent in our data. In this

[24] As another evidence to suggest ineffectiveness of a uniform LTV cap, Justiniano et al. (2015, Figure 1.2.) find in the U.S. that residential mortgage LTVs remained unchanged during the housing boom until 2006, and then spiked after the collapse of housing prices, and Campbell and Cocco (2014, Figure 1) report that origination LTV ratios for residential mortgages were stable from 1984-2008. Furthermore, using U.S. data from 1998-2008 Glaeser et al. (2013, Table 7.13) report that cumulative origination LTVs ratios (using the sum of the loan amounts of up to three mortgages as the numerator) are fairly stable over time, but that origination LTV ratios for first lien residential mortgages are counter-cyclical.

[25] They find with respect to LTV caps that a large increase in asset prices in a boom lowers the LTV ratio. They argue that this lowered LTV ratio makes it 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).

subsection, we discuss possible biases from various dimensions, and consider whether we can still conclude that the LTV ratio is counter-cyclical.

4.4.1 Survivorship bias

One of the most important concerns in our findings is a survivorship bias. Although we have a sample of collateral registrations that spans the period 1975 to 2009, the sample is limited to the registrations that *still exist* in the official real estate registry from 2008 to 2010. This *synthetic* nature of our data could be associated with biases in two directions. On the one hand, firms in our sample may be longer-lived and are likely to be *more* creditworthy, because registrations for borrowers that had defaulted and exited before 2008 are not included in our sample. On the other hand, firms in our sample may be *less* creditworthy, because firms who left the sample may have been stronger borrowers who were able to repay loans before 2008 and had no loan demand thereafter (e.g., because they were strong enough to fund internally). Thus, it is a priori unclear whether firms in our sample are more or less creditworthy, and we cannot predetermine in what direction our findings on the LTV ratio are biased.

However, if any survivorship bias existed throughout our sample period, the LTV ratio should have a monotonically decreasing or increasing trend reflecting the change in the mix of firm quality over time. If, for example, for better- (worse-)quality firms that dominate the earlier periods, banks may be willing to lend more (less) for the same amount of collateral, ceteris paribus, there should be a downward (upward) trend in the LTV ratio. This is not observed in Figure 4, so we can rule out such a bias stemming from consistently increasing or decreasing borrower creditworthiness throughout the period.

However, we must also consider a bias that is period-specific, i.e., a kind of a cohort effect. For example, the low LTV ratio that we found for the bubble period might be because high LTV loans underwritten during the bubble period had higher likelihood of defaulting and disappeared from our sample. Due to data limitations, we cannot directly examine whether our findings suffer from such a bias, but we can address it indirectly.

First, we can decompose loans underwritten in each year by priority. To the extent that firms with high LTV loans underwritten during the bubble period disappeared from the sample were risky, we would expect that these loans would tend to reflect relatively lower collateral seniority (i.e., low-priority loans in the sense of having a junior collateral position). That is, loans underwritten with a lower collateral priority during the bubble period would have been relatively more likely to disappear from the sample. Figure 5 shows the decomposition of our sample firms in each year by priority. The figure clearly shows that loans that were underwritten during the bubble period and remain in the sample are *more* risky in the sense that they have a lower collateral priority, *inconsistent* with the prediction that risky firms disappeared.

Second, although we do not know how many firms went bankrupt and disappeared from the sample before 2008, we do have information on bankruptcy filings for our sample firms *after* 2008. Figure 6 shows the rate of bankruptcy (during the 2008-2010 period) for borrowing firms in our sample depending on the period of their loan origination (for each of the five cohorts). The rate is calculated for loans in each of the four quartiles based on the 25, 50, and 75 percentiles of our LTV ratio (calculated in each year). Although the figure generally shows that younger loans and loans with higher LTV ratios are more likely to go bankrupt from 2008 to 2010, we do not observe that the rate of bankruptcy of high-LTV borrowers is exceptionally higher for loans underwritten during the bubble period.

On balance, this indirect evidence suggests that loans underwritten during the bubble period in our sample are not particularly unusual. That is, this indirect evidence is not consistent with a survivorship bias driven by the exit of relatively risky firms whose loans were underwritten during the bubble period.[26]

[26] Also consistent with our findings, some existing studies on residential mortgages report counter-cyclical LTV ratios using complete (i.e., non-synthetic) data that is immune to survivorship bias (Justiniano et al. 2015 and Campbell and Cocco 2014 using aggregate data, and Glaeser et al. 2013 using the universe of micro data from the 89 U.S. metropolitan areas).

4.4.2 Value of buildings

Another concern in our findings is that the omission of the value of buildings from the denominator of our LTV ratio due to data limitations may drive counterfactual counter-cyclicality. Omitting the value of buildings may not be as problematic in Japan as would be in other countries because in Japan, the value of buildings is generally smaller relative to the value of land. Consistent with this view, the durability of Japanese buildings is relatively short, and hence the rate of real depreciation is high, as compared with Europe or the U.S.[27] Also, the fact that we found countercyclicality in our univariate aggregate analysis using aggregate data *that included the value of buildings* (Panel (B) of Figure 2) suggests that omitting buildings in our multivariate analysis may not be a big concern. To push this even further, we decompose V in Figure 2 using the SNA data

into the value of land and of buildings in Figure 7. We find that the value of buildings is smaller, especially during the bubble period.

Nevertheless, we cannot entirely exclude the possibility that the omission of the value of the

[27] 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 (e.g., office buildings), we do not 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. Yoshida (2016) finds that depreciation rates for housing and commercial properties are respectively 6.2%-7.0% and 9.1%-10.2% in Japan, while the housing depreciation rate in the U.S. is merely 1.5%.

buildings still matters. As we theoretically demonstrate in Appendix D, LTV ratios with and without the value of buildings may exhibit different patterns of cyclicality if the magnitude of the underestimation of V due to the omission of the value of buildings (as expressed by the ratio of the true value of V over the value of land only) exhibits significant pro-cyclicality. Although we cannot directly calculate this underestimation, evidence based on the SNA data suggests that the magnitude of the underestimation does not exhibit significant cyclicality, and would not reverse the counter-cyclicality that we have found in our univariate and multivariate analyses (see Appendix D).

4.4.3 Future value of collateral

Another possible methodological concern is that lenders might take into account expected future land values when underwriting loans, which makes it inappropriate to define V as the current value of land. To address this, we calculate and compare the LTV ratios under two different alternative definitions of V. The first definition uses land value one year later, V(t+1), reflecting perfect (one year) lender foresight in loan underwriting.[28] The second definition uses a V that is interpolated from its previous year's growth rate, i.e., $V(t-1)\cdot\{V(t-1)/V(t-2)\}$, which assumes a naive prediction based on its past values. In unreported results, we find that under either of these

[28] Using V(t+1) might also be appropriate because there might be a lag in reporting the land price in the data that we used to predict land values (i.e., PNLP).

alternative definitions of V, the LTV ratios still exhibit almost similar counter-cyclicality as shown in Figure 4.[29]

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 borrower ex post performance. Here we examine the validity of another important underlying assumption behind LTV caps: high-LTV loans perform worse than low-LTV loans (see, for example, FSB 2012) – a necessary condition for a simple LTV cap to dampen the risk channel. If this argument is valid, then imposing an LTV cap would inflict little or no harm on the economy while minimizing bank losses by constraining loans to poorly performing borrowers. While some evidence on the ex post performance of high LTV loans justifies this argument for residential mortgages, there is little evidence on business loans.[30]

To address this, we construct samples of *treatment* observations (high-LTV borrowers) and of *control* observations (low-LTV borrowers), and compare the ex post performance of these two groups using several alternative performance measures. We define our treatment (high-LTV)

[29] See Ono et al. (2013, subsection 3.1.3.) for these results.

[30] For the recent evidence on the positive relation between origination LTV ratios and default rates for residential mortgages, see, for example, Campbell and Cocco (2014) and FSA (2009) and references therein. As far as we know,

observations as loans that are in the fourth quartile of the entire sample in terms of their LTV ratios.[34]

The control observations are defined using two alternative procedures. In the first, we simply consider the control group to be all firms with non-treatment loans (non-high-LTV loans) and compare them with the treatment group (firms with high LTV loans). In the second alternative, we define as control firms those that have similar ex-ante characteristics with each treatment firm by employing a propensity score matching approach. Using the matching approach allows us to control for the differences in ex-post performance between high- and low-LTV firms stemming from their ex ante differences. [35] Matched controls also 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 probability that a borrower obtains a

Agarwal and Ben-David (2014) is the only paper that examines the effect of LTV ratios on loan performance for business loans, although it is not the main focus of the paper.

[34] In the case where a firm obtained multiple secured loans in a year, we use the one with the highest LTV ratio. The number of observations is thus reduced from 59,125 loans in the previous section to 48,334 firms for this analysis.

[35] Following studies that employ propensity score matching DID approach, we assume unconfoundedness, i.e., the treatment/control choice is independent of the outcome. Our rich set of covariates employed for the propensity score matching justifies this assumption.

high-LTV loan conditional on the covariates that are used in section 4.2.[31] For each treatment (high-LTV) observation, the matched observation is selected from the non-treatment firms by having the closest propensity score.[32]

As for the variables that indicate ex post performance of the firms, we use four firm characteristics: (1) the number of employees and (2) the log amount of sales to represent firm growth (in terms of size), (3) ROA to represent firm profitability, and (4) the capital-asset ratio to represent credit risk. The analysis for these variables is similar, at least in *spirit*, to a difference-in-differences (DID) approach. That is, for each treatment or control firm, we take differences in the performance variables from year t (when the loan was originated) to year $t+k$ (k = 1 to 5), and eliminates time-invariant firm-fixed effects. We then calculate the average difference in these differences within the treatment and control firms (either unmatched or matched). As in the quantile regressions in the previous section, the sample period begins in 1990 due to data availability. Because we take five year differences at maximum in the performance variables, the sample period ends in 2004.

In addition to these four variables, we also focus on an incidence of bankruptcy as a measure

[31] The results of the probit estimation are similar to those of the quantile regressions in Table 4, and so we do not report them (which are available from the authors).

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

of ex post performance. In this analysis, we simply compare the rates of ex post bankruptcy between the treatment and the control groups. As explained in subsection 4.4.1 (Figure 6), we only have information on bankruptcy after 2008. Thus, we use an indicator that takes the value of one if the loan went into bankruptcy during the 2008-2010 period. When using the unmatched control, this analysis is essentially the same as the final robustness test in subsection 4.4.1 (Figure 6), but we also compare the rate of bankruptcy using matched control.

5.2. Results

Tables 3 and 4 show the results of the ex-post performance analysis. Table 3 is for the analysis a la DID using the four firm performance variables except for the variable on bankruptcies. Rows (1) and (2) respectively report the results using the unmatched and the matching estimators, and the four columns respectively report the results using the whole sample, and the subsamples of 1990-94, 95-99, and 2000-04. In each column, we show the average ex-post performance of treatment groups (high LTV firms) and control groups (non-high LTV firms), and their differences. We also show 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.

When we focus on the ex post performance at the firm level (Row (1) of Table 3), the results for the unmatched estimator show that treatment firms (high LTV firms) perform better than control firms (low LTV firms) in terms of employment growth (*d_F_EMP* in years t+1 and t+2) and in terms 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 in terms of sales growth (*d_F_lnSALES*) and changes in the capital-asset ratios (*d_F_CAP*). Also, significant and positive estimators for *d_F_EMP*, *d_F_lnSALES*, and *d_F_ROA* in the second column show that the high LTV borrowers perform better especially in years 1990-94 (during and after the bubble burst). However, as shown in the third and the fourth columns, we no longer find that treatment firms performed better after the bubble burst. Such firms sometimes exhibit worse performance (e.g., negative estimators for *d_F_lnSALES*).

Turning to the matched estimators shown in Row (2), from the first column using the whole sample, we find that high LTV firms performed better in terms of employment growth. However, we find no significant differences in other ex-post performance variables. These findings suggest that the performance of high and low LTV borrowers with similar ex-ante characteristics are almost comparable. The other three columns show that the average performance of treatment firms was better during 1990-94, but the differences almost disappeared afterwards.

Table 4 shows the results for the analysis on ex post bankruptcy (during the 2008-2010 period). In this table, we report the rates of ex post bankruptcy for treatment firms (high LTV firms in the 4^{th} quartile) and control firms (low LTV firms in the 1^{st} quartile) and their difference together with the significance level for the test of the equivalence in the ratios. The figures in Rows (1) and (2) are respectively the results using the unmatched and the matched controls. As shown in the six columns, we report the results using the whole sample, and the subsamples of 1990-94, 95-99, 2000-04, 2005-2007, and 2008-2009.[33]

We find that the rate of bankruptcy is higher for high-LTV loans for the whole sample and for all the subsamples when using unmatched controls (Row (1)). This finding is mostly consistent with the finding in Figure 6, and suggests that if we had set a uniform cap on the LTV ratio, we might have been able to purge risky loans. However, the significant differences disappear for the subsamples after 1995, when we use the matched controls (Row (2)). This finding implies that after 1995, high LTV loans are riskier not because the LTV ratios are higher, but because high LTV loans are underwritten to observably riskier borrowers.

To summarize, we find mixed results on the ex-post performance of firms with higher LTV loans. We find that in terms of ex-post performance measured by employment growth, sales growth and an increase in ROA, firms that obtain high LTV loans do not perform worse, and actually better, during or just after the bubble period. These findings suggest that a high LTV ratio does not reflect by itself lax lending standards. However, we also find that firms that obtain high LTV loans

[33] The quartiles are calculated in each subsample. The results do not qualitatively change even if we calculate the quartiles for the whole sample.

do perform worse in terms of subsequent bankruptcy, although our analysis is confined to bankruptcy after 2008. This finding is consistent with conventional wisdom that loans with high LTV ratios are risky. Taken together, our findings suggest that firms that obtain high LTV loans are risky but growing firms.[34]

These mixed findings on ex post performance for high LTV loans has an important policy implication, because it speaks to the issue of unintended consequences from imposing a cap on the LTV ratio. In the previous section, we find evidence suggesting that a simple cap on business loans would have been ineffective in dampening lending booms in Japan. In addition to this ineffectiveness, the findings in this section imply that imposing a simple LTV cap might have curbed lending to risky but growing firms. Thus, when deliberating on the deployment of an LTV cap as a macroprudential policy tool in business lending, policy-makers need to be careful about its institutional design and consider the possibility that an LTV cap might reduce the debt capacity of growing firms.

[34] The findings of no worse ex-post performance, and even better performance after the bubble, for high LTV borrowers would seem to be inconsistent with the findings on zombie firms (e.g., Peek and Rosengren 2005, Caballero, Hoshi, and Kashyap 2008). However, we need to keep in mind that what these studies find is evidence suggesting that poor-performing firms could survive due to evergreening loans by banks, which cannot be directly compared with our findings for ex-post performance of high versus low LTV borrowers. Also, these studies focus mostly on late 1990s, but we find the better ex-post performance for early 1990s.

6. Conclusion

Using unique data from the official real estate registry in Japan, this paper looks at the LTV ratios of business loans secured by real estate. 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 are at the same time riskier but faster growing than those granted low LTV loans.

Our findings have important policy implications, because they are inconsistent with the two underlying assumptions for LTV caps to be effective: the pro-cyclicality of LTV ratios and a worse performance for loans with higher LTV ratios. Our findings rather suggest that imposing LTV caps on business loans in Japan would likely have been unsuccessful as a macroprudential policy tool to mitigate the risk build-up in the financial system during the bubble period. Our findings also imply that the efficacy of an LTV cap may depend crucially on how it is conditioned.

While our analysis focuses on business lending, our results on LTV cyclicality and LTV loan performance could conceivably apply to residential mortgages as well. However, two caveats are worth mentioning in generalizing our findings to the residential real estate market. First, inconsistent with the results from our ex post performance analysis, high LTV lending in the U.S. residential mortgage market in the form of subprime mortgages appears to have resulted in higher losses. Second, first (i.e., senior) residential mortgages are usually used to purchase the real estate itself, unlike most secured business loans in our sample. To examine LTV ratios of mortgages, we need to take into account their direct link with asset pricing (the pricing channel). However, it should also be noted that the pricing channel is not likely to have operated through 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.

References

Agarwal, S., and I. Ben-David, (2014). Do Loan Officers' Incentives Lead to Lax Lending Standards? NBER Working Paper No. 19945.

Aiyar, S., C.W. Calomiris, and T. Wieladek. (2014). Does Macro-Prudential Regulation Leak? Evidence from a UK Policy Experiment, Journal of Money, Credit, and Banking 46, 181-214.

Almeida, C., M. Campello, and C. Liu. (2006). The Financial Accelerator: Evidence from International Housing Markets, Review of Finance 10, 321-352.

Bank of Japan. (2012). Financial System Report. October.

Barlevy, G. and J.D.M. Fisher, (2012). Mortgage Choices and Housing Speculation, mimeo.

Basten, C., and C. Koch. (2014). Higher Bank Capital Requirements and Mortgage Pricing: Evidence from the Countercyclical Capital Buffer (CCB). Working paper.

Beck, T., A. Demirgüç-Kunt, and M. S. Martinez Peria. (2008). Bank Financing for SMEs around the World: Drivers, Obstacles, Business Models, and Lending Practices, World Bank Policy Research Working Paper Series No. 4785.

Berger, A.N., and G.F. Udell. (2004). The Institutional Memory Hypothesis and the Procyclicality of Bank Lending Behavior, Journal of Financial Intermediation 13, 458-495.

Berger, A.N., and G.F. Udell. (2006). A More Complete Conceptual Framework for SME Finance, Journal of Banking and Finance 30, 2945-2966.

Borio, C., Furfine, C., and P. Lowe. (2001). Procyclicality of the Financial System and Financial Stability: Issues and Policy Options, BIS.

Caballero, R.J., T. Hoshi, and A.K. Kashyap, (2008). Zombie Lending and Depressed Restructuring in Japan, American Economic Review 98, 1943-77.

Campbell J. Y., and J. F. Cocco, (2014). A Model of Mortgage Default, Journal of Finance, forthcoming.

Cerutti, E., S. Claessens, and L. Laeven, (2015). The Use and Effectiveness of Macroprudential Policies: New Evidence. IMF Working Paper WP/15/61.

CGFS (Committee on the Global Financial System). (2010). Macroprudential Instruments and Frameworks: a Stocktaking of Issues and Experiences, CGFS Papers No 38, BIS.

CGFS. (2012). Operationalising the Selection and Application of Macroprudential Instruments, CGFS Papers No 48, BIS.

Council of Land Policy, the National Land Agency, the Government of Japan. (1990). Report: How the Land Policy in the Future Ought to be Based on the Basic Act for Land, October 29. (in Japanese)

Council for Social Infrastructure, the Ministry of Land, Infrastructure, Transport and Tourism, the

Government of Japan. (2005). Reference for the Report “On the Institutional Framework to Accommodate New Housing Policy.” (in Japanese)

Crowe, C., G. Dell’Ariccia, D. Igan and P. Rabanal. (2013). How to Deal with Real Estate Booms: Lessons from Country Experiences, Journal of Financial Stability 9, 300-319.

FSA (Financial Services Authority, U.K.). (2009). Mortgage Market Review, FSA Discussion Paper 09/3.

FSB (Financial Stability Board). (2011). Thematic Review on Mortgage Underwriting and Origination Practices: Peer Review Report. 17 March.

FSB. (2012). Principles for Sound Residential Mortgage Underwriting Practices, 18 April. Gan, J. (2007). Collateral, Debt Capacity, and Corporate Investment: Evidence from a Natural Experiment. Journal of Financial Economics 85, 709-734.

Gerlach, S., and W. Peng. (2005). Bank Lending and Property Prices in Hong Kong, Journal of Banking and Finance 29, 461-481.

Glaeser, E., J. D. Gottlieb, and J. Gyourko (2013), Can Cheap Credit Explain the Housing Boom? in E. L. Glaeser and T. Sinai eds., Housing and the Financial Crisis, Chapter 7, University of Chicago Press, pp. 301-359

Goodhart, C. A. E., A. K. Kashyap, D. P. Tsomocos, and A. P. Vardoulakis. (2012). Financial Regulation in General Equilibrium, NBER Working Paper No. 17909.

Iacoviello, M. (2005). House Prices, Borrowing Constraints, and Monetary Policy in the Business Cycle, American Economic Review 95, 739-764.

Igan, D., and H. Kang. (2011). Do Loan-to-Value and Debt-to-Income Limits Work? Evidence from Korea, IMF Working Paper.

Illueca, M., L. Norden, and G.F. Udell. (2016). Do Changes in the Timeliness of Loan Loss Recognition Affect Bank Risk Taking? Working paper.

IMF-FSB-BIS. (2016). Elements of Effective Macroprudential Polices: Lessons from International Experience.

Justiniano, A., G.E. Primiceri, and A. Tambalotti. (2015). Household Leveraging and Deleveraging, Review of Economic Dynamics 18, 3-20.

Kashyap, A., and J. Stein. (2004). Cyclical Implications of the Basel-II Capital Standards, Federal Reserve Bank of Chicago, Economic Perspectives 28, 18-31.

Kuttner, K.N. and I. Shim. (2013) Can Non-Interest Rate Policies Stabilize Housing Markets? Evidence from a Panel of 57 Countries, NBER Working Paper No. 19723.

Lamont, O.A. and J.C. Stein. (1999). Leverage and House-Price Dynamics in U.S. Cities, Rand Journal of Economics 30, 498-514.

Laufer, S. (2014). Equity Extraction and Mortgage Default, paper presented at the 50th Bank Structure and Competition Conference.

Lim, C., F. Columba, A. Costa, P. Kongsamut, A. Otani, M. Saiyad, T. Wezel, and X. Wu. (2011). Macroprudential Policy: What Instruments and How to Use Them? IMF Working Paper 11/23.

Ono, A., H. Uchida, S. Kozuka, and M. Hazama. (2015). A New Look at Bank-Firm Relationships and the Use of Collateral in Japan: Evidence from Teikoku Databank Data, in T. Watanabe, I. Uesugi, and A. Ono eds., The Economics of Interfirm Networks, ch.12, Springer.

Peek, J., and E.S. Rosengren. (2005). Unnatural Selection: Perverse Incentives and the Misallocation of Credit in Japan, American Economic Review 95, 1144-66.

Petersen, M. A., and R. G. Rajan. (2002). Does Distance Still Matter? The Information Revolution in Small Business Lending, Journal of Finance LVII, 2533-2570.

Stein, J. (1995). Prices and Trading Volume in the Housing Market: A Model with Down Payment Effects, Quarterly Journal of Economics 110, 379-406.

Ueda, K. (2000). Causes of Japan’s Banking Problems in the 1990s, in T. Hoshi and H. Patrick (eds.), Crisis and Change in the Japanese Financial System, Boston: Kluwer Academic Publishers.

Uemura, S. (2012). Japan's Experience with Credit Ceilings for Real Estate Lending, RIETI Policy Discussion Paper 12-P-019 (in Japanese).

Vandenbussche, J., U. Vogel, and E. Detragiache. (2013). Macroprudential Policies and Housing Prices – A New Database and Empirical Evidence for Central, Eastern, and Southeastern Europe, mimeo. IMF and Bundesbank.

Yoshida, K. (1994). Faults of Japanese Bank Management, Toyo-Keizai Inc., Tokyo (in Japanese).

Yoshida, J. (2016). Structure Depreciation and the Production of Real Estate Services, HITREFINED Working Paper Series, No. 44.

Tables and Figures

Figure 1 Number of observations

This figure reports the numbers of observations (NOB) in each year that are used for our univariate and regression analyses.

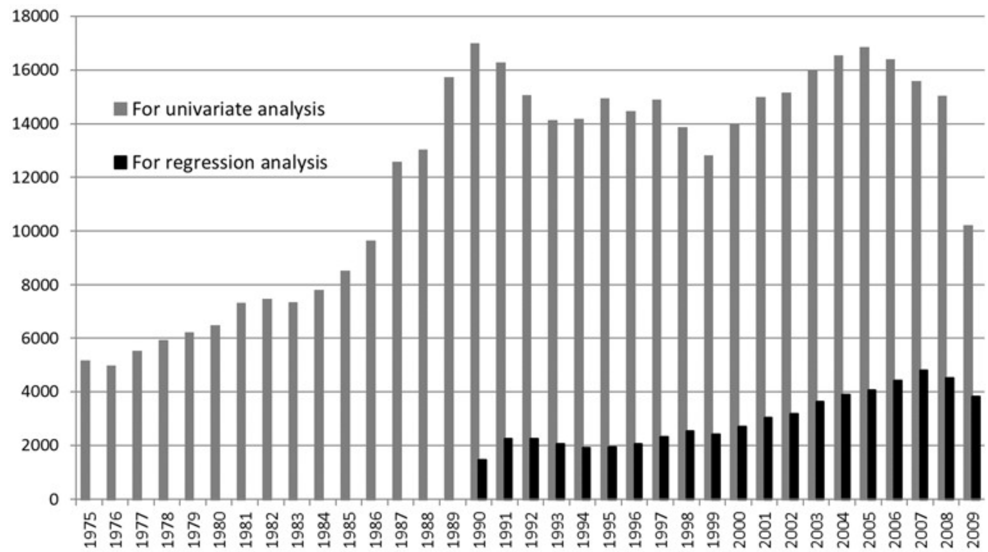
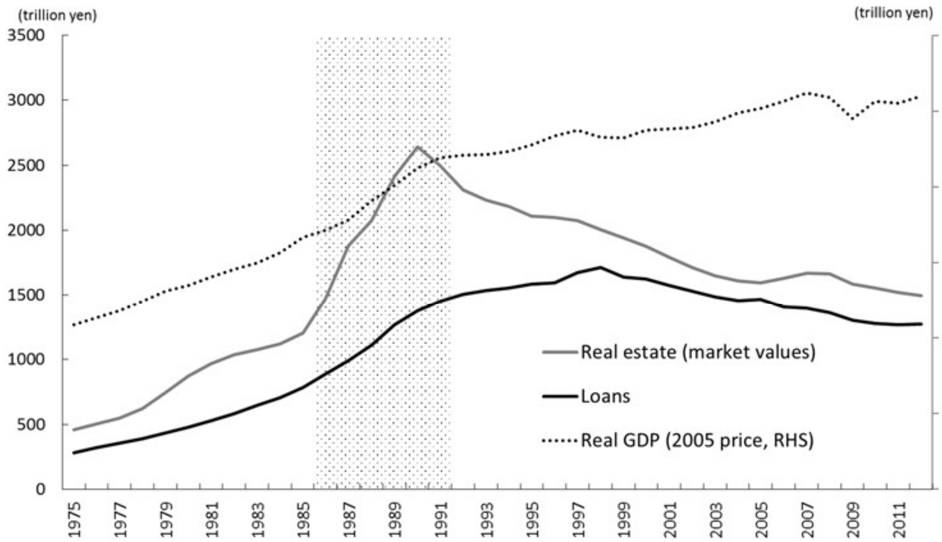


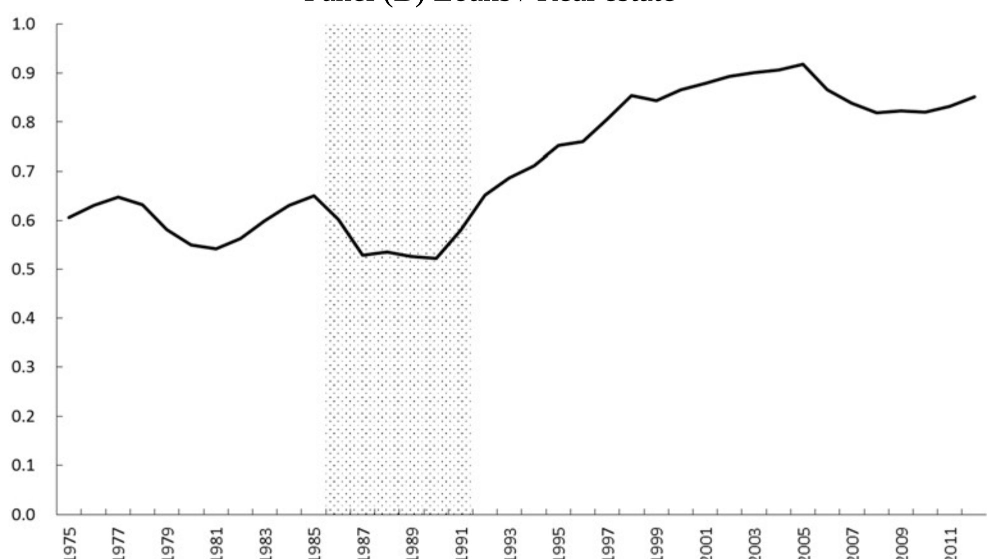
Figure 2 GDP, land price, and bank loans (2005 = 100)

Panel (A) shows the time-series path of the real GDP, the market value of real estate (sum of land and buildings), and the stock of loans outstanding at the aggregate level. Panel (B) shows the ratio of the stock of bank loans outstanding over the market value of real estate. The so-called “bubble” period from late 1980s to early 1990s is shaded.

Panel (A) Aggregate indicators



Panel (B) Loans / Real estate

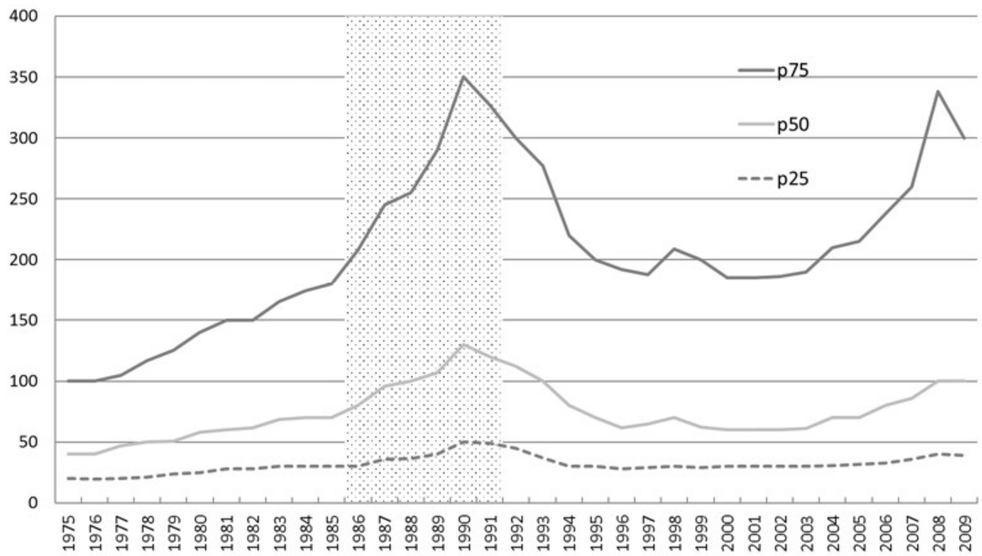


Note: Figures for years 1994-2012 are taken from "National Accounts for 2014". Figures for years 1975-1993 are calculated based on year-on-year growth rates obtained from "National Accounts for 2009" (1980-1993) and from "National Accounts for 1998 (68SNA, benchmark year = 1990)" (1975-1979).
Source: Cabinet Office, “National Accounts”

Figure 3 Loans and values over the business cycle

Panel (A) shows the time-series path of the amount of loans in our sample at its three percentile points. Panel (B) shows the time-series path of the value of collateralized land in our sample at its three percentile points. The so-called “bubble” period from late 1980s to early 1990s is shaded.

Panel (A) Loans



Panel (B) Value of collateral

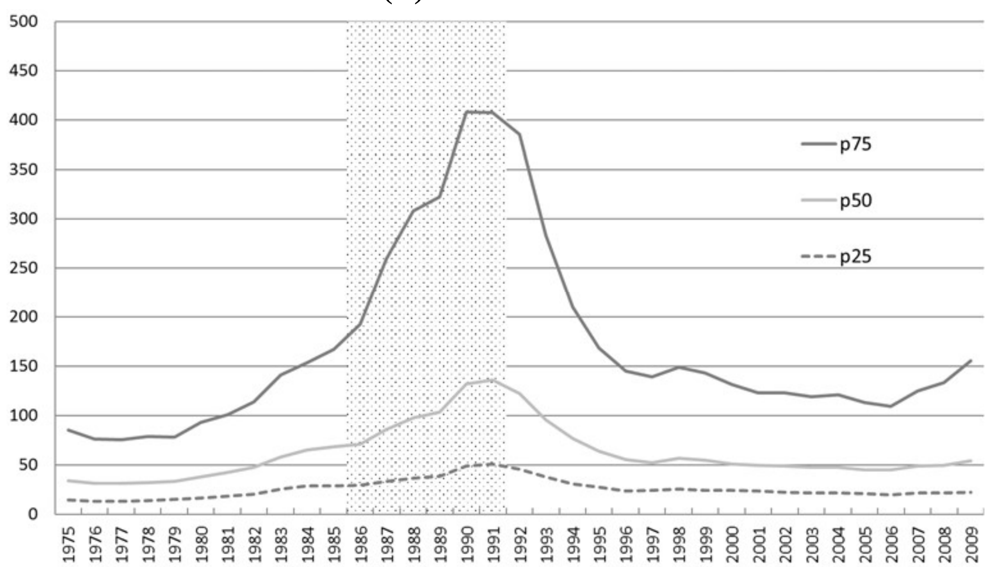


Figure 4 LTV ratios over the business cycle

This figure shows the time-series path of the LTV ratio in our sample, which is calculated as the amount of loans over the value of collateralized land. The so-called "bubble" period from late 1980s to early 1990s is shaded.

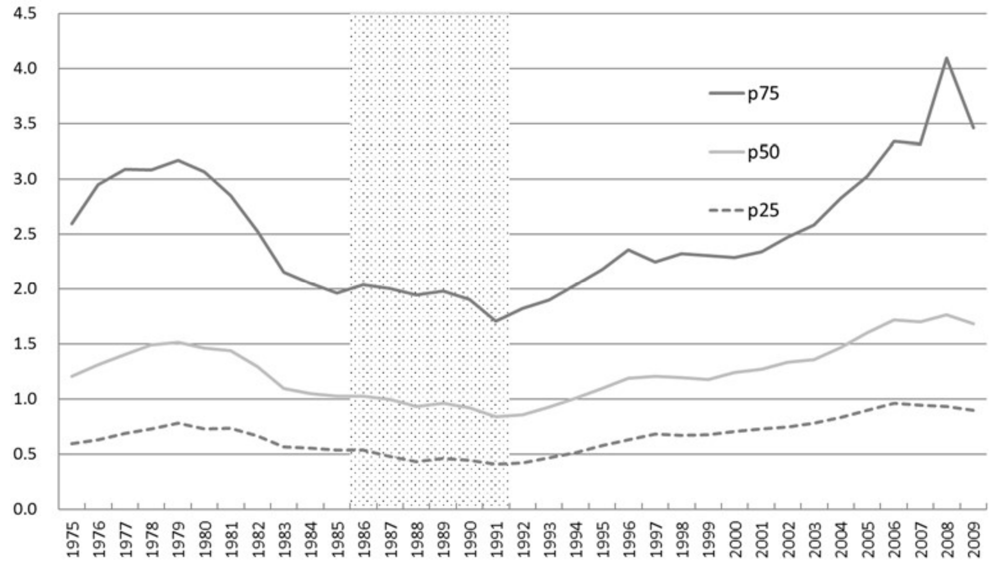


Figure 5 Decomposition of loans by priority

This figure shows the shares of loans in each year by different priorities.

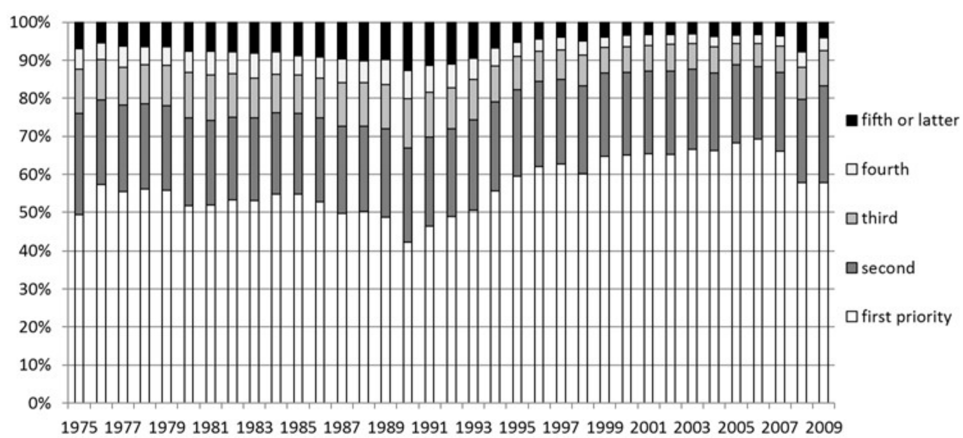


Figure 6 Rate of ex post bankruptcy during the 2008-2010 period

This figure shows the rate of bankruptcy (during the 2008-2010 period) of firms depending on the origination period (five-year cohort) of the loans. The rate is calculated for each quartiles of the LTV ratio.

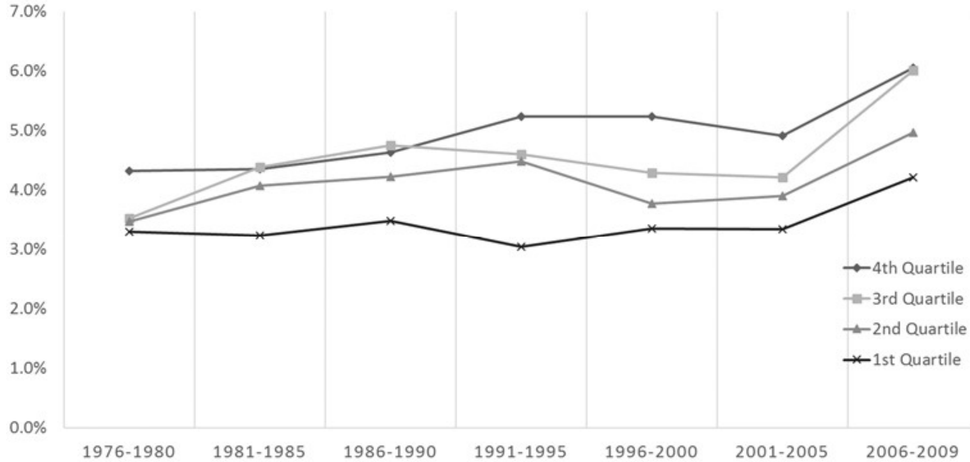


Figure 7

This figure shows the decomposition of the values of buildings and land based on the National Account Statistics by the Cabinet Office of Japan.

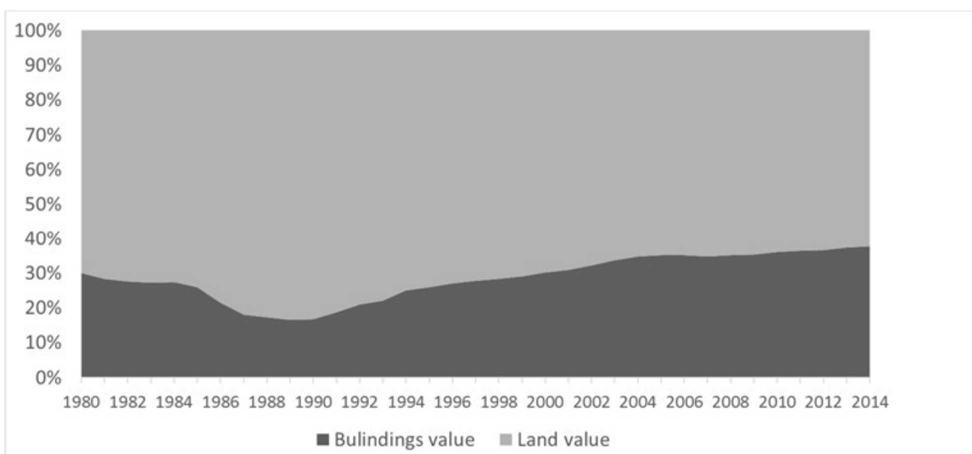


Table 1 Variable definitions and summary statistics

This table shows definitions and summary statistics of the variables used in the main analysis except for the year dummies. The number of observations is 59,125.

Label	Definition	mean	sd	min	p50	max
Dependent variable						
LTV	Loan-to-value ratio	7.718	434.32	0.000	1.385	99681.8
Loan characteristics						
L_netanpo	Ne-tanpo dummy: =1 if the collateral is ne-tanpo	0.660	0.474	0	1	1
Loan priority dummies						
L_PR0	Fifth or lower priority (default)	0.070	0.255	0	0	1
L_PR1	First priority	0.586	0.492	0	1	1
L_PR2	Second priority	0.219	0.413	0	0	1
L_PR3	Third priority	0.085	0.278	0	0	1
L_PR4	Fourth priority	0.040	0.197	0	0	1
Firm characteristics						
F_lnSALES	Log of gross annual sales	13.924	1.296	0	13.904	21.915
F_ROA	Return on Asset: = operating profit / total asset	0.032	0.084	-6.457	0.027	2.429
F_CAP	Capital-asset ratio: = net worth / total asset	0.181	0.257	-13.801	0.155	0.999
F_BUILD	Building-asset ratio: = building / total asset	0.288	0.268	0	0.246	9.942
F_AGE	Firm age	29.769	15.753	1	29	119
Borrower industry dummies						
F_IND0	Other industries (default)	0.003	0.057	0	0	1
F_IND1	Construction	0.317	0.465	0	0	1
F_IND2	Manufacturing	0.212	0.409	0	0	1
F_IND3	Wholesale	0.252	0.434	0	0	1
F_IND4	Retail and restaurant	0.052	0.222	0	0	1
F_IND5	Real estate	0.051	0.220	0	0	1
F_IND6	Transportation and communication	0.032	0.176	0	0	1
F_IND7	Services	0.080	0.272	0	0	1
	onal dummies					
F_REG0	Hokkaido and Tohoku (default)	0.133	0.340	0	0	1
F_REG1	North Kanto	0.030	0.170	0	0	1
F_REG2	South Kanto	0.298	0.458	0	0	1
F_REG3	Koshin-etsu	0.070	0.255	0	0	1
F_REG4	Tokai	0.106	0.307	0	0	1
F_REG5	Keihanshin	0.164	0.371	0	0	1
F_REG6	Other kinki	0.015	0.120	0	0	1
F_REG7	Chugoku	0.067	0.250	0	0	1
F_REG8	Shikoku	0.026	0.158	0	0	1
F_REG9	Kyushu and Okinawa	0.092	0.289	0	0	1
Lender characteristics						
BK_MAIN	Main bank dummy: = 1 if the lender is a main bank (top-listed bank) of a borrower firm.	0.269	0.443	0	0	1
Lender type dummies						
BK_TYPE0	City banks (default)	0.146	0.353	0	0	1
BK_TYPE1	Regional or second-tier regional banks	0.296	0.456	0	0	1
BK_TYPE2	Shinkin banks	0.153	0.360	0	0	1
BK_TYPE3	Credit cooperatives	0.016	0.126	0	0	1
BK_TYPE4	Government-affiliated financial institutions	0.174	0.379	0	0	1
BK_TYPE5	Other banks, security companies, or insurance companies, etc.	0.013	0.112	0	0	1
BK_TYPE6	Others (non-banks, credit guarantee corporations, non-financial firms, etc.)	0.202	0.402	0	0	1
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).	0.222	0.415	0	0	1

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.	0.001	0.035	0	0	1

Table 2 Estimation results - Quantile regressions

This table presents the results for the analysis on counter-cyclicality of the LTV ratios by controlling for a variety of factors. The quintile regression results are shown, in which the dependent variables are the LTV ratios (*LTV*). Columns (A) through (C) respectively report the results at the 50 (median), the 10, and the 90 percentile points of the LTV ratios. The main independent variables are the year dummies (*YEAR1991-2009*). For the definitions of the other variables, see Table 1. ***, **, and * respectively indicate that the relevant coefficients are statistically significant at the 1, 5, and 10% level.

Estimation method: Quantile regression		(A) Median (p50)	(B) p10	(C) p90
Dependent variable: *LTV*		Coef.	Coef.	Coef.
Registration year	*YEAR1991*	-0.017	-0.052 **	-0.051
	YEAR1992	0.000	-0.030	-0.147
	YEAR1993	0.074 *	-0.005	0.016
	YEAR1994	0.223 ***	0.064 **	0.611 ***
	YEAR1995	0.412 ***	0.148 ***	0.807 ***
	YEAR1996	0.545 ***	0.209 ***	0.928 ***
	YEAR1997	0.463 ***	0.207 ***	0.916 ***
	YEAR1998	0.480 ***	0.217 ***	0.814 ***
	YEAR1999	0.521 ***	0.260 ***	0.854 ***
	YEAR2000	0.618 ***	0.279 ***	0.948 ***
	YEAR2001	0.629 ***	0.293 ***	1.242 ***
	YEAR2002	0.704 ***	0.350 ***	1.096 ***
	YEAR2003	0.810 ***	0.355 ***	1.399 ***
	YEAR2004	0.898 ***	0.409 ***	1.854 ***
	YEAR2005	1.043 ***	0.458 ***	1.754 ***
	YEAR2006	1.090 ***	0.486 ***	2.124 ***
	YEAR2007	1.066 ***	0.471 ***	2.186 ***
	YEAR2008	1.016 ***	0.436 ***	2.201 ***
	YEAR2009	1.012 ***	0.432 ***	2.211 ***
Loan characteristics	*L_netanpo*	-0.062 ***	0.014 *	-0.201 ***
	L_PR1	-0.846 ***	-0.284 ***	-7.613 ***
	L_PR2	-0.205 ***	-0.052 ***	-4.758 ***

	L_PR3	0.077 ***	0.011	-2.852 ***
	L_PR4	0.084 **	0.044 **	-1.960 ***
Firm characteristics	*F_lnSALES*	0.187 ***	0.055 ***	0.875 ***
	F_ROA	0.292 ***	0.220 ***	0.031
	F_CAP	-0.148 ***	-0.076 ***	-0.450 ***
	F_BUILD	0.108 ***	-0.004	0.138
	F_AGE	-0.008 ***	-0.004 ***	-0.009 ***
Bank characteristics	*BK_MAIN*		-0.014 *	-0.163 **
Policy measures	*PL_ACTION*	-0.015	-0.012	0.311 ***
	PL_CEILING	-0.049 **	-0.074 -	-0.954
constant		-0.106	0.015	17.622 ***
Bank type, Industry, and Regional dummies		0.227 * Yes	Yes	Yes
NOB		59125	59125	59125
Pseudo R2		0.0201	0.0134	0.0347

Table 3 Ex-post performance for high- versus low-LTV borrowers

This table presents the results for the comparison of the ex-post performance between high- versus non-high LTV borrower groups, where high-LTV loans are defined as those in the fourth quartile of the entire LTV ratios. Year t refers to the year in which a loan was extended, and spans from 1990 to 2004. We evaluate the ex-post performance in years t+k (k=1, 2, …, 5) and use the differences (from year t to t+k) in the number of employee (*d_F_EMP*), in sales in logarithm (*d_D_lnSALES*), in return on asset (*d_F_ROA*), and in capital-asset ratio (*d_F_CAP*). DID (difference-in-differences) indicates the difference in the average ex-post performance variable between the treatment group (firms with high LTV loans) and the control group (firms with non-high LTV loans). ***, **, * respectively indicate that the null hypothesis of the differences being zero is rejected at the significance level of 1, 5, and 10% levels. In panel (1), control observations are simple unmatched non-treatment firms. In panel (2), control observations are the 5-nearest matched non-treatment firms that have the closest propensity scores to each treatment observation.

			(A) Entire sample			(B) 1990-1994			(C) 1995-1999			(D) 2000-2004		
			Treat.	Control	DID	Treat.	Control	DID	Treat.	Control	DID	Treat.	Control	DID
(1) Unmatched Control	d F EMP	t+1	0.417	0.217	0.200 ***	1.463	0.673	0.789 ***	-0.022	-0.155	0.133	0.165	0.087	0.078
		t+2	0.487	0.283	0.204 **	2.070	1.001	1.069 ***	-0.477	-0.575	0.098	0.387	0.288	0.100
		t+3	0.278	0.137	0.141	2.128	0.817	1.311 ***	-1.497	-1.252	-0.246	0.459	0.486	-0.027
		t+4	0.194	0.054	0.140	2.074	0.402	1.672 ***	-2.472	-1.857	-0.614 *	0.640	0.809	-0.169
		t+5	0.108	-0.136	0.244	1.427	-0.337	1.764 ***	-3.009	-2.326	-0.682	0.816	1.042	-0.226
	d F lnSALES	t+1	0.008	0.007	0.001	0.027	0.002	0.025 ***	0.001	0.003	-0.002	0.014	0.019	-0.006
		t+2	0.010	0.008	0.002	0.031	-0.005	0.036 ***	-0.018	-0.007	-0.011	0.036	0.038	-0.002
		t+3	0.008	0.009	-0.001	0.048	-0.004	0.052 ***	-0.043	-0.029	-0.014 *	0.049	0.057	-0.009
		t+4	0.005	0.008	-0.003	0.047	-0.008	0.055 ***	-0.074	-0.051	-0.023 **	0.059	0.080	-0.021 ***
		t+5	-0.003	0.002	-0.005	0.029	-0.023	0.052 ***	-0.085	-0.059	-0.026 **	0.042	0.072	-0.030 ***
	d F ROA	t+1	-0.005	-0.005	0.000	-0.007	-0.007	0.001	-0.002	-0.003	0.001	-0.003	-0.002	-0.001
		t+2	-0.005	-0.006	0.001	-0.010	-0.013	0.003 *	-0.001	-0.002	0.001	-0.001	-0.002	0.000
		t+3	-0.006	-0.008	0.001 **	-0.012	-0.017	0.005 ***	0.000	-0.002	0.002	-0.003	-0.002	-0.001
		t+4	-0.006	-0.008	0.002 **	-0.014	-0.019	0.005 ***	0.000	-0.002	0.002	-0.003	-0.002	-0.001
		t+5	-0.007	-0.009	0.003 ***	-0.018	-0.022	0.004 **	0.001	-0.001	0.002	-0.006	-0.006	0.000
	d F CAP	t+1	-0.003	-0.002	-0.001	-0.003	-0.001	-0.002 *	0.001	0.000	0.002	0.000	0.000	0.000
		t+2	0.001	0.002	-0.001	0.001	0.004	-0.003	0.009	0.005	0.004 **	0.004	0.004	0.000
		t+3	0.006	0.007	-0.001	0.007	0.008	-0.001	0.015	0.012	0.003	0.008	0.008	0.000
		t+4	0.013	0.013	-0.001	0.012	0.014	-0.002	0.023	0.020	0.003	0.013	0.012	0.000
		t+5	0.020	0.019	0.001	0.019	0.019	0.000	0.032	0.026	0.006 **	0.018	0.017	0.001
(2) Matched Control	d F EMP	t+1	0.417	0.274	0.143 *	1.463	0.804	0.658 ***	-0.022	-0.360	0.338	0.165	0.270	-0.105
		t+2	0.487	0.193	0.294 **	2.070	1.139	0.931 **	-0.477	-1.118	0.641 **	0.387	0.351	0.036
		t+3	0.278	-0.014	0.292 *	2.128	0.921	1.207 **	-1.497	-2.119	0.622	0.459	0.497	-0.038
		t+4	0.194	-0.192	0.386 *	2.074	0.262	1.812 ***	-2.472	-3.269	0.798 *	0.640	0.802	-0.162
		t+5	0.108	-0.570	0.678 ***	1.427	-0.804	2.231 ***	-3.009	-3.624	0.615	0.816	0.885	-0.069
	d F lnSALES	t+1	0.008	0.001	0.007 **	0.027	0.006	0.022 ***	0.001	-0.003	0.004	0.014	0.010	0.004
		t+2	0.010	0.004	0.006	0.031	-0.001	0.032 ***	-0.018	-0.014	-0.003	0.036	0.031	0.005
		t+3	0.008	0.003	0.004	0.048	0.004	0.044 ***	-0.043	-0.041	-0.002	0.049	0.047	0.001
		t+4	0.005	0.001	0.004	0.047	0.001	0.046 ***	-0.074	-0.069	-0.005	0.059	0.069	-0.010
		t+5	-0.003	-0.007	0.004	0.029	-0.015	0.044 ***	-0.085	-0.077	-0.009	0.042	0.051	-0.009
	d F ROA	t+1	-0.005	-0.005	0.001	-0.007	-0.006	-0.001	-0.002	-0.002	0.000	-0.003	-0.002	-0.001
		t+2	-0.005	-0.005	0.000	-0.010	-0.012	0.002	-0.001	-0.002	0.001	-0.001	0.001	-0.002
		t+3	-0.006	-0.007	0.001	-0.012	-0.016	0.004 **	0.000	-0.001	0.001	-0.003	0.000	-0.003 *
		t+4	-0.006	-0.007	0.001	-0.014	-0.018	0.004 **	0.000	-0.002	0.002	-0.003	0.000	-0.003 *
		t+5	-0.007	-0.008	0.001	-0.018	-0.019	0.001	0.001	0.001	0.000	-0.006	-0.004	-0.002
	d F CAP	t+1	-0.003	-0.002	0.000	-0.003	0.000	-0.003 **	0.001	0.002	0.000	0.000	0.000	0.000
		t+2	0.001	0.002	-0.001	0.001	0.004	-0.003	0.009	0.008	0.002	0.004	0.004	0.000
		t+3	0.006	0.007	-0.001	0.007	0.009	-0.002	0.015	0.013	0.002	0.008	0.006	0.001
		t+4	0.013	0.012	0.000	0.012	0.014	-0.001	0.023	0.021	0.002	0.013	0.011	0.002
		t+5	0.020	0.018	0.002	0.019	0.020	-0.001	0.032	0.025	0.007 **	0.018	0.015	0.003

Table 4 Ex-post performance (bankruptcies) for high- versus low-LTV borrowers

This table presents the results for the comparison of the ratio of bankruptcies between high- versus non-high LTV borrower groups, where high-LTV loans are defined as those in the fourth quartile of the entire LTV ratios. It measures the number of bankruptcies that occur between 2008 and 2010 for each cohort years a loan was extended: 1990-1994, 1995-1999, 2000-2004, 2005-2007, and 2008-2009 and compares its frequency between the treatment firms (high LTV firms in the 4th quartile) and the control firms (low LTV firms in the 1st quartile). DID (difference-in-differences) indicates the difference in the average bankruptcy ratio between the treatment group (firms with high LTV loans) and the control group (firms with non-high LTV loans). ***, **, * respectively indicate that the null hypothesis of the differences being zero is rejected at the significance level of 1, 5, and 10% levels. In panel (1), control observations are simple unmatched non-treatment firms. In panel (2), control observations are the 5-nearest matched non-treatment firms that have the closest propensity scores to each treatment observation.

(1) Unmatched control

	Entire sample			1990-1994			1995-1999			2000-2004		
	Treatment	Control	DID	Treatment	Control	DID	Treatment	Control	DID	Treatment	Control	DID
2008-2010	0.074	0.055	0.019***	0.057	0.043	0.014***	0.065	0.055	0.010*	0.078	0.055	0.023***
				2005-2007			2008-2009					
				Treatment	Control	DID	Treatment	Control	DID			
2008-2010				0.090	0.062	0.027 ***	0.074	0.054	0.020 ***			

(2) Matched control

	Entire sample			1990-1994			1995-1999			2000-2004		
	Treatment	Control	DID	Treatment	Control	DID	Treatment	Control	DID	Treatment	Control	DID
2008-2010	0.074	0.064	0.010 ***	0.057	0.044	0.014 **	0.065	0.064	0.001	0.078	0.071	0.007
				2005-2007			2008-2009					
				Treatment	Control	DID	Treatment	Control	DID			
2008-2010				0.090	0.079	0.011	0.074	0.065	0.009			

Appendices for Online Publication

Appendix A: Identification of business loans

To identify business loans, we first classify all of the loans secured by ne-tanpo (see the next paragraph) 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. The number of observations thereby identified as residential loans is 37,352.

Ono et al. (2013) discuss the evolution of LTV ratios for these residential loans.[35]

Appendix B: Estimation of the current value of land

B.1 Hedonic approach

As explained in section 3.2, the denominator of the LTV ratio, V (the per-acreage price of the land), is estimated using the hedonic approach that is widely used in the field of real estate economics. This approach assumes that the price of a land is the sum of the values of its attributes such as size, a floor area ratio, a physical distance to metropolis in the region, and so on (see Ohnishi et al., 2011).[36] In particular, we assume that the log price of a land *i*, $\log P_i$, is the sum of K components:

$$\log P_i = \sum_{k=1}^{K} x_{ik} .$$

In the actual estimation, we follow the following steps. First, using the dataset of "Public notice of land prices (PNLP)" provided by the Land Appraisal Committee of the Ministry of Land, Infrastructure, Transport and Tourism of the Government of Japan, we estimate a hedonic model where the log price of lands complied in PNLP is explained by different explanatory variables. The explanatory variables in this estimation are:

- the size of land in logarithm
- a regulatory upper limit of the floor area ratio

[35] Ono, A., H. Uchida, G. Udell, and I. Uesugi. (2013). Lending Procyclicality and Macro-prudential Policy: Evidence from Japanese LTV Ratios, Available at SSRN: http://ssrn.com/abstract = 2262575.

[36] Ohnishi, T., T. Mizuno, C. Shimizu, and T. Watanabe (2011). The Evolution of House Price Distribution, RIETI Working Paper Series 11-E-019.

- an Euclidean distance from the relevant land to the one whose price is the highest in the same prefecture
- the square term of the Euclidean distance
- an Euclidean distance from the land to the one whose price is the highest 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
- dummy variables representing the type of land districts where the land is located (i.e., whether the land is located in a residential, commercial, or industrialized district).

We run a large number of regressions for different combinations of land district type (3 types: residential, commercial, or industrialized), year (35 years: 1975-2009), and region. As for the regions, we in principle use 47 prefectures in Japan, but in the case when the number of observations in a prefecture is not large enough to warrant trustable estimation results, we use an area including several neighboring prefectures as a unit for the region (15 areas). However, this is the case only for a subset of the industrialized lands.

Second, based on the parameters obtained from the estimation of the above regressions, we project (predict) the current prices of the land in our dataset. We need to predict these prices because the number of the pieces of land in our dataset is far larger than that in the PNLP dataset. We have different sets of parameters depending on land district type, year, and region (obtained from the first stage estimations). When we project the price of a particular piece of land in our dataset, we use the parameters for the same land district type, year, and region.[37]

Finally, the value of the land is obtained by multiplying its projected price and the acreage obtained from the TDB database.

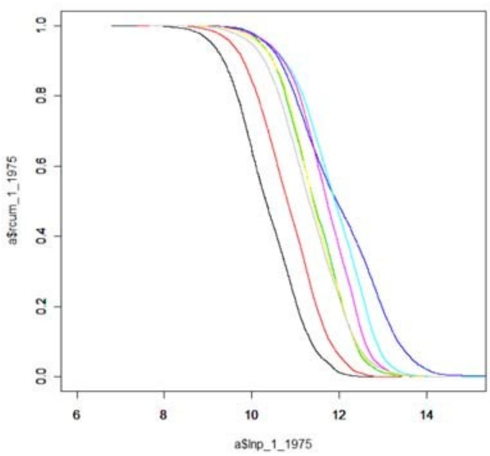
B.2 Estimation results (first stage)

As for the first stage estimation of hedonic models, the numbers of the regressions that we run for lands in residential districts and in commercial districts are both 1,738 (= 47 prefectures times 37 years, except for Okinawa in year 1975), and that for lands in industrial districts is 555 (15 regions times 37 years).

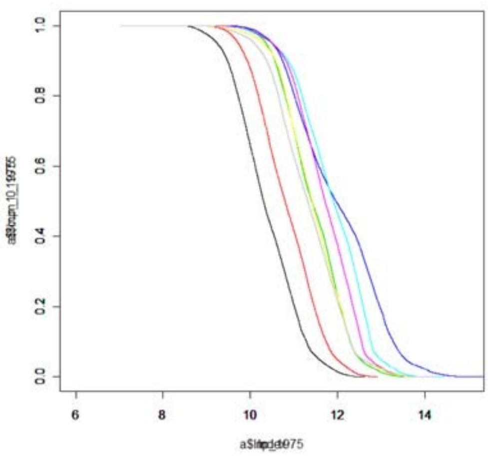
Figure B-1: In-sample comparisons between cumulative distributions of estimated and actual prices (PNLP)

[Residential land]

[Estimation]



[Actual]

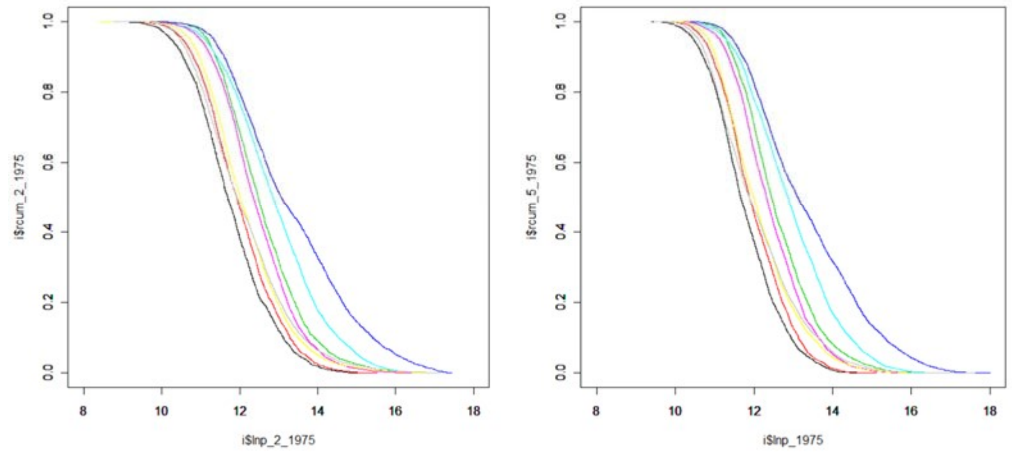


[Commercial land]

[Estimation]

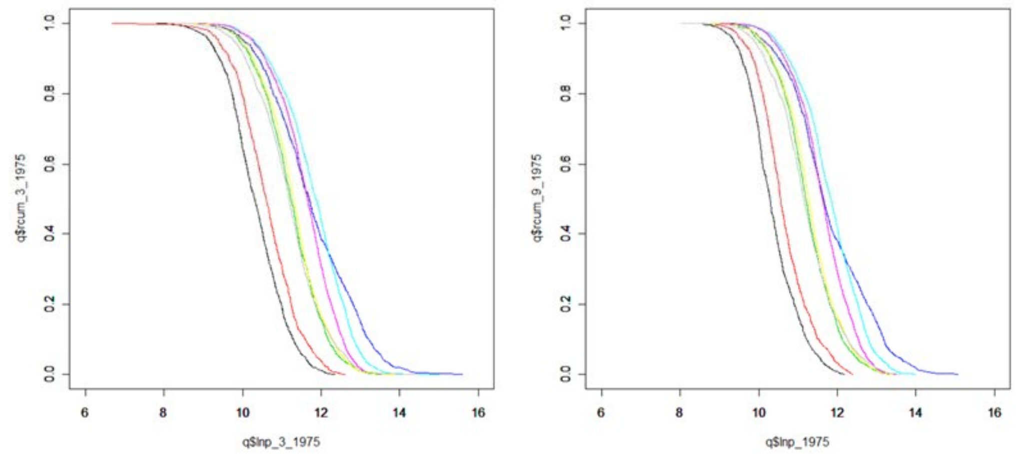
[Actual]

[37] For example, suppose land A in the TDB dataset is in a residential district in Tokyo prefecture in year 1990. In this case, its current price is projected using the parameters estimated for the sample in the residential district in Tokyo in 1990 (same-district, same-prefecture, and same-year) using the PNLP dataset.



[Industrial land]

[Estimation] [Actual]



Notes: Each colored-line represents the following year: black 1975, red 1980, green 1985, blue 1990, light blue 1995, purple 2000, yellow 2005, and grey 2010.

To confirm the accuracy of the prediction using the coefficients obtained from the hedonic estimation, in-sample comparisons are shown in figure B-1. In the figure, we show the cumulative distributions of the predicted prices (left panels) and the actual PNLP prices (right panels) of the lands in the PNLP dataset for each of the three types of land districts. We find that the distributions are similar in all the panels, which justifies the prediction using the estimated coefficients.

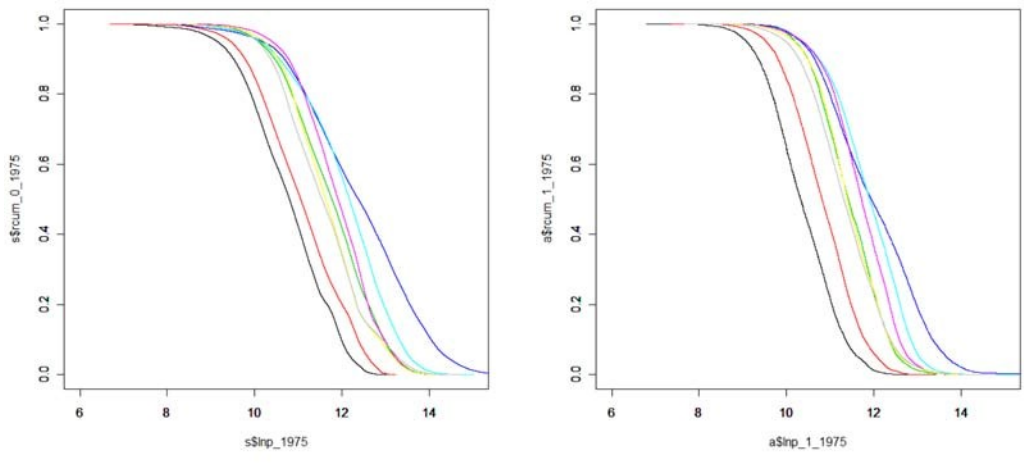
B.3 Projection results (second stage)

Based on the coefficients estimated in the first stage, we project the prices of each piece of land in our dataset. In doing so, we excluded outliers from our sample in the following manner. For each combination of land district type, prefecture, and year, we dropped observations whose projected prices were higher than the highest price of lands in the corresponding combination in the PNLP dataset. We also dropped those observations whose projected prices were lower than the lowest price in the PNLP database in the relevant year.

Figure B-2: Out-of-sample comparisons between cumulative distributions of projected prices on the TDB dataset and actual prices in the PNLP dataset

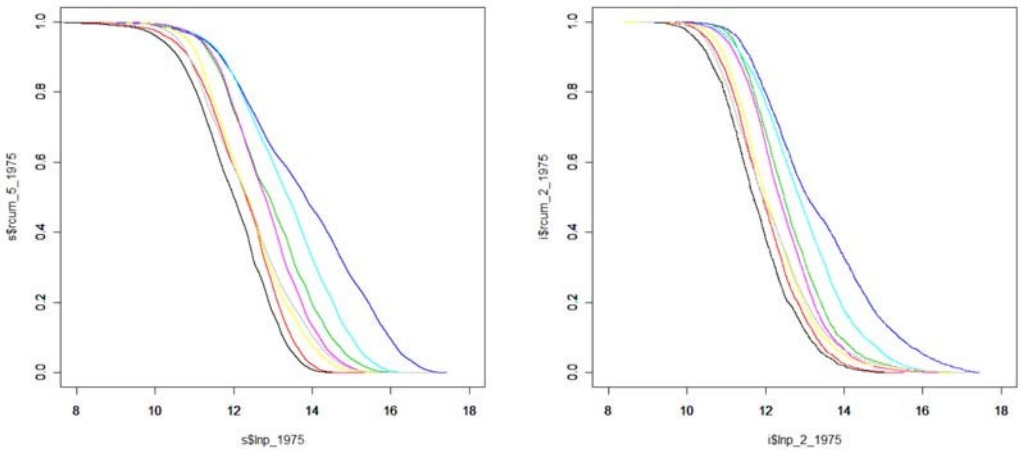
[Residential land]

[Projection (TDB)] [Actual (PNLP)]



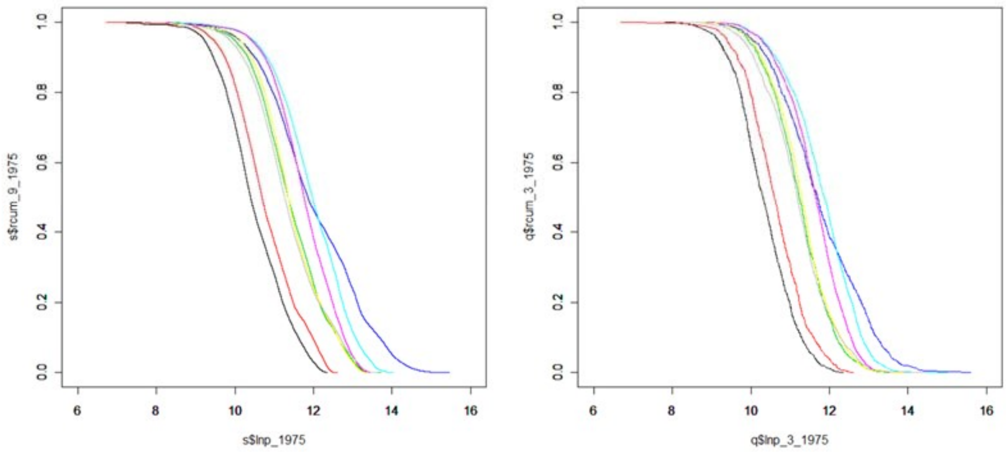
[Commercial land]

[Projection (TDB)] [Actual (PNLP)]



[Industrial land]

[Projection (TDB)] [Actual (PNLP)]



Notes: Each colored-line represents the following year: black 1975, red 1980, green 1985, blue 1990, light blue 1995, purple 2000, yellow 2005, and gray 2010

Figure B-2 shows the cumulative distributions of the projected prices of lands in our TDB dataset (left panels) and the actual prices in the PNLP data (right panels) for each type of land districts. Although the projected prices are available for a larger number of lands than the actual prices in the PNLP data, their distributions are similar, which supports our use of the projected prices to calculate the LTV ratios.

Appendix C: 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 C-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.

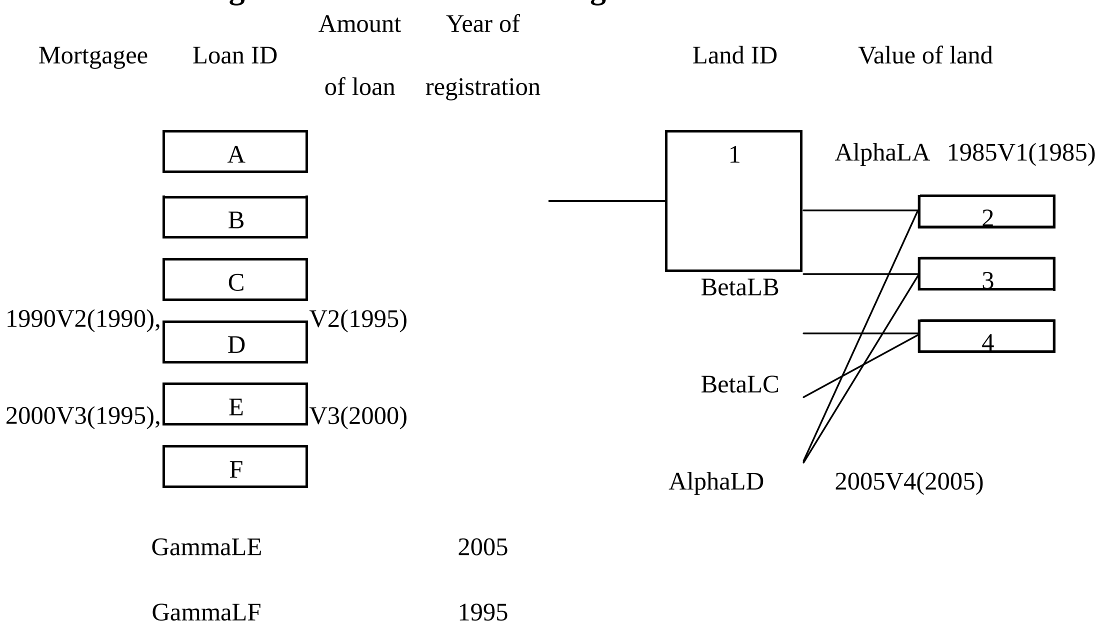
Calculation is fairly simple if a land is pledged to only one claim holder. In the example above, this is the case for loan A. Information about the amount of loan A, represented by LA, is provided by TDB database. The value of land A in year 1985, V1(1985), is estimated by the hedonic approach described in Appendix A. 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 C-1 Illustrative setting for LTV calculation



Appendix D: Difference in cyclicality between LTV ratios with and without the value of buildings

Let us denote the amount of loan, the value of buildings and of land that are pledged as collateral at time *t* by , , and respectively. We assume pro-cyclicality of and , and without loss of generality, let us focus on the bubble period, i.e., and .

Our finding of the counter-cyclical LTV can be expressed as:

— > —,

while the pro-cyclical LTV for the *real* LTV as:

—— < ———.

These two inequalities are reduced to:

——— < —— < ——.   (1)

For expositional simplicity, let us define to satisfy . The indicates

how many times the true *V* (i.e.,) is larger than our *V* (i.e.,), or to what extent we underestimate *V*, and is larger than 1 for secured loans underwritten in year *t*. Using , we can

rewrite Inequality (1) as:

—— —— < —— < ——.

Comparing the leftmost and rightmost terms, we find that for this inequality to hold, or for

our finding of the counter-cyclical LTV to be flawed, / (or the rate of increase in our

underestimation) must be small enough, at least sufficiently smaller than one. Following the

same procedure (with reverse inequalities), we can also demonstrate that the increase in our

LTV ratio after the bubble is flawed if / is large enough, at least sufficiently larger than

one. On balance, our finding of counter-cyclical LTV ratio is flawed if increases (decreases)

when our LTV ratio decreases (increases), i.e., if exhibit significant pro-

cyclicality.

Although we cannot directly quantify this underestimation, Table D-1 provide us with

closely related evidence. Column (1) of this table 1 report the amount of the value of land and of buildings using the SNA data that are depicted in Figure 7. Based on these figures, we can

calculate , the magnitude of underestimation, as in Column (2), and / as in Column (3). As this

column shows, / deviates very little from 1, suggesting that the omission

of buildings is not consequential.

Table D-1 Value of land and buildings from the SNA

Column (1) of this table shows the amount of the value of buildings (= housing and other buildings) and of the land (= land for housing) in Japan at the end of each calendar year, which are calculated based on the National Account Statistics issued by the Cabinet Office. Column (2) reports the resulting indicator of our underestimation of V (the denominator of LTV ratios). If the ratio of an annual increase in the indicator (reported in Column (3)) is significantly larger than 1, the omission of the value of buildings produces flawed cyclicality in LTV ratios. Note that the statistics until 1993 employ the benchmark year of 2000, while those after 1994 employ the benchmark year of 2005.

	(1) Amount (billion yen) Buildings	Land	(2)	(3) $x_t = x_{t+1} / x_t$
Year	V^B_t	V^L_t	$(V^L t + V^B t)/V^L t$	
1980	250,364.50	586,157.20	1.427	0.977
1981	262,800.60	666,945.10	1.394	0.991
1982	274,768.10	719,185.20	1.382	0.995
1983	281,407.80	751,389.20	1.375	1.000
1984	293,051.80	781,751.10	1.375	0.982
1985	300,156.70	857,219.50	1.350	0.944
1986	303,771.40	1,109,005.20	1.274	0.957
1987	324,274.20	1,479,659.80	1.219	0.990
1988	341,322.50	1,646,434.90	1.207	0.992
1989	382,074.70	1,933,500.40	1.198	1.001
1990	420,058.60	2,114,790.60	1.199	1.026
1991	448,302.40	1,949,387.60	1.230	1.028
1992	463,532.60	1,753,557.10	1.264	1.015
1993	472,983.60	1,670,172.10	1.283	1.038
1994	543,685.10	1,639,295.30	1.332	1.013
1995	546,213.10	1,562,946.40	1.349	1.017
1996	569,333.80	1,530,644.90	1.372	1.009
1997	576,170.20	1,497,280.60	1.385	1.007
1998	566,906.20	1,438,334.30	1.394	1.011
1999	563,732.40	1,376,773.50	1.409	1.016
2000	564,822.90	1,308,599.40	1.432	1.011
2001	553,808.80	1,237,547.40	1.448	1.017
2002	549,785.30	1,163,611.20	1.472	1.022
2003	553,347.30	1,094,917.80	1.505	1.018
2004	558,779.70	1,049,447.70	1.532	1.006

2005	560,401.40	1,034,700.00	1.542	1.000
2006	571,807.50	1,055,020.60	1.542	0.994
2007	579,101.00	1,088,463.30	1.532	1.008
2008	585,251.80	1,076,481.10	1.544	1.003
2009	561,100.10	1,024,279.90	1.548	1.009
2010	559,364.10	996,384.40	1.561	1.007
2011	554,151.80	967,554.30	1.573	1.003
2012	546,571.80	947,147.80	1.577	1.013
2013	561,062.40	939,870.00	1.597	1.006
2014	569,447.00	939,279.70	1.606	(NA)