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

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Abstract

Using unique micro data compiled from the real estate registry in Japan, we examine more than 400,000 loan-to-value (LTV) business loan ratios 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: LTV ratios, pro-cyclicality, macroprudential policy, bubble

JEL classification codes: G28, G21

Declarations of interest: none

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1. Introduction

The global 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 in Japan and residential mortgages to the household sector in the U.S. Following 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).

Perhaps the most prominent tool in the macroprudential policy toolbag is a cap on the LTV (loan-to-value) ratio (see e.g., FSB 2012), a tool that has already been implemented in a number of countries.[1] Most of the policy emphasis on LTV caps to date has been on residential mortgages, but there has been an increasing emphasis on LTV caps on commercial real estate loans used to finance real property, as reflected in the IMF's inclusion of the caps in the corporate sector macroprudential toolkit.[2] 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, because given L, a lower V exposes the lender to a greater loss given default *ceteris paribus*. Caps on LTV ratios potentially work

[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).

[2] In its annual macroprudential policy survey conducted in February of 2018, the IMF reported that LTV caps on commercial real estate loans used to finance real property have been implemented in 12 respondent countries (IMF 2018).

through two channels (CGFS 2012, p.27): (1) "increas[ing] the resilience of the banking system by decreasing the probability of default (PD) and loss-given-default (LGD) of loans", 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, regulators are making 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.[3]

The aim of this paper is to examine these implicit assumptions, by looking retrospectively at loans to firms secured by real estate in Japan during the bubble and bust periods. Using a very large dataset that includes detailed information on over 400,000 business loans secured by real estate underwritten 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.[4] 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

[3] See section 2.1 for more discussion on these implicit assumptions.

[4] While we focus on a simple (i.e., unconditional) LTV cap, not all LTV cap regulations and proposals take this form. Lim et al. (2011) show that among 20 countries that impose the caps, 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. Also, although we do not deal with them in this paper, higher risk weights for high LTV loans are another form of regulation designed to make high LTV loans more expensive to lenders (see, e.g., Jácome and Mitra 2015, pp. 8 and 11 for the combination of LTV caps and risk weights in Brazil, Malaysia, Hong Kong, Korea, and Poland).

of credit to risky borrowers – a condition for a simple LTV cap to dampen the risk channel.

Our analysis is motivated by the question of whether the LTV cap macroprudential policy tool would have worked in the context of the Japanese financial crisis. In Japan, there was an excessive lending boom (credit expansion) in the late 1980s that fueled an asset pricing bubble, during which banks were thought to have underwritten many high-LTV business loans with lax lending standards anticipating surging real estate prices. The consequence of this buildup of risk was a huge amount of non-performing loans (NPLs), accompanied by significant bank failures, and ultimately the crisis in the late 1990s.[5] Against this background, our analysis addresses the question of whether LTV caps - had they been employed - would have dampened the lending boom, and would have dampened the two channels indicated above (the pricing and the risk channels) in the sense of mitigating pro-cyclicality.

We note that our focus is on loans secured by real estate to *all* industries for *all* purposes. More precisely, we do not limit our analysis to commercial real estate loans where the purpose of the loan is to finance the property itself, nor do we limit our analysis to firms in the real estate industry. We acknowledge that the current policy emphasis on LTV caps as a macroprudential policy tool is confined to residential mortgages and commercial real estate lending used to finance real property (e.g., IMF 2014). However, there is general agreement among researchers and policymakers that not only were commercial real estate

[5] See section 2.2 for more detail on the crisis in Japan.

loans used to finance real property a prime driver of the financial crisis in Japan, but so were other business loans secured by real estate where the purpose was other than financing the property (e.g., Economic Planning Agency of the Government of Japan 1993, 1998).[6] Thus, the relevant policy question in the context of the Japanese crisis is whether counterfactually LTV caps would have been effective in closing the risk channel and the pricing channel with respect to *all* business loans secured by real estate. And, even if LTV caps on real estate-based lending to firms for general purposes are not currently included in the macroprudential policy toolkit, policymakers might consider adding them in the future. Such a policy initiative might be driven by the ubiquitous nature of this type of lending globally.[7,8]

On the other hand, we do recognize that in terms of LTV caps as a corporate sector macroprudential policy tool, the current policy focus is on commercial real estate loans used to finance real property (e.g., IMF 2014). Fortunately, our data allow us to investigate the (counterfactual) imposition of LTV caps used

[6] See Section 2.2 for more detailed institutional background on loans to firms secured by real estate whose proceeds are used for general business purposes.

[7] For example, using a survey of 91 banks in 45 developing and developed economies Beck et al. (2008) report that "real estate is the most frequently accepted type of collateral for business lending and close to 40% of banks rank real estate as the most important type of collateral used for small, medium and large firm financing." Somewhat surprisingly, they also found that it is even more important in developed economies where 56% ranked it as the most important. The importance of real estate in general small business lending has also been highlighted in the U.S context (e.g., Berger and Udell 2006, and Berger and Black 2011). There is also evidence that real estate as collateral may be important in explaining U.S. corporate behavior, although LTV caps have not been imposed in the U.S. on real estate in this sector. Chaney et al. (2012), for example, find a direct link between changes in the value of a firm's real estate and the amount of its corporate investment.

[8] Other potential types of lending to which LTV caps could be applied include, for example, lines of credit secured by accounts receivable and inventory, and loans secured by equipment. In both of these cases lenders routinely use LTV ratios in loan underwriting.

for this purpose. In that regard, we run separate tests on Japanese real estate businesses that obtain loans to finance income-producing property, i.e., firms that are in the construction industry and the real estate industry. These tests can add to the unsettled research on the efficacy of *existing* tools in the macroprudential policy toolkit.

By way of preview, using data during the period from 1975 to 2009, we first find in a univariate analysis that from the beginning of the real estate bubble, the LTV ratio was counter-cyclical, not pro-cyclical, at least until the early 2000s, although its numerator (L) and its denominator (V) were both pro-cyclical. This was not only true for the full sample on all nonfinancial industries, but also true for firms in the real estate business (firms in the real estate and the construction industries). In addition, we find in a multivariate analysis over a shorter period from 1990 to 2009 similar counter-cyclicality in the LTV ratio both for the full sample of nonfinancial industries as well as for the subsample of firms in the real estate business.[9] Additional analysis finds little evidence that our results are driven by possible biases due to data limitations. Our finding of counter-cyclicality is inconsistent with the regulators' first implicit assumption behind implementing LTV caps: the assumption that LTV ratios are pro-cyclical.

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

[9] Data limitations do not permit a multivariate analysis that covers the pre-bubble period.

correlated with the choice of high versus low LTV ratios. The finding of higher risk is consistent with the second implicit assumption behind LTV caps. However, the finding of greater growth is inconsistent with high-LTV loans being necessarily associated with lower quality firms as regulators assume.

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 the bubble period. The second finding, higher risk and greater growth for firms that obtain high-LTV loans, suggests that the simple (counterfactual) LTV cap might have produced an important unintended consequence by limiting access to finance to firms with higher growth opportunities. Although we cannot directly generalize these implications for LTV caps for other types of loans, our findings at least suggest the need to reconsider 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 because they are based on a very 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 (not limited to commercial mortgages used to purchase real property) using a micro dataset.[10] Also, most existing

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

studies of LTV ratios use aggregate data and so inevitably confine their analysis to the ex post performance of macro-performance variables such as aggregate credit growth (e.g., Cerutti et al. 2017). Unlike these studies, we can evaluate performance at the borrower level.[11]

Finally, our findings on loans to firms in the real estate business – either the construction industry or the real estate industry – mimic our findings in the full sample. This similarity is informative on two dimensions. First, because loans to firms in these industries prominently include loans that are now explicitly included in the IMF corporate sector toolkit, our findings in the context of the Japanese financial crisis speak to the potential *in*-efficacy of LTV caps on commercial real estate loans used to finance real property that policy currently emphasizes. Second, these findings show that the efficacy of (counterfactually) implementing LTV caps in Japan would likely have been lacking.

The remainder of our paper is composed as follows. Section 2 discusses the context of our analysis. Section 3 provides details on our data and the definition of our LTV ratios. 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: Institutional background and LTV caps

2.1. Macroprudential policies and LTV caps

[11] Igan and Kang (2011) (for Korea) and Laufer (2018) (for U.S.) use micro data to study the effect of LTV caps on residential mortgages on housing demand and pricing. Basten (2019) 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.

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).

There are various instruments available to implement macroprudential policy. For example, IMF (2014) provides a useful guide to the instruments by grouping them into five categories: Broad-based tools, Household sector tools, Corporate sector tools, Liquidity tools, and Structural tools. LTV caps (limits on LTV ratios) are one of the main Household sector tools, because historically much of the macroprudential policy debate has been centered on problems in the residential mortgage market. However, LTV caps have more recently become an important part of the Corporate sector toolkit.[12]

To meet policy objective (i), LTV caps are expected to operate through the *risk channel* through which they directly decrease the probability of default

[12] In addition to LTV caps on commercial real estate used to finance real property, the Corporate sector toolkit includes debt service coverage ratios, tools affecting broad corporate credit (e.g., capital requirements linked to overall corporate sector portfolio exposure), and tools related to foreign exchange risks. Lim et al. (2011) list 10 macroprudential policy instruments: caps on the loan-to-value (LTV) ratio; limits on maturity mismatch; caps on the debt-to-income (DTI) ratio; reserve requirements; caps on foreign currency lending; countercyclical capital requirements; ceilings on credit or credit growth; time-varying/dynamic provisioning; limits on net open currency positions/currency mismatch; and, restrictions on profit distribution. Igan and Kang (2011) compare the effectiveness of LTV caps and DTI caps, and Kuttner and Shim (2016) compare nine non-interest rate policies including LTV caps in regard to the pricing channel for residential mortgages.

(PD) and the loss-given-default (LGD) of the banks' 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.[13] In other words, LTV caps address objective (ii) through their "impact on the credit cycle" (CGFS 2012).[14]

With regard to both channels, proponents of LTV caps make two implicit assumptions. First, for caps on LTV ratios to be effective during a bubble, LTV ratios must be pro-cyclical, so that a fixed cap can curb lending more in booms than in busts. Second, for caps to reduce risky lending, loans with higher LTV ratios must be riskier, due to a higher PD and larger LGD. Whether these assumptions actually hold - and LTV caps are effective - is ultimately an empirical question. And, in the case of business loans, it is the question for which there is an acute paucity of work. We address this gap.

2.2. The Japanese crisis

The financial (banking) crisis in Japan started in the mid-1990s, escalated in the late 1990s, and continued into the mid-2000s. Starting with the failures of

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[13] 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.

[14] On the pricing channel, some studies on residential mortgages examine the relationship 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, Kuttner and Shim 2016). There are also studies that find that higher house prices drive up loan values (Bhutta and Keys 2016, Mian and Sufi 2011, Brown et al.2015, and Laufer 2018).

two credit cooperatives in 1994, total bank failures ultimately reached 171 by 2003. The crisis is closely associated with the formation of an asset pricing bubble in the late 1980s, fueled by excessive bank lending, that led to a precipitous collapse in asset prices beginning in 1990.[15]

In this subsection, we briefly review key characteristics of the lending boom in Japan and the nature of the NPLs that followed it. These characteristics justify our focus on *all* business loans in our main tests as opposed to just focusing on loans to firms in the real estate business to purchase income-producing property. Specifically, the broader focus on all business loans is justified by the fact that the lending boom included a substantial amount of commercial loans secured by real estate for general business purposes (including working capital) in all industries; and that these loans were important in driving bank NPLs (along with commercial real estate loans used to finance real property).

Below, we look first at the lending boom in Japan in the late 1980s. This period is relevant to our paper because it is the period when LTV caps would have been implemented to dampen the pricing and risk channels. We subsequently discuss bank NPLs. This focus is important because NPLs represent an *ex post* measure of bank risk-taking (i.e., the consequence of the risk channel).

[15] Cargill (2000) points to five related indirect, or underlying causes of the crisis, which can be viewed as the drivers of the excessive lending in the banking sector: i) a rigid financial regime; ii) the failure of the monetary policy of the Bank of Japan; iii) a slow and indecisive regulatory response to emerging problems; iv) the lack of public and political support to deal with troubled financial institutions with public funds; and, v) the intransigence of financial institutions in accepting criticism of management policies. See Uchida and Udell (2019, section 36.3.3) for more details on the Japanese crisis and its causes.

2.2.1. The lending boom

We begin our discussion of the lending boom by documenting its magnitude and its breadth. Figure 1 shows outstanding bank loans in Japan from 1980 to 2010. The steep rise in total lending, particularly in the late 1980s, clearly reflects a boom. Table 1 shows a breakdown of the lending boom by industry from 1985 to 1993, and from 1993 to 2001. This table reflects a significant increase in bank lending during the boom in the late 1980s to the two industries that are involved in the real estate business – the construction industry and the real estate industry.

In addition to this *direct* lending, there was also an increase in *indirect* lending to these two industries via "nonbanks". Nonbanks lend to firms and individuals but do not take deposits, and in the late 1980s, some large nonbanks called "Jusen" borrowed from commercial banks and other financial institutions in order to lend to the real estate industry.[16] Thus, the increase in total (direct plus indirect) lending to the real estate and construction industries reached a maximum over the period of 99.9 trillion yen, which is the sum of the increases in "construction," "real estate" and "nonbank." Importantly, however, Table 1 also shows significant increases across all other industries. Lending to these industries increased by 75.5 trillion yen.

[16] Nonbanks called Jusen had originally underwritten residential mortgages, but they increased lending to real estate businesses in the late 1980s. These loans, in turn, became non-performing and produced NPLs at banks that lent to the Jusen.

Although it is not shown in Table 1, there was also a surge in lending to small and midsized enterprises (SMEs) during the boom. This surge partly reflects disintermediation, a shift away from large firms who left the bank loan market for the securities market (Hoshi and Kashyap 2001 and Shimizu 2000).[17]

Another notable aspect of the lending boom is the increasing emphasis on real estate as collateral in loan underwriting. Securing business loans with real estate had been a standard practice of banks in Japan for over a century (Shimizu 2000, Ogawa and Kitasaka 2000, Gan 2007b, and Lian and Ma 2020). It was often referred to as lending based on the "collateral principle" (yuu-tanpo gensoku) (e.g., Shimizu 2000).[18] During the 1980s, there was also a sharp rise in loans secured by real estate (Figure 1, the darkened area at the bottom). This emphasis on the value of real estate as collateral not only applied to firms in the real estate business, but also to firms in other industries who could pledge real

[17] Hoshi and Kashyap (2001) report a surge in loans to the real estate sector and to SMEs as evidence for their view that slow and incomplete deregulation of the financial system in the 1980s was an important contributor to the crisis. This deregulation process prompted a flight of high quality larger borrowers from the banking system to the capital markets. This, in turn, incentivized banks to underwrite loans to firms in the real estate business, and to SMEs, with whom they were less familiar.

[18] It should be noted that prior to, and during, the financial crisis, Japan did not have a "legal infrastructure" conducive to the type of cash-flow based lending found in the U.S. and thus Japan developed a corporate lending tradition focused on physical assets, especially real estate (Lian and Ma 2020). Moreover, Japan did not have modern commercial laws on business collateral (i.e., on security interests) that allowed lending against moveable assets, particularly accounts receivable and inventory. In the mid 2000's the Financial Services Agency (FSA) opened the door for this type of lending by allowing accounts receivable and inventory to be re-classified as "ordinary collateral". Thus, real estate-based corporate lending was more important in Japan – and real estate was more important as collateral in Japan - prior to the crisis than in countries like Canada, the U.S. and the U.K. that had modern laws on security interests in moveable assets against which banks routinely lend in these countries and a lending culture that facilitated cash-flow lending. See Calomiris et. al. (2017) for an analysis of lending against moveable assets in a global context.

estate as collateral in loans underwritten for general business purposes (e.g., Ueda 2000).

Compounding this general increase in secured loans were bank loans to finance speculative investments in real property. Both firms that were, and were not, in the real estate business were involved in these investments (see, for example, Economic Planning Agency 1993). For firms outside the real estate business, this was essentially a side business to seek speculative returns from real estate appreciation.[19]

Secured loans also mattered for SMEs. Unfortunately, contemporaneous data are not available to generate a separate collateral chart for SMEs, who had become an increasing fraction of bank portfolios over the boom period (Hoshi and Kashyap 2001). However, ex post data indicate that the amount of loans secured by real estate surged for SMEs as well. Data from the post-crisis period (2007-2010) in a sample of firms that are mostly SMEs show that 51.9% of these firms pledged real estate as collateral (Ono et al. 2015). Given that the steady decrease in the fraction of loans secured by real estate after the collapse of the bubble shown in Figure 1, we can reasonably infer that a far larger fraction

[19] Such side-business investment by non-real estate firms in land (and in equities) in anticipation of a further price increase was called "*zaiteku*" (new asset management technology). Anecdotal evidence shows that banks even lent to unprofitable firms as long as they owned land in the middle of the bubble formation ("Distortion spreading due to surging land prices: Firms rushing to *tochiteku*," Nikkei Newspaper April 5, 1987, in Japanese). Furthermore, the Bank of Japan started to monitor excessive lending to *zaiteku* firms after observing the rise in corporate bankruptcy ("The Bank of Japan also monitor *zaiteku* loans in response to increased corporate bankruptcy," Nikkei Newspaper, November 16, 1990, in Japanese). The Japanese Bankers Association (1992) also noted at that time the existence of "fictitious" land demand for *zaiteku* investment (i.e., the purchase of land for financial return instead of for development) and banks' willingness to fuel this demand in their loan underwriting.

of smaller firms (and thus a large number of firms) borrowed using loans secured by real estate during the bubble period.[20]

As we mentioned in the introduction, our main tests examine the efficacy of LTV caps on business loans to *all* firms, not just firms in the real estate business. As we have shown above, the lending boom was associated with an overall increase in lending secured by real estate. Also, the literature on this period emphasized the increase in loans secured by real estate underwritten to all firms (not just firms in the real estate business). Further, there was speculative investment in real estate among firms in all industries that was fueled by secured loans. In light of these, we feel that conducting our main tests on all business loans secured by real estate is well-founded.

Moreover, the Japanese government took this same inclusive view when it considered introducing measures to curtail an increase in loans secured by real estate (including loans that were not underwritten to finance commercial real estate) to deal with the real estate bubble (e.g., Council of Land Policy, the National Land Agency, the Government of Japan 1990).[21] Even banks recognized the need to focus on a wider range of loans in addressing public

[20] Although the percentage share of the amount of loans secured by real estate in Figure 1 is not particularly high, it is likely that most of the unsecured and non-guaranteed loans in Figure 1 reflect those of large firms that continued borrowing from banks. Economic Planning Agency (1998) also points out that loans secured by real estate make up a large share of SME loans, and are about 3/4 of secured loans.

[21] Ultimately the government only implemented a ceiling on the amount of loans to firms in the real estate industry (see Uemura 2012).

criticism about excessive lending during the boom (Japanese Bankers Association 1992).[22]

However, as noted above, we nevertheless conduct separate tests on secured loans to firms in the real estate business as well. We do this for two reasons. First, as we noted earlier, loans to firms in the real estate business generally fall under the current IMF corporate sector toolkit for LTV caps. Second, some of these loans are likely to be non-recourse loans to finance income-producing property. This is in contrast to real estate-based loans for general business purposes to firms in other industries. These loans, by definition, are recourse loans and thus may behave differently (see section 2.4).

2.2.2. Bank NPLs

Excessive business lending against real estate has been identified as a key causal factor driving the buildup during the boom of bank portfolio risk that ultimately materialized in the massive increase in NPLs, business failures and bank failures. Ueda (2000) finds that the share of a bank's loans backed by land collateral in 1986 was statistically and economically associated with bank NPL ratios in 1996, although Ueda (2000) does not make a distinction among

[22] Japanese Bankers Association (1992), a report to respond to social criticism on the banks' excessive lending to support land investment, provides a classification of real estate-related loans in Japan and distinguishes among three types of loans: (a) loans to raise funds for real estate-related businesses (including those to purchase, build, develop, and hold real estate); (b) loans to real estate firms (including real estate brokers, leasing agents, real estate management firms, and construction firms to develop land or to build or renovate structures), and (c) loans secured by real estate. Because type (a) loans are defined based on their purposes, type (b) are defined based on borrower types, and type (c) loans are defined based on the presence or absence of real estate collateral, there is overlap among the three types. Japanese Bankers Association (1992) states that not only type (a) and (b) but also type (c) loans are important for their analysis of the lending frenzy and the associated surge in real estate prices, and that including them in their analysis is necessary to respond to social criticism.

industries. Hoshi (2001, Table 5), on the other hand, breaks out loans secured by land collateral to the construction industry, to the real estate industry, and to other industries and finds that only loans to the construction and real estate industries had a significant effect on the March 1998 NPLs. Thus, the evidence on early NPLs in Hoshi (2001) suggests that only loans to firms in the real estate business were the key causal factor of the crisis.

In light of our discussion above, however, this conclusion is not consistent with the general view shared among researchers and policymakers. This view emphasized that excessive lending based on real estate not only characterized lending to firms in the real estate business but also lending to firms in other industries.[23] If this view is correct, it would be reflected in the subsequent NPLs caused by this excessive lending. We argue that the evidence supports this view that the accumulation of NPLs was not limited to firms in the real estate business. We make this argument in three parts.

First, although the early evidence shows that only loans to real estate businesses mattered, there were problems with the early NPL data used in Ueda (2000) and Hoshi (2001). The Financial Services Agency (FSA) reports significant differences between its estimates of NPLs and the banks' own estimates of their NPLs. In 2000 and 2001 when the FSA started stringent manual-based bank examinations, the major banks were found to have

[23] Even Hoshi (2001) states that land was considered the "most secure" as collateral until the 1990s, and Ueda (2000) echoes the same view: "Because land is a common form of collateral, declining urban land prices affected more than just loans made directly for real estate-purposes" (p.66).

underestimated their bad loans by 36%, with the five largest banks underreporting by more than 50%.[24]

Second, data on NPLs in the later period also show a very different story. Figure 2 shows that NPLs in other industries were not only important but were, in aggregate, greater than the combination of NPLs in the real estate and construction industry. In addition, related to the surge of loans to SMEs discussed above (Hoshi and Kashyap 2001), Hoshi and Kashyap (2010) find that the ratio of loans to SMEs had a positive effect on (i.e., drove) non-performing loans after 2000. Although no data are available to break down these loans into secured and unsecured loans, given the prevalence of loans secured by real estate (Shimizu 2000, Ogawa and Kitasaka 2000, Gan 2007b, and Lian and Ma 2020), and indirect evidence on the high fraction of loans secured by real estate, especially for smaller firms, we can expect that many real estate-related loans to industries other than real estate businesses also caused NPLs.

Finally, Gan (2007a) examines a real estate "collateral channel" in Japan in the context of companies that *were not* in the real estate business – specifically companies in the manufacturing industry. This paper finds that during the five-year period after the collapse in land prices, firms with more land holdings more significantly reduced their corporate investment, were less likely to sustain their bank relationships, and tended to obtain less bank credit. Taking this all together, the evidence is mostly consistent with a causal link between the crisis and real

[24] See the FSA webpage: https://www.fsa.go.jp/news/newse/e20021108-1.html.

estate-based lending to all firms (as opposed to just firms in the real estate business).

2.3. Crisis in the U.S.

For the purpose of comparison, and to help draw implications from our findings, we briefly review the U.S. crisis as well. The Japanese financial crisis shared one important common denominator with the financial crisis in the U.S. – causality in both crises ran through the real estate market, although causality ran through real estate-based *business* lending in the case of Japan while in the U.S. it ran through the *residential* mortgage market. In both countries, LTV caps were not imposed during the boom that preceded the bust.

While there is little debate in the U.S. about whether mortgage lending and the housing market were at the center of the crisis (see Adelino et al. 2018a), there is considerable debate about factors that drove the expansion of residential mortgage lending during the boom. Adelino et al. (2018b) articulate two competing narratives: the *subprime view* and the *expectations view*. The former emphasizes financial innovation (i.e., securitization), deregulation, and lending to poor-credit-quality borrowers. The latter view emphasizes the role of house price expectations. Importantly, Adelino et al. (2018b) point out that the policy implications for these two narratives are quite different. The subprime view emphasizes constraints on securitization and stricter screening, while the latter emphasizes macroprudential policies related to inflated house price expectations.

Interestingly Adelino et al. (2018b) find empirical evidence more in line with the expectations view in that credit expansion was greater in areas with more rapid house price growth and that credit expansion was not concentrated in borrowers with low-income and low-credit scores.[25] As we do in this paper, they also examined the behavior of LTV ratios. Interestingly, consistent with the expectations view, they found that LTV ratios at origination did not climb.[26]

2.4. LTV ratios and recourse

Recourse is an important dimension in collateralized lending particularly as it relates to real estate. Recourse is relevant to our analysis because some loans in our sample are with recourse and others are likely without recourse. The underwriting associated with each, and the ex post behavior of each, could vary depending on recourse.

In residential mortgage lending, lenders have "recourse" if the homeowner is personally liable for any deficiency caused by the insufficient liquidation value (in foreclosure) to cover the mortgage balance. There is considerable variation in recourse in residential mortgage lending across, and even within countries. In the U.S., for example, mortgages in some areas are typically with recourse, and in other areas they are typically without.[27]

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[25] See Adelino et al. (2018a,b) and DeYoung (2019) for more detailed discussion of the chain of events associated with, and the causes of, the U.S. financial crisis.

[26] Justiniano et al. (2019) find empirically that overall mortgage debt and house prices increased in parallel in the pre-crisis period. In a general equilibrium framework they model an economy to explain this parallel trend and the fact that mortgage rates also declined in the pre-crisis period. In their framework, a collateral constraint limits household demand as in Kiyotaki and Moore (1997) and a lending constraint limits the savings flow on the supply side. "A progressive relaxation of this lending constraint, which generates an expansion in the supply of credit" generates the observed pre-crisis parallel debt/price trend and the decrease in mortgage rates.

[27] Residential mortgage lending in Canada, Ireland, and Spain for example, is conducted on a recourse basis. The U.S. residential mortgage market is more complicated. Ghent and Kudlyak (2011) show that while mortgages in 67% of the states in the U.S. are effectively made with recourse, mortgages in 33% of the states are on a non-recourse basis.

In our setting, business lending against real estate collateral for purposes other than the purchase of real estate is by definition lending *with recourse.* If the loan defaults, the lender is entitled to the proceeds from the liquidation of the pledged real estate. If these liquidation proceeds are insufficient to pay off the balance, then the lender has recourse against the firm itself. Thus, in estimating loss-given-default, loan underwriters can take into account both the expected liquidation value of the pledged real estate and the lender's unsecured claim against the expected proceeds from the liquidation of the other assets of the firm. It will also be the case that most loans – if not all – to firms outside of the real estate business to purchase real estate (e.g., purchase a factory building or a warehouse) will also be recourse loans.

However, some of the loans in our sample are very likely to be without recourse. These would occur exclusively in the real estate and construction industries when the purpose of the loan is to finance income-producing real estate that is pledged as collateral and when the only asset owned by the borrowing firm is the real estate itself.[28] In underwriting these non-recourse commercial mortgages, the loan underwriter can only claim the liquidation proceeds of the pledged collateral. Interestingly, it appears that in Japan, the mix between recourse and non-recourse in these two industries has trended over the

[28] There are firms that are classified as being in the real estate industry and in the construction industry that do not own real estate. These would include, for example, real estate agents (in the real estate industry) and sub-contractors (in the construction industry). This is not a particularly problematic issue in our context because our dataset consists exclusively of firms who pledge real estate as collateral.

sample period from very little recourse lending to more.[29]

While our main tests are conducted on all industries, we also conduct (as noted earlier) separate tests on the real estate and construction industries. This allows us to examine differences in lending against real estate based on recourse. We acknowledge that not all loans extended to the real estate and construction industries are non-recourse loans to finance income-producing properties. However, it is the case that for firms in the other industries, *all* real estate-based loans are recourse loans. So, given that our data do not include information on recourse, these separate tests on the real estate and construction industry at least better isolate the non-recourse segment of real estate-based business lending in Japan.

3. Data and LTV ratios

3.1. Data

Our data contain 420,889 total observations on collateral registrations for business firms during the period from 1975 to 2009. Our dataset is constructed from a very large database on Japanese firms compiled by the Teikoku Databank (TDB), the largest credit information provider in Japan. The TDB database covers almost one third of the entire universe of firms in Japan (see Ono et al. 2015). Most of firms in the database are SMEs reflecting the fact that SMEs dominate the business sector in Japan – as in virtually every other economy.[30]

[29] The first non-recourse loan in Japan was originated by JP Morgan in April 1998 (Nikkei Newspaper, August 18, 1998, in Japanese). Otani et al. (2007) state that non-recourse loans to the real estate industry have markedly increased since 2003.

[30] For example, the Japanese government's 2019 SME White Paper reports that 99% of the firms in Japan are SMEs and they employ 69% of the labor force.

The database contains very detailed information on collateral registrations that TDB extracts from the official real estate registry.[31] 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), and its ownership. Most importantly, we can identify whether the property is pledged as collateral, together with the associated claim holder(s), the debtor(s), the amount of loans against which the collateral is pledged, and the registration date.[32]

We can also identify the type of collateral. In Japan, collateral takes one of two types: ordinary collateral and *ne-tanpo*. The former is similar to 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*), once pledged, *ne-tanpo* remains pledged and will automatically secure any future loans extended by the same lender up to a specified maximum, unless its registration is "released" (i.e., terminated).[33] Thus, the loan balance secured by *ne-tanpo* fluctuates, although the property

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

[32] Unfortunately, TDB does not collect all the information in the official registry. First, there is no information on seniority (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. Second, TDB only records information that is effective when it conducts credit research on the firm. Terminated registration information is erased from the database, so we cannot trace the history of registration information. Finally, the TDB database does not specify whether a piece of real estate is pledged to a business loan or a loan for CEO/owners to finance their residences. However, as explained in Appendix A, other information allows us to make this distinction.

[33] 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.

that is pledged stays the same. The main motivation to use *ne-tanpo* is to avoid the transactions cost for serial borrowings in the spot market.

Although the richness of the information on real estate registrations is highly unusual (particularly with respect to its coverage of SMEs), 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. TDB always collects registration information on a firm's headquarters and its CEO's residence, but only collects data on the other real estate in response to a customer request. As for SMEs, TDB always collects this information. But, as for listed and/or large firms (equity capital exceeding 100 million yen and more than 100 employees), it only collects the information upon request from its customers.

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.[34] That is, all of the registrations in our sample consist of those that existed in the registry from 2008 to 2010, and those registered before 2007 are included only when they *remained* registered until at least 2008. Thus, our data are *synthetic* in nature.[35]

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[34] If TDB conducted credit searches on a firm several times during the 2008-2010 period, we only use the most recent data. It is difficult to track the same land in constructing a panel data set due to changes in the names of the addresses (e.g., street and city names), which most likely occur because of municipal mergers.

[35] 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.

This *cross-sectional*-like nature of our data has two shortcomings. First, we cannot exploit data variation in time series dimensions to control for time-invariant fixed effects. Second, we might suffer from a survivorship bias. In our dataset, "bad" firms that went bankrupt and liquidated before 2008 are not included.[36] Also, registration information on repaid "good loans" that were removed from the registry are not included. In our regressions, we try to address these shortcomings by controlling for many firm- and loan-characteristics.[37]

We use information on LTV ratios for the full 420,889 observations in our univariate analysis (section 4.1). For our regression analysis we use a 59,125 subset for which we also have financial statement information (section 4.2).[38]

Figure 3 reports the number of observations for our analyses, and indicates the magnitude of missing observations that might drive a survivorship bias. The number of the full observations 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 the sample's first

[36] In this regard, it is highly likely that our data do not include loans that Jusen underwrote, because these nonbanks went into bankruptcy in the mid-1990s. On the other hand, our data include loans that banks and other depository financial institutions underwrote to nonbanks, but the number of these observations is very small.

[37] It might appear that another bias arises because the observed earlier loans all appear at first blush to be super long-term loans. However, as we noted earlier in the paper, most of these earlier loans are (unreleased) ne-tanpo loans for repeated short-term lending for working capital. Again, the security interest (i.e., the collateralization) on these loans can survive indefinitely until pro-actively terminated.

[38] We have additional variables for lender characteristics from lenders' financial statements, but the statements are only 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.

year.

3.2. Definition of LTV ratios

LTV ratios are defined as the ratio of the amount of the loan, either being extended or committed (maximum), to the current value of real estate being pledged as collateral.[39] The numerator (L) of the LTV ratio is from the TDB database as explained above.

V, the denominator, is not available in the database, but information on acreage is available. So we multiply the acreage by a predicted per-acreage land price using an hedonic model, which is an approach widely used in real estate economics.[40] The hedonic model estimates the price by assuming 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 the estimation we use price information obtained from the *Public Notice of Land Prices* (PNLP) (the Land Appraisal Committee of the Ministry of Land, Infrastructure, Transport and Tourism of the Government) dataset, which reports land prices for a limited number of locations in Japan on an annual frequency.[41] Using all data available in this dataset, we estimate more than 3,000 hedonic model

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[39] The calculation of the LTV ratio becomes complicated when there are multiple loans and multiple lenders with different levels of priority. Appendix B provides an illustrative explanation of how we define the LTV ratio in these and other cases.

[40] There are other approaches to estimating land value such as the DCF (discounted cash flow)-based model. However, because our sample observations are spread out widely over the country, and over a long period of time, data availability (including on cash flows) prohibits us from applying other methods.

[41] PNLP captures the land value based on the hypothetical highest and best use (HBU), i.e., it takes into account the option to redevelop a site into an alternative use. The appraisers estimate the land value by subtracting the value of a hypothetical new building from the appraised property value.

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. The details of this estimation are summarized in Appendix C.[42] 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.[43]

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 also commonly pledged as collateral in Japan together with the land on which they are built, we have no information on the value of buildings. 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 on the land. 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 section 4.4.2.

4. Cyclicality of LTV ratios

4.1. Univariate analysis

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[42] The explanatory variables include dummy variables for the type of land districts where the land is located (i.e., whether the land is located in a residential, commercial, or industrialized district). These variables control for much of the variation in the value of land due to its use, as well as the types of buildings built on the land.

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

4.1.1. Background information: The business cycle and the bubble

In this section, we address the primary focus of our paper – cyclical changes in LTV ratios. In order to provide some context, we first take a brief look at Japanese economic conditions and land prices using *aggregate* statistics. Figure 4 shows the market value of real estate (land and buildings) and the amount of bank loans outstanding at the aggregate level.[44] 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 notable, together with a rapid increase in the amount of loans during that period.

The figure also shows the ratio of loans to the value of real estate as a crude measure of the LTV ratio at the aggregate level. This ratio is decreasing during the bubble period, although it has an increasing trend after the burst of the bubble. Thus, this ratio exhibits counter-cyclicality at least in the period around the bubble.[45] However, this aggregate indicator may not capture the evolution of loan level LTV ratios.[46] Below, we will check the cyclicality using detailed micro loan level data.

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[44] Note that the L that we use to calculate origination LTV ratios is in flow terms, while the aggregate amount of loans outstanding in Figure 4 is in stock terms.

[45] Note that we focus on cyclicality with respect to real estate prices and not with respect to general business conditions, 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.

[46] This ratio differs from our loan level LTV ratios in a number of respects. First, the figures for loans and real estate in this ratio are in stock terms. Second, the loans for this aggregate ratio include unsecured loans and residential loans, and the value includes real estate that does not secure loans. In contrast, the loan level LTV ratio that we use below focuses on secured business loans (in flow terms) and land used for securing those loans.

4.1.2. Cyclicality of loans, land values, and LTV ratios

Loans and land values

We begin our micro loan level analysis by first examining separately the evolution of the numerator and the denominator of the LTV ratio, i.e., the amount of loans originated (L) and the estimated value of the collateralized land (V). Figure 5 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 individually are not particularly surprising. They each have an increasing trend until around 1991 when the bubble burst, and a decreasing trend 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 4.[47]

LTV ratios

Turning to the key focus of our analysis, Figure 6 shows the LTV ratio by quartile (25th, 50th, and 75th percentiles). The LTV ratio clearly exhibits counter-cyclicality, until it disappears early in the 2000s. 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.

To check the cyclicality of the LTV ratio more formally, we also calculate the coefficient of correlation between these LTV ratios (25th, 50th, and 75th

[47] The finding of pro-cyclicality is consistent with the existing evidence on commercial lending (e.g., Berger and Udell 2004).

percentiles) and the aggregate real estate market values obtained from the SNA (System of National Accounts) (shown in Figure 4). The coefficients for the LTV ratio at the 25th, 50th, and 75th percentiles are respectively -0.3464, -0.4674, and -0.5165, and are statistically significant at least at the 5% level. When we use a 1-year lag (or lead) for the real estate values, the coefficients for the LTV ratios at the 25th, 50th, and 75th percentiles are -0.2782 (-0.4102), -0.4153 (-0.5183), and -0.4739 (-0.5626), and are statistically significant at least at the 5% level, except for the one between the LTV ratio at the 25 percentile and the 1-year lag of the real estate value that is not significant even at the 10% level.[48] These findings suggest that the LTV ratios are counter-cyclical.

Although our finding of a counter-cyclical LTV ratio is consistent with the finding in Figure 4 (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 (e.g., Shimizu 2000, Yoshida 1994).

LTV ratios for firms in the real estate industry and construction industry

In this part we focus on LTV ratios for firms in the real estate business. We do this for three reasons. First, of all the firms in our sample, real estate firms are more likely to obtain commercial real estate loans whose purpose is to

[48] Because we cannot reject the null hypothesis of unit root for each of these variables, we also calculate the coefficients for correlation using one-year differences of the variables. The coefficients are still negative in all the cases (the LTV ratios at the 25th, 50th, and 75th percentiles and the real estate value in the same, previous, and next year), although they are statistically less significant. But we still find some strong and statistically significant coefficients, e.g., -0.5422 and -0.4854 between the difference in LTV ratio at the 25th and the 50th percentiles respectively and the 1-year lag of the difference in real estate value, both are statistically significant at the 1% level. In any case, we find no positive correlations.

provide permanent financing for the property that they own. And, as we noted in the introduction, LTV caps on these commercial real estate loans are now prominently part of the policy dialogue on LTV caps and explicitly part of the corporate sector macroprudential toolbag discussed in IMF (2014).[49] Second, while we provided compelling evidence that the build-up of bank NPLs was driven by firms in all industries (see section 2.2), loans to the real estate industry secured by real estate have been directly and explicitly linked to NPLs in the empirical literature (Hoshi 2001). We also break out the construction industry because of its close association with commercial real estate and the fact that it has also been explicitly linked to NPLs (Hoshi 2001).[50] Third, as we noted above the non-recourse loans in our sample all come from these two industries.

Figure 7 shows the LTV ratios by the three quartiles (25th, 50th, and 75th) for real estate firms (Panel (A)) and construction firms (Panel (B)). Similar to Figure 6, we find V-shaped LTV ratios for all these panels. This means that the ratios exhibit counter-cyclicality. However, we do see some differences in the extent of the ratios' decrease up to, and their increase after, the burst of the

[49] Taking the purpose into account might also be important because of differences in valuation methodology. For residential mortgages, FSB (2011, p.21) states that in most FSB countries, "Supervisory guidelines generally direct institutions to obtain credit support for high LTV residential mortgages, typically defined as greater than 80% of the property's appraised value." The IMF (IMF 2014) mentions both appraised value and transactions price when it states explicitly that "limits on LTV ratios cap the size of secured loans relative to the appraised (or transaction) value of the property" (p. 32). Studies, for example, on Hong Kong indicate that either the purchase price or appraised value can be used (Wong et al. 2011, Gerlach and Peng 2005).

[50] Another approach to capturing the effect of loans to purchase real estate involves splitting working capital loans from other loans in our sample. In that regard, we also calculate the LTV ratios by splitting the loans into ne-tanpo loans and other loans. As indicated in section 3.1, ne-tanpo loans are used for financing working capital and not likely to finance the purchase of real estate. We find that the results are qualitatively the same and both types of loans exhibit counter-cyclicality. See Appendix D for this analysis.

bubble. On balance, however, the results reflect the same countercyclical findings as the entire sample.

This finding of counter-cyclicality for firms in the real estate business suggests that the efficacy of simple LTV caps in the corporate sector toolbag (IMF 2014) may be challenging. It is also interesting that this countercyclicality result also applies beyond just the two industries – the real estate and construction industries – where non-recourse lending occurs. Separate tests for the other industries show the same pattern of countercyclicality (see Appendix E).

Some caution should be exercised in extrapolating these findings to different scenarios and different countries. The issue here is that the Japanese pre-crisis lending environment could be quite idiosyncratic given the emphasis on secured loans in all industries (see section 2.2).

Frequency of business cycle

Before turning to our multivariate analysis where we control for a number of factors, it is worth mentioning the issue of the higher frequency component extracted from the LTV ratios.[51] The counter-cyclicality above was based on data for the period leading up to the bubble bursting in the early 1990s when real estate prices surged and after which prices steadily decreased. One may wonder if this counter-cyclicality of the LTV ratios also holds for de-trended counterparts. In Appendix F, we calculate higher-frequency de-trended LTV ratios, and their correlation coefficients with real estate values.

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[51] We thank an anonymous referee for alerting us to this.

We find that the coefficients are negative and statistically significant for a substantial range of the smoothing parameter, although they are insignificant for smaller parameter values (see Appendix F). This finding indicates that the counter-cyclicality is also present for de-trended LTV ratios at relatively lower frequencies, but not for those at higher ones. Although this finding might suggest a path for future research, we will continue to focus on the original series of LTV ratios. We do this because it is our sense that regulators would not implement the LTV caps in business lending based on de-trended series. However, we note that any implication from our findings may not apply to higher-frequency cycles.

4.2. Multivariate analyses

4.2.1. Methodology and variables

We now investigate the cyclicality of LTV ratios by controlling for a variety of factors, including many of the same variables that could be used in practice by lenders in loan underwriting. 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.

Table 2 shows variable definitions and summary statistics. Our dependent variable is the LTV ratio. The main independent variables are the registration year dummies (*YEAR1991-2009*, with 1990 as the default). We control for loan, borrower (firm), and lender characteristics, using variables shown in Table 2.[52]

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 the median is better than focusing on the mean because as Table 2 shows, the mean LTV ratio (7.7) is relatively higher than the median (1.4), suggesting that there are outliers with large LTV ratios.[53]

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.

4.2.2. Results

Table 3 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 and 90 percentiles. In each column, most of the variables are significant and reflect their expected signs.

The key finding here is that the year dummies consistently exhibit an

[52] See Appendix G for more information on the control variables. 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.

[53] When we run OLS regressions after dropping observations that fall into the 1% tails of the LTV ratio distribution, the results (not reported) are qualitatively the same as those of the median regression below. This conclusion does not change even if we cluster standard errors by time and industry.

increasing trend from 1993 or 1994 to 2009 (as compared to 1990). Even after controlling for a variety of factors that might affect the LTV ratio, and eliminating potential survivorship bias to some extent, the LTV ratios in the midst of, or just after, the bubble period were low compared with those afterwards.[54] This finding is consistent with the counter-cyclical LTV ratio that we found in our univariate analysis. 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. 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, 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.

4.3. Discussion

Our finding of counter-cyclicality implies that bank risk exposure with respect to the LTV ratio was decreasing, not increasing, during the boom period. This is inconsistent with conventional wisdom that blamed banks for fueling the bubble by lowering their underwriting standards – at least with respect to this important element of the loan pricing menu.

Our finding has an important implication for the efficacy of implementing a

[54] Unreported results (available from the authors) show that the multivariate results are qualitatively unchanged even if we split the sample by industry (when a sufficient number of observations are available), including the real estate and construction industries.

cap on the LTV ratio, because regulators have implicitly assumed – as we noted earlier – that the LTV ratio is pro-cyclical. Our finding implies that this may not be the case, and 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. Curbing the volume of credit during the boom would have required a very low initial LTV cap, and/or a cap that varied in a counter-cyclical manner.

To curb the volume of credit during the boom, the imposition of a ceiling on the volume of secured loans, or on total real estate exposure, rather than imposing a cap on LTV ratios makes sense in light of our findings. As we noted above, a ceiling on the amount of loans to firms in the real estate industry was imposed by the Ministry of Finance in 1990, and it allegedly had a direct effect in bursting the real estate bubble.[55] Some critics argue that the ceiling should have been introduced far earlier (see Uemura 2012).

Our findings also suggest that the implementation of LTV caps – or at least simple LTV caps – on real estate-based business lending in other countries may not be effective to the extent that loan underwriting follows the Japanese pattern. In this respect, it is interesting to note that our results are consistent with findings in other countries that LTV ratios in residential mortgage lending were counter-cyclical – or, at least, not pro-cyclical. For example, LTV pro-cyclicality in residential mortgage lending was not evident in the U.S. in the

[55] Even the Economic White Papers of the Government of Japan (for years 1992 and 1993) report this effect of the ceiling, although it is empirically difficult to identify specific causality.

pre-crisis period (e.g., Adelino et al. 2018a).[56] Counter-cyclicality was also observed in the residential mortgage market, for example, in Japan from 1994 to 2009 (Bank of Japan 2012) and in the U.K. from 1997 to the late 2000s (FSA 2009)). Our findings are also consistent with results in Goodhart et al. (2012) who calibrated the effects of different macroprudential policy measures on credit expansion and house prices in a general equilibrium model.[57] Thus, the ineffectiveness of a simple LTV cap can be a concern for residential mortgages and potentially other types of loans beyond the real estate-based business loans that we examine in this paper.

4.4. Some methodological issues

4.4.1. Survivorship bias

Although our finding is robust to controlling for observable loan-, borrower-, and lender-characteristics, it may still suffer from biases due to limitations inherent in our data. We now consider possible biases from two sources that could affect our conclusion that the LTV ratio is counter-cyclical.[58]

One of these concerns is a survivorship bias. Although we have a sample of

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[56] Justiniano et al. (2015, Figure 1.2.; 2019, Figure 3) similarly 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. Campbell and Cocco (2015, Figure 1) report that origination LTV ratios for residential mortgages in the U.S. were stable from 1984-2008. Using U.S. data from 1998-2008 Glaeser et al. (2013, Table 7.13 and Figure 7.6) report that cumulative origination LTVs ratios are fairly stable over time, but that origination LTV ratios for first lien residential mortgages are counter-cyclical. See also Ferreira and Gyourko (2015, Figures 4 and 5) and Adelino et al. (2018b, Figure 9) that report stable LTV ratios in the U.S. in the pre-crisis period. Note that these studies use complete (i.e., non-synthetic) data for residential mortgages that is immune to the survivorship bias that we will discuss in section 4.4.1.

[57] Goodhart et al. (2012) find with respect to LTV caps that a large increase in asset prices lowers the LTV ratio. The authors 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”.

[58] In addition to the two concerns discussed here, Appendix H deals with the issue of measuring the value of collateral using its current value.

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 *more* creditworthy, because registrations for borrowers that had defaulted and exited before 2008 are not included. On the other hand, these firms may be *less* creditworthy, because stronger borrowers who were able to repay loans before 2008 and had no loan demand thereafter might have left the sample. Thus, we cannot predetermine the net direction of the bias.

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. This is not observed in Figure 6, 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 for the bubble period might be because high LTV loans underwritten during the bubble period had a 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 8 shows the decomposition of our sample firms in each year by priority. 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 – a finding *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 9 shows their rate of bankruptcy during the 2008-2010 period depending on the period of loan origination (for each of the five-year cohorts). We calculate the rate in each of the four quartiles based on the 25, 50, and 75 percentiles of our LTV ratio (calculated in each year). Although younger loans and loans with higher LTV ratios are more likely to go bankrupt, the rate of bankruptcy of high-LTV borrowers is not exceptionally higher for loans underwritten during the bubble period.

4.4.2. Value of buildings

Another concern in our findings is the omission of the value of buildings from the denominator of our LTV ratio due to data limitations that could potentially drive counterfactual counter-cyclicality. This omission may not be as problematic because in Japan, the value of buildings is generally smaller relative to the value of land. 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.[59] Also, we already found counter-cyclicality using aggregate data that included the value of buildings (Figure 4), which helps mitigate concern over the omission of buildings in our multivariate analysis. Further, when we decompose V in Figure 4 into the value of the land and the value of the buildings (Figure 10), we find that the value of buildings is smaller, especially during the bubble period.[60]

Nevertheless, we cannot entirely exclude the possibility that the omission of the value of buildings still matters. In theory, as we demonstrate in Appendix I, 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 counter-cyclicality. Although we cannot directly calculate this underestimation, evidence based on the aggregate SNA data suggests that the magnitude of the underestimation does not exhibit significant cyclicality, and would not reverse the counter-cyclicality that we found in our univariate and multivariate analyses (see Appendix I).

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[59] 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. We are not aware of similar cross-country evidence on commercial property (e.g., office buildings). However, if commercial real estate depreciation maps that of residential real estate depreciation in Japan, then the durability of Japanese buildings would be relatively short. 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%.

[60] Yoshida (2016, Table 7) also reports that the share of the structure is about 30% to 40% at median building ages, although it depends on location, within or outside of Tokyo, property type (residential or commercial), and building age.

Another important assumption that we make is that the types, and thus values, of buildings do not differ significantly across our observations. This assumption does not hold if some firms did not have buildings on their land (e.g., agricultural firms), and others owned complex factories (e.g., manufacturing firms). This problem has already been mitigated to some extent because we have no observations for firms in the agriculture, forestry, and fishing industry. In addition, we already control for differences in building values that are related to the type of land (land district types) in our hedonic regressions (see Appendix C). To control for any remaining differences, we also conduct the analysis by splitting our sample by industries. The sub-sample results reported in Appendix E do not change our conclusion that the LTV ratios are counter-cyclical.

5. LTV ratios and the ex post performance of borrowers

5.1. Methodology

We now turn to an analysis of the risk channel. As we noted in the introduction, the literature on macroprudential policy emphasizes that LTV caps are designed, in part, by regulators to reduce borrower risk by decreasing the probability of default (PD) and loss-given-default (LGD) of individual loans. Of course, we cannot directly test this assumption by regulators, because we do not observe the counterfactual in Japan given that LTV caps were not implemented. However, we can test the association between high LTV loans, which would not have been made had LTV caps been implemented, and ex post borrower performance, which is, at least partly, a reflection of ex ante risk. Put differently, we test whether high-LTV loans perform worse than low-LTV loans (as implicitly assumed, for example, in FSB 2012) – and indirectly whether an LTV cap would have dampened the risk channel in Japan. 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.[61]

In this analysis, 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) observations as loans that are in the fourth quartile of the entire sample in terms of their LTV ratios.[62]

We define the control observations using two alternative procedures. In the first, we simply consider the controls to be all firms with non-treatment loans (non-high-LTV loans) and compare them with the treatment group. In the second alternative, we define as control firms those that have similar ex-ante characteristics as each treatment firm by employing a propensity score matching approach. Using this approach allows us to control for the differences in ex-post performance between high- and low-LTV firms stemming from their ex ante differences.[63] Matched controls also eliminate, at least partially, the

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[61] For recent evidence on the positive relation between origination LTV ratios and default rates for residential mortgages, see, for example, numerical simulations by Campbell and Cocco (2015, Table VI) and the descriptive statistics in FSA (2009, Exhibit 4.5) and references therein. As far as we know, Agarwal and Ben-David (2018, Table 6) is the only paper that shows the positive relation between LTV ratios and default rates for business loans, although it is not the main focus of their paper and their paper does not address the issue of firm quality and LTV ratios.

[62] For firms that obtained multiple secured loans in a year, we use the one with the highest LTV ratio. This reduces the number of observations from the previous 59,125 to 48,334.

[63] Following studies that employ a 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.

survivorship bias that a simple unmatched control group might suffer from. To calculate the propensity scores, we run a probit regression for the probability that a borrower obtains a high-LTV loan conditional on the covariates used in section 4.2.[64] For each treatment observation, the matched observation is selected from the non-treatment firms with the closest propensity score.[65]

To capture ex post performance we use four firm characteristics: (1) the number of employees, (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), which eliminates time-invariant firm-fixed effects. We then calculate the average difference in these differences within the treatment and control firms. 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 the maximum in the performance variables, the sample period ends in 2004.

In addition to these four variables, we also focus on the incidence of bankruptcy as a measure of ex post performance. Here, we simply compare the rates of ex post bankruptcy between the treatment and the control groups.

[64] The results of the probit estimation are similar to those of the quantile regressions in Table 3, and so we do not report them. These estimations are available from the authors.
[65] We employ 5-nearest matching, in which 5 observations whose propensity scores are the closest to each treatment observation are chosen.

Because we only have information on bankruptcy after 2008 (section 4.4.1: Figure 9), we use an indicator that takes the value of one if the firm 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 section 4.4.1 (Figure 9), but we also use the matched control.

5.2. Results

Tables 4 and 5 show the results of the ex-post performance analysis. Table 4 is the analysis that employs our "pseudo" 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 sets of columns respectively report the results using the whole sample, and the subsamples of 1990-94, 1995-99, 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 unmatched estimations (Row (1) of Table 4), the results show that treatment (high LTV) firms perform better than control (low LTV) firms in terms of employment growth (*d_F_EMP* in years t+1 and t+2) and profitability (*d_F_ROA* in years t+3, t+4, and t+5). We find no significant differences 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. These 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 the 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. These results in Table 4 suggest that while high LTV lending may be associated with higher loan risk after 1995, the underwriting of high LTV loans in Japan was not associated with lower firm quality and may have rather been associated with growing and profitable firms.

Table 5 shows the results for the analysis on ex post bankruptcy. In this table, we report the rates of ex post bankruptcy (during the 2008-2010 period) for treatment and control firms, their difference, and 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.[66]

We find that the bankruptcy rate 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 9, and suggests that had a uniform cap been set on the LTV ratio, Japan 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. In terms of ex post employment growth, sales growth and an increase in ROA, firms that obtain high LTV loans do not perform worse - and actually perform better - during or just after the bubble period. However, firms that obtain high LTV loans do perform worse in terms of subsequent bankruptcy, although our analysis is confined to bankruptcy after 2008. Taken together, our findings suggest that firms that obtain high LTV loans are risky but growing firms.[67]

These mixed findings on ex post performance for high LTV loans have an

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

[67] The findings of no worse, and even better, ex-post performance for high LTV borrowers would seem to be inconsistent with the findings on zombie firms in other studies (e.g., Peek and Rosengren 2005, Caballero et al. 2008). However, what these studies find is evidence suggesting that poor-performing firms survived because their banks extended evergreening loans, which cannot be directly compared with our findings for ex-post performance of high versus low LTV borrowers. Also, these studies focus mostly on the late 1990s, while we find better ex-post performance for the early 1990s.

important policy implication, because they speak to the issue of unintended consequences from imposing a cap on the LTV ratio. 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 consider the possibility that an LTV cap might reduce access to debt financing for growing firms.

6. Conclusion

Using unique data from the official real estate registry in Japan, this paper looks at the LTV ratios of business loans. 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 while firms that were granted loans with high LTV ratios are ex post riskier, they were also faster growing than those granted low LTV loans.

Although our main analysis is on all business loans secured by real estate, we also conduct analyses on loans just to firms in the real estate business (the real estate and construction industries). In Japan, securing loans by real estate is prevalent in lending to *all* firms, not just firms in the real estate business, and evidence indicates an economically important link between the crisis and lending against real estate to firms in *all* industries, not just firms in the real estate industry (see section 2.2). However, separate robustness tests on just firms in the real estate business are meaningful because the current policy focus on the LTV cap component of the IMF corporate sector toolkit is centered around real estate loans to purchase real estate, and such loans are much more likely to be distinct because of their non-recourse structure. Our main results are robust to these separate tests.

Our findings have important policy implications, because they are inconsistent with the two underlying assumptions that regulators make to motivate the efficacy of LTV caps: the pro-cyclicality of LTV ratios and the lower quality of firms with higher LTV ratios. This inconsistency suggests 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, and might have had the unintended consequence of curbing loans to growing firms. Our findings also imply that the efficacy of an LTV cap may depend crucially on how it is conditioned. Although it is beyond our focus in this paper, these findings suggest that when researchers and regulators compare LTV caps with other macroprudential tools, they should take into account these potential effects.

While the purpose our paper is not to identify the cause of the financial crisis in Japan, the paper's main finding that collateralized lending is counter-cyclical is inconsistent with a causal link from loans with high LTV ratios (their pro-cyclicality) to the crisis. Our findings rather indicate that although Japanese banks decreased an intensive margin (LTV ratios) during the property-price bubble in the 1980s, they increased an extensive margin (the proportion of collateralized loans). We cannot exclude the possibility that this bank behavior is a more important cause of the crisis.

Although our analysis focuses on business lending, our results on LTV cyclicality and LTV loan performance could conceivably apply to residential mortgages as well. Our finding that LTV ratios were not pro-cyclical, for example, is consistent with findings on the residential mortgage market in the U.S. during the pre-crisis period (Adelino et al. 2018b). However, three caveats are worth mentioning in generalizing our findings.

First, inconsistent with our results on ex post performance in business lending, high LTV lending in the U.S. residential mortgage market in the form of subprime mortgages appears to have resulted in higher losses. Second, the relationship between the risk in high LTV real estate-based business loans and high LTV residential mortgages can differ depending on whether the residential mortgage is on a recourse or non-recourse basis. And third, even in comparing business loans secured by real estate with recourse residential mortgages, the nature of business income is quite different than personal income.

Finally, our analysis suggests that more research is needed on the efficacy of implementing LTV caps and on potential unintended consequences. While our study focuses on the corporate sector, some of our findings could apply in implementing LTV caps on residential mortgages and even other types of business loans. Although we have not addressed the issue of alternatives to LTV caps or the interaction of LTV caps with other macroprudential tools, we agree with the IMF's guidance on this that emphasizes that "country-specific circumstances" matter and that the "interactions between different tools" and "the sequencing of tools" matter (IMF 2014).

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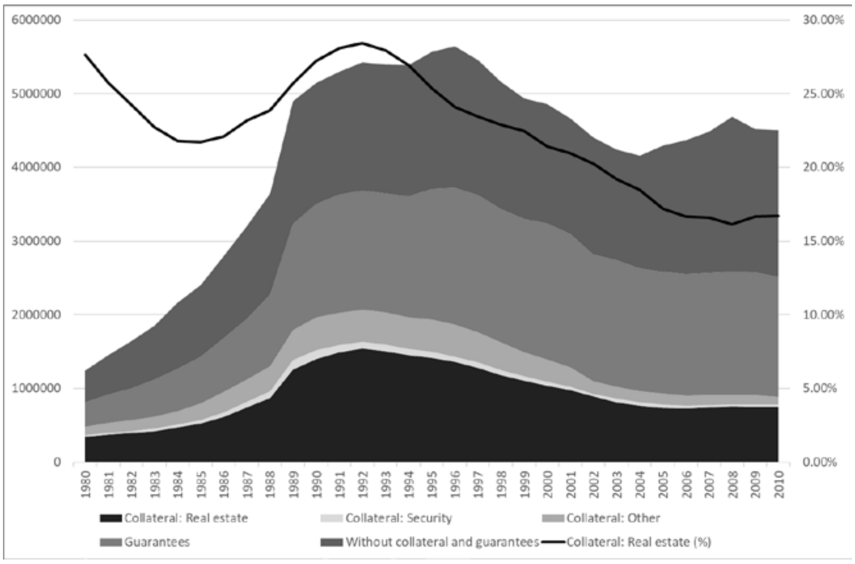
Tables and figures

Figure 1 Decomposition of total bank loans outstanding by collateral

This figure shows the decomposition of total bank loans outstanding in Japan depending on whether and how they are secured (100 million yen, the left hand/side axis). The solid line represents the percentage share of loans with real estate collateral (%, the right hand/side axis).

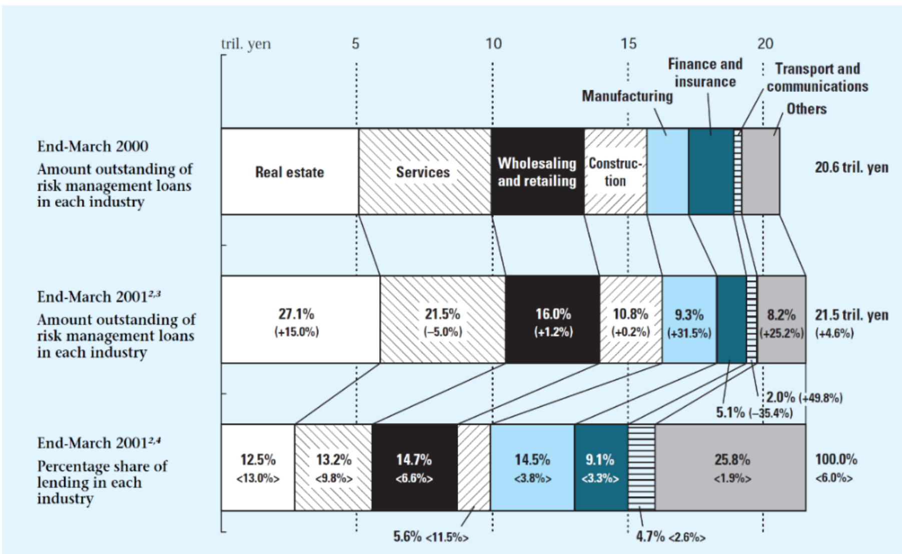


Source: Bank of Japan, Outstanding of Loan (Others) − Amounts of Loans by Collateral Type, BOJ Time-Series Data Search

https://www.stat-search.boj.or.jp/ssi/cgi-bin/famecgi2?cgi=$nme_a000_en&lstSelection=LA03

Figure 2 Non-performing loans by borrowers’ industry

This figure shows the decomposition of the amount of non-performing loans (NPLs) defined as “the risk management loans” by borrowers’ industry.



Source: Bank of Japan (2001).

Figure 3 Number of observations

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

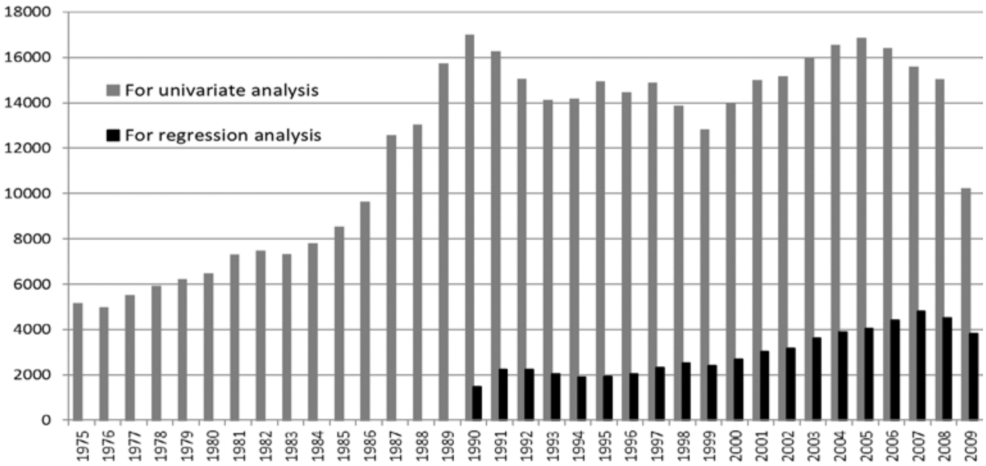
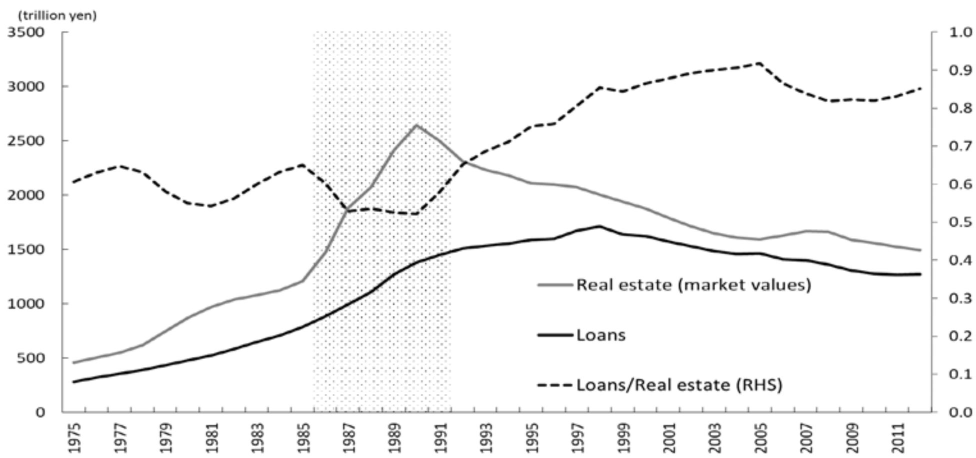


Figure 4 Value of real estate, bank loans, and aggregate LTV ratio

This figure shows the market value of real estate (land and buildings), and loans outstanding at the aggregate level, together with the ratio of the loans to the value of real estate. The so-called "bubble" period is shaded. As for the value of real estate, figures for years 1994-2012 are from the National Accounts for 2014, and those for years 1975-1993 are calculated based on year-on-year growth rates obtained from the National Accounts for 2009 (1980-1993) and for 1998 (1975-1979).

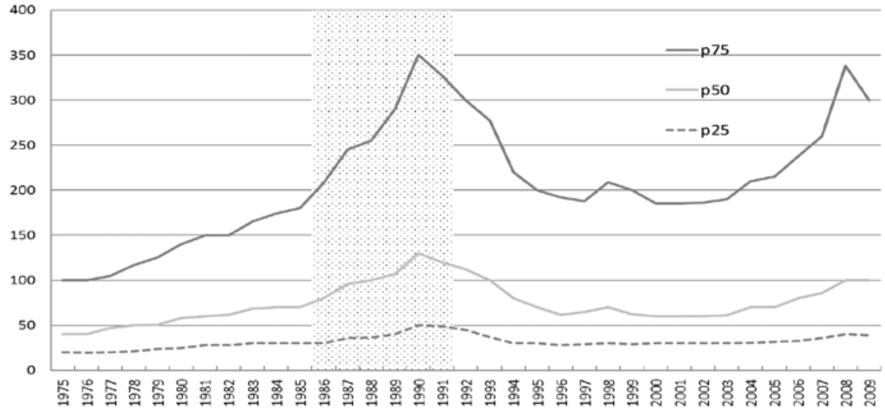


Source: Cabinet Office, National Accounts for 2014, 2009, and 1998.

Figure 5 Loans and values over the business cycle

Panel (A) and (B) show the time-series path of the amount of loans and the value of collateralized land in our sample at its three percentile points. The so-called “bubble” period is shaded. The number of observations is 420,889.

Panel (A) Loans



Panel (B) Value of collateral

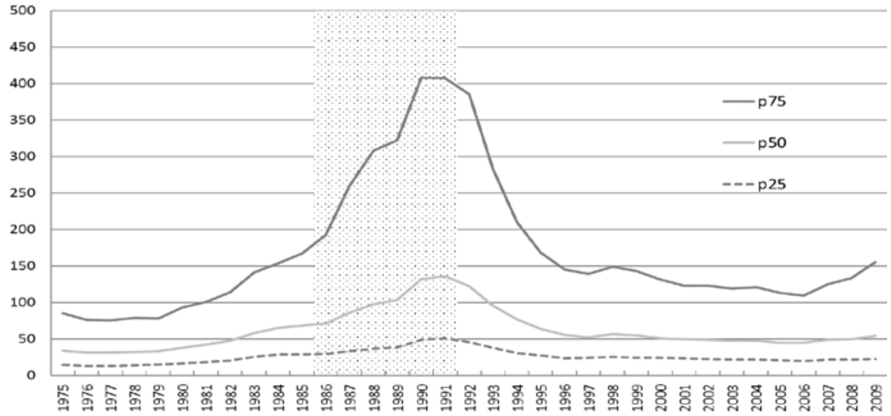


Figure 6 LTV ratios over the business cycle

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. The number of observations is 420,889.

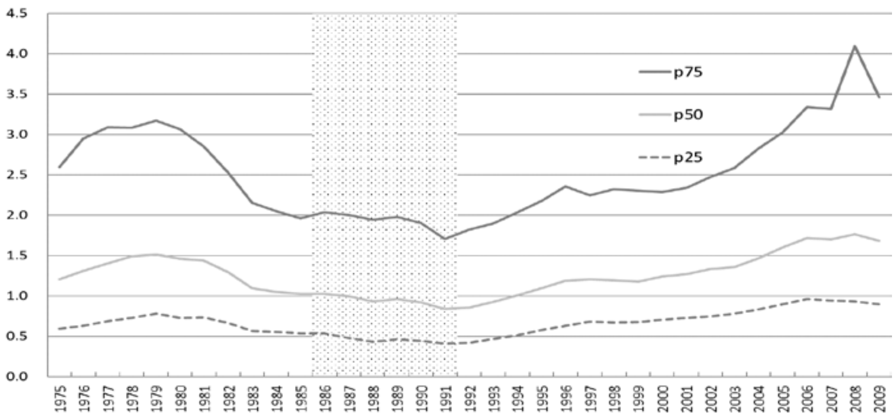


Figure 7 LTV ratios for real estate and construction firms

This figure shows the time-series path of the LTV ratio for real estate and construction firms in our sample, which is calculated as the amount of loans over the value of collateralized land. The number of observations for Panel (A) and (B) is 15,845 and 55,948, respectively.

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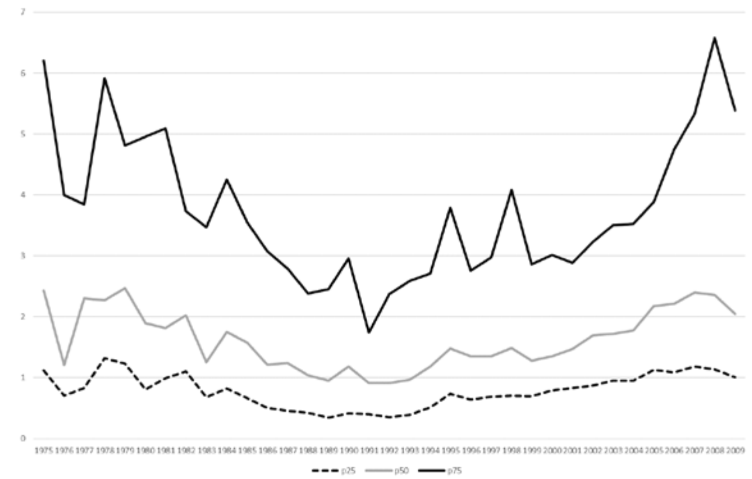
Panel (A) Real estate firms



Panel (B) Construction firms

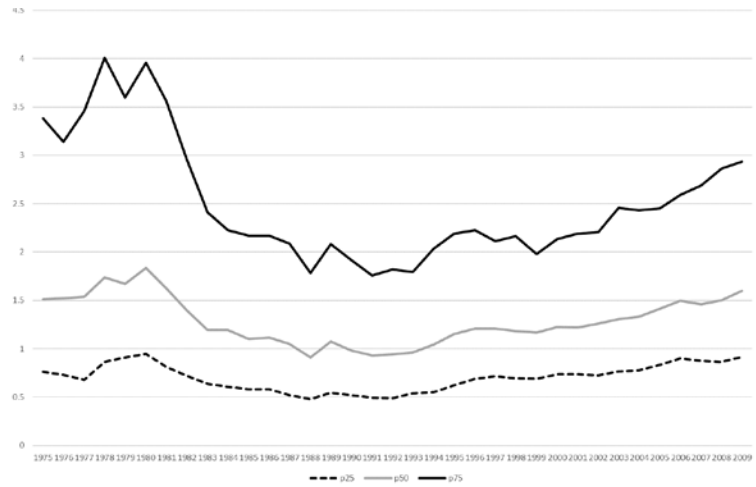


Figure 8 Decomposition of loans by priority

This figure shows the shares of loans in each year by different priorities. The number of observations is 443,379.

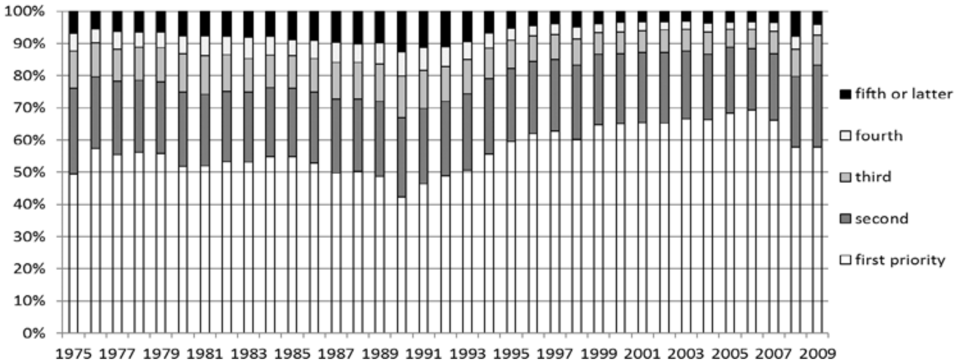


Figure 9 Rate of ex post bankruptcy during the 2008-2010 period

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 by LTV ratio quartiles. The number of observations is 412,166.

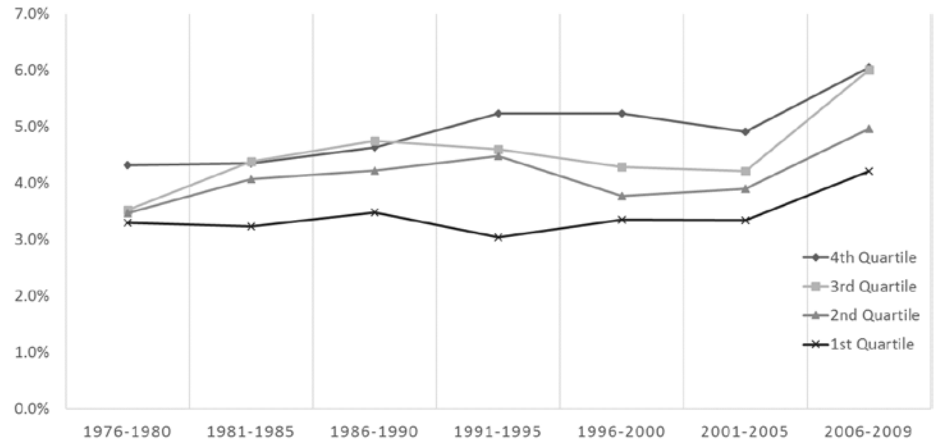


Figure 10 Relative value of buildings and land

This figure decomposes the values of buildings and land (National Account Statistics).

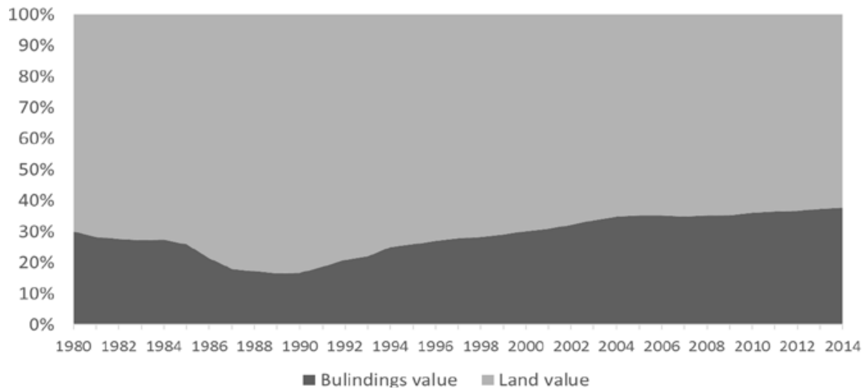

100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
1980 1982 1984 1986 1988 1990 1992 1994 1996 1998 2000 2002 2004 2006 2008 2010 2012 2014
Bulindings value
Land value

Table 1 Decomposition of the amount of total bank loans outstanding by industry

This table shows the amount of bank loans in Japan by industry.

tril. yen

	End-March 1985	End-March 1993	Changes from end-March 1985	End-March 2001	Changes from end-March 1993
Total of all banks	250.8	481.7	+230.8	450.0	–31.6
Manufacturing	67.5	77.7	+10.1	66.4	–11.3
Construction	16.1	30.0	+13.8	28.1	–1.9
Transport and communications	9.7	18.0	+8.3	20.4	+2.4
Wholesaling and retailing	58.7	78.2	+19.5	65.4	–12.8
Nonbanks	26.5	74.5	+48.0	38.7	–35.8
Real estate	20.6	58.6	+38.0	58.0	–0.6
Services	18.8	56.3	+37.5	49.8	–6.4
Individuals	28.4	82.3	+53.9	96.7	+14.4

Source: Bank of Japan (2001).

Table 2 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
Borrower regional 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_CEILINC	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 3 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 2. ***, **, 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*	-0.015		-0.014	*	-0.163	**
Policy measures	*PL_ACTION*	-0.049	**	-0.012		0.311	***
	PL_CEILING	-0.106		-0.074		-0.954	
constant		0.227	*	-0.015		17.622	***
Bank type, Industry, and Regional dummies		Yes		Yes		Yes	
NOB		59125		59125		59125	
Pseudo R2		0.0201		0.0134		0.0347	

Table 4 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. The number of observations is 48,334.

			(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 5 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. The number of observations is 48,334.

(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 not typically used for residential loans. Second, loans are also classified as business loans if the debtor is a firm (not its CEO). 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 we cannot determine whether the loan is a business loan or a residential loan. 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.

References for Appendix A

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.

Appendix B: 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 B-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.

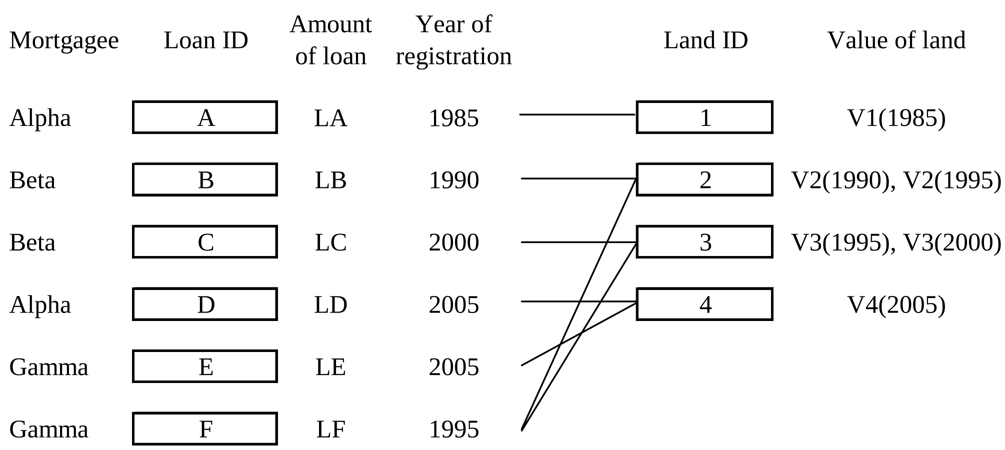
Calculation is fairly simple if a land is pledged to only one claim holder. In the example above, this is the case for loan A. Information about the amount of loan A, represented by LA, is provided by TDB database. The value of land A in year 1985, V1(1985), is estimated by the hedonic approach described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



Appendix C: Estimation of the current value of land

C.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). In particular, we assume that the log price of a piece of land *i*, $\log P_i$, is the sum of its K components:

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

In the actual estimation, we follow these 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 land complied in PNLP is explained by different explanatory variables. The explanatory variables in this estimation are:

- the size of land in logarithms
- a regulatory upper limit of the floor area ratio
- the 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
- the 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 for the industrialized land districts, we group some neighboring prefectures into one area to form 15 areas because in this case, the number of observations in some prefectures is not large enough to warrant reliable estimation results.

Second, based on the parameters obtained from the estimation of the above regressions, we estimate (predict) the current prices of the land in our dataset. We need to estimate these prices because the number of 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 estimate the price of a particular piece of land in our dataset, we use the parameters for the same land district type, year, and region.[68]

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

C.2 Estimation results (first stage)

As for the first stage of hedonic model estimations, the number of regressions that we run for land in residential districts and land in commercial districts are both 1,738 (= 47 prefectures times 37 years, except for Okinawa in year 1975). The number for land in industrial districts, the third type of land district, is 555 (15 regions times 37 years).

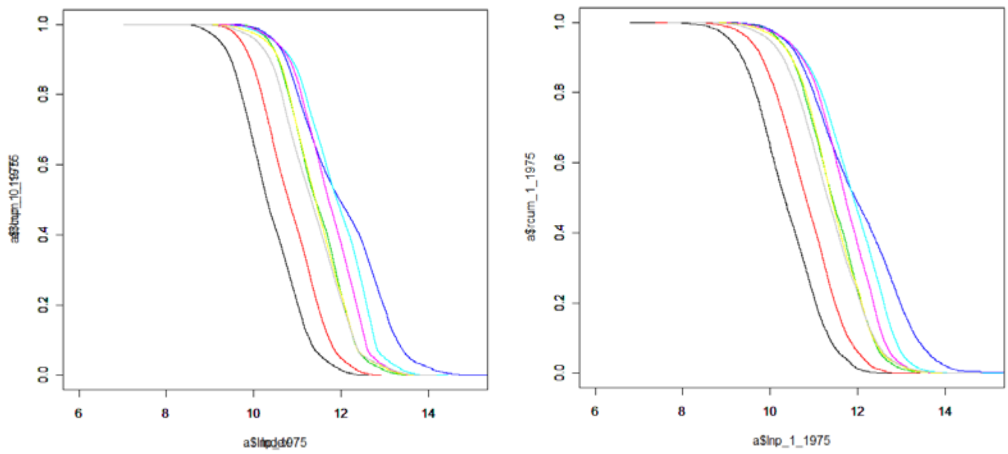
To confirm the accuracy of these estimations using the coefficients obtained from the hedonic estimation, in-sample comparisons are shown in figure C-1. In the figure, we show the cumulative distributions of the predicted prices (left panels) and the actual PNLP prices (right panels) of the lands in 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 our approach.

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

[Residential land]

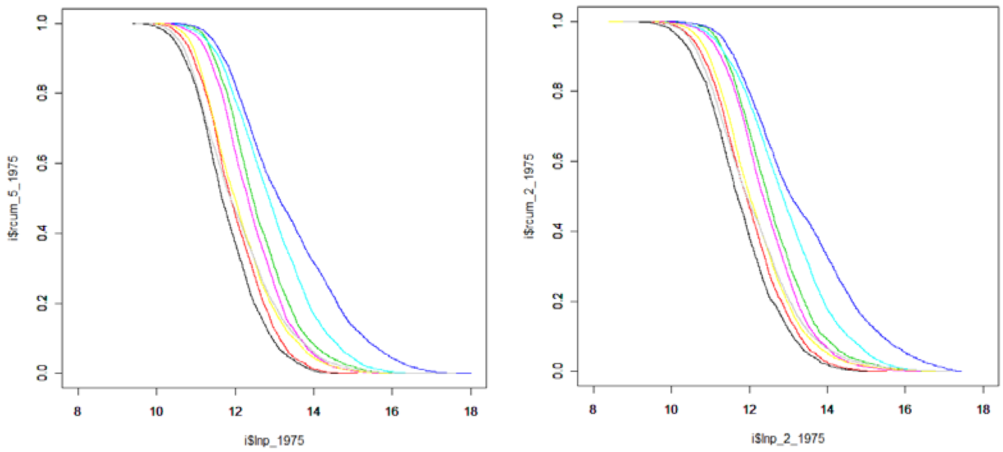
[Estimation] [Actual]

[68] For example, suppose land A in the TDB dataset is in a residential district in the Tokyo prefecture in year 1990. In this case, its current price is estimated 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.



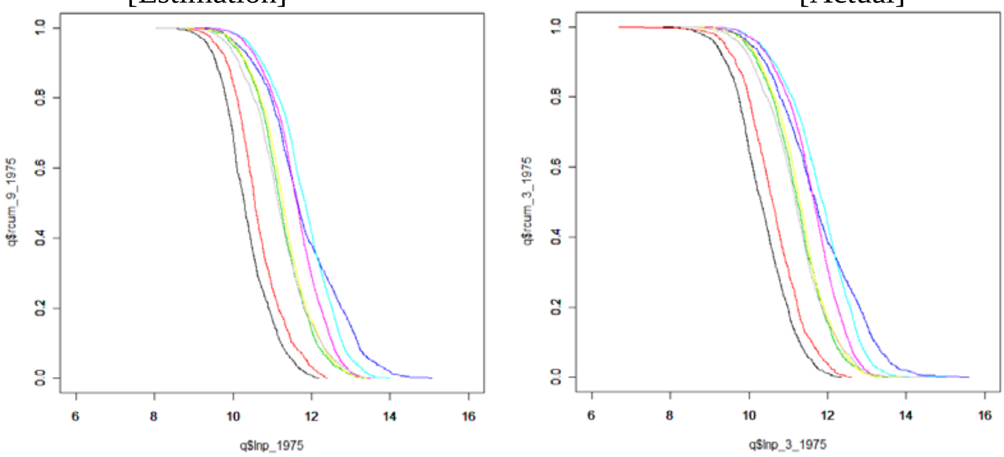
[Commercial land]

[Estimation] [Actual]



[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.

C.3 Projection results (second stage)

Based on the coefficients estimated in the first stage, we estimate 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 estimated prices were higher than the highest price of lands in the corresponding combination in the PNLP dataset. We also dropped those observations whose estimated prices were lower than the lowest price in the PNLP database in the relevant year.

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

Figure C-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)]

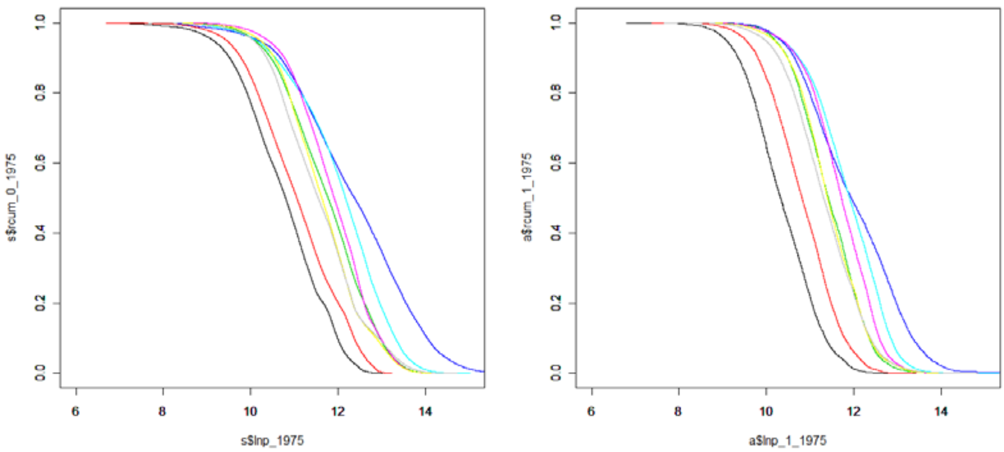
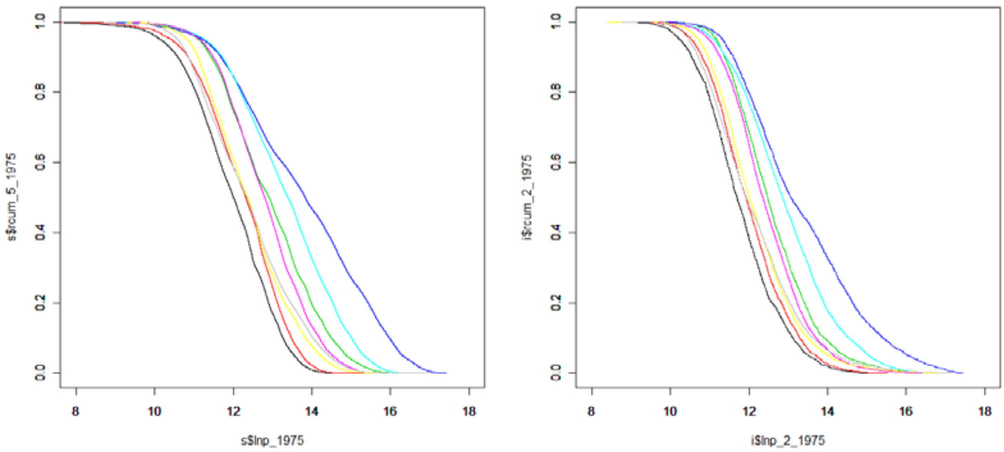


Figure C-2 (continued)

[Commercial land]

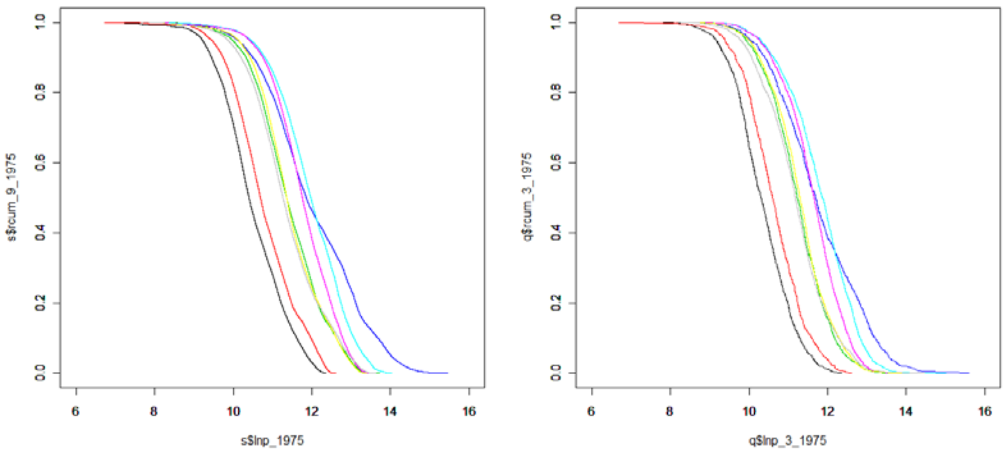
[Projection (TDB)] [Actual (PNLP)]



[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

References for Appendix C

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

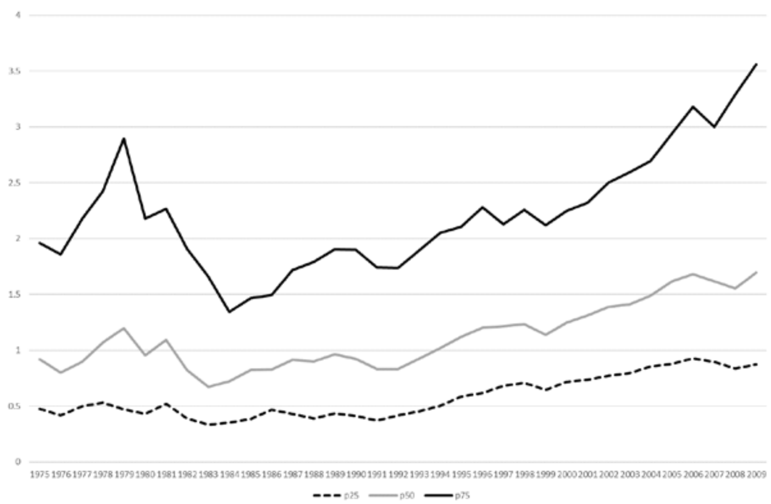
Appendix D: Ne-tanpo loans

To address the concern that the meaning of the LTV ratios differ depending on the purpose of the loans, here we calculate the ratios by splitting our sample into “ordinary” loans and ne-tanpo loans. This is because it is highly likely that ne-tanpo loans are used for financing working capital (see section 3.1), so secured business loans used to purchase the underlying (associated) real estate would be confined to “ordinary” loans. Note that this analysis also addresses another concern on whether our finding on counter-cyclical LTV ratios is caused by the maturity of loans because it may be the case that the observed earlier loans are long-term loans. By looking at LTV ratios on ne-tanpo loans, we can eliminate such a possible bias.

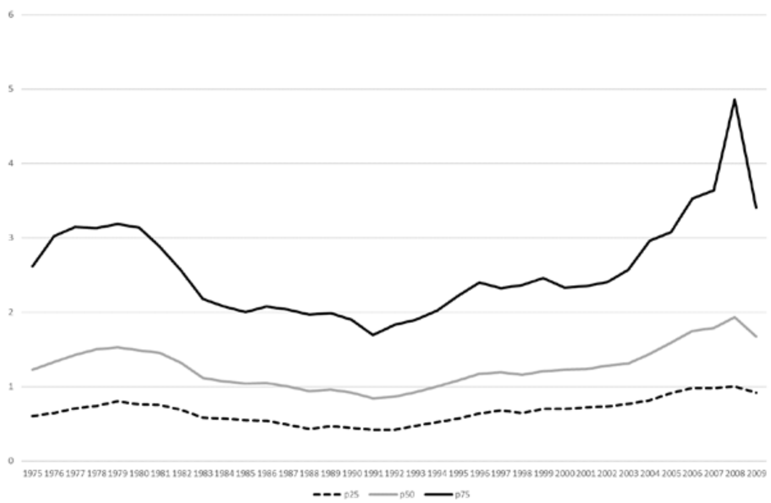
In Figure D-1, we report the time-series path of the LTV ratios as Figure 6 for the split sample. We find that the shape of the paths is mostly V-shaped, meaning that the LTV ratios for split samples exhibit counter-cyclicality. However, we see some differences in the V-shape across the samples. First, the paths for the subsamples of Non-ne-tanpo loans do not exhibit a clear V-shape. Second, the bottom of the paths for the subsamples of Non-ne-tanpo loans is around the middle of 1980s, while that of Ne-tanpo loans is around 1990. Examining the causes of these differences across industry and loan types might be an interesting research issue for a further study.

Figure D-1 LTV ratio for ne-tanpo and non-ne-tanpo loans

Panel (A) Non ne-tanpo loans



Panel (B) Ne-tanpo loans



Appendix E: Industry split

In this Appendix, we report the results of splitting our sample in order to address a shortcoming in our data that alluded to in the paper. Specifically, our denominator V does not include the value of buildings, only the value of the land. This would not affect our analysis so long as building values do not differ significantly across observations. As we discuss in Appendix I using aggregate data, this assumption is justified at least across years. However, this assumption might not hold within each year if, for example, some firms do not have much building infrastructure on their land (e.g., agricultural firms) while others typically have complex factories and building infrastructure on their land (e.g., manufacturing firms). The lack of firms in primary industries in our sample diminishes concern about this problem, but to control for any remaining differences, we conduct the analysis by splitting our sample into industries.[69]

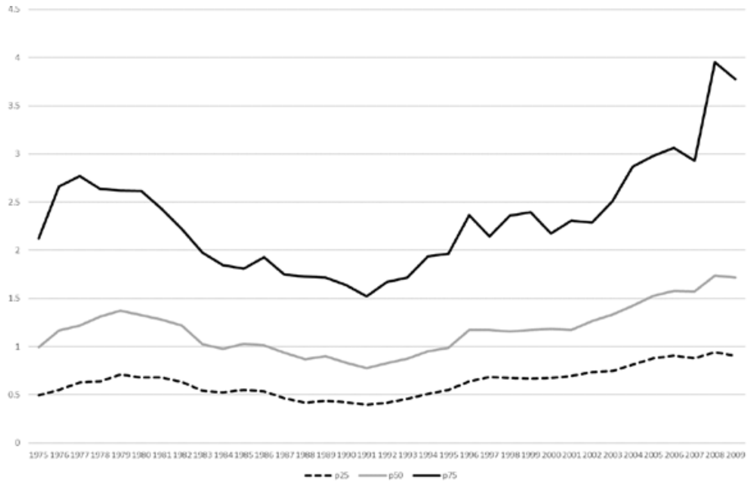
In Figure I-1, we report the time-series path of the LTV ratios as in Figure 6 for the split sample. We find that the shape of the paths is mostly V-shaped, meaning that the LTV ratios

[69] We do not report the results for real estate and construction firms because we have already reported them in section 4.1.2.

for split samples exhibit counter-cyclicality. However, we see some differences in the V-shape across the samples, and the paths for the subsamples of Services do not exhibit a clear V-shape.

Figure E-1 LTV ratios by industry

Panel (A) Manufacturing



Panel (B) Wholesale

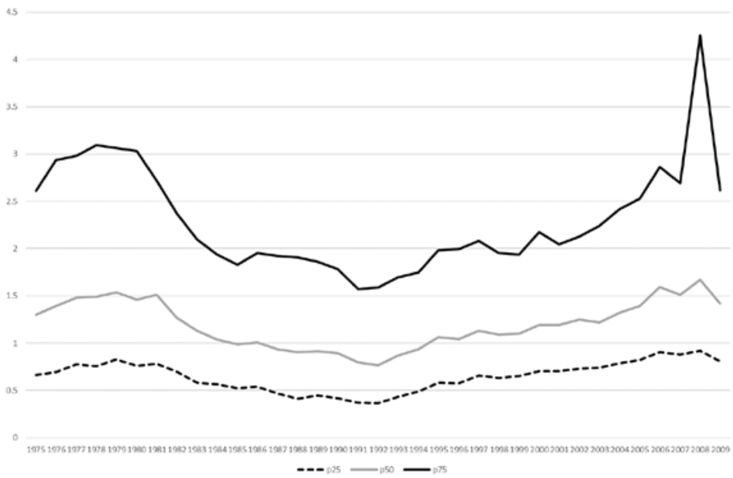
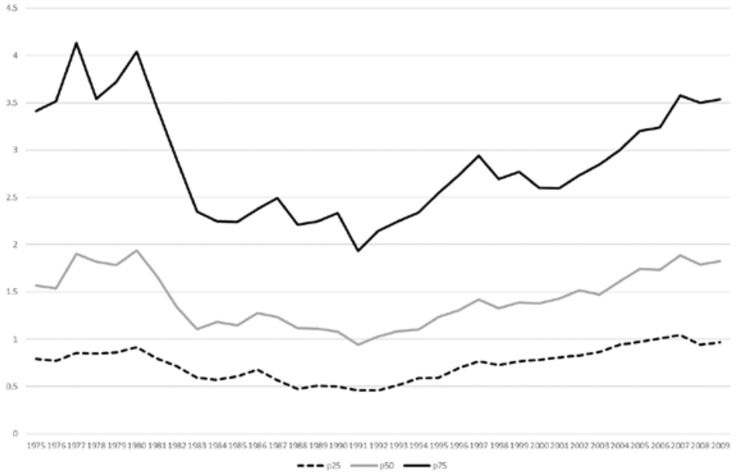
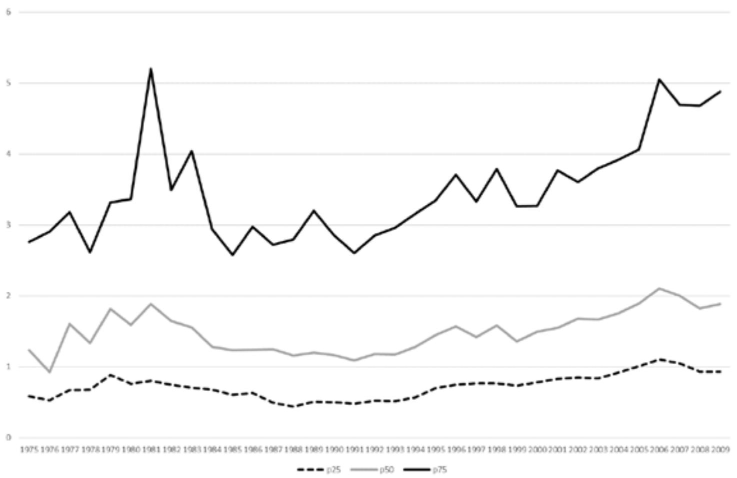


Figure E-1 (continued)

Panel (C) Retail and restaurants



Panel (D) Services



Appendix F: De-trended LTV ratios and their correlation with real estate values

To construct de-trended LTV ratios, we employ the Hodrick-Prescott filter. First, we calculate a trend component (g_t) of the original series of our LTV ratios (y_t) by following this formula,

$$\min_{\{g_t\}_{t=-1}^{T}} \{\sum_{t=1}^{T}(y_t - g_t)^2 + \lambda \sum_{t=1}^{T}[(g_t - g_{t-1}) - (g_{t-1} - g_{t-2})]^2\}.$$

We then take the difference $y_t - g_t$ to generate a de-trended series of LTV ratios.

One important methodological issue in this de-trending is the choice of the smoothing parameter λ. Hodrick and Prescott (1997) motivate their choice of $\lambda = 1600$ for the quarterly data based on their belief that a large change in the cyclical component within a quarter is around 5 percent, while a large change in the trend component is around $1/8$ percent. This led them to select $\lambda = (5/(1/8))^2 = 1600$. Ravn and Uhlig (2002) show that time aggregation changes the parameter by the fourth power of the observation frequency. Provided that they start from the value of 1600 for the quarterly frequency, they use $\lambda = 1600/(4)^4 = 6.25$ for the annual frequency. However, Marcet and Ravn (2003) emphasize the possible heterogeneity of λ across countries. They theoretically show that uniformly applying the same λ to all countries might distort the results when a de-trended component is highly serially correlated, and suggest that researchers should check if results are "reasonable" in light of common wisdom. They find that reasonable values for λ at the quarterly frequency in some OECD countries including Italy, Japan, and Spain are substantially larger than the conventionally used value of 1600.

Against this background, we use alternative values for λ when we generate de-trended LTV ratios. Specifically, we varied the values for λ from 6.25 to 1600. We then examine the correlation between these de-trended LTV ratios and aggregate real estate values. A negative (positive) correlation coefficient indicates counter- (pro-)cyclicality of the de-trended series.

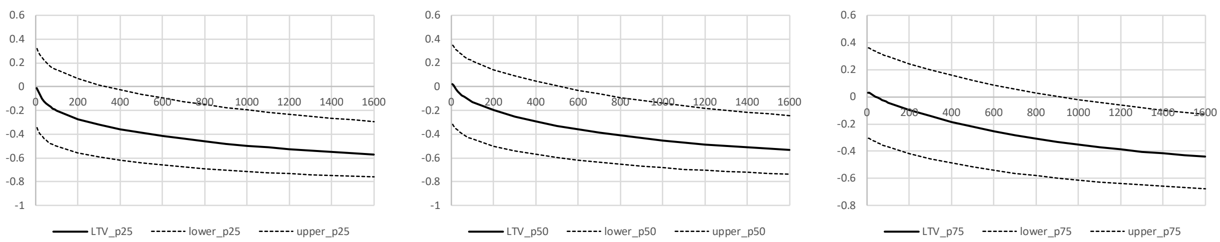
Figure F-1 shows the correlation coefficients that we obtain as a result, where panels (A), (B), and (C) respectively use contemporaneous, one-year lagged, and one-year lead values of real estate. In panel (A), the correlation coefficients are negative for the entire range of λ s for the de-trended LTV ratio at the 25 percentile, negative only for λ being no smaller than 20 for the ratio at the 50 percentile, and no smaller than 50 for the 75 percentile LTV ratio. In panel (B), all the correlation coefficients are negative throughout the entire range of λ and for all the de-trended LTV ratios. Panel (C) is similar to panel (A) in that correlation coefficients are negative only for a limited range of λ. Note, however, that in all the panels the negative coefficients are statistically significant only for the higher range of λ. In sum, this finding indicates that the counter-cyclicality is also present for de-trended LTV ratios at relatively lower frequencies, but not for those at higher ones.

Although the HP filter is most commonly used in studies on business cycles, to check the robustness of the results, we also employ other, less-frequently used filters: the Christiano-Fitzgerald filter and the Butterworth filter. Table F-1 shows the result, indicating that correlation coefficients between the de-trended LTV ratios and real estate values are not statistically significant.

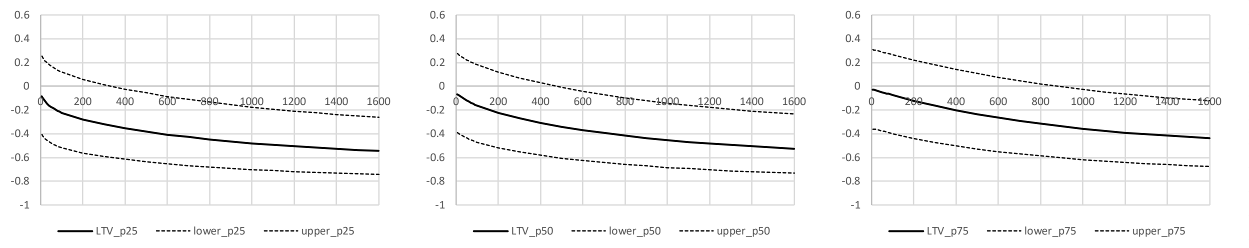
Figure F-1 Correlation coefficients between de-trended LTV ratios and real estate values (the Hodrick-Prescott filter)

This figure reports the correlation coefficients between de-trended LTV ratios and real estate values. In panels (1) to (3) we apply the HP filter for de-trending and employ a range of values from 6.25 to 1600 for the smoothing parameter λ . For real estate values, we employ the market value of real estate (land and buildings) in Figure 4. We measure values of smoothing parameters along the horizontal axis, while we have correlation coefficients with the 95% confidence interval along the vertical axis.

Panel (A) HP filter: Contemporaneous real estate values



Panel (B) HP filter: One-year lagged real estate values



Panel (C) HP filter: One-year lead real estate values

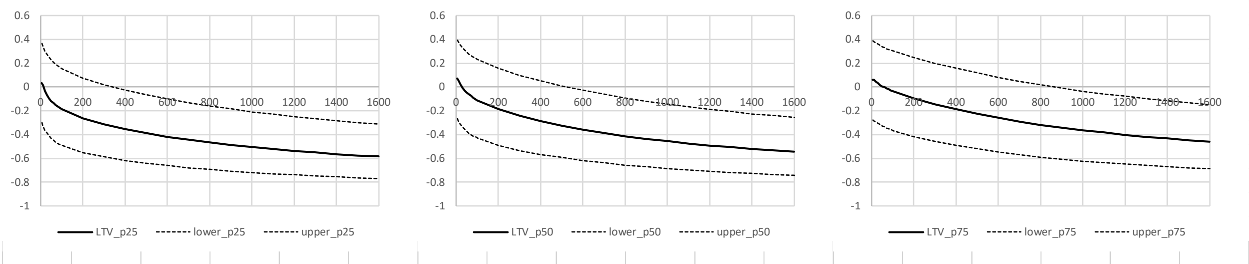


Table F-1 Correlation coefficients between de-trended LTV ratios and real estate values (the Butterworth filter and the Christiano-Fitzgerald filter)

This table reports the correlation coefficients between de-trended LTV ratios and real estate values. We apply two types of filters for the de-trending: (1) the Butterworth filter (we set the maximum period of 8), and (2) the Christiano-Fitzgerald filter (we set the minimum and maximum periods of 2 and 8, respectively). ***, **, and * respectively indicate that the relevant coefficients are statistically significant at the 1, 5, and 10% level.

Butterworth filter			Chiristiano-Fitzgerald filter		
LTV_p25	LTV_p50	LTV_p75	LTV_p25	LTV_p50	LTV_p75
with contemporaneous real estate values					
0.0119	0.0344	0.0291	0.0778	0.092	0.0363
with one-year lagged real estate values					
-0.062	-0.0546	-0.0236	0.0281	0.0302	0.0118
with one-year lead real estate values					
0.0572	0.0852	0.0508	0.1078	0.1271	0.0423

References for Appendix F

Hodrick, R.J. and E.C. Prescott. (1997). Postwar U.S. Business Cycles: An Empirical Investigation, Journal of Money, Credit, and Banking 29(1), 1-16.

Marcet, A. and Ravn, M.O. (2003). The HP-Filter in Cross-Country Comparisons, Working paper.

Ravn, M.O. and H. Uhlig. (2002). On Adjusting the Hodrick-Prescott Filter for the Frequency of Observations, The Review of Economics and Statistics 84(2), 371-380.

Appendix G: Control variables for the multivariate analysis on cyclicality

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 2 shows that 66% of our sample loans are *ne-tanpo* loans. Because loans secured by *ne-tanpo* are usually used to finance working capital, *L_netanpo* is a proxy for short maturity. The *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 2).

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 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 firm's lenders.[70] This controls for the likelihood that the main banks assume 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 2 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 boom in 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.

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 is one of the these three types. This controls for the possible change in the willingness of banks to lend on an unsecured basis.

Appendix H: Future value of collateral

Another possible methodological concern in our findings 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.[71] The second

[70] The banks on the list are ordered based on their importance as subjectively determined by TDB.
[71] 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).

definition uses a V that is interpolated from its previous year's growth rate, i.e., V(t-1)·{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 alternative definitions of V, the LTV ratios still exhibit almost similar counter-cyclicality as shown in Figure 6.[72]

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

Let us denote the amount of the loan, the value of pledged buildings, and the value of pledged land at time *t* as L_t, V_t^B, and V_t^L respectively. We assume pro-cyclicality of L_t and V_t^L, and without loss of generality, let us focus on the bubble period, i.e., $L_t < L_{t+1}$ and $V_t^L < V_{t+1}^L$.

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

$$\frac{L_t}{V_t^L} > \frac{L_{t+1}}{V_{t+1}^L},$$

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

$$\frac{L_t}{V_t^L+V_t^B} < \frac{L_{t+1}}{V_{t+1}^L+V_{t+1}^B}.$$

These two inequalities are reduced to:

$$\frac{V_{t+1}^L+V_{t+1}^B}{V_t^L+V_t^B} < \frac{L_{t+1}}{L_t} < \frac{V_{t+1}^L}{V_t^L}. \quad (1)$$

For expositional simplicity, let us define x_t to satisfy $x_t V_t^L = V_t^L + V_t^B$. The x_t indicates how many times the true *V* (i.e., $V_t^L + V_t^B$) is larger than our *V* (i.e., V_t^L), or the inverse of x_t indicates to what extent our *V* underestimates the true *V* , and x_t is larger than 1 for secured loans underwritten in year *t*. Using x_t, we can rewrite Inequality (1) as:

$$\frac{x_{t+1}}{x_t}\frac{V_{t+1}^L}{V_t^L} < \frac{L_{t+1}}{L_t} < \frac{V_{t+1}^L}{V_t^L}.$$

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, x_{t+1}/x_t (or the rate of increase in our

[72] See Ono et al. (2013, section 3.1.3.) for these results.

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 x_{t+1}/x_t is large enough, at least sufficiently larger than one. On balance, our finding of counter-cyclical LTV ratio is flawed if x_t sufficiently decreases (increases) when our LTV ratio decreases (increases), i.e., if x_t exhibit significant counter-cyclicality.

Although we cannot directly quantify this underestimation, Table H-1 provides us with closely related evidence. Column (1) of this Table H-1 reports the amount of the value of land and of buildings using the SNA data that are depicted in Figure 10. Based on these figures, we can calculate x_t, the magnitude of underestimation, as in Column (2), and x_{t+1}/x_t as in Column (3). As this column shows, x_{t+1}/x_t deviates very little from 1, suggesting that the omission of buildings is not consequential. We can reconfirm this conclusion by depicting the figure of the adjusted LTV ratios that uses $x_t V_t^L$ as the denominator (see Figure I-1).

References for Appendix I

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.

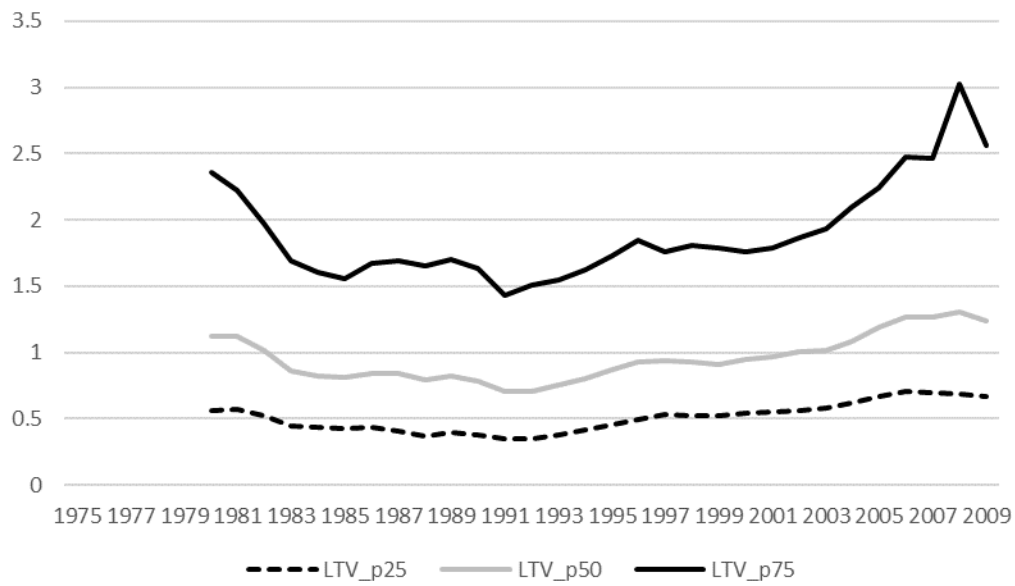
Table I-1 Value of land and buildings from the SNA

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)		(2)	(3)
	Buildings	Land	$x_t =$	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)

Figure I-1 Adjusted LTV ratio

This figure shows the time-series path of the adjusted LTV ratio that is defined as the amount of loans over the estimated value of property, i.e., $x_t V_t^L (= V_t^L + V_t^B)$ shown in Table I-1.


3.5
3
2.5
2
1.5
1
0.5
0
1975 1977 1979 1981 1983 1985 1987 1989 1991 1993 1995 1997 1999 2001 2003 2005 2007 2009
LTV_p25
LTV_p50
LTV_p75