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Credit Constraint and Synchronous House Price Movements: Evidence from the Mortgage Credit Regulation in Korea

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Abstract

This study aims to offer empirical evidences on causal linkage between credit constraints in residential mortgage lending and synchronicity of local housing returns, by testing effects of the time- and location-varying LTV regulations in Korea in a quasi-experimental design. Specifically, by employing the similar R^2-based testing statistics used by Cotter et al (2015), we investigate drivers of co-movement in local housing prices in Korea, and the results of our data analyses show that: first, as in the U.S., the extent of the price co-movement in Korea has been rising in the recent years, reducing the diversification benefit for national investors on housing (e.g., large mortgage lenders); second, the longer the duration of the LTV restriction in given geographical areas, the lower the level of the price co-movement in the Korean cities, implying that macroeconomic shocks tend to have a smaller impact on housing markets with more strict lending restrictions and the LTV regulations tend to work as a market stabilizer in the Korean case. We test the robustness of this result with various alternative regression models, and find a reasonably robust testing outcome. We also show that the results hold after controlling potential endogeneity between imposition (or removal) of the regulation and local housing price dynamics.

JEL Classifications:

Keywords: House Price Co-movement, LTV, Macro-prudential Policy

1. Introduction

During the last financial crisis, the synchronous movements of housing prices became a deep policy concern in destabilizing financial markets and weakening real economy. Considering a long line of literature on banks' geographic diversification, even from pre-Great Depression era such as Sprague (1903), it is not surprising that accelerating housing price co-movement could potentially impose substantial risks for financial institutions and investors. Obviously, financial markets do price this risk by asking for ex ante risk premiums of insurer and bank stocks load on undiversifiable real estate factor (Mei & Saunders 1994; Mei & Saunders 1995). However, during the last financial crisis, this risk was heightened to an unprecedentedly level and became a major systemic risk domestically (Cotter *et al.*, 2014) as well as globally (Hirata et el. 2012). In particular, Cotter *et al.* (2014) (CGR henceforth) documents considerbale variations in synchronicity among the metropolitan area housing prices over time in the U.S. and pointed out that, even before the recent financial crisis, sharply increased synchronicity from the early to mid-2000s weakened the effectiveness of GSEs' and large Wall Street firms' diversification efforts through securitization such as MBS and REITs. They identified mortgage securitization and ease of mortgage underwriting as the main drivers.

Even though those two factors identified are consistent with the literature that emphasizes the role of the (imperfections in the) credits markets for the financial crisis (Mian and Sufi, 2009), the causalities between the synchronicity of housing prices and the factors identified in CGR are not yet clear. First, the LTV measures used in their study might not be exogenous. CGR use the conforming loan-to-value ratio reported by FHFA as the measure for the national LTV ratio. The minimum LTV ratios are set by GSEs, but the actual level of LTV is determined by the borrower.

Moreover, since lenders adjust their lending standards, including LTV ratios, for conforming loans based on various factors including their expectations about the housing market and financial market conditions, identification of effects of exogenous changes in lending standards would be difficult in studies that rely on aggregate data such as CGR. Second, the effects of securitizations might substantially overlap with those of eased lending standards. At least in the context of U.S. housing markets, private label securitizations were driven by proliferation of subprime lending which occurred at the same time as loosened lending standards. Third, the precise channel through which weakened lending standards affect housing price synchronicity is not clear. The relationship between lending standards and the level of housing prices are well-documented, especially during the last financial crisis (Duca et. al. (2011), Adelino et. al. (204), Favara and Imbs (2015)). However, there has not been much attention on how lending standards affect the co-movements of regional house prices. It is difficult to apply the findings in the literature cited above to the relationships between lending standards and house price synchronicity since changing synchronicities implies asymmetric effects of lending standards on house prices.

This paper empirically investigates the causal link between credit constraints and housing return synchronicity in Korea by using cross-sectional and time-series variations in loan-to-value regulations exogenously imposed by the government. After the Asian financial crisis, Korean government instituted a comprehensive package of aggressive anti-speculation policy instruments under the title of designation of speculative zone (DSZ) policy. The policy instruments of the DSZ policy include higher effective capital gains taxes, DTI restrictions, and LTV restrictions. Among them, LTV restrictions are the most potent; the government imposes

the maximum LTV ratios (LTV caps) on metropolitan housing markets where housing prices are deemed "speculative."

Taking advantage of experiences with the DSZ policy in Korea, this paper have several advantages over the existing studies. First, the time-varying, locally based maximum LTV provides a quasi-natural experiment for cleaner testing grounds for the effects of lending standards on synchronicity of housing prices. The LTV restriction policy by Korean government is unique in global real estate market regulations; not only the target housing markets are narrowly defined (based on administrative districts within a metropolitan area) but also the level of LTV caps are frequently changed over time depending on the developments in the district-level housing markets. Second, in contrast to the strong securitization activities in the States, securitization was quite sluggish in Korea during the sample period. Even though a small number of mortgage loans were securitized through government-driven efforts, but it did not attract interests from banks and other mortgage lending institutions. In addition, most of securitized mortgage loans were of high quality unlike the subprime loan securitized during the credit boom period before the crisis in the States. This implies that our LTV measures will provide a cleaner measure of lending standards than those used by CGR. Third, since LTV restrictions are imposed by district, their effects on the co-movement of the district-level housing price with those of other districts can be more precisely estimated. In CGR where national level LTV are used, it implicitly is assumed that the aggregate level lending standards have differential effects on metropolitan area housing prices, which, in turn, generate time-varying synchronicity of metropolitan housing prices. However, it is not clear through which channel the aggregate lending standards can have locally differential effects. In our paper, it is district- and time-

varying LTV caps that render asynchronous effects on district housing markets with a given national shock. Extant theoretical studies show that credit constraints can radically alter the link between macroeconomic factors and housing price movements (Stein 1995; Ortalo-Magné & Rady 2006). They model the demands for "starter" and "trade-up" homes by borrowing constrained buyers. They commonly predict that relaxation of credit constraints boosts the impact of macroeconomic shocks on housing price movements because the demand for "trade-up" home depends on the size of down payment (or credit constraint) and the increase in home equity. Since the macroeconomic shocks can be either aggregate shocks such as national income as well as location-specific idiosyncratic shocks, we will focus on the aggregate shocks. A given aggregate shock will have larger (smaller) effects on district housing markets with less (more) restrictive LTV caps.

Since the focus is on interactions among aggregate shocks, local housing price and local LTV cap, we use regression based R^2 as synchronicity measure as in CGR. The R^2 based measure for housing price synchronicity is relatively new; instead, other studies (Kallberg, Liu and Pasquariello (2012), Landier, Sraer, and Thesmar (2013)) have used correlation coefficients as co-movement measure. For our purpose, correlation based measures are less useful and difficult to use since we relate housing price synchronicity to the degree of transmission of national level shocks to local housing markets.

Using an R^2 based measure on Korean housing markets entails another advantage. R^2 is often used as asset price synchronicity measure (Morck, Yeung and Yu (2000), Chan and Hameed (2006), Chan, Hameed and Kang (2013), Chan and Chan (2014)), but is also used as a measure of market integration. In certain contexts, it is obvious what an R^2 captures, but not always. For

example, Cotter et. al. use the measure interchangeably between two interpretations. It certainly is plausible in the U.S. case where deregulations on bank branching gradually took effects on housing markets. In our case, however, the interpretation is clearer. Since the Korean housing market was already integrated throughout the sample period, an R^2 only captures housing price synchronicity not the degree of market integration. In Korea, there was no restrictions on bank branching, and housing finance has been dominated by a few large national banks. In addition, Korean housing market is substantially more liquid due to homogeneity of housing units and to Chonsei system, ownership interests in a housing unit and consumption of housing service from the unit are highly separated.

We first compile intra-metropolitan area (district) level housing price indices (on a quarterly basis) for Seoul and six other large metropolitan areas in Korea. To capture the R^2-based synchronicity in housing prices, we iteratively estimate the regression models of housing price returns based on national level housing fundamentals for every five-year period for each location from the first quarter of 2000. Next, we regress those estimated R^2 on a set of district level fundamentals and the DSZ policy variable. The results show that, while the cross-sectional dispersions in housing returns in Korea are larger in the early 2000s, the extent of the price co-movement in Korea has been rising in the more recent years. But more importantly, location- and time- varying LTV regulations significantly affect district level housing price synchronicity.

Closely related to our paper, Lamont and Stein (1999) present evidence that the city areas with a greater proportion of highly levered homeowners yield higher sensitivities in housing price changes with respect to city-level per-capita income. Our study differs from Lamont and Stein

(1999) in the following three aspects. First, Lamont and Stein (1999) focused the effects of local shocks under financial constraints on local house prices, while we investigate how financial constraints affect "nation-wide" synchronicity of housing prices *across* different local markets. Second, our empirical method is less restrictive, in that our empirical specifications allow more than one macro-factors to drive house price synchronicity. For instance, the key market fundamental in Lamont and Stein (1999) was local per capital income; we include a wide array of market fundamentals, demand and supply conditions as well as financial market indicators, but all of them at national level following CGR. Finally, we employ Korean government's DSZ policy as an instrument for LTV restrictions, an it is less likely to subject to the bias due to endogeneity of LTV ratio used in Lamont and Stein (1999).

Our main findings are three-fold. First, we find that the duration of LTV restrictions is negatively associated with house price synchronicity. This implies that district level housing prices tend to co-move less when LTV restrictions are imposed. There has been many discussions on marcoprudential policies of which LTV restrictions are important tools. However, all the discussion has focused on its effects on the level of housing prices, but not on the synchronicity of them. This paper highlights the possible additional effect of macroprudential policies, that is, enhancement of geographic diversification benefits for lenders and financial institutions. Second, we consider the plausible drivers of LTV regulations. It is an important question because it directly addresses potential endogeneity issues. Endogeneity is much less of concern for our paper than for Lamont and Stein (1999) since we focus on synchronicity of housing prices in different housing markets than on price impacts in each individual markets. In addition, LTV regulations are mostly determined in the political process outside the housing markets, leaving

considerably small room for endogeneity problems. However, we identify some factors that are highly associated with the regulation: the duration of conservative party in power at the central government (judged by presidency) and the duration of conservative congress member of the district. Even though the policy decisions are mostly made at the central government, we found evidence that local, district level political process has some effects. Statistical analysis shows that only the interaction effect between the two political processes is significant. Third, we found robust evidence that LTV restrictions are the most effective policy instruments compared to DTI regulations or higher capital gains taxations in terms of reducing house price co-movement and increasing geographic diversification effects for investors and lenders.

The rest of the paper consists of the following five sections: the housing price dynamics and mortgage lending restrictions in Korea (Section 2), empirical strategies (Section 3), data description (Section 4), empirical results and robustness tests (Section 5), and concluding remarks (Section 6).

2. Housing Market and Lending Restrictions in Korea

The Korean housing sector has exhibited dramatic growth. Since 1960s, due to the rapid industrialization and urbanization, the Korean housing market experienced chronic shortages of affordable housing units, resulting in accelerated housing price appreciations in Seoul (capital) and other major metropolitan areas. In 1989, Korean government instituted the aggressive housing supply policies, which increased the housing stock by 30 percent by the early 1990s, which brought in stabilized housing prices in Seoul and other metropolitan areas throughout the rest of 1990s (Figure 1). However, the sharp economic downturn by the Asian Financial Crisis

(AFC) in 1997~99 substantially brought down housing market activities and depressed the housing prices. Korean government responded by lifting many restrictions from mortgage lenders and took important deregulation measures. This played a pivot role in turning the Korean housing finance system into a competitive, market oriented institution which allowed easier access to credit for borrowers.

As the crisis was short-lived and the economy experienced a quick recovery, the housing price started to rapidly appreciate, which made Korean government concerned about housing market being overheated.

In response, Korean government initiated a potent "anti-speculation" measure called "designation of speculative zones (DSZ)." Under this policy, Korean government designates as "speculative zone," the districts within Seoul and other metropolitan areas where they believes that the local housing markets are in the midst of the speculative bubble. Once a district is designated as speculative zone, home buyers and investors in the district face more restrictive LTV and DTI constraints as well as heavier capital gains taxes. Those constraints are interacted with other market factors, such as lender type (commercial banks vs. mutual savings banks[1]), mortgage product types (fixed-rate vs. adjustable rate, and amortizing vs. non-amortizing), and property type (high-priced property vs. medium-/low-priced property). The LTV cap can be as low as 40% and the newly purchased real estate cannot be resold unless the original seller is not fully paid, thereby imposing stricter credit constraints on housing demand in the local markets.

[1] Mutual savings banks in Korea are smaller and more locally based than commercial banks which tend to be larger and nationally operated. Mutual savings banks tend to concentrate on small and medium-sized enterprises and middle to lower income households.

Those constrains have been flexibly applied depending on local housing market characteristics and macroeconomic conditions.

The decision on speculative zones are made by the vice minister of Strategy and Finance who consults with Real Estate Stabilization Committee. The Committee includes high level central government officials and outside experts. It is important to note that discussions and decisions of imposition or dismissal of the policy on a given district is largely determined at the central government, not at the local government. This gives an interesting contrast to U.S. or European countries where most of policy decisions on housing markets are made at the local government. However, this does not completely exclude district level effects; it is still possible that local representatives such as congress member might potentially but informally exercise some influences on the policy decision process, which we will explore later as robustness check for endogeneity.

Note that anti-speculation policies are not unique in Korea. It is not uncommon among East Asian countries to employ a similar mortgage lending restriction as a counter-cyclical policy instrument, as observed in Hong Kong, Taiwan, Singapore, and China. What distinguishes the cap in Korea from those of other countries is the fact that the restriction is not only time-varying but also location-varying, providing enough variations in policy to conduct empirical tests for effect of the regulation in a panel regression setting. This provides important advantage in assessing the causal impacts of LTV regulations on housing market integration.

3. Empirical Strategy

Correlation coefficients have been used extensively in asset price synchronicity studies e.g., Kallberg, Liu and Pasquariello (2012), Landier, Sraer, and Thesmar (2013)), partly because they are simple and intuitive. However, the correlation coefficients are not universally accepted (e.g., Forbes and Gibbons (2002), Rukthuanthong and Roll(2009)). In particular, Pukthuanthong and Roll(2009) show that the simple correlation measure is likely to yield distorted information as to price co-movement when multiple market factors determine expected asset returns. In particular, they use the following example to demonstrate the potential bias of the simple correlation statistics:

$$R_{j,t} = \alpha_j + \beta_{j,\omega} \times f_{j,\omega} + \beta_{j,s} \times f_{j,s} + \varepsilon_{j,t}, \qquad (1)$$

where $R_{j,t}$ is a log growth rate of return in holding asset j at time t, $\beta_{j,\omega}$ and $\beta_{j,s}$ are the sensitivities of markets factors (or determinants) under two states of the market ω and s, respectively, and $\varepsilon_{j,t}$ represents an idiosyncratic risk. They demonstrate through a simulation analysis that, assuming $E[\varepsilon_{j,t}] = 0$ for all j, the systematic risk factors ω and s solely determine real estate returns with $\beta_{B,\omega} = \kappa\beta_{A,\omega}$, $\beta_{B,s} = (1-\kappa)\beta_{A,s}$, and that, except when the special case of $\kappa \neq 0.5$, the simple correlation coefficient drops below one and under-estimates extent of the co-movement. Pukthuanthong and Roll (2009) propose a simple intuitive measure of synchronous movement of asset prices, the R^2 from a multifactor model of asset price return[2]. The suggested measure represents the proportion of total variation in asset price returns

[2] The price synchronicity is measured based on the R2 ratios as adopted by prior studies. (Roll (1988), Morck et al. (2000), Jin and Myers (2006), An and Zhang (2012), Chan, Hameed, and Kang (2013), Chan and Chan (2014).

explained by the market variables, and thus a value close to unity indicates that the asset returns closely co-move with market-wide macroeconomic fundamentals.

CGR extend the literature to housing market in order to measure the extent of co-movement in the metropolitan level housing price changes. They report that the integration of U.S. housing markets has increased in recent years, which substantially lowers potential for geographical diversification. Although reporting the increasing correlation between house price returns and macro factors such as mortgage loan securitization and national income growth during the recent years, they do not formally test any causal link between them.

We follow CGR and estimate the house price co-movement in Korean metropolitan areas with R^2 from estimating multi-factor asset price models. For each quarter, we first calculate quarterly log returns of district housing price, and estimate the degree of co-movement based on 30 quarter observations.

$$r_i = \beta_{i0} + \sum_{j=1}^{k} \beta_{ij} f_j + v_i \,. \qquad (2)$$

r_i in (3) is log return in district i following Cotter, Gabriel and Roll (2014). For systemic risks, f_j, we consider log growth rate in MBS issuance, log of 91 day CD rate, log growth rate in industrial production, log growth rate in population, log of building permit, log growth rate in producer price index, log consumer sentiment, log return in KOSPI 2000 index and log of per capital income. The factors used in our estimation are almost identical to those used in CGR except for LTV ratio.

Figure 5 plots the average and quartiles of R^2 from estimating (3) at district level for six

metropolitan areas with the rolling window of 30 quarters. We will henceforth call the R^2 measure for housing price synchronicity as *CGR synchronicity measure*. From 2007 to the first quarter of 2008, the CGR synchronicity measure moderately increased. The trend reversed briefly during the rest of 2008, but the moderate growth in CGR measure resumed in 2009 and continued until the end of the sample period, 2012. This indicates that Korean housing markets increasingly co-moved for the most of time since 2007.

Despite the advantage of the R^2 based measures, it is well known that higher R^2 does not necessarily indicates the stronger integration of segmented markets; higher factor volatility may temporarily drive higher R^2. Bekaert and Wang (2009) argue that controlling for the period fixed effects is necessary to reduce this "volatility bias" but such control does not ensure that the results are completely free from the potential bias. Indeed, the potential "volatility bias" requires exploiting cross-sectional variations of determinants when we test the impact of financial constraint on the synchronous movements of asset prices. Whenever possible, we consider period effects in our regression specifications. We also check for robustness of our findings by estimating the cross-sectional regressions for each period separately.

3.1 Empirical specification

To understand the effects of LTV regulations on house price synchronicity, we estimate the following regression:

$$ln\left(\frac{R^2}{1-R^2}\right)_{it} = \beta_0 + \beta_1 \times \ln(1 + Duration\ of\ LTV\ restriction)_{it}$$

$$+ Local\ Market\ Characterisitcs_{it} + \eta_i + \tau_t + \epsilon_{it} \quad (3)$$

Since R^2 measure is bounded between zero and one so not appropriate for usual regressions, we transform the R^2 measure into $\ln\left(\frac{R^2}{1-R^2}\right)$. Our measure for credit constraint is the number of quarters during which the DSZ policy is imposed. During the period of the DSZ policy in effect, LTV caps are imposed by the central government on residential mortgage loans originated in the district. We posit the credit constraints as the main channel through which the DSZ policy and LTV caps affect the district level house price co-movements. However, factors unrelated to credit constraints can potentially affect house price co-movement and we include various local housing market characteristics and region- and period-fixed effects. One approach to understand how local characteristics can affect R^2 measure is that Equation (3) is a simplified, reduced form version of a more formal model where the housing price synchronicity depends how similar a local housing market is to the national housing market in terms of market fundamentals. Presumably, a local housing market whose fundamentals are close to the national averages tend to have higher housing price synchronicity. A more formal model will postulate that housing price synchronicity depends on some measures of distance between local fundamentals and their national counterparts. Equation (3) can be thought of a simplified reduced form approach to such models where all the national fundamentals are subsumed in time fixed effects. In this sense, the local market characteristics that need to be considered are those of housing market fundamentals such as income and demographics.

The recent studies show that the change in population can be positively associated with the demand for mortgage loans in the local market (Loutskina & Strahan 2015), but the older (younger) investors tend to have weaker (stronger) demand for housing (Saiz 2010). As instruments for the demand for housing, we consider local economic, demographic factors such as income growth, population growth rate, net flow of population, and proportion of population with age 65 and over. In addition, prior study shows that the housing supply elasticity hinges mainly on land scarcity due to geographic characteristics of terrains such as average land slope and proportion of wetlands. Unfortunately we cannot construct a measure similar to Saiz(2010)'s housing supply elasticity for Koran districts. However, the characteristics that endogenously determine land scarcity are largely time-invariant, so to we include various location-fixed effects to control for the supply for housing.

We use 30 quarter moving window approach to measure house price synchronicity. This procedure is likely to introduce the significant autocorrelation, for instance, the first-order autocorrelation of 0.92. This implies that t-tests without considering such autocorrelations are likely to overstate the significance of the effects. Therefore, we use cluster-robust standard deviations whenever possible following Petersen (2009).

4. Sample and Summary Statistics

The main geographical unit for observations in the analysis is the district (called "Gu" in Korean) within a metropolitan area, similar to municipality or county in the U.S. We use quarterly housing price indices at the district level of six Korean major cities, from the 1st quarter of 2000 to the 3rd quarter of 2012. The indices were compiled by Real Estate 114, one of

the largest real estate information service providers in Korea. Even though the span of the time series is relatively short, the data covers a large number of districts cross-sectionally, with the total number of observations over 6,000. Since there is a considerable difference in liquidity among housing markets in Korea, housing indices of some housing markets potentially suffer from non-synchronous trading problems (Bond and Hwang, 2007). We address this issue by including only the districts in seven largest metropolitan areas where a large enough number of transactions are ensured during the sample period: Seoul (capital), Pusan, Daegu, Inchon, Kwangju, Daejun and Ulsan.

Figure 2 shows the location of each metropolitan area on Korea map. Some of them are adjacent to each other (Seoul – Inchon and Busan – Ulsan) but overall they are reasonably well dispersed geographically over the country. Even though they are the six largest metropolitan areas in Korea, there exist substantial heterogeneity among them in terms of industrial structure, demographics and housing markets. Table 1 provides information about each metropolitan housing market. The cross-sectional unit of observation is the district within each city (called "Gu" in Korean). The districts, "Gu," have close economic ties each other within their metropolitan area, but also can have considerably different housing markets due to location heterogeneity or district level policies. Among all the metropolitan areas, Seoul is distinctively different from the rest. Not only it has been the capital city for several centuries with centralized administrative functions but also disproportionally concentrated economic activities. Table 1 shows that Seoul is by far the largest area; it has 44% of the total population of the seven metropolitan areas. In terms of the number of districts, the share of Seoul is 35%. It also has the highest housing price; its housing price is more than twice the second highest housing price of

Pusan metro area and more than the four times the lowest housing price of Kwangju metropolitan area. In particular, three districts (Seocho, Gangnam and Songpa) in south of the river have had the highest housing prices as well as the largest appreciation rates, and have frequently been enforced with the LTV regulations.

Figure 3 shows housing price indices of the seven metropolitan areas. Even though housing prices were rising all the areas over the sample period, they show divergent patterns of appreciation. Housing price in Ulsan appreciated most overall during the later sample period while those in Seoul and Incheon rose faster than others in the early 2000s, but considerably slowed down after 2007.

Information on speculative zones, the main variable of interest in this study, is provided by Korean Monetary Policy Committee. Panel A of Figure 4 plots proportions of districts under the LTV caps for each quarter. There were effectively no districts under the cap until 2002, but in 2003 close to 40% of districts were affected by the policy. At the peak of 2006, more than 60% of districts were under the cap, indicating overheated housing markets nationwide. In following years of 2007-08, the number of LTV restricted districts decreased moderately, but still more than a half of districts were under the policy. As Korean housing market markedly cooled down after 2008 during the Global Financial Crisis, the number of districts under LTV cap substantially dropped to less than 5%.

Panel (B) of Figure 3 contrasts the districts newly enforced with the LTV policy against the districts dismissed from the policy for each year. The credit constraints policy were strengthened during the period of 2003 through 2005 but it was considerably weakened since 2006.

We supplement district-level house price growth, duration of financial constraints with

information on district characteristics from KOSIS data by Korean bureau of statistics (namely, "Statistics Korea"). Specifically, we augment the district-level housing price data with log (per-capita??) income growth rate, log population growth rate, net flow of population and proportion of population over 65.

Table 2 presents summary statistics for CGR measure (Panel A) along with other variables used in regression analysis (Panel B). The average CGR measure is about 0.4, lower than R^2 for U.S. housing markets in CGR. CGR measures are widely dispersed, ranging from 0.07 to 0.90, which indicates substantially varying degrees of co-movement among housing markets in Korea. On the other hand, the average duration and the longest duration of LTV restriction is 2.24 years and 7.5 years respectively. There are a small number of districts which never had LTV caps placed in their housing markets.

5. Empirical Results

5.1 Effects of the DSZ policy

A simple way to examine if LTV restrictions have material effects on CGR measure is to test whether CGR measures are related to length of LTV regulations. For this, the whole sample is divided into two subsamples, one with duration longer than the median duration (2.03 years) and the other with durations shorter than the median duration. We use the univariate nonparametric Wilcoxon rank sum test to determine if two subsamples are drawn from the same population. If the test cannot reject the null hypothesis of an identical population, it would imply that the LTV restrictions have no effects on CGR measure. Table 3 reports that the null hypothesis is rejected

with a very low p-value (less than 0.1%) suggesting that LTV restrictions are likely to have significant effects on CGR measure and that house prices in districts with longer LTV restrictions tend to co-move less. One obvious shortcoming of the Wilcoxon test in Table 3 is that the null hypothesis is much narrower than what is needed. In particular, the test might capture the effects of some other potential factors on the CGR measure which are correlated with LTV caps, for which we bring in control variables to sort out the net effects of LTV caps.

Table 4 reports on the main estimation results of Equation (3) with control variables. Following housing literature, we consider various control variables related to housing demand such as personal income and demographics; income growth, total population growth, net flow of population and the proportion of population over 65. Since data on the district level personal income is not available, the data on city level personal income is used instead. All the explanatory variables are winsorized ay 1% and 99% ; all the local characteristics variables are standardized . Column (1) reports estimation results without the control variables but with the time fixed effects and the location fixed effects. The estimated coefficient for the DSZ policy is statistically significant at 1%, which implies strong effects of the policy on co-movement of housing prices. With the DSZ policy and the fixed effects, the model explains more than 60% of variations in house price co-movement. As shown later, a substantial portion of the variations are accounted for by the fixed effects, which highlights the importance of fixed effects. Column (2) reports the results with the control variables added to the regression. The local income growth has significant effects at 5%; the house prices of the districts with above the national average of income growth tend to co-move less with national house prices, which is expected. Other control variables such as net flows of population and the proportion of elderly population

have the wrong sign, but insignificant. The population growth has the expected sign, but also insignificant. Even though most of the control variables are not significant, they, as a group, improve the regression fits with adjusted R square increasing from 0.624 to 0.751. This improvement is notable since it measures the additional effects of control variables over the effects captured by time and location fixed effects. From Column (1), 37 percent of variations in house price co-movement are presumably accounted for by factors which are both time and location varying and the control variables can account for about one third of it. Column (3) presents the same results as Column (2) but with standardized DSZ policy durations. The result indicates that the effects of the DSZ policy duration is also economically important; the effect of one standard deviation in the policy duration is more than double the effects of local income shocks. In Column (4) and (5), we include the indicator variable for a district which has ever been designated as speculative zone in addition to the duration of the policy to control for further heterogeneity among districts that the local control variables and time and location fixed effects cannot account for. The coefficient for the DSZ policy duration is still statistically significant and even larger than that in column (1). The estimated coefficient of the indicator variable for DSZ-districts is also statistically significant. The estimated coefficient, 0.787, is a distinctively large number. Assuming that the CGR measure for non-DSZ districts is 0.41, a median value from Table 2, the effect of becoming a DSZ district is to have R^2 increased to 0.60, a substantial increase in housing price co-movement. This indicates that there are markedly different degrees of house price co-movement between the DSZ districts and non-DSZ district. Recall that the DSZ policy is directed toward the districts where house prices are appreciating fast, not directly toward the districts where house prices show high degrees of co-movement. However, out

results indicates that the house prices of the districts subject to price bubbles are more sensitive to national factors and tend to co-move. Those districts impose two types of risks for home buyers and financial institutions; first, those districts are subject to housing price bubbles and bursts which lead to higher default rates for mortgages and capital losses for financial institutions; second, the benefits of regional diversification become considerably weaker. The effects of the first risk would be more direct and of first order, but the effects of the second risk might be potentially large as we have observed during the last housing crisis in the U.S. (Cotter et. al. 2015). It also implies that well-designed macro-prudential policies might be even more important; they not only help contain house price bubbles but also keep the regional diversification effective. There have been many discussions and research efforts to understand the effects of macro prudential policies against asset bubbles, but, as far as we are aware, this paper is the first attempt to understand the macro-prudential policies for enhancing diversification effects.

Column (5) reports the estimation results with the indicator variable for DSZ-districts and the control variables. In contrast to Column (2) and (3), all the control variables are not significant. In particular, the effects of local income shocks are absent. The estimated coefficient is much smaller, has a wrong sign and is not significant. This indicates that the local income shocks in Column (2) indeed might have been more related to characteristics of DSZ-districts. Column (4) and (5) also show that the regression fits as measured by adjusted R squares considerably deteriorated. This is because location fixed effects are not included due multicollinearity with the DSZ-district fixed effect. This suggests that the district fixed effects play a very important role in capturing heterogeneity of house price co-movements, even much larger than the control

variables. It is difficult to tell exactly what those fixed effects capture. However, since all the control variables are demand side variables, it is possible that the remaining heterogeneity captured by the fixed effects might be related to housing supply factors. The most important supply factor that received extensive attentions is housing supply regulations (Glaeser, Gyourko and Saiz 2005, Quigley and Raphael 2005). Recently Saiz (2010) found that geographic structure of land is the most important determinants for housing supply elasticity. It has the direct effects on the supply elasticity through land availability. But it also has indirect effects through increases in land values which create incentives for more regulations. We don't provide any formal analysis using supply side control variables such as relative scarcity of land or local regulations due to lack of available data. However, the effects of both regulatory constraints and geographic constraints should be well-captured by district fixed effects we employ because they are mostly location-variant but time-invariant.

The effects of the DSZ policy in reduction of house price co-movement via credit constraint channel can be understood by examining the effects of the policy on other types of condominium units that were not considered in Table 4. Table 5 presents estimation results of Equation (3) for medium sized units and large units. In Table 4, we focus on the small condominium units in the analysis since owners of those units are likely most affected by changes in lending conditions. If it is the credit constraint channel that makes the policy more effective on the small condominium units, the effects of the policy on larger sized condominium units should be smaller or non-existent since owners and investors of those units are less likely credit-constraint. Table 5 confirms this; the effects of the policy decrease with condominium unit size. In Table 5, the columns under the heading of "Medium" and "Large" provides the estimation results on the

medium sized units and the large units. As conjectured, the effect of DSZ policy on the medium sized units is smaller than the effect on the small sized units, and, in turn, the effect on the medium sized units is smaller than the effect on the large units. All the coefficients on the policy durations are significant. The difference between the policy effects on the small and medium sized units is much larger than that between the effects on medium sized units and the large units. The p-value of the one-tailed test against the null hypothesis of equal effects between the small units and the large units is 9.8%. In the last column under the heading of "Small and Large," we re-estimate the Equation (3) for the small and the large units together with the medium sized units left out. In this regression, we also control for unobserved heterogeneity in house price co-movement between the small size units and the large units by including a fixed effect for the small units, which turns out to be important when testing for different effects of the policy. While the p-value for the one-tailed test is 9.8% without controlling for the heterogeneity, the p-value for the interaction term between the policy duration and the indicator variable for the smaller units is $2.15e^{-37}$, highly significant. In addition, a substantial degree of price co-movement in smaller units provides important implications. Based on the U.S. housing market, Liu, Nowak and Rosenthal (2014) report that prices of different home size segments exhibit almost identical time series patterns during the period of 2001 – 2006 when the housing prices were rising overall but that the prices of smaller housing units fell further than the price of larger housing units during the period of the housing market crash. It highlights a significant risk for smaller housing units compared to larger housing units; prices of the smaller units not only tend to fall more during the bust period but also tend to move together more.

5.2 Robustness Tests

5.2.1 Endogenous DSZ

In this section, we address the issue of potential endogeneity of the DSZ policy. Since much of the decision on designation of a certain district depends on the recent house price development and, in turn, the subsequent house prices will certainly be affected by the DSZ policy imposed, the duration of the DSZ policy in effect, our measure for credit constraints, is much less likely exogenous for the housing price process. However, the regressand in our estimation is not the housing price per se, but the CGR co-movement measure, a relative portion of district level housing price explained by the national factors. Therefore, it is not yet clear if our estimates in Table 4 are biased due to endogeneity of the DSZ policy itself. Nevertheless, we explore this issue here for the following two reasons. First, identification of potentially exogenous factors which are highly correlated with the DSZ policy duration will add to our understanding of a dynamics between the district level housing prices and the policy determination process at the central government. Second, by establishing the same relationship between house price co-movements and the instruments, the remaining concerns for endogeneity, if any, can be addressed, which make our findings and their implications more compelling.

It is known that politician ideology affects mortgage policy decisions during the recent crisis in the U.S.(Mian et al, 2010), implying that political factors are at least partially exogenous. In particular, nation-wide shifts in governing party from progressive party to conservative are orthogonal to the district-level shifts of regulatory changes in LTV caps. More specifically, we note that at the center of DSZ policy decision process is Real Estate Stabilization Committee chaired by the vice-minister of Ministry of Strategy and Finance. The committee consists

mostly of high central government officials (at the level of vice minister) and outside experts. Even though local governments provide some inputs, such as data, for the policy process, but it only plays a supportive and secondary role. This implies that appropriate instruments might include (1) variables that captures factors correlated with the policy decision process of the central government and (2) variables that captures district level factors which indirectly (and perhaps in more subtle ways) affect the decision process at the central government. For this, we use information on the number of quarters during which a congressman and/or the president from a conservative party holds a position from Republic of Korea National Election Commission website (http://www.nec.go.kr).

We consider three instruments: the number of quarters when the president is from the conservative party during the previous 30 quarters; the number of quarters when the congress member of the district is from the conservative party during the previous 30 quarters; an interaction of the those two variables. Table 6 reports the summary statistics on three instrumental variables. The number of years of conservative congress man for each district-quarter is highly skewed to the right; in more than 75% of the observations, the district was represented by conservative congress members throughout the sample period. On the other hand, the periods under the conservative president are shorter and more varied. The maximum period with the conservative president is 5.12 years, which reflects that the conservative president came into power in 2008 around the middle of the sample period (after a 10 year of progressive presidency).

Table 7 reports how effectively these political variables can instrument the duration of DSZ policy. The first column reports the result with one instrument: the number of years with

conservative congress members. The regression includes the local characteristics from Table 4 but without time fixed effects or district fixed effects. The coefficient for conservative parliamentary member is significantly positive, implying that conservative congress member of a district is related to longer DSZ policy on that district, contrary to our hypothesis. Column (2) reports the same regression as in Column (1) but with an additional fixed effect of the DSZ district. The coefficient for the conservative congress member is still positive but insignificant. One potential reason is that DSZ districts are often wealthy and conservative. That is, there might be positive correlations between the length of the DSZ policy and the likelihood of conservative congress members being elected in those districts, which seems to have been captured by the instrument in Column (1). Column (3) reports the result from estimation with all three instrument: conservative congress member, conservative president and their interaction variable. The result in Column (3) confirms what we find in Column (1); the conservative president is a significant factor for imposition of the DSZ policy. However, the result also provide an important condition for the association between conservative presidency and the DSZ policy that the conservative presidency is only effective for the districts which are represented by conservative congress members. With the interaction variable included, either the conservative presidency or the conservative congress members are not significant by themselves. This implies that the decision at the central government is an important factor. The conservative government appears less willing to impose the DSZ policy but conservative congress members by those district seem to play some roles as well. We do not seek to identify how these two political forces interact in the policy decision process, which goes beyond the scope of this paper. However, we would like to confirm that the instrument of the conservative presidency is not a

proxy for unidentified macro variables since this instrument is not district-varying but is subsumed in time fixed effects. In Column (5) we drop the conservative presidency and instead include time fixed effects. The result in Column (5) is almost identical to that in Column (4), which implies that the instrument of the conservative presidency is much less likely proxying other macro variables.

Table 8 reports the results of instrumental variable regressions for Equation (3) using the political instruments. The results strongly confirm the result in Table 4 that the credit constraints imposed by the DSZ policy significantly reduces co-movement of housing prices. In contrast to Table 4, most of local characteristics are significant. The coefficients for instrumented DSZ policy durations have the expected sign and statistically significant, which indicates the instruments selected provide sufficient explanatory power for the DSZ policy variable. In contrary to Table (4), most of control variables are significant with the instruments, which indicates that the portion of variation sin the house price co-movement not explained by instruments are closely correlated with the control variables in the regression.

5.2.2 LTV regulations vs. DTI regulations

The DSZ policy is a comprehensive package of anti-speculation regulations, including LTV regulations, DTI regulations and capital gains taxations. Therefore, the effects of the DSZ policy we identified in Table 4 and 5 might have been resulted from any combination of the policy instruments.

In this section, we focus on the relative effects of LTV restrictions and DTI restrictions for the following reasons. First, after the recent financial crisis, international organizations such as IMF and BIS have recommended adoption of macroprudential policies, and the key policy instruments for real estate market stabilization are DTI and LTV restrictions[3]. However, due to relatively small number of countries with experiences on LTV and DTI regulations, the actual effects of those policies are not well documented[4]. The experiences in Korean housing markets under the DSZ policy might be able to provide some information potentially useful for understanding the relative effects of two policy instruments. Second, the effects of LTV restrictions and DTI restrictions, at least in theory, work in substantially different ways. DTI restrictions largely concerns borrower's liquidity risks, which is less related to borrower's credit constraints. On the other hand, LTV restrictions directly affect credit-constrained borrowers. For example, In Stein (1995), the only way to overcome LTV restrictions for credit-constrained borrowers is housing equity buildup through house price appreciations. Therefore, LTV restrictions become binding only for borrowers with limited options in financing, which is why we posit credit constraints as the main channel of the DSZ policy, especially LTV regulations.

To separate effects from DTI restrictions from those from LTV restrictions, we note that only the districts in Seoul and Incheon (Seoul-Incheon metropolitan area) were subject to DTI restrictions and LTV restrictions while districts in other cities were imposed with LTV restrictions only. Table 9 presents estimation results based on separate data on Seoul-Incheon areas vs. the remaining areas. If DTI restrictions have significant effects, then the co-movement of Seoul-

[3] Committee on the Global Financial System (2012) lists DTI and LTV as the main policy instruments potentially effective for controlling credit cycles and for securing resilience for banking sectors by limiting probability of defaults (PD) and loss given defaults (LGD).

[4] CGFS report (2012) also acknowledges that "the impact on the credit cycle is less well documented, as relatively few countries have instituted LTV and DTI restrictions in a macroprudential fashion."

Incheon districts would be more affected by the DSZ policy compared to that of districts in other cities. However, Table 9 reports that the estimated coefficient of the DSZ policy on Seoul-Incheon district is smaller than that of other districts, inconsistent with the effective DTI restrictions. Third column reports the result of the formal test on the interaction variable between the DSZ policy duration and the indicator variable for districts in Seoul-Incheon. The estimated coefficient on the interaction variable is not significant, which implies that the DTI restrictions do not have additional effects on the house price co-movement of Seoul-Incheon districts.

5.2.3. Credit Constraints Effect vs. Tax Effect

When a district is placed under DSZ, potential home buyers and investors face not only more stringent lending conditions, but also potentially higher capital gains taxations. Since stricter lending conditions and higher capital gains taxations can presumably have similar effects on demand for housing, the estimated effects of duration of the DSZ policy in Table 4 might have been at least partially due to the effects of higher taxations. Yet, it is not clear whether capital gains taxation would unequivocally lead to lower demand for the assets. Investors who are bidding to purchase houses would ask for lower prices due to higher taxes in the future (capitalization effects) but at the same time, existing homeowners would ask for higher prices to offset higher taxes (lock in effects). Examining the Taxpayer Relief Act of 1997, Dai et. al. (2008) report that, during the period leading to actual implementation of the Act, the capitalization effect was dominant, but, once the Act became effective, the lock-in effect dominated the capitalization effect. Therefore, the net effects of the capital gains taxation might

be in the same direction as the lending constraints or, quite possibly, they can be of the opposite direction. If the capital taxation effects dominated, the estimated effects of the policy in Table 4 would be the combined effects of capital gains taxes and lending constraints, casting doubts on our interpretation of the results in Table 4 in terms of credit constraints. If, on the other hand, the lock-in effects dominated, the actual effects of the credit constraints would be larger than the estimated effects in Table 4, providing even stronger support for out hypothesis.

As a part of the DSZ policy, higher capital gains taxations were imposed in three different ways. First, the government implemented a temporary increase in capital gains tax on selective basis, that is, not all the districts designated as speculative zone faced the tax rate increases. Indeed, there were only a few districts which higher taxations were imposed on, which indicates this type of capital gains tax increases would have relative smaller effects. Second, the capital gains taxes were often raised only for homeowners with multiple housing units because policy makers believe that housing transactions by those with multiple housing units are more likely motivated by speculations. However, the data indicates that the portion of homeowners with multiple housing units does not seem to be large enough to have meaningful effects on market outcomes. Table 10 provides the number of homeowners and the number of houses owned by homeowners for each metropolitan areas for the period of 2012 – 14. For each metropolitan area-year pair, we compute the potential portion of homeowners with multiple houses by assuming all the homeowners with multiple houses have 2 houses only and report them in column (C) for each year. The average ratio over seven metropolitan areas for the three year period is 6.8%, but there is a substantial variation across metropolitan areas and over time. There are metropolitan areas with high proportion of homeowners with multiple houses (such as Inchon in 2014 with

15.49%), but for most of the metropolitan areas, the potential proportion is in the range of 6% to 7%, which appears small for the tax effects to have significant effects. In addition, the potential proportions we computed are the upper bounds for the proportions of homeowners with multiple housing units. Depending on the number of homeowners with more than two housing units, the actual numbers might be substantially lower than the potential proportions reported in Table 10. Third, most importantly, the most effective tax policy was to impose the capital gains taxes at the existing rates on the actual market values of the houses. In Korea, capital gains taxes had been imposed on tax assessment values, not on actual transaction values. There were large discrepancies between tax assessment values and actual market values in Korea housing markets; the discrepancy between two values could be close to 50% of the market value. Therefore, imposing capital gains at the transaction values could potentially have large effects. The discrepancies were not uniform over regions or over types of housing units. Yang (2013) reports that, for multifamily housing units, there is a wide regional variation and that the discrepancy in Seoul is larger than any other areas in Korea, which is reproduced in Table 11. Table 11 reports that the discrepancy between market values and tax assessment values range 21.0% (Ulsan) to 31.6% (Seoul). The regional variations in discrepancy allow us to exploit the relative effects of capital gains taxes; if the discrepancy is large, then the effect on the demand for housing would be large, and so would be the size of coefficient on the policy duration. Therefore, the estimated coefficient for Seoul should be larger (in absolute value) than the coefficients for other metropolitan areas if the capital gains tax effects are dominant. On the other hand, the effects of financial constraints dominates, the direction of inequality would be the opposite; since home buyers and investors in Seoul are less financially constrained, which is plausible considering

differences in housing prices and income between Seoul and other areas, the estimated effects of the policy duration for districts in Seoul would be smaller than those of other metropolitan areas. One potential problem in testing this is the unobserved heterogeneity between Seoul and non-Seoul areas that can contribute to house price co-movement. The best strategy would be to choose a city that are as close to Seoul as possible in observable aspects but still large differences in market value-tax assessment value discrepancies. We choose Pusan as semi-control city against Seoul. As Seoul has traditionally been the largest city in Korea, Pusan has also been the second largest city in Korea over history. Both cities have been economic and cultural centers of the surrounding areas with concentrated government functions. Seoul and Pusan are more similar than other possible combinations of cities. Nevertheless Pusan has the second lowest discrepancy between market value and tax assessment value next to Ulsan. Table 12 reports on the tests whether the effects of DSZ policy is larger for districts in Seoul than those in Pusan. Again, we include time fixed effects and location fixed effects as before. The results show that the estimated coefficient is smaller (in absolute value) for Seoul and that the difference between the coefficients is economically larger and statistically significant. The results strongly supports our interpretation of Table 4 that the main channel through which the DSZ policies affect the co-movement of house prices is the credit constraints via LTV restrictions, not the capital gains taxations.

5.2.4. Unobserved time varying factors

Another concern in Table 4 is that we might not have enough control variables to account for variations in house price co-movement that are not attributed by the DSZ policy. To control this, we include time fixed effects and location fixed effects in Table 4 and in most of empirical

exercises that follow. Judging from goodness of fit by adjusted R^2, the fixed effects considerably improve the general performance of Equation (3), which, we believe, helps accurately estimate the effects of the DSZ policy. In particular, some well-known factors, such as geographic conditions (Saiz, 2010) and macroeconomic development, are sufficiently accounted for by those fixed effects. However, it is still plausible that some unobserved time varying factors, such as changing local financial conditions, might affect house price co-movement, which potentially bias our results in Table 4. To partially address this issue, we follow Loutskina and Strahan (2009) and estimate the effects of the DSZ policy on the relative house price co-movement by differencing out the unobservable time varying effects.

More specifically, we have Equation (3) for large units with unobservable time varying factors (UTV) explicitly accounted for,

$$\ln\left(\frac{R^2}{1-R^2}\right)_{it}^{Large} = \beta_0^{Large} + \beta_1^{Large} \times \ln(1 + Duration\ of\ LTV\ restriction)_{it}$$

$$+X_{it}^{Large} + Z_{it}^{Large} + \eta_t^{Large} + \tau_i^{Large} + \epsilon_{it}^{Large} \quad (3'),$$

and for small units,

$$\ln\left(\frac{R^2}{1-R^2}\right)_{it}^{small} = \beta_0^{small} + \beta_1^{small} \times \ln(1 + Duration\ of\ LTV\ restriction)_{it}$$

$$+X_{it}^{small} + Z_{it}^{small} + \eta_t^{small} + \tau_i^{snall} + \epsilon_{it}^{small} \quad (3''),$$

where X_{it} are observable time varying factors and Z_{it} unobservable time varying factors.

Assuming $Z_{it}^{Large} = Z_{it}^{small}$ and taking the difference between (3') and (3''),

$$\ln\left(\frac{R^2}{1-R^2}\right)_{it}^{small} - \ln\left(\frac{R^2}{1-R^2}\right)_{it}^{Large} = \beta_0^{small} - \beta_0^{Large}$$

$$+\left(\beta_1^{small} - \beta_1^{Large}\right) \times \ln(1 + Duration\ of\ LTV\ restriction)_{it}$$

$$+\left(X_{it}^{small} - X_{it}^{Large}\right) + \left(\eta_t^{small} - \eta_t^{Large}\right) + \left(\tau_i^{snall} - \tau_i^{Large}\right)$$

$$+\left(\epsilon_{it}^{small} - \epsilon_{it}^{Large}\right) \quad (3''').$$

In Equation (3'''), the individual effects of the DSZ policy can no longer be estimated, however, the null hypothesis of no effects of the DSZ policy on the house price co-movement can be still tested without the concerns for potential bias due to unobserved time varying factors.

Table 13 reports the results of Equation (3'''), which are consistent with Table 4 and Table 5. The estimated effects of the DSZ policy are statistically and economically significant, which confirms that the estimated effects of the policy reported Table 4 are not seriously biased due to important time varying factor that are not accounted for. The estimated effects of the policy based on relative housing price co-movement are somewhat larger compared to the estimated effects reported in Table 5. Otherwise, the results are similar.

5.2.4 Serial Correlation in the CGR measure.

One potential source of concerns on the results in Table 4 and Table 5 is the potential bias in standard errors for the coefficient since that the measure for house price co-movement is constructed based on 30 quarter overlapping observations and thus highly persistent. In principle, since we cluster standard errors by district and quarter based on Peterson (2009), this should not interfere with our null hypothesis testing as long as population errors are not

correlated beyond districts and 30 quarter periods assuming population error terms are not correlated. However, considering potential persistence in housing price co-movement, it is difficult to assume away the serially correlated regression residuals which could lead to wrong inferences on hypothesis tests. To further investigate for this concern, we estimate equation (5) in cross section quarter by quarter from 2010 Q1 through 2012 Q3 and report the results in Table 14. Due to the cross sectional nature of estimation, standard errors are clustered by district only. The estimated coefficients are comparable to those in panel regressions reported in Table 4. Except for the first quarter of 2010, the estimated effects of duration of the DSZ policy are statistically significant at 5% or lower with the expected sign, which indicates that the statistical significance of the estimated effects of the DSZ policy is not biased due to substantial serial correlations in the house price co-movement measure that were not appropriately controlled for. The estimated coefficients for the districts under DSZ policy are also significant throughout the sample period. The estimated coefficients for the duration of DSZ policy and those for the indicator for districts under do not appear to vary widely over the sample period, indicating that estimates in Table 4 are not based on a few extreme observations. On the other hand, district level characteristics such as income and demographics are rarely significant, probably even more than those from panel regressions in Table 4. The difference in performance of those characteristics is somewhat surprising since the district level characteristics are not much time-varying for each district, but they still exhibit substantial heterogeneity in their explanatory power in cross section.

6. Conclusion

Housing price synchronicity is one of the most important factors for geographical diversification of mortgage lenders and investors, which came to heightened attentions of financial markets and governments at the time of the last financial crisis (Cotter, Gabriel and Roll (2014)). We analyze the effects of LTV restrictions on synchronicity of local housing prices by using the Korean government's DSZ (Designation of Speculative Zone) policy experiences in 2002-2013 as quasi experiment. While endogeneity has been considerable concerns for extant studies on the effects of LTV (Lamont and Stein (1999), Cotter, Gabriel and Roll (2014)), we are able to sidestep this problem by taking advantage of LTV caps in the DSZ policy which the government implements in a time- and location-varying manner.

Our results show that the LTV restrictions substantially reduce the local housing price synchronicity; the longer the DSZ policy in effect, the less the local house prices tend to move together. It implies that the LTV regulations (or macroprudentail policies of which LTV caps are a part of) can have secondary, but important effects that haven't been paid much attention to; they not only help stave off formation of housing price bubbles and reduce mortgage defaults but also enhance the benefits of geographical diversifications for lenders and investors. Our analysis point out that the local housing bubble and the housing price synchronicity are not separate, but might be closely related each other, and that LTV caps can enhance potential effects of geographic diversification by reducing the likelihood of rapid housing price appreciation. In addition, we identify effective instruments for the DSZ policy implementation. In contrast to U.S. or European experiences (Mian and Sufi, 2010), the DSZ policy is driven by the central government with little input from local government, which implies that the ideological inclination of the political party in power potentially might matter. Our analysis shows that the

political disposition of the central government is important for policy implementation decision only when it lines up with that of the congress member who represents the district. Lastly, we assess the effectiveness of LTV restrictions against DTI restrictions and capital gains taxations. The DSZ policy itself is a package of all three policy tools, but each tool was applied differently from each other by time and by location. We find that the LTV restrictions have the most significant effects, which appears consistent with the characteristics of housing units in our dataset.

Our paper has a few important implications. First, our study offers the first empirical evidences on housing price co-movement outside the U.S. Since Korean housing market is quite different from U.S. housing market, our analysis shows how effective the co-movement measure based on R^2 can be utilized in non-U.S. housing markets in academic research as well as for policy analysis. Second, we find that the local housing markets with tendency for price bubbles can disproportionately contribute to housing price synchronicity and potentially to systemic risk. This implies that the LTV restrictions selectively applied to local housing markets vulnerable to asset bubbles might work more effectively than LTV restrictions universally applied to the national housing market.

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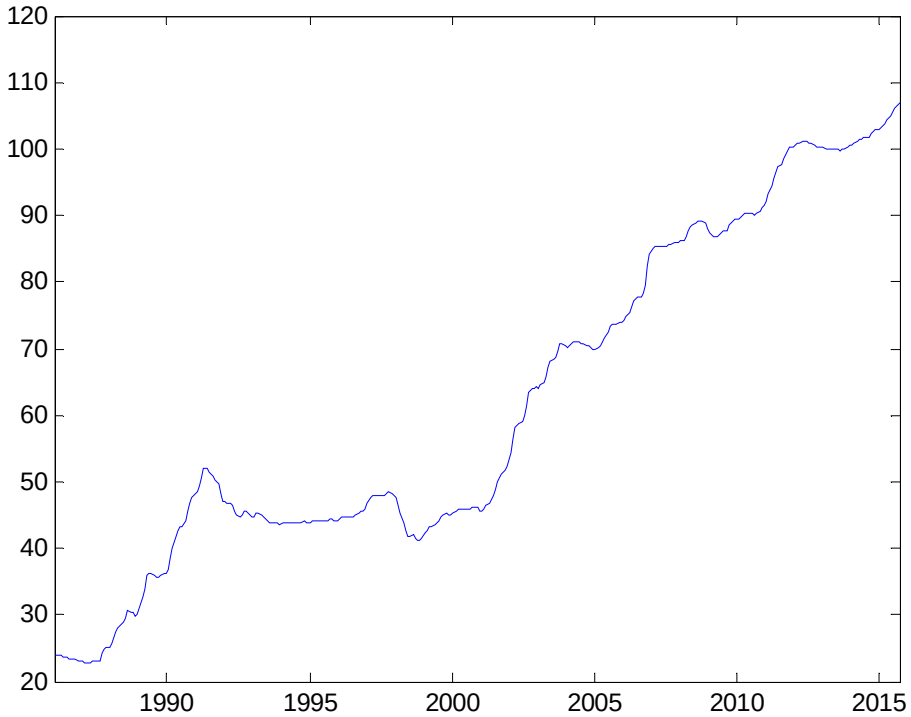
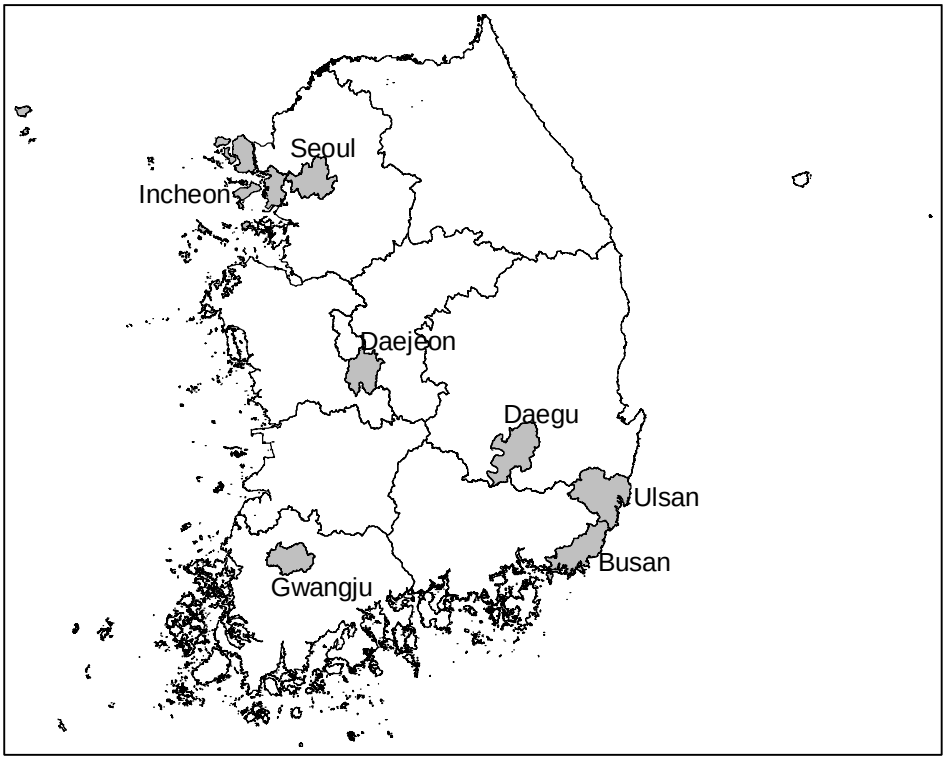


Figure 1: Korean House Price: 1986 – 2015



Figre 2. Seven largest metropolitan areas of Korea

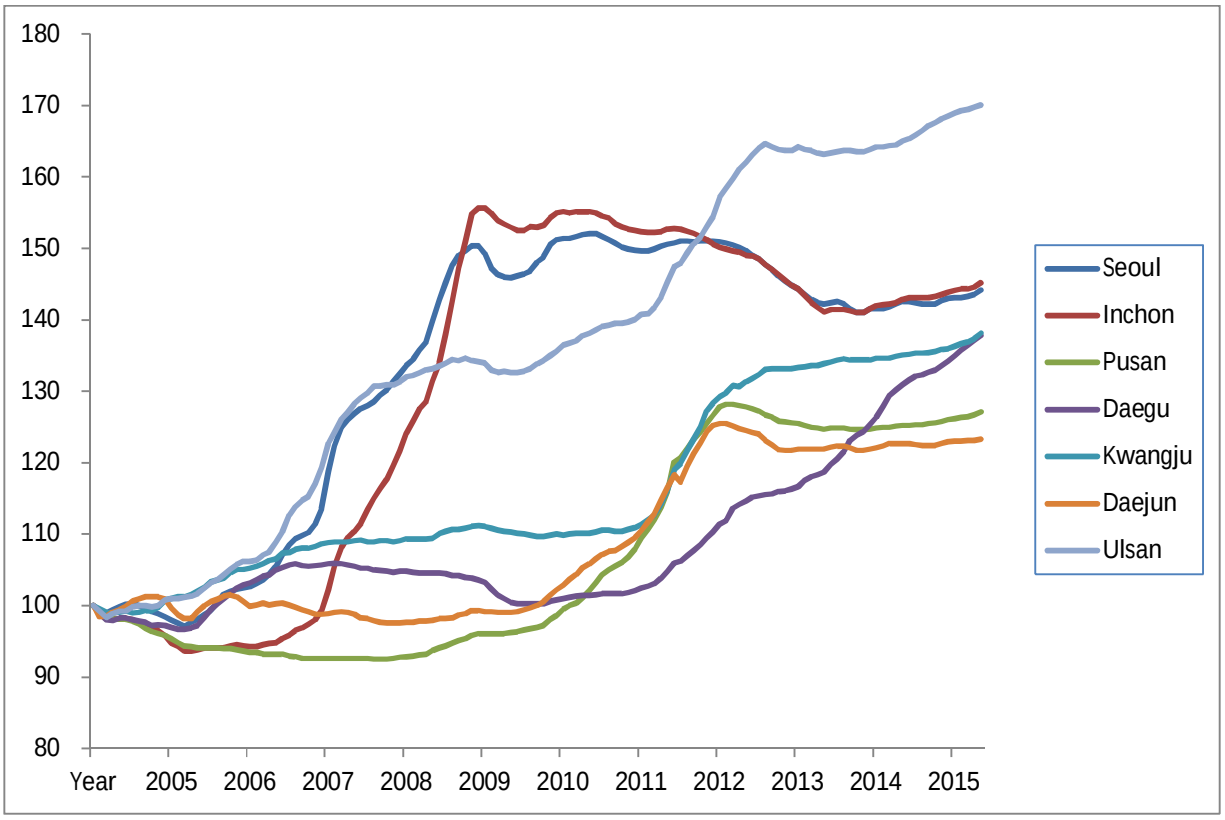


Figure 3. Housing Price by metropolitan areas.

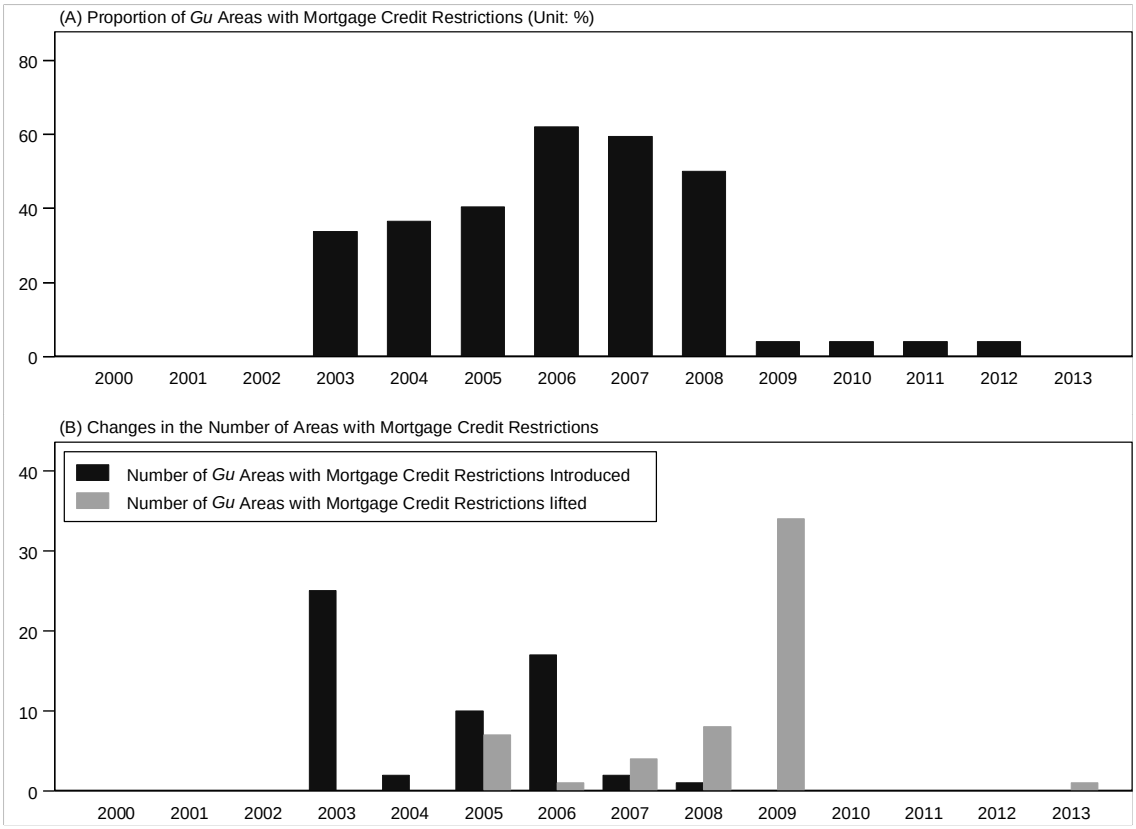


Figure 4. Designation of Speculative Zone (DSZ) Policy in Korea, 2000 - 2013

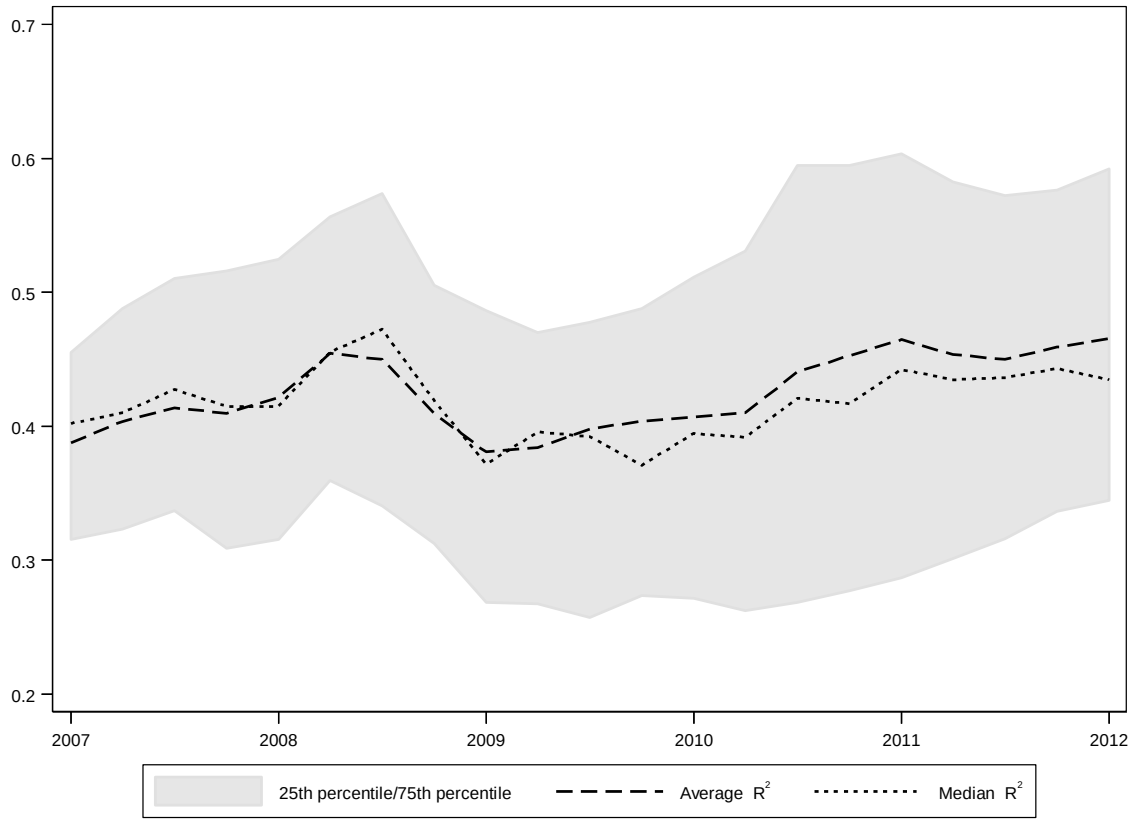


Figure 5. *R^2*'s of Korean House Price Returns

Table 1

Summary Descriptions of Metropolitan Areas

This table reports average characteristics of the seven largest metropolitan areas in Korea. The average housing price is based on data from Real Estate 114, and population and income data are from Korea Statistics Agency.

Metro	# of district	Average housing price	Average district population	personal income
Seoul	25	$318,378	385,259	$14,330
Pusan	15	$108,185	226,213	$11,867
Inchon	9	$121,723	292,448	$11,310
Daegu	8	$94,234	303,972	$11,611
Daejun	5	$97,249	298,032	$12,327
Kwangju	5	$73,202	293,229	$11,685
Ulsan	5	$101,249	214,335	$15,064

Table 2
Summary Statistics

A. House Price Synchronicity

R^2 is calculated following Cotter et al.(2015) for each moving window of 30 quarter observations (see Appendix A). ψ is a logistic transformation of R^2, i.e., $\ln\left(R^2/(1-R^2)\right)$.

	Obs.	Mean	Std. Dev.	Min.	p25	p50	p75	Max.
R^2	1,365	0.42	0.17	0.071	0.30	0.41	0.53	0.90
ψ	1,365	-0.34	0.75	-2.56	-0.85	-0.35	0.12	2.16

B. Local Housing Market Characteristics

Each observation is a 30-quarter moving averages. All the growth rates are quarterly log growth rates for each local housing markets except *Income Growth*. *Income Growth* is the quarterly log growth rate of Gross Regional Domestic Product(GRDP) for a city where local "gu" area belongs. All control variables except the duration of LTV restriction are winsorized at 1st and 99th percentiles.

	Obs.	Mean	Std. Dev.	Min.	p25	p50	p75	Max.
ln(1+Duration of LTV Restriction (Unit :Years))	715	2.21	1.90	0.00	0.77	2.03	3.22	7.50
Income Growth	715	0.05	0.01	-0.00	0.04	0.05	0.06	0.09
Population Growth	715	0.00	0.01	-0.03	-0.01	-0.00	0.01	0.06
Net Flow of Population	715	-0.01	0.01	-0.03	-0.01	-0.01	-0.00	0.04
Proportion of Population over Age 65	715	0.09	0.02	0.04	0.07	0.08	0.10	0.15

Table 3. Univariate Test

Long (*Short)* is a dummy variable which equals one if *Duration of Credit Restriction* is greater (equal to or smaller) than median and zero otherwise.

Duration of (Mortgage) Credit Restriction	R^2							
	Observations	Mean	Std. Dev.	Min.	p25	p50	p75	Max.
Long (>median)	674	0.40	0.16	0.07	0.28	0.40	0.51	0.80
Short	691	0.44	0.17	0.10	0.31	0.42	0.56	0.90

Wilcoxon Rank-sum Test for difference ($H_a : R^2_{Long} \neq R^2_{Short}$): z=3.777 (p-value < 0.001)

Table 4

This table report the results from estimating the following model:

$$\psi_{it} = \beta_0 + \beta_1 \times \ln\left(1 + Duration_{\mathrm{it}}\right) + Local\ Market\ Characteristics_{ij} + \eta_i + \tau_t + \epsilon_{\mathrm{it}}.$$

LTV Restriction Area Dummy equals one if the local housing market had experienced any government restrictions on mortgage lending based on LTV ratio for the sample periods.The local market characteristics are adjusted for mean and further standardized by dividing them by standard errors for each period. The standard errors are in the parentheses. The standard errors in columns (2), (3) and (4) are adjusted for location- and quarter-level clustering. The explanatory variables are winsorized at 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
ln(1+Duration of Credit Restriction)	-0.411***	-0.335***		-0.484***	-0.166***
	(0.150)	(0.097)		(0.170)	(0.043)
Credit Restriction Area Dummy				0.787***	0.586***
				(0.268)	(0.217)
Z(Duration of Credit Restriction)			-0.637***		
			(0.184)		
(Local Market Characteristics)					
Z(Income Growth)		-0.285**	-0.285**		0.029
		(0.141)	(0.141)		(0.180)
Z(Population Growth)		-0.831	-0.831		-0.089
		(0.530)	(0.530)		(0.258)
Z(Net flow of Population)		0.268	0.268		0.197
		(0.434)	(0.434)		(0.255)
Z(Proportion of Population Over Age 65)		0.195	0.195		-0.005
		(0.455)	(0.455)		(0.105)
Time-fixed effects	Yes	Yes	Yes	Yes	Yes
Location-fixed effects	Yes	Yes	Yes	–	–
Time and location clustering	(robust)	Yes	Yes	Yes	Yes
$Adj.R^2$	0.624	0.751	0.751	0.081	0.110
Observations	1,365	715	715	1,365	715

Table 5

Size of House and the Impact of Credit Constraint on House Price Comovement

The local market characteristics are adjusted for mean and further standardized by dividing them by standard errors for each period. The standard errors are in the parentheses. The standard errors in columns (2), (3) and (4) are adjusted for area- and quarter-level clustering. The explanatory variables are winsorized at 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively.

	Apartment Size				
	All sizes	Small	Medium	Large	Small and Large
ln(1+Duration of LTV restriction)	-0.228***	-0.335***	-0.169***	-0.181**	-0.225***
	(0.000)	(0.001)	(0.008)	(0.011)	(0.000)
ln(1+Duration of LTV restriction)× Small Apt. dummy					-0.066***
					(0.005)
Small Apt dummy					0.293***
					(0.001)
(Local Market Characteristics)					
Z(Income Growth)	-0.156*	-0.285**	-0.318*	0.143	-0.074
	(0.094)	(0.141)	(0.055)	(0.320)	(0.372)
Z(Population Growth)	-0.459	-0.831	-0.857*	0.318	-0.260
	(0.131)	(0.530)	(0.087)	(0.337)	(0.337)
Z(Net flow of Population)	0.104	0.268	0.357	-0.315	-0.022
	(0.663)	(0.434)	(0.372)	(0.250)	(0.910)
Z(Proportion of Population Over Age 65)	0.560*	0.195	0.568	0.917*	0.556*
	(0.092)	(0.455)	(0.285)	(0.065)	(0.066)
		p-vaue of $H_a: \beta^{SmallAPT}_{Duration} < \beta^{LargeAPT}_{Duration}$: 0.098			
Time-fixed effects	Yes	Yes	Yes	Yes	Yes
Location-fixed effects	Yes	Yes	Yes	Yes	Yes
Clustering errors	Yes	Yes	Yes	Yes	Yes
$Adj.R^2$	0.746	0.751	0.773	0.667	0.423
Observations	2,145	715	715	715	1,430

Table 6
Summary statistics of Instrument variables

	Obs.	Mean	Std. Dev.	Min	p25	p50	p75	Max
Conservative party congressman	715	6.43	1.14	3.00	7.00	7.00	7.00	7.00
Conservative party president	715	3.95	0.79	2.70	3.20	3.95	4.70	5.21
ln(1+Conservative party congressman)	715	1.99	0.19	1.39	2.08	2.08	2.08	2.08
ln(1+Conservative party president)	715	1.59	0.16	1.31	1.44	1.60	1.74	1.83

Table 7

Political factors of LTV regulation

This table reports the results from estimating the following regressions:

$$z\left(\ln\left(1+Duration_{ij}\right)\right)=\beta_0+Instrument\,Variables+Local\ Market\ Characteristics_{ij}+\phi_i+\kappa_t+\upsilon_{it}.$$

All the explanatory variables are converted to standardized z-scores. The standard errors are in the parentheses. The standard errors are adjusted for area- and quarter-level clustering. The explanatory variables are winsorized at 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively.

	Dep. Var.: ln(1+Duration)				
	(1)	(2)	(3)	(4)	(5)
(Instrument Variables)					
ln(1+Conservative party congressman(unit: years))	0.955***	0.014		0.730	0.726
	(0.079)	(0.012)		(0.483)	(0.494)
ln(1+Conservative party congressman) ×ln(1+Conservative party president)				-0.436***	-0.437**
				(0.168)	(0.178)
ln(Conservative party president(unit: years)			-0.327***	0.537	
			(0.046)	(0.410)	
(Local Market Characteristics)					
Credit restriction area-fixed effect		1.227***	1.230***	1.227***	1.228***
		(0.023)	(0.023)	(0.072)	(0.072)
Z(Income growth)	0.153***	-0.062***	-0.062***	-0.061	-0.061
	(0.011)	(0.016)	(0.016)	(0.082)	(0.082)
Z(Population growth)	-0.677***	-0.153***	-0.151***	-0.149	-0.149
	(0.047)	(0.025)	(0.025)	(0.201)	(0.202)
Z(Net flow of population)	0.542***	0.176***	0.174***	0.171	0.173
	(0.047)	(0.018)	(0.019)	(0.180)	(0.180)
Z(Proportion of population over age 65)	-0.130***	-0.011	-0.011	-0.012	-0.012
	(0.010)	(0.010)	(0.010)	(0.071)	(0.071)
Time-fixed effect	-	-	-	-	Yes
Clustering	Period	Period	Period	Period, location(Gu)	Period, Location(Gu)
Adj. R^2	0.162	0.636	0.643	0.639	0.640
Observations	715	715	715	715	715

Table 8

Endogeneity of LTV restriction

This table reports the results from estimating the following simultaneous model using 2SLS regressions:

$$\psi_{it} = \beta_0 + \beta_1 \times z\left(\ln\left(1+Duration_{it}\right)\right) + +Local\ Market\ Characteristics_{ij} + \eta_i + \tau_t + \epsilon_{it},$$

$$z\left(\ln\left(1+Duration_{ij}\right)\right) = \beta_0 + Instrument\ Variables + Local\ Market\ Characteristics_{ij} + \phi_i + \kappa_t + \upsilon_{it}.$$

All the explanatory variables are converted to standardized z-scores. The standard errors are in the parentheses. The standard errors are adjusted for area- and quarter-level clustering. The explanatory variables are winsorized at 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively.

	Dep. Var.: ψ_{it}		
	(1)	(2)	(3)
ln(1+Duration of LTV restriction)	-1.812***	-1.446***	-1.171**
	(0.357)	(0.266)	(0.511)
(Instrument Variables)			
ln(1+Conservative party congressman)		Yes	Yes
ln(1+Conservative party congressman) × ln(1+Conservative party president)		Yes	Yes
ln(1+Conservative party president)	Yes	Yes	Yes
(Local Market Characteristics)			
Mortgage credit restriction area-fixed effect	2.127***	1.738***	
	(0.437)	(0.338)	
Z(Income growth)	-0.066	-0.019	-0.144***
	(0.080)	(0.055)	(0.055)
Z(Population growth)	0.485***	0.364***	0.037
	(0.066)	(0.074)	(0.188)
Z(Net flow of population)	-0.387***	-0.243***	-0.408**
	(0.115)	(0.081)	(0.195)
Z(Proportion of population over age 65)	-0.087***	-0.053**	0.215
	(0.033)	(0.021)	(0.139)
Location("Gu")-fixed effect	No	No	Yes
Period-fixed effect	Yes	Yes	Yes
Clustering	Period	Period	Period
$Adj.R^2$	N/A	N/A	0.742
Observations	715	715	715

Table 9. Seoul-Incheon versus Other Metropolitan Cities

	Seoul-Incheon (DTI and LTV reg.)	Other cities (LTV reg. only)	Seoul-Incheon vs. others
ln(1+Duration of Credit Restriction)	-0.210***	-0.528**	-0.535**
	(0.061)	(0.228)	(0.229)
(1+Duration of Credit Restriction)×I(Seoul)			0.291
			(0.227)
z(Income Growth)	-0.189***	-0.113	-0.137
	(0.062)	(0.262)	(0.146)
z(Population Growth)	-0.447	0.102	-0.255
	(0.303)	(0.562)	(0.371)
z(Net flow of Population)	0.296	0.880	0.430
	(0.270)	(0.629)	(0.313)
z(Proportion of Polutaion Over Age 65)	0.065	0.779	-0.156
	(0.767)	(1.901)	(1.273)
Time-fixed effects	Yes	Yes	Yes
Location("Gu")-fixed effects	Yes	Yes	Yes
p-value(under $H_0 : \beta_{seoul} = \beta_{others}$)		0.178	
Adj. R^2	0.96	0.53	0.78
Observations	352	363	715

Table 10. Potential Proportion of Homeowners with Multiple Housing Units

A. The number of houses owned by individuals
B. The number of individuals who own houses
C. Potential Number of Homeowners with Multiple Housing Units $\left(= \frac{A-B}{B}\right)$

	2012			2013			2014		
	(A) The number of housing units	(B) The number of homeowners	(C) Homeowners with multiple housing units	(A) The number of housing units	(B) The number of homeowners	(C) Homeowners with multiple housing units	(A) The number of housing units	(B) The number of homeowners	(C) Homeowners with multiple housing units
Nation	12,963	12,033	7.73%	13,431	12,399	8.32%	13,672	12,650	8.08%
Seoul	2,457	2,287	7.43%	2,500	2,307	8.37%	2,419	2,343	3.24%
Pusan	948	874	8.47%	1,002	910	10.11%	1,017	923	10.18%
Daegu	623	594	4.88%	640	612	4.58%	650	623	4.33%
Inchon	732	678	7.96%	755	695	8.63%	820	710	15.49%
Kwangju	368	349	5.44%	376	355	5.92%	402	374	7.49%
Daejun	384	359	6.96%	392	366	7.10%	403	380	6.05%
Ulsan	305	291	4.81%	317	302	4.97%	311	311	0.00%

Table 11.

This table reports the discrepancy between market values and tax assessment values for each metropolitan areas reported in Yang (2013). Discrepancy is calculated as

$$Discrepancy = \frac{Market\ Value - Tax\ Assessment\ Value}{Market\ Value}$$

Metropolitan Area	Discrepancy
Seoul	31.6%
Pusan	23.7%
Daegu	25.2%
Inchon	28.2%
Kwangju	27.6%
Daejun	24.6%
Ulsan	21.0%
National	26.0%

Table 12. Seoul versus Pusan

	Seoul (DTI and LTV reg.)	Pusan (LTV reg. only)	Seoul vs. Pusan
ln(1+Duration of Credit Restriction)	-0.222***	-0.667***	-1.352***
	(0.059)	(0.186)	(0.098)
(1+Duration of Credit Restriction)×I(Seoul)			1.190***
			(0.095)
z(Income Growth)	-0.256	0.514	-0.372
	(0.160)	(0.714)	(0.294)
z(Population Growth)	-0.173	0.873*	-0.099
	(0.313)	(0.438)	(0.414)
z(Net flow of Population)	0.414	-0.647	0.513
	(0.252)	(0.856)	(0.331)
z(Proportion of Polutaion Over Age 65)	1.184	-6.711***	-0.371
	(0.760)	(0.818)	(1.067)
Time-fixed effects	Yes	Yes	Yes
Location("Gu")-fixed effects	Yes	Yes	Yes
p-value(under $H_0 : \beta_{seoul} = \beta_{others}$)		0.022	
Adj. R^2	0.96	0.76	0.82
Observations	264	154	418

Table 13. Unobservable Factors and the Impact of Credit Regulation on House Price Synchronicity

$(\psi_{small\ APT.} - \psi_{large\ APT.})_{it} = (\gamma_{small\ APT} - \gamma_{large\ APT}) \ln(1 + Duration\ of\ LTV\ restriction)_{it} + Period\ fixed\ effects_t + \epsilon_{it}$,
where i indicates "Gu" and t indicates period. All the explanatory variables are converted to standardized z-scores. The standard errors are in the parentheses. The standard errors are adjusted for area- and quarter-level clustering. The explanatory variables are winsorized at 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively

	(1)	(2)	(3)	(4)
Duration of Credit Restrictions	-0.583***	-0.666***		
	(0.195)	(0.177)		
z(ln(1+Duration of Credit Restrictions)			-0.364***	
			(0.073)	
Credit Restriction Area Fixed Effect	1.169***	1.130***	0.859***	0.422**
	(0.317)	(0.297)	(0.227)	(0.183)
z(Income Growth)		-0.039	-0.072	0.045
		(0.165)	(0.166)	(0.160)
z(Population Growth)		0.097	0.048	-0.127
		(0.280)	(0.269)	(0.293)
z(Net flow of Population)		-0.020	0.003	0.245
		(0.285)	(0.270)	(0.308)
z(Proportion of Polutaion Over Age 65)		0.030	0.021	0.091
		(0.086)	(0.085)	(0.085)
Adj. R^2	0.102	0.115	0.141	0.041
Observations	1,365	715	715	715

Table 14

Check for Robustness: Subsamples

Robust standard errors are used. 1% and 99%. *, ** and *** indicate significance at 10%, 5% and 1% levels, respectively.

Period	*ln(1+Duration of LTV Restriction)*	*LTV Restriction Area Dummy*	*Z(Income Growth)*	*Z(Pop. Growth)*	*Z(Net Flow of Pop.)*	*Z(Age 65+)*	R^2
2010 1st Qtr.	-0.116*	0.828***	-0.210	0.711**	-0.559	0.134	0.056
2010 2nd Qtr.	-0.133**	0.848***	-0.152	0.432	-0.264	0.142	0.046
2010 3rd Qtr.	-0.149**	0.869**	-0.195	0.345	-0.156	0.180	0.038
2010 4th Qtr.	-0.194***	1.135***	-0.539	0.517	0.013	0.098	0.131
2011 1st Qtr.	-0.231***	0.846**	-0.302	0.803	-0.485	-0.046	0.135
2011 2nd Qtr.	-0.225***	0.642**	-0.352*	0.622	-0.280	-0.078	0.154
2011 3rd Qtr.	-0.202***	0.660**	-0.347*	0.659	-0.340	-0.083	0.148
2011 4th Qtr.	-0.173***	0.546*	-0.276	0.607	-0.288	-0.148	0.127
2012 1st Qtr.	-0.143***	0.523**	-0.323*	0.605**	-0.232	-0.121	0.102
2012 2nd Qtr.	-0.126**	0.581**	-0.446***	0.240	0.181	-0.004	0.085
2012 3rd Qtr.	-0.141***	0.603***	-0.343**	0.135	0.175	0.005	0.062

Appendix A.

We calculate R^2 following Gabriel et al.(2014).

$$prc\_gro = \beta_0 + \beta_1 PRIVMBS + \beta_1 lncd91d + \beta_2 ind\_prod\_gro + \beta_3 pop\_gro + \beta_4 permit1 + \beta_5 payroll\_gro + \beta_6 lncsent + \beta_7 KOSPI200r + \beta_8 INCOME + \epsilon .$$

LTV ratio is not considered because we examine the impact of cross-sectional (and time-series) variations in LTV restrictions on house price comovement.

	Obs.	Mean	Std. Dev.	Min	p25	p50	p75	Max
prc_gro	3,562	0.02	0.05	-0.63	-0.00	0.00	0.03	0.54
PRIVMBS	3,562	21.49	11.44	0.00	26.19	27.19	27.96	29.51
lncd91d	3,562	1.41	0.27	0.88	1.26	1.43	1.60	1.96
ind_prod_gro	3,562	0.01	0.05	-0.09	-0.03	0.01	0.06	0.09
PAYROLL_gro	3,562	0.00	0.02	-0.04	-0.01	-0.00	0.03	0.06
permit1	3,562	10.73	0.66	9.56	10.14	10.64	11.09	11.97
ppi_gro	3,562	0.01	0.01	-0.03	-0.00	0.00	0.01	0.05
Lncsent	3,562	4.60	0.10	4.30	4.54	4.61	4.66	4.76
KOSPI200r	3,562	0.02	0.14	-0.31	-0.05	0.03	0.11	0.39
INCOME	3,562	16.75	0.21	16.36	16.59	16.75	16.90	17.06

Appendix B.

	Credit Constraints Measure
Duration	For each local area ("Gu," which is similar to the county in the U.S.) and each 30-quarter period, we compute the number of years between the beginning of "Gu"-level LTV restriction and its ending date (source: Mistry of Strategy and Finance, www.mosf.go.kr).
	Political factors of LTV restriction
Conservative party congressman	For each local area and each 7-year period, we compute the tenure of office of conservative party congressmen. In Korea, two-to-four election districts are included in each "Gu" area. We consider a quarter as a conservative party quarter when at least one congressmen's political party in a "Gu" area is classified as conservative party (source: Republic of Korea National Election Commission, http://www.nec.go.kr/)
Conservative party president	For each 7-year period, we compute the tenure of office of conservative party presidents (source: Republic of Korea National Election Commission, http://www.nec.go.kr/)
	Local Housing Market Characteristics
Income growth	Due to data availability, we use metropolitan-level income growth rates as proxies for local-area income growth rates. First, for each metropolitan area (particularly, six "KwangYuk-Si" and Seoul)" and year, we compute GRDP(Gross Regional Domestic Product) per registered residents. Second, we compute their 7-year moving averages and match each observation with "Gu"-level local areas within a metropolitan area (source: Statistics Korea (Korean bureau of statistics)'s kostat.go.kr).
Population growth	First, for each local area ("Gu") and year, we compute the log growth rate of the number of registered residents. Second we compute their 7-year moving averages (source: Statistics Korea (Korean bureau of statistics)'s kostat.go.kr).
Net flow of population	First, for each local area ("Gu") and year, we divide the net flow of population by the number of registered residents. Second, we compute their 7-year averages (source: Statistics Korea (Korean bureau of statistics)'s kostat.go.kr).
Proportion of population over age 65	For each local area ("Gu") and year, we compute moving averages of proportion of population over age 65 (source: Statistics Korea (Korean bureau of statistics)'s kostat.go.kr).