

The Financial Accelerator: Evidence from International Housing Markets [★]

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Abstract. This paper shows novel evidence on the mechanism through which financial constraints amplify fluctuations in asset prices and credit demand. It does so using contractual features of housing finance. Among agents whose housing demand is constrained by the availability of collateral, those who can borrow against a larger fraction of their housing value (achieve a higher loan-to-value, or LTV, ratio) have more procyclical debt capacity. This procyclicality underlies the *financial accelerator* mechanism. Our study uses international variation in LTV ratios over three decades to test whether (a) housing prices and (b) demand for new mortgage borrowings are more sensitive to income shocks in countries where households can achieve higher LTV ratios. The results we obtain are consistent with the dynamics of a collateral-based financial accelerator in international housing markets.

1. Introduction

Theoretical research has argued that endogenous developments in the financial markets can greatly amplify the effects of small income shocks through the economy (e.g., Kiyotaki and Moore, 1997; Bernanke et al., 1996, 1999). Bernanke et al. (1996) call this amplification mechanism the “financial accelerator” or “credit multiplier”. The key idea behind the financial accelerator is the notion that shocks to the net worth of firms and households have a procyclical effect on their borrowing capacity. This can happen either because the information cost wedge between external and internal finance moves countercyclically (Bernanke and Gertler, 1989), or because a procyclical change in the value of collateralizable assets changes the external financing capacity in the same direction (Kiyotaki and Moore, 1997). Following a positive income shock, agents should be able to raise more external finance, and the increase in their borrowing capacity would further boost spending. According to this view, the endogenous procyclicality of

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the external financing capacity of firms and individuals can help explain important features of the business cycle and the transmission of monetary policy.

How can one identify whether there is an independent spending effect coming from an endogenous change in financing capacity following an income or wealth shock? The theory suggests that the effect of an income shock on constrained agents' spending should be greater when debt capacity is more procyclical. Accordingly, one way to pin down the dynamics of the financial accelerator is to explore *cross-sectional differences* in the spending responses of *constrained, cyclical* agents to aggregate income shocks. This study explores the features that characterize housing finance contracts to pursue such an approach. It does so using a unique data set on asset prices and credit in international housing markets. Our results provide novel evidence on the financial accelerator mechanism.

The housing market is an ideal laboratory for conducting a test of the accelerator. The crucial feature of housing finance contracts that we explore is that the availability of mortgage credit to households is typically limited to a specific proportion of the value of the house they own or are about to purchase (the maximum loan-to-value, or LTV, ratio). Our analysis builds on the theoretical framework of Stein (1995) to show how the presence of a maximum LTV ratio (which implies a down payment constraint) affects prices in the housing market. The basic intuition is simple. Suppose that constrained households receive a positive income shock that boosts housing prices. The higher the LTV ratio that households can achieve, the higher the increase in borrowing capacity that is generated by the ensuing increase in prices. Importantly, the procyclical increase in borrowing capacity may allow households to further increase housing spending, amplifying the collateral-based spending cycle. If an accelerator effect is present, then housing prices should respond more to the initial income shock when the maximum LTV is high. In this fashion, the relation between LTV ratios and the income sensitivity of housing prices provides for a direct test of the endogenous mechanism underlying the financial accelerator: the impact of shocks to household income on housing prices is amplified by the higher marginal opportunity to borrow associated with a high LTV ratio.

Testing our hypothesis requires some degree of exogenous variation in borrowing constraints (i.e., in LTV ratios). Fortunately, data from international housing markets can be used to test the accelerator theory. To give a concrete example of what we have in mind, consider a country in which housing finance is not well-developed, such as Italy, where historical maximum LTV ratios do not exceed 60%. On the other hand, take a country such as the UK, where LTV ratios averaged 90% in the last two decades. The accelerator argument suggests that so long as the collateral constraint is binding in both countries, the housing credit multiplier would be much stronger in the UK than in Italy. Simply put, the collateral-based accelerator story that we examine predicts that, because households in high LTV countries have more procyclical debt capacity, housing prices should be more sensitive to income shocks in the UK than in Italy.

The accelerator mechanism of Stein (1995) has a second testable implication. The theory also predicts that the price effect of the income shocks is amplified through changes in the demand for mortgage debt. To wit, if the effect of LTV ratios on housing prices is generated by a credit multiplier, then new mortgage borrowings should also be more sensitive to income shocks in countries with higher maximum LTVs. International data on mortgage borrowings allow us to test this second hypothesis.

Our study further characterizes the financial accelerator by developing a third testable hypothesis. This hypothesis arises from the existence of an additional borrowing constraint in mortgage markets: the *income (or affordability) constraint*. The income constraint stems from real-world features of mortgage contracts that limit the yearly amount of housing expenditures associated with the loan (mortgage payments plus taxes and insurance) to a certain fraction of the household's yearly income. For our purposes, the key difference between the collateral and the income constraints is that only the former constraint gives rise to a credit multiplier. To wit, if the income constraint binds, then a household's marginal opportunity to borrow depends positively on its future income stream. In contrast, the marginal ability to borrow under the income constraint *does not* increase with the current value of the housing unit. Accordingly, whenever the income constraint binds, the positive relation between maximum LTV ratios and the sensitivity of housing prices to income should vanish. Our empirical strategy explores well-known characteristics of international housing markets to identify situations in which the income constraint is more likely to bind (i.e., when housing is less affordable).

In a nutshell, our tests show that housing prices are indeed more sensitive to income shocks in countries with higher maximum LTV ratios. Our estimates indicate that in countries like the UK, where the LTV ratio is around 90%, housing prices decrease by more than 1.2% in the first year following a 1% decrease in per capita GDP. On the other hand, in countries such as Italy, where the LTV ratio is around 60%, housing prices decrease by only 0.8% following a 1% decrease in per capita GDP. Second, fleshing out the financing mechanism underlying the accelerator, we find evidence that new mortgages responses to household income shocks are also increasing in maximum LTV ratios. Finally, and consistent with our conjecture about the joint role of income and collateral constraints, we find that the relation between LTV ratios and income sensitivities is stronger in countries where housing is more affordable.

Our analysis explicitly recognizes a number of alternative factors that could influence the results we report. For example, we control for variables that are likely to be correlated with maximum LTV ratios and that could also explain the cross-country differences in income sensitivities, such as economic development and the propensity for homeownership. We also consider the potential for simultaneity biases in our tests and use an alternative approach in which LTV ratios are instrumented with proxies for financial development (e.g., proxies for the quality of the judicial system and of accounting standards). Finally, we consider the possibility

that cross-country variations in the price-elasticity of housing supply might explain away the effect of LTV ratios on price-income elasticities.

Our empirical analysis borrows from Lamont and Stein (1999), who examine the sensitivity of housing prices to household income across US cities. Using data from the US, they find that housing prices are more sensitive to changes in city-level GDP in years when homeowners in a particular city have high debt (a proxy for liquidity constraints). Our study, in contrast, uses international variation in maximum LTV ratios and in price-to-income ratios to identify procyclicality in debt capacity. Importantly, the key housing finance variable we use (the maximum LTV ratio) is conceptually different from household's existing leverage. The maximum LTV ratio represents the marginal opportunity to borrow as a function of the value of housing, while household's leverage is an endogenous variable determined by past borrowing decisions and planned future spending.¹ Finally, Lamont and Stein do not examine the sensitivity of new mortgages to income shocks, and they do not incorporate an income constraint in their analysis.

The role of financial constraints in housing markets has been examined by a few theoretical papers besides Stein (1995). Ortalo-Magné and Rady (2006), for example, consider the effects of an interaction between household heterogeneity and a collateral-type constraint on housing price fluctuations. Their model features an amplification mechanism that relates to the one we seek to empirically identify in this paper: an income shock gets amplified through its impact on the ability of constrained (young) households to afford down payments. Similarly to Stein, their model predicts that housing prices should initially over-react to income shocks.

The current paper adds to the literature that examines the effects of financial development and financial market liberalization. Existing papers focus primarily on the effects of financial development and liberalization on the corporate sector and on overall growth rates (e.g., Demirgüç-Kunt and Maksimovic, 1998; Rajan and Zingales, 1998; Bekaert et al., 2005).² However, household behavior and housing markets are also likely to be affected by cross-country variations in financial development. Related evidence of such effects are found in Jappelli and Pagano (1989, 1994), who study the relation between financial development and macroeconomic variables, such as savings and consumption. Though not studying the accelerator, these authors use maximum LTV ratios as a measure of financial constraints on households exactly as we do – higher LTV ratios are associated with higher debt capacity and less financial constraints on households. Our results indicate that financial development is a contributing factor to the real-side effects of the financial accelerator. Specifically, they help identify a mechanism through which financial development and liberalization seem to magnify fluctuations in housing prices: when financial development is associated with lower borrowing constraints (higher LTV ratios) the financial accelerator becomes stronger.

¹ While highly levered households are probably more constrained than less levered households, it is hard to argue that households in the UK are more financially constrained than those in Italy.

² See Levine (1997) for a survey of the literature on financial development.

Finally, we stress that the existing literature provides limited evidence on the amplification mechanism that underlies the financial accelerator. Most extant studies look at firm data to explore one insight behind the accelerator: income shocks should affect corporate spending when firms have imperfect (or constrained) access to credit. Empirically, the investment spending of financially constrained firms should be more sensitive to changes in net worth than the investment of unconstrained firms (see Hubbard, 1998). Another working hypothesis is that constrained firms' spending and borrowing should fluctuate relatively more in the aftermath of monetary and other macro shocks (Gertler and Gilchrist, 1994). Unfortunately, while comparisons between constrained and unconstrained firms might show whether one group's investment is more dependent on income and net worth, they *cannot* identify whether differences in spending stem from an endogenous financial mechanism. Because constrained firms are more dependent on internal funds for investment, they should be more sensitive to a shock that affects income and net worth even when the shock has no endogenous, pro-cyclical effect on their borrowing capacity.³ Our analysis contrasts with that of previous studies in that we flesh out the financial accelerator by identifying contracts and markets that engender the mechanism behind the accelerator.

The remainder of the paper is organized as follows. Section 2 develops our empirical hypotheses. In Section 3, we provide a detailed description of the international housing markets data we use in the study. In Section 4, we present empirical results associated with each one of the hypotheses we develop. Section 5 concludes the paper.

2. The Roles of Collateral and Income Constraints on Housing Price Fluctuations

We build on the theoretical framework of Stein (1995) to develop our hypotheses about the effect of collateral and income constraints on housing prices. Stein models the equilibrium of the housing market under the assumption that a minimum down payment is required for the purchase of a new home. Specifically, if the value of a new home is equal to P , then a household must contribute at least a fraction τ of this value to buy the new home. The down payment constraint means that although the household can raise mortgage debt against the value of its housing wealth, the value of the mortgage loan that can be raised cannot be higher than a fraction λ ($= 1 - \tau$) of P .

The parameter λ can be interpreted as the maximum loan-to-value (LTV) ratio. The higher the λ , the easier it is for a household to borrow in order to finance spending. In the real-world, this parameter depends on variables such as the costs of enforcing and disposing of collateral, regulations about housing finance, and the

³ Recent research has further argued that the differential investment-income sensitivity of firms seen as "constrained" – typically small and young – can be explained by models that ascribe *no role* to financial constraints (e.g., Gomes, 2001; Altı, 2003).

amount of information creditors have about borrowers.⁴ The fact that the λ can be lower than 1 represents a credit quantity (collateral) constraint on households.⁵

Stein characterizes the effects of the down payment constraint on the comparative statics of the model and shows that a binding constraint amplifies the effects of shocks to housing demand on equilibrium housing prices (relative to a benchmark case with no down payment constraints). This amplification effect is created by a “credit multiplier”. A shock that increases housing prices, for example, also increases households’ borrowing capacity, because the ability to raise mortgage debt is directly linked to the value of housing through the maximum loan-to-value ratio. As the increase in borrowing capacity shifts out the demand for housing, the impact of the initial shock is amplified, and housing prices increase even more, boosting household wealth and borrowing capacity further, and so on.

One implication of the mechanism described by Stein is that the price–financing amplification effect is increasing in the change in borrowing capacity that follows the initial shock. To see this, consider an extreme case in which households need to pay for the home entirely with their own funds, that is, a case in which the maximum LTV ratio is zero. In this case, despite the extreme nature of the credit constraint, there is *no multiplier effect* being transmitted from the change in housing prices into changes in borrowing capacity – the latter is always equal to zero. The total change in housing prices will then be limited to the effect of the initial shock. In contrast, if the maximum LTV ratio is high, a change in housing prices will have a large effect on borrowing capacity, kickstarting the amplification mechanism. This discussion summarizes the first implication that we seek to test in this paper:

IMPLICATION 1. If the collateral constraint is binding, then the sensitivity of housing prices to shocks to housing demand should be increasing in the maximum LTV ratio available to households.

Besides testing this central implication, our analysis aims at providing evidence for the specific mechanism that explains the link between LTV ratios and housing price fluctuations. According to the theory, when the household is collateral-constrained, the effect of a demand shock is amplified by the associated change in borrowing capacity, and this amplification effect is larger the higher is the maximum LTV ratio. If this argument is correct, then *new collateral-based borrowings* by households should also be more sensitive to demand shocks in countries with high LTV ratios:

⁴ See Japelli and Pagano (1994) for a detailed discussion. Spiegel (2001) endogenizes down payment requirements, and argues that LTV ratios can be used to forecast future housing returns.

⁵ There is ample evidence from micro data that down payment requirements constrain household behavior. Stein (1995), Genesove and Mayer (1997), and Ortalo-Magné and Rady (2006) provide references and discussion.

IMPLICATION 2. If the collateral constraint is binding, then the sensitivity of new mortgage borrowings to shocks to housing demand should be increasing in the maximum LTV ratio available to households.

The amplifying effect of a higher loan-to-value ratio is conditional on the down payment constraint continuing to bind for the higher LTV ratios. If the maximum LTV ratio becomes so high that the household can easily afford the minimum down payment on the house, then we effectively revert to the benchmark case of no constraints in Stein's model. In this benchmark case, the effect of the shock is again limited to the effect of the change in fundamentals, given that borrowing capacity is inconsequential for housing demand. Our empirical analysis explicitly addresses the possibility that households might be largely unconstrained in high maximum LTV countries.

In addition, it is possible that the collateral constraint is not binding even in situations in which the maximum loan-to-value ratio is relatively low. This possibility arises from the fact that in real-world mortgage contracts households face an additional constraint that limits the amount of debt that they can raise against the house: the *income*, or *affordability constraint*. The affordability constraint essentially limits the yearly amount of expenditures associated with the mortgage contract (loan payments plus property taxes and insurance) to a certain fraction of the household's expected future yearly income. In the US, this fraction is around 28%.

The presence of income constraints means that households might be financially constrained even if they haven't reached the maximum loan-to-value ratio. A simple example can illustrate this possibility. Suppose that the value of the desired housing unit is equal to 100, and the maximum loan-to-value ratio is 70%. Suppose, in addition, that the household's current wealth level is equal to 30, so that it can afford the required down payment. In order to qualify for the loan, however, the household's future labor income must be $\frac{1}{0.28}$ times greater than the required level of housing expenditures associated with the loan of 70 (assuming US income limits). If we assume that yearly housing expenditures amount to 10% of the value of the mortgage, then expected future household income must be higher than 25, or else the household will not qualify for the loan. In such a case, the household's demand for housing would be constrained, but *not* by the collateral constraint.⁶

Stein's model does not explicitly treat the idea of an income constraint. However, it is easy to gauge the implications of such a constraint in the context of the amplification mechanism described above. Essentially, if the income constraint binds (instead of the collateral constraint), then the self-reinforcing mechanism that links changes in housing prices to changes in borrowing capacity should vanish. To wit, when the income constraint binds, the household's marginal debt capacity will increase with future income, but it will *no longer increase* with the value of

⁶ For example, if expected future income is equal to 15, the household can only afford a loan of 42, and thus the maximum amount that the household can pay for the housing unit is $42 + 30 = 72$.

the housing unit.⁷ Because the collateral-based amplification effect goes away, the link between maximum loan-to-value ratios and housing price fluctuations should disappear. This discussion summarizes the third implication that we seek to test in the empirical analysis:

IMPLICATION 3. The effect of the maximum LTV ratio on the sensitivity of housing prices to housing demand shocks is driven by country-years in which the income constraint is less likely to bind.

We discuss the details of the tests of our three hypotheses shortly. First, however, we describe the data that we use to test our predictions. This is done in the next section.

3. Data Description

We gather data for our analysis from a total of 26 countries over the 1970–1999 period (see Table II for the list of countries). The housing price data are summarized in Table I together with the data on per capita GDP (the main driving variable in our empirical tests) and annual new mortgages (which we use to assess the credit effects of the accelerator). We use yearly changes in the logs of GDP and housing prices, deflating the data with consumer price index series taken from the IMF's *International Finance Statistics* database. New mortgages are expressed as a fraction of nominal GDP. The data on housing prices and new mortgages are hand-collected from a number of different sources, while the GDP data are taken from the IMF financial statistics. We list all of our data sources and provide detailed information about the different indices used in the Appendix. Our sample has 754 country-year observations.

Table II displays country-level data on maximum LTVs over three decades. The maximum LTV ratio is the empirical counterpart of the parameter λ described in Section 2. Most of the LTV data are taken from Jappelli and Pagano (1989, 1994), who also use the maximum LTV ratio as a measure of the availability of credit to households in international housing markets. Those authors argue that the maximum LTV ratio is a direct measure of constraints on households that is comparable across countries. We were able to augment the Jappelli and Pagano data set using data from Chiuri and Jappelli (2003), and by looking into the sources cited therein. We collect additional data on LTV ratios from a number of other sources as well (see the Appendix for details). Table II shows that maximum LTV ratios vary significantly around the world. Developing countries, such as Korea and Taiwan, generally have lower LTV ratios (as low as 30%). However, there is variability even among developed economies, as evidenced by the case of Italy, where the LTV ratio is 60% during the 1990's, and the UK, where the LTV is 95%

⁷ In our example, if expected future income is equal to 15, then the maximum loan amount equals 42, irrespective of the value of the housing unit.

Table I. Summary statistics of housing price changes, income growth, and new mortgages

This table displays summary statistics for housing prices changes, income growth, and new mortgages for 26 countries over the 1970–1999 period. $\Delta\text{Log}(\text{Price})$ is the log change in the real housing price index. $\Delta\text{Log}(\text{Income})$ is the log change in real per capita GDP. New mortgages are net new lending against mortgage in residential property divided by nominal GDP. GDP, population, and inflation data are from the IMF's *International Financial Statistics*. The housing price and new mortgage data are described in the Appendix.

| | Mean | Std. Dev. | Pct 5 | Pct 25 | Median | Pct 75 | Pct 95 | N. Obs |
|-----------------------------------|-------|-----------|--------|--------|--------|--------|--------|--------|
| $\Delta\text{Log}(\text{Price})$ | 0.020 | 0.116 | −0.150 | −0.034 | 0.015 | 0.072 | 0.210 | 718 |
| $\Delta\text{Log}(\text{Income})$ | 0.030 | 0.045 | −0.033 | 0.007 | 0.027 | 0.051 | 0.102 | 754 |
| New Mortgages | 0.030 | 0.022 | 0.002 | 0.015 | 0.027 | 0.040 | 0.069 | 278 |

during that same period. Importantly, a number of countries register significant time variation in maximum LTVs (examples are Belgium, Denmark, Germany, Hong Kong, Sweden, Spain, and the UK). This allows us to explore both within- and cross-country variations in the dynamics of the financial accelerator.

4. Empirical Tests

Our base tests focus on cross-country-year differences in the sensitivity of housing prices to income shocks. According to the financial accelerator hypothesis, that sensitivity should be especially strong when the maximum LTV ratio is high, because of the endogenous change in debt capacity following a positive shock to income. In addition, new mortgage borrowings should also be more sensitive to income shocks if the maximum LTV ratio is high. Finally, our discussion of income constraints implies that the relation between LTV ratios and income sensitivities should be stronger in countries with relatively cheap housing. Finding that these patterns are present in the data is consistent with evidence of the financial accelerator in housing markets.

4.1. HOUSING PRICE DYNAMICS

We need to estimate empirical models of housing price dynamics to test our hypotheses. Following Lamont and Stein (1999), we use the log change in the housing price index as the endogenous regressor in our tests. Besides including current household income in our specifications, we look at the literature for additional determinants of housing prices. For instance, there is ample evidence of a consistent autoregressive pattern in housing prices. There is positive autocorrelation at short lags (e.g., Poterba, 1991; Lamont and Stein, 1999), but negative serial correlation

Table II. Maximum loan-to-value ratios (LTVs) by country-decade

Maximum LTV ratios represent the highest mortgage loan that households can get from lenders as a fraction of the value of the property owned. The data period is from 1970 through 1999.

| Country | LTV Ratio | | |
|-------------|-----------|--------|--------|
| | 1970's | 1980's | 1990's |
| Australia | 0.70 | 0.80 | 0.80 |
| Belgium | 0.65 | 0.75 | 0.80 |
| Canada | 0.75 | 0.80 | 0.80 |
| Chile | N/A | N/A | 0.78 |
| Denmark | 0.85 | 0.95 | 0.80 |
| Finland | 0.80 | 0.85 | 0.80 |
| France | 0.80 | 0.80 | 0.80 |
| Germany | 0.65 | 0.65 | 0.80 |
| Hong Kong | N/A | 0.90 | 0.70 |
| Ireland | 0.80 | 0.90 | 0.80 |
| Israel | 0.50 | 0.70 | N/A |
| Italy | 0.50 | 0.56 | 0.60 |
| Japan | N/A | 0.60 | 0.55 |
| Korea | 0.30 | 0.40 | 0.40 |
| Malaysia | 0.65 | N/A | 0.85 |
| Netherlands | 0.75 | 0.75 | 0.75 |
| New Zealand | 0.66 | 0.80 | 0.80 |
| Norway | 0.75 | 0.80 | 0.80 |
| Singapore | N/A | N/A | 0.85 |
| Spain | 0.60 | 0.80 | 0.80 |
| Sweden | 0.90 | 0.95 | 0.75 |
| Switzerland | N/A | N/A | 0.90 |
| Taiwan | 0.40 | N/A | N/A |
| Thailand | 0.65 | N/A | 0.75 |
| UK | 0.81 | 0.87 | 0.95 |
| US | 0.80 | 0.89 | 0.80 |

at longer lags (Case and Shiller, 1989; Lamont and Stein, 1999). We experiment with the use of these lag structures in turn.

In Table III we pool the sample in a panel regression and search for models to fit our data on housing prices. The only sampling restriction we impose is that we have data on LTV ratios for the data points considered (this allows us to gauge the marginal impact of LTV in the next set of tests). Model (1) shows that real housing prices are indeed correlated with real current income (proxied by real per

capita GDP). In models (2) and (3) we experiment with longer lag structures for changes in income (two additional lags) and also include two lags of price changes. These models differ with respect to the use of fixed effects (the latter model includes country effects). Consistent with earlier research on the US housing market, those estimations show that there is positive price autocorrelation at short lags, but negative autocorrelation at longer lags (long-term reversal) in international data.⁸

Model (4) adds other macroeconomic variables to the empirical specification. Both the real interest rate and the inflation rate have negative effects on housing prices, but their effects are not always significant. Finally, in model (5) we use the specification proposed by Lamont and Stein (1999) in their study of housing price dynamics in US cities. Essentially, they replace longer lags of price and income changes with the start-of-period ratio of price to per capita income ($\text{Price}_{t-1}/\text{Income}_{t-1}$). The more parsimonious specification of model (5) seems to capture well the effects of longer price and income lags.⁹

In what follows, we introduce LTV ratios and income constraints in our analysis, employing *all* of the specifications used of Table III. Our approach might seem tedious, but it will demonstrate that our findings do not hinge on the selection of a particular specification for housing price dynamics.

4.2. COLLATERAL CONSTRAINTS AND THE INCOME SENSITIVITY OF HOUSING PRICES

We introduce collateral constraint effects in our analysis by allowing the price-sensitivity of income to vary according to the maximum LTV ratio (Implication 1). This amounts to augmenting our empirical price models by adding an intercept term for the LTV ratio and another term capturing the interaction between LTV and per capita GDP growth. When we use multiple lags of GDP growth, we interact the LTV ratio with all of those lags, besides the current GDP growth (lag 0). This approach will capture the effect of the accelerator even if it takes longer for it to feed through the economy. We then test whether an increase in LTV increases price-income sensitivities by testing whether the parameters on those interaction terms are significantly greater than zero.

Table IV presents some of the main results of our paper. Model (1) (first column) shows that the correlation between changes in housing prices and changes in household income is indeed higher in countries with higher maximum LTVs.

⁸ Following the standard approach in the literature, most of our models are estimated via OLS and include both lagged dependent variables and fixed effects (see, e.g., Lamont and Stein, 1999). We, however, recognize the potential for biases in this procedure. In an earlier version of the paper, we emphasized results from the Arellano-Bond dynamic panel GMM estimators. As it turns out, the results from those estimations (available upon request) lead to the same conclusions we achieve using standard OLS estimations.

⁹ In unreported regressions, we show that longer lags of price and income become insignificant once we include the lagged ratio of price to per capita income. This result is also reported by Lamont and Stein (1999).

Table III. Housing price dynamics

The dependent variable is $\Delta\text{Log}(\text{Price})$, the log change in the real housing price index. $\Delta\text{Log}(\text{Income})$ is the log change in real per capita GDP. $\text{Price}_{t-1}/\text{Income}_{t-1}$ is the start-of-period ratio of the real housing price index to real per capita GDP. Real interest rate is the nominal long-term interest rate on the government benchmark bond yield, from the IMF's *International Financial Statistics* or from the OECD's *Economic Outlook*, minus the inflation rate in the same year. Inflation rate is the change in the consumer price index for the current year, taken from the IMF's *International Financial Statistics*. The estimation period is 1970–1999. The estimations correct the error structure for heteroskedasticity using the White-Huber estimator. t -stats (in parentheses).

| Indep. Variables | Model (1) | Model (2) | Model (3) | Model (4) | Model (5) |
|--|--------------------|--------------------|----------------------|--------------------|--------------------|
| $\Delta\text{Log}(\text{Income})_t$ | 1.083 (7.93)*** | 0.853 (5.38)*** | 1.023 (6.12)*** | 0.984 (5.46)*** | 0.954 (6.72)*** |
| $\Delta\text{Log}(\text{Income})_{t-1}$ | | 0.377 (2.13)** | 0.533 (3.10)*** | 0.159 (1.01) | |
| $\Delta\text{Log}(\text{Income})_{t-2}$ | | 0.028 (0.19) | 0.287 (1.82)* | 0.058 (0.39) | |
| $\Delta\text{Log}(\text{Price})_{t-1}$ | | 0.242 (2.90)*** | 0.185 (2.01)** | 0.327 (4.44)*** | 0.343 (5.05)*** |
| $\Delta\text{Log}(\text{Price})_{t-2}$ | | -0.065 (-1.16) | -0.080 (-1.34) | -0.095 (-1.83)* | |
| Interest Rate | | | -0.366 (-1.80)* | | |
| Inflation Rate | | | -0.116 (-0.83) | | |
| $\text{Price}_{t-1}/\text{Income}_{t-1}$ | | | -0.243 (-9.10)*** | | |
| $\sum_{j=0}^2 \Delta\text{Log}(\text{Income})_{t-j}$ | | 1.258 | 1.843 | 1.201 | |
| Summation Test p -value | | 0.00 | 0.00 | 0.00 | |
| Exclusion Test p -value | | 0.00 | 0.00 | 0.00 | |
| Country Effects? | No | No | Yes | No | Yes |
| Year Effects? | Yes | Yes | Yes | Yes | Yes |
| Observations | 611 | 567 | 567 | 531 | 589 |
| Adj- R^2 | 0.198 | 0.274 | 0.299 | 0.328 | 0.358 |

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Importantly, the positive effect of the LTV ratio remains strong after we include further lags of price and income in the specification. This is shown in model (2). In that model, the sum of the interaction terms of the LTV with the current and past lags of the change in income is positive and significant at the 1% level. When we include country effects in the specification (model (3)), the sum of the interaction terms increases. Model (4) shows that the inclusion of inflation and interest rates in the specification reduces the effect of the LTV ratio, but the interaction effects are still positive and significant. Finally, the interaction of the LTV ratio with the current change in income is also significant (p -value of 8%) when we use the Lamont and Stein specification (model (5)). This last specification makes it easier to assess the implied magnitude of the effect of the LTV ratio on income sensitivities. The coefficient returned for $\Delta\text{Log}(\text{Income})_t \times \text{LTV}_t$ suggests that if the LTV goes from 0.60 to 0.90, the income-price sensitivity increases from 0.84 to 1.23. These estimates imply, for example, that a 2% drop in per capita GDP will depress housing prices by some 1% *more* in the UK than in Italy.

Next, we report results that obtain after we impose multiple modifications to our empirical price models. To save space, we limit the tabulation of these checks to the results from three of our previous five models. In particular, amongst the more standard housing price specifications in Table IV (models (1) through (4)), we choose models (2) and (3) based on standard exclusion tests.¹⁰ In addition, since the Lamont and Stein (1999) model is of a somewhat different genre and because it has the highest R^2 , we also highlight the results from further experimentation with this model. We note that our conclusions are similar when we consider the other specifications featured in Table IV. We discuss the rationales for our additional tests in turn.

Our interpretation of the positive relation between the LTV ratio and the income sensitivity of housing prices is that this effect is driven by differences in the availability of mortgage finance to households in different countries. To provide further evidence that our results are indeed driven by differences in financial constraints (as opposed to a trivial simultaneity story), in our first round of checks we adopt an instrumental variables approach. In particular, we instrument the LTV ratio in our model with variables that we expect to be related to the level of financial development across countries. In countries with higher financial development it should be easier for both firms and households to raise outside finance. In the context of mortgage finance, a higher level of financial development should arguably be reflected in the availability of higher LTV ratios for households.

Our set of instruments for LTV includes the index of accounting standards computed by the Center for International Financial Analysis and Research. Accounting standards have been used as a measure of financial development in Rajan and Zingales (1998), among others. The second variable included in our instrument

¹⁰ Since our specifications are nested, we can perform standard χ^2 statistics-based exclusion tests for model selection. For example, the additional macroeconomic variables included in model (4) are found to be jointly statistically insignificant. That model is dominated by models (2) and (3).

Table IV. House prices and the multiplier effect: Baseline regressions

The dependent variable is $\Delta\text{Log}(\text{Price})$, the log change in the real housing price index. $\Delta\text{Log}(\text{Income})$ is the log change in real per capita GDP. $\text{Price}_{t-1}/\text{Income}_{t-1}$ is the start-of-period ratio of the real housing price index to real per capita GDP. Real interest rate is the nominal long-term interest rate on the government benchmark bond yield, from the IMF's *International Financial Statistics* or from the OECD's *Economic Outlook*, minus the inflation rate in the same year. Inflation rate is the change in the consumer price index for the current year, taken from the IMF's *International Financial Statistics*. LTV_t is the maximum LTV ratio for year t . The estimation period is 1970–1999. The estimations correct the error structure for heteroskedasticity using the White-Huber estimator. t -stats (in parentheses).

| Indep. Variables | Model (1) | Model (2) | Model (3) | Model (4) | Model (5) |
|--|--------------------|--------------------|---------------------|--------------------|----------------------|
| $\Delta\text{Log}(\text{Income})_t$ | -0.437 (-0.96) | -0.787 (-1.31) | -0.622 (-1.00) | -0.273 (-0.42) | 0.051 (0.10) |
| $\Delta\text{Log}(\text{Income})_{t-1}$ | | 1.174 (1.11) | 1.029 (1.03) | 0.132 (0.17) | |
| $\Delta\text{Log}(\text{Income})_{t-2}$ | | -0.470 (-0.62) | -0.199 (-0.24) | 0.504 (0.91) | |
| $\Delta\text{Log}(\text{Price})_{t-1}$ | | 0.228 (3.01)*** | 0.174 (2.08)** | 0.299 (4.21)*** | 0.332 (4.88)*** |
| $\Delta\text{Log}(\text{Price})_{t-2}$ | | -0.070 (-1.35) | -0.081 (-1.47) | -0.089 (-1.71)* | |
| Interest Rate | | | | -0.287 (-1.39) | |
| Inflation Rate | | | | -0.092 (-0.69) | |
| $\text{Price}_{t-1}/\text{Income}_{t-1}$ | | | | | -0.231 (-8.71)*** |
| LTV_t | -0.065 (-1.52) | -0.037 (-0.81) | -0.214 (-2.40)** | 0.007 (0.16) | -0.068 (-0.82) |
| $\Delta\text{Log}(\text{Income})_t \times \text{LTV}_t$ | 2.276 (3.58)*** | | | | 1.315 (1.80)* |
| $\sum_{j=0}^2 \Delta\text{Log}(\text{Income})_{t-j} \times \text{LTV}_t$ | | 2.152 (2.45)*** | 2.414 (1.96)** | 1.420 (1.75)* | |
| Country Effects? | No | No | Yes | No | Yes |
| Year Effects? | Yes | Yes | Yes | Yes | Yes |
| Observations | 611 | 567 | 567 | 531 | 589 |
| Adj- R^2 | 0.220 | 0.297 | 0.316 | 0.342 | 0.362 |

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

set is a proxy for the effectiveness of the country's judicial system. This proxy is taken from LaPorta et al. (1998). Arguably, the higher the standards of financial disclosure and the more advanced the judicial system in a country, the easier it is for firms to raise funds from a wider circle of investors. The results in models (1), (2), and (3) of Table V show that the effect of the LTV ratio on income sensitivities actually increases after instrumenting for overall financial development.¹¹ The new estimates of the multiplier (LTV interaction) effects are all significant at better than the 1% test level. They suggest that our previous findings are indeed driven by variables affecting the availability of finance.

An additional set of explanations for our results is also considered. In particular, to the extent that maximum LTVs and economic development might be correlated, one could argue that the results in Table IV are not driven by financial development, but simply by cross-country differences in *economic* development. It is likely that the fraction of wealth spent in housing increases with wealth. Then, one could argue that richer countries have larger income sensitivities, even if financial constraints are never binding. This provides for an "unconstrained explanation" for the observed pattern in sensitivities. Another possible explanation for our results is that the relation between maximum LTV ratios and income sensitivities is driven by cross-country differences in homeownership. One could argue, for example, that countries with large rental markets have lower sensitivities and lower LTV ratios because the rental market helps absorb the effect of an income shock, or because only the wealthiest households own homes in countries with low LTV ratios. If this argument explains our results, then the cross-country differences in income sensitivities we observe should be absorbed by variations in the homeownership ratio.

Models (4) through (9) in Table V address the relevance of these competing stories by adding time-varying proxies for economic development (levels of per capita GDP in constant international prices) and homeownership to our specifications.¹² In models (4), (5), and (6) we add our economic development proxy together with all of its interactions with lags of income change (lags 0 through 2). In models (7), (8), and (9) a similar approach is used to control for homeownership. The results from these tests show that the strong positive effect of LTV on sensitivities remains mostly unchanged after controlling for homeownership and economic development.¹³ The sum of the interaction terms of the income changes with the LTV ratio is positive and significant at better than the 5% test level in 6 of the 5 added

¹¹ The first-stage regressions show that our set of instruments – which also include lags of the predetermined regressors – and the maximum LTV are strongly positively related. The R^2 of the first-stage regression is 0.39.

¹² The coefficients returned for these controls are mostly insignificant and are thus omitted from the table.

¹³ Results are similar if we use both of these variables and all of their interactions with LTV together in one specification. The same holds under a more parsimonious approach where we only use the LTV ratio and its interactions with income change in the specification after expunging economic development and homeownership main effects from LTVs (i.e., using a "residual LTV").

Table V. House prices and the multiplier effect: Alternative specifications

The dependent variable is $\Delta\text{Log}(\text{Price})$, the log change in real housing price index. $\Delta\text{Log}(\text{Income})$ is the log change in real per capita GDP. $\text{Price}_{t-1}/\text{Income}_{t-1}$ is the start-of-period ratio of the real housing price index to real per capita GDP. In models (1) through (3) we instrument LTV_t with proxies for the quality of accounting standards and judicial efficiency. The measure of judicial efficiency is compiled by the Business International Corporation. The index of accounting standards computed by the Center for International Financial Analysis and Research. In models (4) through (6) we control for the level of economic development (PPP-adjusted per capita GDP) by including the intercept variable as well as its interactions with each of the lags of $\Delta\text{Log}(\text{Income})$ (coefficients omitted). Likewise, in models (7) through (9) we add intercept and interaction terms for homeownership (homeowners as a fraction of total households) and $\Delta\text{Log}(\text{Income})$. The estimation period is 1970–1999. The OLS estimations correct the error structure for heteroskedasticity using the White-Huber estimator. t -stats (in parentheses).

| Indep. Variables | IV | | | | | | | | |
|--|-----------------------|---------------------|----------------------|----------------------|-------------------|----------------------|----------------------------------|-------------------|----------------------|
| | Financial Development | | | Economic Development | | | Added Controls for Homeownership | | |
| | Model (1) | Model (2) | Model (3) | Model (4) | Model (5) | Model (6) | Model (7) | Model (8) | Model (9) |
| $\Delta\text{Log}(\text{Income})_t$ | -2.940 (-2.51)** | -2.717 (-2.36)** | -1.664 (-1.73)* | -0.671 (-1.17) | -0.512 (-0.85) | -0.019 (-0.04) | -0.971 (-0.88) | -0.900 (-0.79) | -0.354 (-0.36) |
| $\Delta\text{Log}(\text{Income})_{t-1}$ | 1.802 (1.04) | 1.884 (1.13) | | 1.194 (1.13) | 1.054 (1.07) | | 1.044 (0.86) | 0.828 (0.68) | |
| $\Delta\text{Log}(\text{Income})_{t-2}$ | -1.583 (-1.35) | -1.357 (-1.09) | | -0.411 (-0.53) | -0.187 (-0.23) | | -0.113 (-0.13) | 0.001 (0.01) | |
| $\Delta\text{Log}(\text{Price})_{t-1}$ | 0.227 (2.86)*** | 0.188 (2.14)** | 0.324 (4.50)*** | 0.230 (3.05)*** | 0.184 (2.21)** | 0.290 (3.81)*** | 0.233 (2.90)*** | 0.164 (1.77)* | 0.235 (2.90)*** |
| $\Delta\text{Log}(\text{Price})_{t-2}$ | -0.088 (-1.64) | -0.099 (-1.49) | | -0.076 (-1.45) | -0.091 (-1.64) | | -0.049 (-0.83) | -0.068 (-1.08) | |
| $\text{Price}_{t-1}/\text{Income}_{t-1}$ | | | -0.219 (-8.07)*** | | | -0.216 (-8.03)*** | | | -0.200 (-7.59)*** |

Table V. House prices and the multiplier effect: Alternative specifications (continued)

| Indep. Variables | IV | | | | | | | | |
|--|-----------------------|--------------------|----------------------|----------------------|---------------------|-------------------|----------------------------------|---------------------|-------------------|
| | Financial Development | | | Economic Development | | | Added Controls for Homeownership | | |
| | Model (1) | Model (2) | Model (3) | Model (4) | Model (5) | Model (6) | Model (7) | Model (8) | Model (9) |
| LTV_t | -0.077 (-0.97) | -0.079 (-1.01) | -0.764 (-3.11)*** | -0.056 (-1.17) | -0.175 (-2.10)** | -0.084 (-1.09) | -0.038 (-0.79) | -0.217 (-2.40)** | -0.130 (-1.58) |
| $\Delta \text{Log}(\text{Income})_t \times LTV_t$ | | 3.308 (2.63)*** | | | 1.926 (1.96)** | | | 1.921 (2.54)** | |
| $\sum_{j=0}^2 \Delta \text{Log}(\text{Income})_{t-j} \times LTV_t$ | 6.078 (3.66)*** | 5.924 (2.83)*** | | 2.085 (2.04)** | 2.275 (1.56) | | 2.048 (2.15)** | 3.214 (2.60)*** | |
| Country Effects? | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year Effects? | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 540 | 540 | 540 | 567 | 567 | 572 | 510 | 510 | 514 |
| Adj- R^2 | 0.313 | 0.322 | 0.392 | 0.307 | 0.323 | 0.363 | 0.289 | 0.316 | 0.375 |

(a) F -statistic. ***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

specifications; in the remaining specification (model (5)) the sum of the interaction terms is still marginally significant (p -value $< 12\%$).

In untabulated tests we also estimate our income multiplier models using the GMM estimator for dynamic panel data proposed by Arellano and Bond (1991). Specifically, we implement the one-step Arellano-Bond estimator with each of the base model variables instrumented by two of their own lags (in levels). As is generally the case, the Arellano-Bond estimator returns coefficients that are smaller than those from the OLS regression. Yet, the effect of the maximum LTV ratio on income sensitivities is still positive and statistically significant at the 1% test level.¹⁴

4.3. COLLATERAL CONSTRAINTS AND THE INCOME SENSITIVITY OF NEW MORTGAGES

In Section 2 we argue that if households are collateral-constrained, then the income sensitivity of *new mortgage borrowings* should also be higher in countries with high LTV ratios (Implication 2). This happens because the credit multiplier is generated by endogenous changes in collateralized debt capacity. To examine this implication, we consider the yearly ratio of new mortgages to GDP as the endogenous variable in a set of multiplier regressions. Importantly, one should be concerned with the potential for nonstationarity in the ratio of new mortgage to income.¹⁵ Accordingly, for each individual country in our raw data set, we perform (both) Dickey-Fuller and Phillips-Perron tests to check for unit root process in the mortgage-income ratio.¹⁶ With only one exception, all of the individual country series containing 20 or more consecutive observations pass those stationarity tests. We only keep in our sample those country-level mortgage series that are stationary.

In the absence of priors about other determinants of the dynamics of new mortgages, and to maintain consistency with our previous tests of the multiplier mechanism, we perform our new borrowings tests using the same exogenous variables of the models of Section 4.2 (Table IV). The results from OLS-FE estimations of those specifications are reported in models (1) through (5) in Table VI. Using now a smaller sample, we find evidence that new mortgages respond more to changes in household income when LTV ratios are higher. The interaction between income and LTV is positive in all of the models we experiment with, and statistically significant at better than the 5% level in all but one of our five specifications (the p -value associated with the remaining model is 8%).

These new borrowings results, too, are consistent with the existence of a credit multiplier in international housing markets.

¹⁴ GMM estimates can be found in earlier versions of our paper.

¹⁵ We thank the referee for raising this point to us.

¹⁶ See Gallin (2003, 2004) for empirical applications of cointegration tests to housing panel data sets.

Table VI. The multiplier effect in new mortgages

The dependent variable is New Mortgages, computed as the net new lending against mortgage in residential property divided by GDP. $\Delta\text{Log}(\text{Income})$ is the log change in real per capita GDP. $\text{Price}_{t-1}/\text{Income}_{t-1}$ is the start-of-period ratio of the real housing price index to real per capita GDP. Real interest rate is the nominal long-term interest rate on the government benchmark bond yield, from the IMF's *International Financial Statistics* or from the OECD's *Economic Outlook*, minus the inflation rate in the same year. Inflation rate is the change in the consumer price index for the current year, taken from the IMF's *International Financial Statistics*. LTV_t is the maximum LTV ratio for year t . The estimation period is 1970–1999. The estimations correct the error structure for heteroskedasticity using the White-Huber estimator. t -stats (in parentheses).

| Indep. Variables | Model (1) | Model (2) | Model (3) | Model (4) | Model (5) |
|--|---------------------|---------------------|--------------------|---------------------|--------------------|
| $\Delta\text{Log}(\text{Income})_t$ | -0.464 (-2.35)** | -0.619 (-2.22)** | -0.434 (-1.61) | -0.615 (-2.15)** | -0.335 (-1.14) |
| $\Delta\text{Log}(\text{Income})_{t-1}$ | | 0.184 (0.75) | 0.169 (0.83) | 0.209 (0.85) | |
| $\Delta\text{Log}(\text{Income})_{t-2}$ | | -0.319 (-1.10) | -0.379 (-1.64) | -0.266 (-0.95) | |
| $\Delta\text{Log}(\text{Price})_{t-1}$ | | 0.019 (1.04) | 0.018 (1.34) | 0.018 (0.96) | 0.025 (2.25)** |
| $\Delta\text{Log}(\text{Price})_{t-2}$ | | 0.007 (0.33) | 0.001 (0.07) | 0.010 (0.45) | |
| Interest Rate | | | | -0.149 (-2.44)** | |
| Inflation Rate | | | | -0.049 (-1.56) | |
| $\text{Price}_{t-1}/\text{Income}_{t-1}$ | | | | | -0.013 (-1.67)* |
| LTV_t | 0.038 (3.35)*** | 0.035 (2.80)*** | 0.096 (4.83)*** | 0.040 (3.39)*** | 0.113 (5.56)*** |
| $\Delta\text{Log}(\text{Income})_t \times \text{LTV}_t$ | 0.874 (2.46)** | | | | 0.434* (1.73) |
| $\sum_{j=0}^2 \Delta\text{Log}(\text{Income})_{t-j} \times \text{LTV}_t$ | | 1.073 (2.39)** | 0.915 (2.31)** | 0.968 (2.21)** | |
| Country Effects? | No | No | Yes | No | Yes |
| Year Effects? | Yes | Yes | Yes | Yes | Yes |
| Observations | 169 | 165 | 165 | 165 | 167 |
| Adj- R^2 | 0.369 | 0.388 | 0.583 | 0.399 | 0.581 |

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

4.4. ARE HOUSEHOLDS IN HIGH LTV COUNTRIES FINANCIALLY UNCONSTRAINED?

One might wonder whether a significantly large fraction of households in countries with high maximum LTV ratios such as the US or the UK are really constrained by the availability of collateral. After all, down payment requirements that are as low as 5% might imply that only the poorest and youngest households would be constrained by the size of the down payment. Recall, if households become collateral-unconstrained, the amplification mechanism associated with the financial accelerator would die out. At first sight, this possibility should make it *harder* for us to find the results that we have reported, given that the absence of collateral constraints reduces the extent of housing price fluctuations (Stein, 1995). However, one could argue that maximum LTV ratios are proxying for other (possibly unobservable) variables that are correlated with the sensitivity of housing prices to income shocks; variables that are unrelated to the financial accelerator mechanism.

A direct way to address this concern is to check whether the ratio of collateral-constrained households is systematically related to maximum LTV ratios. One empirical proxy for the ratio of households that are subject to binding collateral constraints is the ratio between the average and the maximum LTVs. A high ratio means that a greater fraction of mortgages are close to the maximum allowed. We have data on total outstanding mortgages that can be used to estimate average LTV ratios.¹⁷ If higher maximum LTVs increase the fraction of households that are unconstrained, then we would expect the ratio of average-to-maximum LTV to be *negatively* related to maximum LTVs. As it turns out, we find that countries with higher maximum LTVs have higher ratios of average-to-maximum LTVs. The correlation between the ratio of average-to-maximum LTV and the maximum LTV is 0.29 (p -value < 1%).

Our second take on the proposed alternative story involves testing the sensitivity of our findings to the presence of observations with very high maximum LTV ratios in the sample. Accordingly, we rank observations according to either overall country or country-decade maximum LTV ratios.¹⁸ For each of these two LTV ranking schemes, we then discard from the sample, alternatively, observations in the top decile, quintile, or quartile of the maximum LTV distribution and then reestimate all of the models of Table IV. This procedure allows us to check whether our results are driven by those (potentially “unconstrained”) countries with the highest maximum LTVs. All such estimations (a total of 30) return a positive significant relation between LTV ratios and income sensitivities. These findings are also inconsistent with the proposed story that country-years with higher maximum LTVs may have more unconstrained households.

¹⁷ The average LTV ratio is computed as the ratio of mortgage debt outstanding to the value of owner occupied housing, with the latter equal to the stock of housing times the homeownership ratio times the housing price level.

¹⁸ In the first case, we use the average maximum LTV ratio over our entire 30-year sample to rank the countries.

4.5. HETEROGENEITY IN PRICE-ELASTICITIES OF SUPPLY

Our interpretation of the positive effect of LTV ratios on price-income elasticities in Tables IV through VI is that the financial accelerator increases the sensitivity of housing demand to income shocks. Because the financial accelerator is stronger in high LTV countries, housing prices respond more to income shocks in such countries. However, the extent to which housing prices respond to changes in housing demand is also influenced by the price-elasticity of housing supply. In countries with less elastic housing supply, prices should change more with underlying changes in housing demand. Thus, it is possible that heterogeneity in supply elasticities across countries partly explains our results if housing supply turns out to be less elastic in high LTV countries.

We believe that it is unlikely that the elasticity of supply and the LTV ratio are systematically negatively related. We have shown that the LTV ratio is positively related to the overall level of financial development in an economy (Section 4.2). In addition, the literature on housing supply suggests that greater availability of credit should increase the flexibility of housing supply. According to Mayer and Sommerville (1996), for example, the typical residential builder relies on banks to finance land acquisition and other construction expenses. Poterba (1984) explicitly includes a measure of credit availability in his empirical model of construction activity and finds that greater credit availability is positively associated with residential investment. Hence, if anything, we would expect the LTV ratio to be positively related to the elasticity of housing supply.

Unfortunately, empirically identifying the elasticity of housing supply is not an easy task. As discussed by DiPasquale (1999), there is no agreement on the best methodology to identify supply elasticities (see also Malpezzi and Maclennan, 2001; Mayer and Sommerville, 2000). Researchers have reported a wide range of estimates for the price elasticity of housing supply, even when using only US data. Estimating supply elasticities is harder when one needs to go beyond the US, given the more limited data availability.

Despite this difficulty, we have attempted to address the potential impact of heterogeneity in supply elasticities in two different ways. One simple idea is to include proxies for the elasticity of supply in the empirical models that we estimate in Tables IV through VI, and to interact these proxies with our main driving variables (changes in household income) in the same way that we have done with the LTV ratios. One potential determinant of supply elasticities is suggested by Voith (1996). He provides evidence that US communities with high population density have lower construction rates, because there is less open space available for new housing developments. In an international context, this argument would suggest that countries with higher population density might have lower elasticities of housing supply. We implement this idea by adding population density and its interaction with income changes to each of the empirical specifications of Table IV, but we find that the inclusion of these variables has no impact on our findings.

We have also collected data that allows us to gauge the price-elasticity of supply for a subset of countries in our sample. First, we compute the changes in the log of the housing stock to measure changes in the supply of housing. We then regress our supply proxy on changes in housing prices and on changes in the real cost of construction and interest rates. Crucially, these regressions explicitly recognize that changes in housing prices are endogenous via a two-stage least squares procedure. In particular, following Blackley (1999), we assume that changes in household income only affect supply through their effect on housing demand, and use changes in income as instruments to identify the model. We do this separately for the 16 countries for which we have sufficient data on construction costs and the changes in the housing stock. The results show that housing supply is reasonably elastic for 13 of those countries. More importantly, there does not seem to be any systematic positive relation between supply elasticities and LTV ratios: the cross-country correlation between price-elasticities and LTV ratios is -0.24 (p -value of 41%).

These empirical findings, together with our theoretical priors, suggest that cross-country heterogeneity in supply elasticities is unlikely to explain the patterns that we have reported in Tables IV through VI.

4.6. THE INCOME CONSTRAINT

The third prediction of Section 2 helps us further characterize the collateral-based financial accelerator. In particular, it states that if the relation between price-income sensitivities and the maximum LTV ratio is driven by the collateral constraint, then it should be especially strong in countries where the income constraint is less likely to bind.

Identifying which of those constraints is most likely to bind in each of the countries we study is not an easy task. Clearly, the income constraint is more likely to bind when the maximum fraction of mortgage expenditures (loan service, taxes, and insurance) to expected household income is low. Unfortunately, however, we do not have mortgage contract data on income limits for countries other than the US. In order to implement a test of the income constraint we need a source of cross-country variation that is arguably exogenous and that changes the likelihood that the income constraint will bind. In the absence of data on mortgage-income limits, we conjecture that the income constraint is more likely to bind when the relative cost of housing in the households' consumption bundle is high. The basic assumption is that, if housing is more expensive, then mortgage expenditures should be more likely to reach the households' income constraint.¹⁹ In other words, the income constraint should bind in countries with less affordable housing.

¹⁹ We note that the appropriateness of this assumption depends on a subtle condition: in countries with less affordable housing, the income constraint must be more likely to bind than the collateral constraint. We believe this condition holds generally. To wit, in a country with expensive housing, households will (on aggregate) also tend to be wealthier, since they own the country's housing stock. This suggests that housing price increases tighten income constraints before they tighten wealth constraints. For example, it is true that an increase in the value of the existing housing stock raises

A standard way of measuring housing affordability is to calculate the ratio of the price of a typical dwelling unit to yearly median household disposable income (*price-income ratio*). However, there is strong evidence that price-income ratios are not stationary (see Gallin, 2003).²⁰ We confirm this evidence in our sample by submitting our price-income time series data to standard stationarity tests. Virtually all country series contain unit roots.

An alternative way to measure housing affordability is to use price-rent ratios, instead of price-income ratios. The assumption of stationarity of price-rent ratios can be justified theoretically, and in fact the literature has shown that house prices and rents are cointegrated (Gallin, 2004). Intuitively, price-rent ratios are less likely to increase dramatically with demographic shifts or changes in property taxes. Accordingly, we measure housing affordability using the ratio between the price of a typical dwelling unit and the rent per unit, where the rent per unit is computed as actual aggregate rent expenditures divided by the size of the non owner-occupied housing stock (one minus the home ownership ratio, times the size of the total housing stock). We compute the price-rent ratio for each year for which we have data (see data description in the Appendix), and then we average price-rent ratios across time for each country. Noteworthy, there is a large cross-country variation in price-rent ratios. For example, in countries such as Switzerland and Hong Kong (average price-rent ratios ranging from 80 to 120), typical housing units are substantially more expensive relative to rents than in other countries such as Australia, US and Canada (average price-rent ratios ranging from 20 to 30). Importantly, these differences seem to be driven by factors such as country geography and size as well as population growth and density, and thus are at least partially exogenous.

In the final set of tests of our paper we estimate models of income multiplier separately for countries with cheap and expensive housing. To perform those tests, we use our country-level data on price-rent ratio for the period 1970–1999 (subject to data availability and stationarity). In particular, we rank countries according to their average price-rent ratios and assign to the “cheap” (“expensive”) housing category those countries ranked at the bottom (top) third of the price-rent distribution.²¹ The countries in the cheap housing category are: Australia, Canada, Denmark, Malaysia, New Zealand, Singapore, Sweden, Taiwan, and the US. The expensive housing countries are: France, Germany, Hong Kong, Italy, Japan, Korea, Spain, Switzerland, and Thailand.²² We then estimate cheap–expensive regression pairs

the required down payment for new buyers; however, it also increases the wealth of would-be movers who own houses (see Stein, 1995).

²⁰ In a previous version, we used price-income ratios to distinguish cheap and expensive housing countries. We thank our referee for suggesting the alternative proxy used below.

²¹ Our main conclusions are insensitive to whether we partition our panel data according to the median price-rent ratio or, alternatively, according to quartiles, quintiles, or deciles. As could be expected, the latter partitions produce stronger but noisier estimates.

²² Despite the aforementioned stationarity problem, using price-income ratios results in a very similar ranking of countries. Thus, the results reported in Table VII below are similar to those that we obtained using price-income ratios.

Table VII. The income constraint effect

For each country, we use the average price-rent ratio for the period 1970–1999 (subject to data availability and stationarity) to classify countries in the “cheap” and “expensive” categories. The price-rent ratio is the price of a typical home divided by rent per rented unit, where rent per rented unit is aggregate rent divided by the stock of housing that is not owner occupied (stock of housing times 1 minus the home ownership ratio). Cheap (expensive) housing countries are those ranked in the bottom (top) third of the cross-country distribution of the price-rent ratio. The countries in the cheap housing category are: Australia, Canada, Denmark, Malaysia, New Zealand, Singapore, Sweden, Taiwan, and the US. The expensive housing countries are: France, Germany, Hong Kong, Italy, Japan, Korea, Spain, Switzerland, and Thailand. The estimation period is 1970–1999. Each of the cells in this table reports the coefficients for the interaction between changes in income and LTV ratios for models (2), (3), and (5) from Table IV. The first two of these models includes three lags of income and housing price changes, as well as the interactions of income changes and the LTV ratio. The third model is the Lamont-Stein specification, which includes the current change in per capita GDP and its interaction with LTV. The estimations correct the error structure for heteroskedasticity using the White-Huber estimator. *t*-stats (in parentheses).

| | Model (2) of Table IV (Year Effects) $\sum_{j=0}^2 \Delta \text{Log}(\text{Income})_{t-j} \times \text{LTV}_t$ | Model (3) of Table IV (Year and Country Effects) $\sum_{j=0}^2 \Delta \text{Log}(\text{Income})_{t-j} \times \text{LTV}_t$ | Model (5) of Table IV Lamont-Stein $\Delta \text{Log}(\text{Income})_t \times \text{LTV}_t$ |
|-----------------------------|--|--|---|
| Cheap Housing Countries | 3.999 (1.98)** | 5.778 (2.83)** | 7.528 (4.45)*** |
| Expensive Housing Countries | 1.056 (0.74) | 0.487 (0.15) | 0.912 (0.46) |

***, **, * indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

for models (2), (3), and (5) from Table IV. These are the same models used in the robustness tests of Table V. To recap, the first two of these models include three lags of income and housing price changes, as well as the interactions of income changes and the LTV ratio – these models differ in that the second model includes country effects. The third model is the Lamont and Stein specification, which includes the current change in per capita GDP and its interaction with LTV. The results from these tests are reported in compact form in Table VII.

Each of the cells in Table VII displays the coefficients for the interaction between changes in income and LTV ratios (with associated t -statistics). Consistent with our hypothesis, there is a broad positive association between the LTV ratio and income sensitivities, however this relation is particularly strong and significant in countries with relatively cheaper housing. This cheap–expensive differential pattern holds steady across all of the specifications that we consider. This last set of results gives additional evidence that our results are driven by binding collateral constraints.

5. Concluding Remarks

This study explores the features that characterize housing finance contracts and international housing markets to provide novel evidence supporting the “financial accelerator”. In particular, we use international variation in maximum loan-to-value (LTV) ratios to identify, within a group of constrained agents, those with more procyclical borrowing capacity. Since the procyclicality in the borrowing capacity of constrained agents is the amplification mechanism at the heart of the financial accelerator, our empirical strategy allows us to provide a direct test of the endogenous mechanism that underlies the accelerator. Inspired by the model developed by Stein (1995), we propose three implications of the financial accelerator hypothesis for housing price dynamics. Our empirical results are consistent with these three implications; namely that (a) housing prices are more sensitive to aggregate income shocks in countries with higher maximum LTV ratios; (b) new mortgage borrowings, too, are more sensitive to aggregate income shocks in countries with higher LTVs; and (c) the empirical relation between LTV ratios and income sensitivities is stronger in countries in which the income constraint is less likely to bind. These results indicate that debt capacity is more strongly procyclical in countries with high LTV ratios, and that the procyclicality of debt capacity affects housing price dynamics through a collateral constraint. Our empirical analysis explicitly addresses a number of factors that could potentially influence the results we obtain.

Besides being a nice laboratory to study the economic effects of the financial accelerator, the housing market is also one of the markets where the economic significance of such effects is likely to be high. Previous literature has shown that consumer spending is intimately linked to housing wealth (see, e.g., Case et al., 2005; Shiller, 2004), that housing investment plays a major role in the business

cycle (Mishkin, 1978; Bernanke and Gertler, 1995), and that housing collateral and house price fluctuations play an important role in explaining time series and cross-sectional variations in asset risk premia (Lustig and Van Nieuwerburgh, 2005). This paper shows that the endogenous effect of the financial accelerator in housing markets help characterize the mechanism through which what seem to be small, localized shocks get amplified and transmitted throughout the economy.

Data Appendix

This appendix describes in detail several of the data items we use in the paper.

HOUSING PRICE INDICES

Most of the data for developed countries are supplied by Peter Englund, which are the same data used in Englund and Ioannides (1997). Below we refer to this source as “EI”. Their data covers the period 1970–1992. We update their data set using the Annual Reports from the Bank of International Settlements (BIS), which give information on the same indices used by Englund and Ioannides. For countries not included in the Englund and Ioannides data set, we use other sources described below. We list all the specific sources for each country, and the information we have about the respective indices.

Australia. EI, and BIS. Weighted average index of prices for all capital cities and other areas; obtained from quarterly national census of home loan approvals, available annually. Updated using the AUEHPI index from the Australian Bureau of Statistics.

Belgium. EI, and BIS. Index based on annual transactions reports on small and medium sized dwellings from entire country, with outliers excluded, available annually.

Canada. EI, and BIS. Average annual transaction prices reported by multiple listing services for entire country, covering 70% of all transactions. Updated using the New House Price Index from the Statistics Canada, available at <http://cansim2.statcan.ca>.

Chile. Data provided by Felipe Morande, from Morande, F. and R. Soto (1992) updated by R. Soto. Based on standardized dwellings in the area of Santiago, annual average.

Denmark. EI, and BIS. Average value of single-family homes, including only arms’ length sales, available annually.

Finland. EI and BIS. Average price per apartment and terraced houses, obtained per square meter, as recorded by realtors (including 30% of all transactions), weighted by region, available quarterly.

France. EI and BIS. Index based on BIS’ own estimate, based on annual values for the Paris region, adjusted by four-year survey for entire country.

Germany. EI and BIS. Transaction prices per square meter, obtained from realtors for the four largest cities, available annually.

Hong Kong. Index constructed by the Rating and Valuation Department, from the Hong Kong Property Review, data from Chou and Shih (1995), updated using data on the same index available at <http://www.info.gov.hk>.

Ireland. EI and BIS. Average transactions price for existing homes, based on all loan approvals, available annually.

Israel. Property price index representative of the entire country, from the Social Sciences Data Archive (data used in Bar Nathan et al., 1998), updated using data from the Israel Central Bureau of Statistics (<http://www.cbs.gov.il/srcer.cgi>).

Italy. EI and BIS. Average price for new and completely refurbished dwellings in large and middle-sized cities and tourist areas, reported by realtors, available annually.

Japan. Urban Residential Land Price Index, from the Japanese Real Estate Institute, available at <http://www.reinet.or.jp>.

Korea. Land Price of Housing, from the Korea Appraisal Board, Appraisal Research & Development Center, available at <http://www.kreic.com>.

Malaysia. IHRM (Malaysian House Price Index % change from previous year). Data provided by Steve Malpezzi and used in Malpezzi and Mayo (1997), updated using data from the Countrywide's Sourcebook 2000.

Netherlands. EI and BIS. Weighted average sales price for existing single and multi-family homes, reported by realtors, including 50-60% of all transactions, available annually.

New Zealand. Data from Dalziel and Lattimore (1999), Valuation New Zealand Housing Price Series, average prices of free-hold house sales, adjusted for quality, updated using BIS data.

Norway. EI and BIS. Average sales price of existing homes, weighted by type of dwelling, reported by Property Owner's Association, covering about 50% of all transactions.

Singapore. Data from Phang and Wong (1997). Value weighted average of current prices of five types of property in five planning districts. Excludes public housing. Updated using the SIPRIRES index of the Singapore Department of Statistics.

Spain. Data provided by O. Bover. Prices per square meter of new dwellings in Madrid, used in Bover (1993). Updated with the Price Index for Existing Dwellings, from Hypostat 1999.

Sweden. EI and BIS. Index based on owner-occupied one- and two-dwelling buildings, based on reports of title registrations for arm's length transactions, weighted by type of dwelling, available annually.

Switzerland. Real estate price index for 3-5 bedroom single family homes, from the Swiss National Bank (<http://www.snb.ch/e/search/index.html>).

Taiwan. Median of Housing Prices in Taipei, provided by Shiawee Yang.

Thailand. Real housing price index used in Malpezzi and Mayo (1997). Data provided by S. Malpezzi covering the period from 1970–1986. Updated using the series on Land Price Increases in Bangkok, from the Agency for Real Estate Affairs.

UK. EI and BIS. Index based on survey of all dwellings with building societies mortgages, weighted by type of dwelling, available annually.

US. EI and BIS. Index based on sales price of existing single-family homes, based on realtor reports, adjusted by regional availability of single-family homes and homeowner mobility, available annually.

NEW MORTGAGES

Data for net new mortgage lending for Belgium, Denmark, Germany, Ireland, Finland, Netherlands and Spain are from Hypostat 1989–1999, and data for Canada, France, Italy, Japan, Norway, Sweden, UK and US is from the OECD, also used by Girouard and Blondal (2001), and kindly provided to us by Nathalie Girouard.

MAXIMUM LTV RATIOS

Data is from Jappelli and Pagano (1994), updated with data from Chiuri and Jappelli (2003). The data are given in 10-year averages. We extended this data as follows: for Denmark, Japan, New Zealand and Norway we assumed the Jappelli and Pagano 1980–1987 data extends to 1990. We took 1991–1999 data for Denmark and Norway from MacLennan, Muellbauer and Stephens (1998). Singapore 1991–1999 data are from Phang and Wong (1997). The data for Chile, Hong Kong, Korea (1980–1999), Japan (1991–1999), New Zealand (1991–1999) and Switzerland are from the Countrywide's Sourcebook, 1995 and 2000. Malaysia and Thailand 1991–1999 data are from the Asian Development Bank, 1999.

HOMEOWNERSHIP RATIOS

Data for Australia, Belgium, Canada (1970–1989), France (1970–1980), Germany (1970–1980), Italy (1970–1980), Netherlands (1970–1980), Spain (1970–1980), and Taiwan are from Chiuri and Jappelli (2003). Data for Chile, Denmark, Finland, France (1981–1999), Germany (1981–1999), Hong Kong, Ireland, Italy (1981–1999), Japan, Netherlands (1981–1999), Norway, Sweden and Spain (1981–1999) are from the Countrywide's Sourcebook 2000. Data for Korea and Malaysia is from the Asian Development Bank, 1999. Data for Canada (1991–1999), New Zealand, Singapore, Switzerland, Thailand, UK and US are from the Euromonitor (available at <http://www.euromonitor.com>).

PERSONAL DISPOSABLE INCOME DATA

The data on personal disposable income are from the Economic Outlook No 70: Annual and Semiannual data (Source: OECD), with the following exceptions: the data for Denmark and Thailand is from DRI-Wefa (<http://www.dri-wefa.com/>), the data for Taiwan is taken from the Government statistics at <http://www.stat.gov.tw>.

PRICE OF A TYPICAL DWELLING UNIT

We collected the nominal housing price for a particular year, and then we used the housing price index described above to extrapolate the series for all years. The data for Belgium, Denmark, Finland, France, Italy, Netherlands, Spain and Sweden represents the typical price for a flat of 150 square meters in 1999, and are taken from the Countrywide's Sourcebook 2000. The data for Canada (average price of all dwellings, 1995–1999), Ireland (average new house price for the whole country, 1996–1998), Korea (median price of typical 710 square feet apartment in Seoul in 1990), New Zealand (median price of a home, 1999), UK (Mix-adjusted average house price in 1999), and the US (average existing single family house price from 1990–1999), are also taken from the Countrywide's Sourcebook 2000. Below we list the sources and definitions for the remaining countries:

Australia. typical house price in 1999, from <http://www.amp.com.au/au/ampweb.nsf/Content>.

Chile. Price of an standardized dwelling in selected areas of Santiago, 1975–1998, from Morande and Soto (1992).

Germany. Price of existing detached houses, 1970–1993, from Muelder and Wagner (1998).

Hong Kong. Price of a 100 square meter flat, 1982–1992, from Chou and Shih (1995).

Israel. Typical apartment price in 1999, from <http://www.jpost.com>.

Japan. Typical apartment price in 1999, from <http://www.pricechecktokyo.com>.

Malaysia. Typical price of a single-story detached home in 1998, from <http://www.jp-ph.gov.my>.

Norway. Average price of a 150 square meter flat, from Statistics Norway (<http://www.ssb.no>).

Singapore. 1999, average house price from Asia Week, <http://www.asiaweek.com>.

Switzerland. Price of an average 4 bedroom semi-detached house with parking in 1999, from <http://www.expatacess.com>.

Taiwan. Actual average housing purchase price, 1981–1989, from Lin (1993).

Thailand. 1994–1997 average house price, from the Asian Development Bank.

PRICE-RENT DATA

We compute the ratio between the price of a typical dwelling unit and the rent per unit, where the rent per unit is computed as actual aggregate rent expenditures divided by the size of the non owner-occupied housing stock (one minus the home ownership ratio, times the size of the total housing stock). The data for aggregate rents are accessed from the DRI database, which is now known as Global Insight. The data refer to actual rentals for housing. We complement these data with data on consumer expenditures on actual rentals for housing, from the National statistical offices/OECD/Eurostat/Euromonitor International, for the following countries: Hong Kong, Malaysia, Taiwan, Singapore, and Thailand.

CONSTRUCTION COSTS

Unless otherwise noted, all data on construction costs are accessed from the DRI database, which is now known as Global Insight. For construction costs, the following sources were also used since DRI did not have any building cost data for these countries:

Hong Kong. Tender Price Indices for the Private Sector are obtained from Levett and Bailey, Chartered Quantity Surveyors Ltd (<http://www.levettandbailey.com>). Cost trends in the construction industry are based on tender prices for builder's works. As an alternative to the Levett and Bailey data, the building cost index from <http://www.cedd.gov.hk/eng/publications/construction/index.htm> is also gathered.

Malaysia. The Building Materials Cost Index for Low Rise Residential Building (up to 5 stories) constructed in Kuala Lumpur. This index is obtained from the Construction Industry Development Board, Malaysia (<http://www.cidb.gov.my/>)

Netherlands. OECD. Data are accessed using the McGill University website (<http://www.arts.mcgill.ca/oecd/>). The last quarter in each year is used.

New Zealand. New Zealand Building Economist Material Construction Costs from the New Zealand Building Economist magazine. Construction cost for a dwelling of standard quality associated with various cities in New Zealand (August of each year) were averaged.

Singapore. RHLB Building Cost Index produced by Rider Hunt Levett & Bailey and reported in their Research & Development Report, Issue 36, June 2005 (<http://www.rhlab.com.sg>)

United Kingdom. Eurostat.

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