RAND Journal of Economics
Vol. 30, No. 3, Autumn 1999
pp. 498–514

Leverage and house-price dynamics in U.S. cities

Owen Lamont*

and

Jeremy C. Stein**

We use city-level data to analyze the relationship between homeowner borrowing patterns and house-price dynamics. Our principal finding is that in cities where a greater fraction of homeowners are highly leveraged—i.e., have high loan-to-value ratios—house prices react more sensitively to city-specific shocks, such as changes in per-capita income. This finding is consistent with recent theories that emphasize the role of borrowing in shaping the behavior of asset prices.

1. Introduction

■ This article studies the relationship between city-level house-price dynamics and homeowner borrowing patterns. We ask whether house prices respond more sensitively to economic shocks—in particular, to changes in per-capita income—in cities where a large fraction of homeowners are highly leveraged. For the purposes of this study, the basic concept of individual homeowner leverage is the ratio of the outstanding mortgage balance to the current value of the house, commonly known as the loan-to-value (LTV) ratio.

Our work picks up on an old and recurring theme in the literature on asset-market fluctuations, a theme that centers on the role of leverage in shaping the behavior of asset prices. In its most general form, the proposition is that when buyers finance the purchase of assets by borrowing, this can lead the prices of these assets to become more sensitive to exogenous changes in fundamentals. Specific versions of this story have been told in the context of a variety of markets, including those for stocks (Garbade, 1982); corporate asset sales (Shleifer and Vishny, 1992); land (Kashyap, Scharfstein, and Weil, 1990; Kiyotaki and Moore, 1997); as well as the market we examine here, that for houses (Stein, 1995). The common mechanism in all these articles is that

* University of Chicago and NBER; owen.lamont@gsb.uchicago.edu.
** Massachusetts Institute of Technology and NBER; jcstein@mit.edu.

This research is supported by the FMC Faculty Research Fund at the Graduate School of Business, University of Chicago (Lamont); the National Science Foundation grant nos. SBR-9733979 (Lamont) and SBR-9512649 (Stein), and the Finance Research Center at MIT (Stein). We are grateful to Anil Kashyap, Chris Mayer, Julio Rotemberg, Michelle White, the referees, and especially to the Editor, David Scharfstein, for helpful comments. Freddie Mac graciously provided some of the data. We also thank Amy C. Ko, Yin-Toa Lee and Peter Simon for research assistance.

the ability to borrow is directly tied to asset values, which imparts an upward tilt to asset-demand schedules. That is, over some regions, a fall in asset prices can actually lead to *reduced* asset demands, because it impairs the ability of potential buyers to borrow against the assets—this is the key amplifying effect.[1]

Our decision to study the housing market is motivated by three considerations. First, as we argue below, housing represents an asset category where—in contrast to, say, stocks—it seems *a priori* plausible to posit that the effect of leverage on prices might be large. A second motivation is that if house prices are in fact significantly affected by leverage, the ultimate economic consequences are likely to be important. Topel and Rosen (1988) show that movements in prices exert a powerful influence on housing starts, and it is well known that housing investment in turn plays a major role in business-cycle fluctuations. Finally, from a practical perspective, city-level housing markets offer good data for the sort of test we wish to conduct: the markets are arguably distinct from one another, we can get reasonable price indices, and there is a good deal of variation across cities in homeowner leverage.

Our main results can be briefly summarized. We find both an economically and statistically significant correlation between leverage at the city level and the elasticity with which house prices respond to shocks to per-capita income. To generate a concrete example, take our preferred measure of leverage, which is the fraction of households in a given city with LTVs exceeding 80%, and which we denote by *HIGHLTV*. First, note that the median value of *HIGHLTV* across all cities is 14%—that is, in the median city, 14% of households have LTVs greater than 80%. Now imagine two cities at extreme ends of the *HIGHLTV* distribution: one a ''low-leverage'' city where *HIGHLTV* is 5% (corresponding to the 10th percentile of the distribution) and the other a ''high-leverage'' city where *HIGHLTV* is 25% (corresponding to the 90th percentile). Finally, assume that both cities are hit with a positive 1% shock to per-capita income.

Our estimates imply that in the high-leverage city, house prices go up by .64% in the first year after the shock, as compared to only .19% in the low-leverage city. By the third year, the corresponding cumulative price movements are 1.23% and .68% respectively. Clearly, these are economically meaningful differences.

One important qualification in interpreting these results is that leverage is an endogenous variable. Given the limitations inherent in our data, it is difficult for us to make an airtight case that our results are not tainted by some degree of endogeneity bias. Nevertheless, we make two distinct attempts to confront the endogeneity problem. First, we lay out what we consider to be the most plausible specific endogeneity-bias story and design variations of our tests that directly control for it. Second, we reestimate our basic regressions using an instrumental variables (IV) approach. The instruments here are state-by-state bankruptcy-law variables that appear to influence the supply of and the demand for mortgage credit. Clearly this latter approach is *a priori* more attractive, as it could potentially insulate us against any unspecified form of endogeneity bias. Unfortunately, our instruments are quite weak, so the power of our IV tests is limited.

The remainder of the article is organized as follows. In Section 2 we articulate in more detail the theory we are testing and use this to help better motivate our choice of empirical variables. Section 3 describes our dataset. Section 4 contains our principal

[1] This direct two-way feedback—from asset prices to borrowing limits back to asset prices—distinguishes this particular class of models from the rest of the broader, and much larger, literature on credit constraints and economic activity. Prominent examples of this latter body of work include Fisher (1933) and Bernanke and Gertler (1989).

results, as well as a couple of robustness checks. In Section 5 we attempt to come to grips with the problems caused by the endogeneity of leverage. Section 6 concludes.

2. Theory

■ What is the mechanism by which homeowner leverage might be expected to influence the behavior of house prices? The model in Stein (1995) offers one possible answer. The model is one of repeat buyers—families who already own a home but have reasons to want to move (e.g., new job, better schools, etc.). These families are never forced to sell their homes under adverse conditions, but they may choose to if the gains from moving are large enough. In particular, at any level of house prices, families can be divided into three groups: ‘‘unconstrained movers,’’ ‘‘constrained movers,’’ and ‘‘constrained nonmovers.’’

Families in the first group are sufficiently wealthy that financial constraints have no effect on their behavior. So for them, housing demand is a decreasing function of price, and they perform a stabilizing role. Families in the second group have an intermediate level of wealth, and they face binding financial constraints. In equilibrium, they each choose to sell their old house and buy a new one, but the new one is smaller than they would like because they do not have enough money for a larger down payment. It is this second group that plays the crucial destabilizing role in the model, because their net demand for housing is an increasing function of price: if house prices were to rise, a constrained-mover family would be able to realize more from the sale of their old house and would use this extra money to make a down payment on a larger new house. Finally, families in the third group are so wealth-constrained that in spite of the potential gains from moving, they are better off sitting tight. Thus they neither buy nor sell, and they have no effect at all on house prices.[2]

The key implication that follows from this line of reasoning is that for leverage effects to have meaningful consequences for house prices, there must be a relatively high ratio of constrained movers to unconstrained movers in the population. This occurs when a large fraction of homeowners are packed into a narrow range of ‘‘high’’ LTV ratios, where ‘‘high’’ represents a value—perhaps 80% or so—where down payment constraints begin to bind. Thus in testing the theory empirically, we would ideally like a measure of leverage at the city level that captures this ‘‘packing’’ concept. That is, something like the median LTV ratio in a city is not the most theoretically desirable measure; it would be better to have something like the percentage of homeowners with LTVs in excess of 80%.

Subject to this ‘‘packing’’ condition being met, simulations in Stein (1995) suggest that the impact of fundamental shocks on house prices can be greatly magnified relative to the benchmark case of no financial constraints. The reason that leverage can matter so much in the housing market is that the potential for stabilizing arbitrage is limited. If house prices begin to drop, it is unlikely that a small set of arbitrageurs will buy up a large chunk of the housing stock, because in contrast to equities, there are obvious diminishing returns to owning more than one house.[3]

---

[2] Although they have no effect on equilibrium prices in this model, constrained-nonmover families are central to understanding another important housing-market phenomenon: the fact that trading volume is strongly correlated with prices.

[3] Similarly, Shleifer and Vishny (1992) offer a reason why arbitrage will have limited stabilizing effect in the corporate asset sales market: those potential buyers most likely to be unaffected by shocks to an industry, and hence to be financially unconstrained in a downturn, are industry outsiders, who cannot extract as much value from the assets. See Pulvino (1998) for evidence from the airline industry that supports this view.

Before proceeding, we should highlight one caveat about the mapping from the theory to the empirical work. While the model in Stein (1995) is useful in motivating our tests, and in giving some qualitative guidance on the choice of variables, it also suffers from a crucial weakness for our purposes in that it is static. Thus all the model really predicts is that prices will, in some timeless sense, react ''more'' to fundamental shocks in high-leverage cities; it is silent on the dynamic nature of the adjustment process. In other words, the model provides no guidance for thinking about whether any price discrepancies between otherwise identical high- and low-leverage cities ought to open up quickly or slowly in response to fundamental shocks, be long-lived or eventually decay away, etc. Since our empirical work must inevitably confront these dynamic issues, we cannot claim that we are disciplined by a fully articulated theoretical model.

3. Data sources

■ Our data on borrowing patterns at the city level come from the American Housing Survey (AHS) for 44 metropolitan areas between 1984 and 1994. The AHS is administered jointly by the Bureau of the Census and the Department of Housing and Urban Development. Each city is surveyed approximately once every four years; every year 11 cities are surveyed. The cities surveyed are Anaheim, Atlanta, Baltimore, Birmingham, Boston, Buffalo, Chicago, Cincinnati, Cleveland, Columbus, Dallas, Denver, Detroit, Fort Worth, Hartford, Houston, Indianapolis, Kansas City, Los Angeles, Memphis, Miami, Milwaukee, Minneapolis, New Orleans, New York, Norfolk, Northern New Jersey, Oklahoma City, Philadelphia, Phoenix, Pittsburgh, Portland (Oregon), Providence, Riverside, Rochester, Salt Lake City, San Antonio, San Diego, San Francisco, San Jose, Seattle, St. Louis, Tampa, and Washington.

In each city survey, data are obtained from several thousand randomly selected households. Among other questions, the AHS asks homeowners whether they have one (or more) mortgage(s), and what the monthly payments and other terms of the mortgage(s) are. Using the owner-reported terms of the mortgage, the AHS calculates the principal remaining on the mortgage; using this estimate of the principal and what the owner estimates to be the market value of the property, the AHS then calculates LTV ratios.[4]

Drawing on the summary statistics published by the AHS, we obtain several different measures of leverage at the city level. The first, which we call *HIGHLTV,* is the fraction of all owner-occupants with LTV ratios exceeding 80%. We use 80% as a cutoff because it is a standard benchmark for ''excessive'' LTV ratios, used for example to determine private mortgage insurance requirements. As discussed above, the *HIGHLTV* measure probably comes closest to capturing the relevant theoretical construct in Stein (1995)—namely, the extent to which a city has a large fraction of the population ''packed'' into a narrow range of high LTVs. Thus we focus most of our attention on the specifications that use this measure of leverage.

However, one potential problem with *HIGHLTV* is that its value might be quite sensitive to any errors homeowners make in estimating the value of their homes. In light of this concern, we also work with two other measures that are less likely to be subject to such errors: *MEDIAN*, the median LTV ratio among those owner-occupants who have a mortgage, and *YESLOAN*, the fraction of all owner-occupants having a

[4] Homeowners' estimates of the value of their own home, while biased upward, do not appear to be related to the characteristics of the owner, the house, or the local housing market. See Goodman and Ittner (1992).

mortgage of any size. Again, we stress that these alternative measures are less well motivated theoretically, and we use them primarily in the spirit of a robustness check.

Table 1 gives some basic summary statistics for our three leverage measures. On average across the entire sample, 66% of homeowners have mortgages. Among these mortgage holders, the median LTV ratio is 52%. Of all homeowners, 14% have LTVs exceeding the 80% threshold. Fortunately for our purposes, there is also a good deal of variation across cities in the leverage measures. For example, the *HIGHLTV* variable ranges from a minimum of 3% in Northern New Jersey in 1986 to a maximum of 35% in Denver in 1989. Similarly, the median LTV ranges from a minimum of 24% to a maximum of 75%, with the same two cities representing the extreme points.[5]

The bottom half of Table 1 also investigates the extent to which our three leverage measures are correlated with one another. The correlation between *HIGHLTV* and *MEDIAN* is very high, at .89. *HIGHLTV* is also quite correlated with *YESLOAN*, with a coefficient of .46. The weakest correlation is between *MEDIAN* and *YESLOAN*, at .30.

The AHS-derived leverage measures are available for each city only at four-year intervals. Other metropolitan-area variables are available annually. For house prices, we use the Conventional Mortgage Home Price Index, jointly created by researchers at Freddie Mac and Fannie Mae using repeat-sales prices from mortgage transactions. For population and income per capita, we use data from the Bureau of Economic Analysis. In all cases, we deflate nominal variables by the aggregate U.S. consumer price index to obtain real values, and we compute annual changes by taking log differences. Table 1 also provides some summary statistics for our data on house prices, demographics, and income.

Given that we wish to exploit the annual data we have on house prices, income, and demographics, the once-every-four-year nature of the AHS survey is a substantial weakness. There are two basic approaches we can take to deal with this problem. In most of our analysis, we use a ‘‘stale data’’ method: we run annual regressions, and in each city-year we use for a leverage variable the most recent value we have for that city. This means that at any point in time, we can have a leverage measure that is as much as three years out of date. Because the staleness of the data effectively amounts to measurement error, we would expect this approach to yield downward-biased estimates of the impact of leverage on house-price dynamics—i.e., our estimates using this approach are likely be too conservative.

In an effort to mitigate this potential bias, we also experiment with an alternative ‘‘projected data’’ method. The idea here is to use the annual series that we do have to create annual projected values of our leverage variables, and to use these projected values in place of the stale data. As one would expect, this method tends to boost our estimates of the importance of leverage.

4. Empirical results

■ **A benchmark model of house-price dynamics.** Our ultimate goal is to see how house-price dynamics vary across cities with different measures of homeowner leverage. But before we can do this, we need to select a benchmark model of city-level house-price dynamics. Ideally, this model should capture in a simple and robust way three key features of house prices that have repeatedly been documented in prior empirical work: (1) prices respond to contemporaneous economic shocks, (2) there are

[5] For New York City in 1986, leverage was so low that the AHS did not report either *MEDIAN* or the data needed to construct *HIGHLTV*. We thus omit the four years using this survey, except when using the *YESLOAN* measure of leverage.

TABLE 1 **Summary Statistics, 44 cities, 1985–1994**

	Description	Mean	Standard Deviation	Minimum	Maximum
House Prices					
dNOMPRICE	Change in log nominal house price	.03	.05	−.11	.27
dCPI	Inflation, change in log of annual U.S. consumer price index	.04	.01	.02	.05
dP	Real price change = *dNOMPRICE* − *dCPI*	−.01	.05	−.13	.23
Mortgage					
HIGHLTV	Percent of mortgages with loan/value >.8	.14	.07	.03	.35
MEDIAN	Median loan/value of all mortgages	.52	.12	.24	.75
YESLOAN	Percent of homes having any mortgage	.66	.08	.44	.80
Demographics and Income					
DNOMINC	Change in log nominal income per capita	.05	.02	−.03	.12
dI	Real change in income per capita = *DNOMINC* − *dCPI*	.01	.02	−.05	.07
dPOP	Change in log population	.01	.01	−.01	.07

Correlation Matrix

	HIGHLTV	*MEDIAN*
MEDIAN	.89	
YESLOAN	.46	.30

Statistics shown for annual observations pooled across cities. Except for inflation, variables are for a specific city. The debt variables come from 111 different surveys taken in staggered four-year periods in each city and are thus constant in each city for up to four years.

short-run ‘‘momentum’’ effects, and (3) there is a long-run tendency for ‘‘fundamental reversion.’’[6]

Table 2 details our search for a benchmark specification. It has six columns. In each one, we regress annual real house-price appreciation at the city level (denoted by dP_t) on some combination of the following six right-hand-side variables: the contemporaneous change in real per-capita income dI_t, and two of its lagged values, dI_{t-1} and dI_{t-2}; two lagged price appreciation terms, dP_{t-1} and dP_{t-2}; and the start-of-period ratio of price to per-capita income, P_{t-1}/I_{t-1}. All the regressions in this table and those that follow also include fixed effects for each year and each city (not shown in the tables), so that we are always working with deviations from both national averages in any year and long-run city averages. The data strongly reject the hypothesis that these year and city dummy variables are zero.

Column (1) shows that in a univariate regression, the elasticity of prices with respect to contemporaneous income is about .8. This univariate regression achieves an adjusted R^2 of .34. In columns (2)–(5) we begin adding in the other variables. These

[6] See, e.g., Case and Shiller (1989, 1990), Cutler, Poterba, and Summers (1991), Poterba (1991), and Abraham and Hendershott (1996) for empirical models of house-price dynamics.

TABLE 2 Candidate Models of House-Price Dynamics Dependent Variable: dP_t

Variable	(1)	(2)	(3)	(4)	(5)	(6)
dI_t	.813	.484	.298	.268	.426	.356
	(5.05)	(3.17)	(2.49)	(1.98)	(3.32)	(3.49)
dI_{t-1}		.553		.296	.161	
		(3.17)		(1.79)	(1.42)	
dI_{t-2}		.275		−.038	−.105	
		(2.22)		(.33)	(1.31)	
dP_{t-1}			.737	.718	.530	.495
			(8.37)	(8.33)	(7.19)	(9.59)
dP_{t-2}			−.165	−.179	−.003	
			(2.166)	(2.88)	(.06)	
P_{t-1}/I_{t-1}					−.220	−.195
					(6.83)	(8.08)
Number of observations	418	330	330	330	330	374
Adjusted R^2	.34	.43	.64	.65	.74	.74

Observations for all 44 cities, 1985–1994, are pooled in one fixed-effects regression. All regressions in all tables include city and year dummies, not shown. Robust *t*-statistics in parentheses.

tend to reduce the coefficient on the contemporaneous income term dI_t, but they substantially enhance the explanatory power of the regression. Column (5) shows that when all six variables are used simultaneously, the adjusted R^2 rises to .74. However, in this regression only three variables, dI_t, dP_{t-1}, and P_{t-1}/I_{t-1}, are statistically significant. The other variables are apparently subsumed by these three.

This suggests that we can do almost as well with a more parsimonious specification that uses only the three most important variables from column (5). This sparer regression is run in column (6). As can be seen, there is no loss in explanatory power to speak of—the adjusted R^2 remains at .74. The coefficient on dI_t is .356, that on dP_{t-1} is .495, and that on P_{t-1}/I_{t-1} is $-.195$. This three-variable model captures nicely and simply the three features of house prices alluded to above: (1) sensitivity to contemporaneous shocks (the dI_t term), (2) short-run momentum (the positive dP_{t-1} term), and (3) long-run fundamental reversion (the negative P_{t-1}/I_{t-1} term).

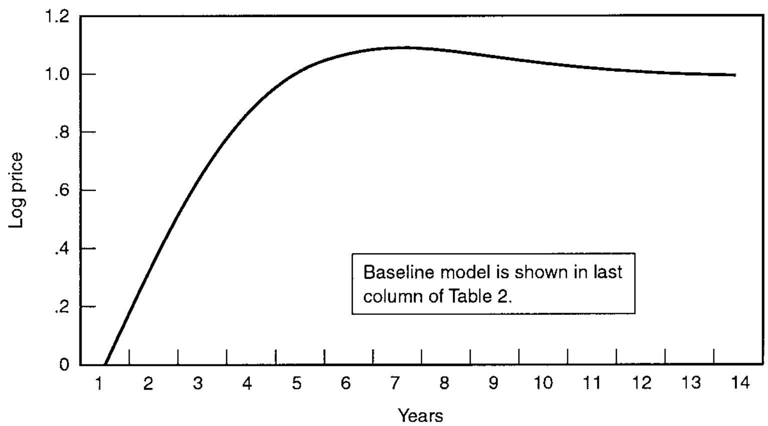
To illustrate this point, Figure 1 depicts the impulse response of house prices to a permanent 1% increase in per-capita income, using the parameter estimates from column (6) of Table 2.[7] As can be seen from the figure, the first year's effect on house prices is .36%. This increase then feeds positively into the next year's prices both by raising the lagged price-change term and lowering the price-to-income term. After about four years, the adjustment is complete, so that house prices have risen 1% and are back in line with income levels.[8]

[7] Preliminary analysis suggests that income shocks at the city level are in fact permanent—i.e., that income roughly follows a random walk. In particular, when we run the change in income on the city and year dummies and two lags of the change in income, the coefficients on these first two lags are .13 (*t*-statistic = 1.3) and .02 (*t*-statistic = .3) respectively.

[8] By including the lagged price-to-income ratio, we are assuming that the long-run elasticity of prices with respect to city-specific income shocks is one. Evidence in Poterba (1991) suggests that this is reasonable. Note, however, that we do allow for cities to have different average price-income ratios and for general trends in national price-income ratios.

FIGURE 1

DYNAMIC RESPONSE OF PRICE TO INCOME, BASELINE MODEL



From this point forward, we use the three-variable specification in column (6) of Table 2 as our baseline. Everything that follows asks in one way or another whether some or all of the coefficients in this simple model are related to the measures of leverage.

□ **The impact of leverage.** Table 3 presents a first test of our central hypothesis. We begin with the three-variable specification and add a single interaction term, given by $dI_t{*}DEBT_{t-1}$, where $DEBT_{t-1}$ is a once-lagged leverage measure. In column (1) we use *HIGHLTV* as the leverage measure; in column (3) we use *MEDIAN*; and in column (5) we use *YESLOAN*. In words, we are asking if prices are more sensitive to contemporaneous income shocks in high-leverage cities.

As can be seen from the table, the answer to this question is yes. Whichever measure of leverage is used, the interaction terms are always positive and statistically significant.[9] Moreover, the magnitude of the leverage effect is quite large in economic terms. This is perhaps easiest to see by comparing the impulse response of house prices to an income shock for cities with different leverage levels, shown in Figure 2. The figure uses the parameter estimates from column (1) of Table 3 and compares a city with the 10th percentile value of *HIGHLTV* (which is approximately 5%) to a city with the 90th percentile value of *HIGHLTV* (which is approximately 25%).

The figure shows a dramatic difference in the implied reaction of the two cities to a 1% income shock. In the high-leverage city, prices are up by .64% in the first year, as compared to only .19% in the low-leverage city. By the third year, the corresponding cumulative price movements are 1.23% and .68%. Thus in the high-leverage city, prices actually overshoot their new long-run value by a substantial margin. This overshooting reaches a peak in the fourth year, when the price increase hits 1.29% in the high-leverage city before turning around.

As a slight variation on the specifications in columns (1), (3), and (5) of Table 3, we also try including the lagged measure of leverage $DEBT_{t-1}$ itself in the regression

[9] In Table 3 and those that follow, our standard errors allow for both heteroskedasticity and correlation within each city-survey cluster. There are 111 of these clusters in our dataset. Intuitively, one does not want to treat four observations for which the right-hand-side leverage variable is the same (because it comes from a single survey) as being independent.

TABLE 3 The Effects of Leverage: Interactive Specificiation

Variable	*DEBT* = *HIGHLTV*		*DEBT* = *MEDIAN*		*DEBT* = *YESLOAN*	
dI_t	.077	.088	−.373	−.338	−1.451	−1.436
	(.46)	(.53)	(1.09)	(.98)	(2.05)	(1.96)
$dI_t{*}DEBT_{t-1}$	2.268	1.784	1.460	1.332	2.687	2.666
	(2.23)	(1.89)	(2.30)	(2.10)	(2.58)	(2.46)
$DEBT_{t-1}$		.071		.039		.010
		(2.00)		(1.76)		(.12)
dP_{t-1}	.516	.510	.526	.521	.500	.501
	(9.02)	(9.41)	(9.02)	(9.10)	(9.35)	(9.29)
P_{t-1}/I_{t-1}	−.192	−.188	−.192	−.190	−.189	−.189
	(7.77)	(8.17)	(7.76)	(7.92)	(8.53)	(8.48)
Implied average slope on dI_t	.39	.39	.41	.41	.32	.32
Number of observations	370	370	370	370	374	374
Adjusted R^2	.74	.74	.74	.74	.75	.75

Robust *t*-statistics in parentheses, allowing both for heteroskedasticity and for the residuals to be correlated within each of the 111 survey periods.

as an additional control variable. This is done in columns (2), (4), and (6) of the table. To some extent, leverage represents the outcome of an endogenous choice on the part of borrowers and lenders. If these agents are forward looking, they may be more willing to enter into high-LTV loans when house prices are expected to rise. Thus one might expect higher values of leverage to predict higher price appreciation. Indeed, for all three of our measures, high leverage today is positively correlated with future price appreciation; this conditional correlation is strongest (and either statistically significant or close to it) for those two measures that directly capture high LTV ratios, *HIGHLTV* and *MEDIAN*. However, for our purposes the important point is that including this

FIGURE 2

DYNAMIC RESPONSE OF PRICE TO INCOME, HIGH- VERSUS LOW-LEVERAGE CITIES: ESTIMATES BASED ON INTERACTIVE SPECIFICATION

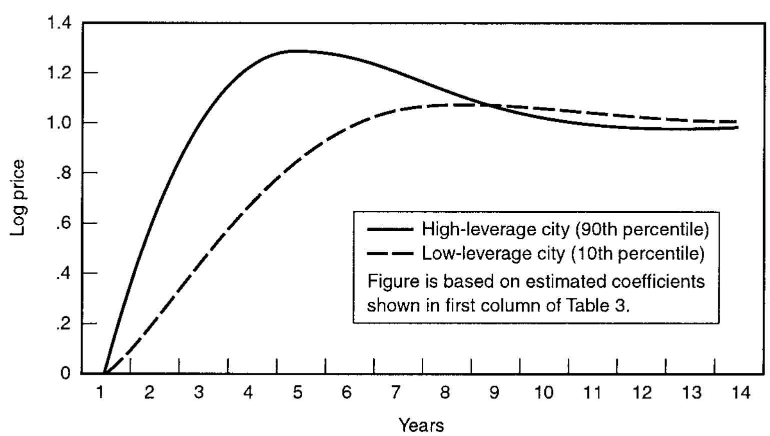
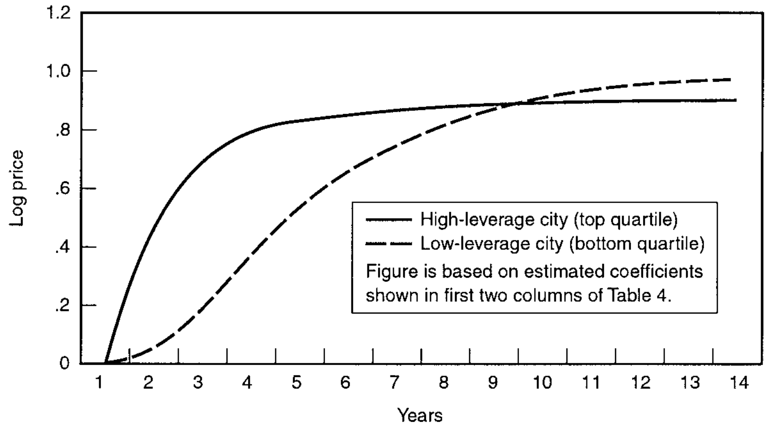


FIGURE 3
DYNAMIC RESPONSE OF PRICE TO INCOME, HIGH- VERSUS LOW-LEVERAGE CITIES: ESTIMATES BASED ON SPLIT-SAMPLE APPROACH



extra variable in the regression does not materially change the estimated coefficients on the key $dI_t{*}DEBT_{t-1}$ interaction term.

One concern with the regressions in Table 3 is that they are very tightly parameterized. First, they allow only the dI_t coefficient to vary with leverage, and they force the dP_{t-1} and P_{t-1}/I_{t-1} coefficients to be constant across cities with different leverage. Second, they impose a linear relationship between the leverage measures and the dI_t coefficient. Since some of these restrictions may not be warranted, we experiment in Table 4 with a much more loosely specified version of the same basic test. We now divide our sample into quartiles, sorted on the leverage variable $DEBT_{t-1}$, and run separate versions of the benchmark regression from column (6) of Table 2 for each quartile. The table reports the results for the top and bottom quartiles, using sorts based on each of our three definitions of leverage.

Three basic conclusions emerge from Table 4. First, prices still seem to respond more sensitively to income shocks in high-leverage cities: the coefficient on dI_t is always substantially larger in the high-leverage quartile. Moreover, even with the loss in statistical power that this method entails, the difference is strongly significant for the *YESLOAN* measure of leverage, and marginally significant for the *HIGHLTV* measure. Second, there is much less of a discernible pattern across quartiles in terms of the coefficients on dP_{t-1} and P_{t-1}/I_{t-1}. For example, the coefficient on dP_{t-1} is about the same across quartiles when we use *HIGHLTV*; it is higher in the high-leverage quartile when we use *YESLOAN*; and it is lower in the high-leverage quartile when we use *MEDIAN*.

Finally, consistent with these first two observations, the regressions in Table 4 yield impulse response functions that look quite similar to those implied by the regressions in Table 3. This is illustrated in Figure 3, which plots the impulse responses for the high and low quartiles according to our *HIGHLTV* measure of leverage.[10] The only noteworthy difference from Figure 2 is that while the high-leverage city still reacts much faster to an income shock, it no longer overshoots its new long-run value. Overall, then, the two types of specifications point to the same basic conclusions.

[10] Figure 3 is more or less directly comparable to Figure 2, because the midpoints of the bottom and top quartiles in Figure 3 are the 12.5th and 87.5th percentiles respectively, whereas in Figure 2 we define low and high leverage as the 10th and 90th percentiles respectively.

TABLE 4 The Effects of Leverage: Split-Sample Approach

Variable	*DEBT = HIGHLTV* High	Low	Difference
dI_t	.489	.039	.45
	(1.89)	(.28)	(1.54)
dP_{t-1}	.398	.376	.02
	(1.93)	(8.19)	(.11)
P_{t-1}/I_{t-1}	−.028	−.145	.12
	(.15)	(10.28)	(.63)
Number of observations	92	92	
Adjusted R^2	.53	.89	

"High" and "Low" indicate the top and bottom quartiles sorted on the lagged debt variable. Robust *t*-statistics in parentheses, allowing both for heteroskedasticity and for the residuals to be correlated within each of the 111 survey periods.

□ **Robustness checks.** Next, we investigate the extent to which our baseline results are robust to a couple of variations in estimation technique. In the interests of brevity, detailed tables are not provided; they can be found in a previous version of this article (Lamont and Stein, 1997). Moreover, the tests we discuss below represent modifications of our more tightly parameterized specification from Table 3. We have also examined the analogous modifications of the looser specification in Table 4; as one might expect based on the comparisons above, these yield very similar conclusions.

First, we check whether the results in Table 3 are due primarily to a few influential outliers. We sort the observations on both dP_t and dI_t, and discard the top and bottom 1% of the realizations for these two variables. This procedure actually results in a fairly substantial increase in the $dI_t{*}DEBT_{t-1}$ interaction coefficients in both the *HIGHLTV* and *MEDIAN* specifications. The point estimates in the *YESLOAN* case are somewhat reduced but still statistically significant. In sum, it seems clear that our results are not due to a handful of outliers, but rather reflect the central tendencies of the data.

Second, we take an alternative tack in dealing with the fact that the AHS survey occurs only once every four years for a given city, so that in our work with annual data we often have outdated measures of leverage. Thus far we have ignored this problem, using the stale data with no adjustments. We now try to do better. One approach is to construct an annual proxy for leverage using the four-year AHS data and other data that we have available annually. In doing so, we do not want to simply interpolate the four-year data, since this could potentially make the constructed leverage variable at any point in time contain information about future price movements.[11]

Instead, for the approximately 110 city-years in which we do have fresh measures of leverage, we run a "kitchen sink" first-stage regression of leverage on the start-of-period price-to-income ratio, as well as on current and once-lagged values of house price changes, growth in income per capita, and population growth. Using the estimated coefficients from this regression, we then can construct an annual projected leverage measure for each city and year. The advantage of this approach is that the projected leverage measure at any time t now only contains information available at that time.[12]

[11] To see why, suppose we proxy for *HIGHLTV* in year $t + 2$ by averaging observations of *HIGHLTV* at t and $t + 4$. The value of *HIGHLTV* at $t + 4$ may contain information about price movements after $t + 2$—e.g., if a sharp price rise in year $t + 3$ reduces LTVs in subsequent years.

[12] Except, of course, to the extent that the regression coefficients themselves are based on data from the entire sample period.

TABLE 4 ***Extended***

DEBT = *MEDIAN*			*DEBT* = *YESLOAN*		
High	Low	Difference	High	Low	Difference
.267	.001	.27	.477	−.215	.69
(1.12)	(.01)	(.78)	(2.66)	(1.60)	(3.10)
.360	.510	−.15	.708	.330	.38
(1.49)	(6.37)	(.60)	(6.34)	(4.04)	(2.74)
−.003	−.214	.21	−.211	−.112	−.10
(.01)	(6.76)	(1.12)	(2.80)	(4.14)	(1.25)
92	90		90	91	
.55	.80		.75	.80	

Next, we rerun the regressions of Table 3 but substitute in our projected leverage measures for the actual stale data. As one might have expected based on the idea that we are fixing a measurement error problem, the coefficients on the key $dI_t{*}DEBT_{t-1}$ term increase in all six specifications. In many cases, the magnitude of this increase is quite substantial. For example, in the first specification using the *HIGHLTV* measure, the coefficient of interest rises from 2.27 in column (1) of Table 3 to 3.03, an increase of approximately 33%.

5. The endogeneity of leverage

■ As discussed above, the biggest concern raised by our empirical approach is the possibility that our measures of leverage at the city level may be endogenous. We now discuss two ways in which this endogeneity problem can be addressed. First, we articulate a specific endogeneity-bias mechanism and try to show that it is not coloring our results. Second, we adopt an instrumental-variables estimation approach.

□ **The emerging-city hypothesis.** To see concretely how an endogeneity bias might arise, consider the following story, which we label the ‘‘emerging-city hypothesis.’’[13] The premise of this story is that some cities are in the process of undergoing fundamental transitions. Moreover, such transitions are purported to have two distinct effects. First, they are accompanied by increased migration into the city. This migration in turn impacts city measures of leverage; for example, it is plausible that newcomers to a city will buy homes with higher loan-to-value ratios, perhaps because they tend to be younger and thus have accumulated less wealth.[14]

Second, for cities in the process of transition, current economic shocks such as changes in per-capita income contain more information about future growth prospects. Consequently, forward-looking asset prices such as house prices should rationally respond by more to these economic shocks. If these two assertions are both correct, there will be a correlation between city measures of leverage and the sensitivity of house prices to income shocks, even if leverage plays no causal role.

We make two attempts to distinguish between our leverage-based hypothesis and this alternative. One approach is to assume that the extent to which a city can be characterized as ‘‘emerging’’ is more or less fixed over the ten-year duration of our

[13] We thank Anil Kashyap for pointing out this alternative hypothesis to us.

[14] Indeed, the data support the idea that increases in a city’s population growth are associated with significantly higher levels of homeowner leverage. See Lamont and Stein (1997) for more details.

TABLE 5 Interactive Specification with City-Specific Income Terms

Variable	*DEBT = HIGHLTV*		*DEBT = MEDIAN*		*DEBT = YESLOAN*	
$dI_t{*}DEBT_{t-1}$	3.950	2.900	3.854	3.862	1.824	2.829
	(2.17)	(1.36)	(2.61)	(2.68)	(.36)	(.49)
$DEBT_{t-1}$		.048		−.000		−.034
		(1.02)		(.02)		(.31)
dP_{t-1}	.513	.509	.509	.509	.510	.509
	(6.85)	(7.13)	(7.08)	(7.08)	(7.09)	(6.92)
P_{t-1}/I_{t-1}	−.182	−.177	−.183	−.183	−.180	−.180
	(6.34)	(6.45)	(6.36)	(6.29)	(6.17)	(6.15)
Number of observations	370	370	370	370	374	374
Adjusted R^2	.74	.74	.74	.74	.74	.74

Each of the 44 cities has its own separately estimated coefficient on real income changes (not shown). Robust *t*-statistics in parentheses, allowing both for heteroskedasticity and for the residuals to be corralated within each of the 111 survey periods.

sample period. If this identifying assumption is correct, we can completely control for the emerging-city phenomenon by using a city fixed-effects approach—i.e., by only looking at the effects of within-city variations in leverage and dummying out across-city variations.[15]

We implement this approach in Table 5. The specifications are the same as in Table 3, except that we allow each of the 44 cities to have its own coefficient on dI_t. Thus if some cities are more ''emerging'' than others over the entire sample, and hence have house prices that are more sensitive to income shocks, this will now be picked up in the city-specific dI_t coefficients, *not* in the $dI_t{*}DEBT_{t-1}$ interaction term. As it turns out, this specification does not reduce the interaction coefficients. In fact, in five of six cases, the interaction terms *increase* relative to Table 3, in some cases by quite a bit. Naturally, by removing all the across-city variation in our leverage measures, we reduce the precision of our estimates. Still, the interaction coefficients remain statistically significant in three of the six specifications.

One objection to this methodology is that the ''emerging'' characteristic is not fixed for cities over the entire ten-year sample period. For example, a city that was not emerging in 1984 may begin to emerge in 1990. If this is the case, things become more difficult. Now the best we can do is to control directly for any observable variables that are likely to proxy for the extent to which a city is emerging. One natural such candidate variable is population growth.

In Table 6, we run a horse race that effectively asks: Are our previous interaction results truly due to leverage effects, or merely to the fact that leverage is correlated with population growth? The regressions are similar to those in Table 3, with the following modifications. In columns (1), (3), and (5), we add a second interaction term, $dI_t{*}dPOP_{t-1}$, where $dPOP_t$ is defined as a city's population growth in the year from $t - 1$ to t. In columns (2), (4), and (6), we also add $dPOP_{t-1}$ by itself. Thus we treat the *dPOP* variable exactly symmetrically to the *DEBT* variable and let the data tell us which one better explains variation in the coefficient on dI_t. The answer is clear-cut.

[15] The city fixed effects that we add here are above and beyond those already in the baseline model. We have already allowed the average degree of price appreciation to vary city by city; now we are proposing to allow the sensitivity of prices to income to also vary city by city. Note that this approach could potentially deal with a variety of (though not all) endogeneity stories, beyond the specific one we focus on here.

TABLE 6 Competing Leverage and Population Growth Interactions

Variable	DEBT = HIGHLTV		DEBT = MEDIAN		DEBT = YESLOAN	
dI_t	.072	.077	−.375	−.339	−1.535	−1.537
	(.41)	(.44)	(1.10)	(.99)	(2.18)	(2.09)
$dI_t{*}DEBT_{t-1}$	2.241	1.718	1.481	1.320	2.868	2.883
	(2.19)	(1.81)	(2.29)	(2.04)	(2.70)	(.258)
$DEBT_{t-1}$		.072		.039		.004
		(2.01)		(1.75)		(.06)
$dI_t{*}dPOP_{t-1}$	.565	1.313	−.622	.388	−2.700	−3.059
	(.10)	(.26)	(.11)	(.07)	(.52)	(.61)
$dPOP_{t-1}$		−.012		−.024		.054
		(.06)		(.12)		(.26)
dP_{t-1}	.516	.510	.526	.521	.500	.498
	(9.06)	(9.66)	(9.11)	(9.43)	(9.31)	(9.47)
P_{t-1}/I_{t-1}	−.192	−.188	−.192	−.190	−.189	−.189
	(7.72)	(8.11)	(7.69)	(7.81)	(8.43)	(8.33)
Number of observations	370	370	370	370	374	374
Adjusted R^2	.74	.74	.74	.74	.75	.75

Robust *t*-statistics in parentheses, allowing both for heteroskedasticity and for the residuals to be correlated within each of the 111 survey periods.

The interaction terms involving $dPOP_{t-1}$ are completely insignificant, while those involving $DEBT_{t-1}$ are almost exactly identical to the ones in Table 3.[16]

Overall, Tables 5 and 6 are good news for the proposition that leverage exerts a causal influence on house-price dynamics. However, because they do not involve exogenous instruments for leverage, the possibility remains that this inference is muddled by some other as-yet unspecified endogeneity problem.

□ **An instrumental variables approach based on state bankruptcy laws.** Ideally, of course, we would like to have exogenous instruments for our leverage variables. One approach to generating such instruments is to take advantage of state-by-state differences in bankruptcy laws.[17] As it turns out, there is substantial variation across states in bankruptcy exemptions. Loosely speaking, these exemptions govern the amount that a debtor can shield from his unsecured creditors.

Gropp, Scholz, and White (1997) find that generous exemptions have the effect of limiting the aggregate flow of credit to some households; the idea is that unsecured lenders don't want to lend if they cannot recover much in bankruptcy. But more important for our purposes is the somewhat subtler point that large exemptions may actually increase the flow of secured mortgage lending, via a substitution effect. Exemptions do not prevent mortgage lenders from having access to their collateral in bankruptcy—effectively, a mortgage lender is senior to any bankruptcy exemptions. Thus one might expect that in states

[16] Our results are not sensitive to using just one year's lag of population growth. For example, we obtain similar numbers when we instead use population growth over the previous five years. We have also experimented with adding yet another competing interaction term—in this case, lagged price changes interacted with income growth. This change also has no discernible impact on the key *DEBT* interaction coefficients.

[17] We thank David Scharfstein for suggesting that we pursue this approach.

with generous exemptions, there would be more in the way of mortgage credit, as borrowers and lenders substitute away from unsecured credit. Evidence supportive of this hypothesis is documented by Hynes and Berkowitz (1998).

This insight—that there will be more mortgage lending in states with generous exemptions—forms the basis of our identification strategy.[18] We collected data on state-level homestead exemptions in 1983 from Gropp, Scholz, and White (1997). Including the District of Columbia, there are 29 states in our sample, of which four (Florida, Minnesota, Oklahoma, and Texas) had unlimited homestead exemptions in 1983. Eight of our total of 44 cities are located in these four states. Using Elias, Renauer, and Leonard (1995), we found that none of the states in our sample had changed the law on unlimited exemptions during our sample period. Thus our instrument for leverage does not vary across time, a substantial drawback.

As a first step, we need to check whether our leverage measures are (cross-sectionally) higher in cities with unlimited exemptions. Consistent with our hypothesis and with the earlier evidence of Hynes and Berkowitz, this turns out to be the case. More precisely, for each of our three *DEBT* measures, we run a purely cross-sectional regression (with 44 observations) where the dependent variable is the average of the *DEBT* measure in a given city over our sample period, and where the only explanatory variable is *DUMMY*, an indicator that takes on the value one if the city is located in a state with unlimited exemptions. We find that for each of the three measures, *DEBT* is positively correlated with *DUMMY*, significantly so for the measures *HIGHLTV* and *MEDIAN*. For example, with *HIGHLTV*, the coefficient on *DUMMY* is .07, with a t-statistic of 3.19. In this specification, the regression achieves a cross-sectional R^2 of .20.

Given the success of this first-step regression, we next proceed to run an IV version of the specification in Table 3. Everything is exactly as before, except we use $dI_t * DUMMY$ as an instrument for $dI_t * DEBT_{t-1}$. It should be noted that this instrumenting technique is a weak one, in the sense that we will be throwing away all the time-series variation in our *DEBT* measures, thereby greatly reducing the power of our tests. Unfortunately, this is the best we can do with this approach.

Table 7 displays the results of the IV estimation. The good news is that the point estimates on the key interaction term are still all positive, and for the most part they are quite similar in magnitude to what was seen in Table 3. For example, in column (2), using our favored *HIGHLTV* measure, the point estimate goes from 1.784 in Table 3 to 1.444 in Table 7. However, the bad news is that none of the estimates in Table 7 are even close to being statistically significant. Evidently, the power loss inherent in our IV approach is substantial. The bottom line is that while this method is clearly attractive in the sense of providing a clean ‘‘natural experiment,’’ and while it tends to produce estimates in line with what we obtained previously, its ability to provide a sharp and definitive answer to the endogeneity problem is limited.

6. Conclusions

■ Our empirical results are compactly summarized by the impulse responses depicted in Figure 2. The reaction of house prices to income shocks is markedly different across high- and low-leverage cities. In high-leverage cities, our baseline estimates suggest that prices react quite quickly to an income shock. In contrast, the price reaction in low-leverage cities is much more gradual. These differences are robust to a range of

[18] One might argue that what should matter for homeowners is their total debt (mortgage plus unsecured). However, unlike most unsecured debt, a mortgage loan is callable when the house is sold. Thus a big mortgage is more likely than credit card debt to deter a homeowner from trading up, since it makes the degree of liquidity constraint a function of whether or not his house is sold.

TABLE 7 Interactive Specification Using Bankruptcy Exemption Instrument

Variable	*DEBT = HIGHLTV*		*DEBT = MEDIAN*		*DEBT = YESLOAN*	
dI_t	.237	.129	.9513	−.086	−3.976	−3.514
	(.62)	(.34)	(.12)	(.11)	(.33)	(.40)
$dI_t{*}DEBT_{t-1}$	.993	1.444	.531	.827	6.442	5.756
	(.33)	(.47)	(.35)	(.54)	(.36)	(.44)
$DEBT_{t-1}$		.074		.041		−.043
		(1.86)		(1.84)		(.18)
dP_{t-1}	.506	.507	.508	.511	.507	.503
	(8.34)	(8.56)	(8.14)	(8.29)	(8.01)	(9.09)
P_{t-1}/I_{t-1}	−.193	−.188	−.193	−.190	−.181	−.183
	(7.67)	(8.12)	(7.70)	(7.89)	(4.44)	(5.41)
Number of observations	370	370	370	370	374	374
Adjusted R^2	.78	.78	.78	.78	.77	.78

The debt/income interaction term (but not the lagged level of debt) is instrumented for using a dummy variable indicating the presence of an unlimited homestead bankruptcy exemption. Robust *t*-statistics in parentheses, allowing both for heteroskedasticity and for the residuals to be correlated within each of the 111 survey periods.

variations in estimation technique. Moreover, to the best of our (admittedly limited) ability to sort it out, it appears that the relationship reflects causality running from leverage to house prices, as opposed to a spurious byproduct of the endogeneity of our city-level measures of leverage.

These results are consistent with the broad spirit of recent theoretical models that emphasize how borrowing can make asset prices more sensitive to fundamental shocks. At the same time, the results also serve to underscore weaknesses in some of the existing models. In particular, the empirical phenomenon documented in this article is an inherently dynamic one: as can be seen in Figure 2, the price gap between a high- and low-leverage city widens in the first couple of years after an income shock, and then gradually narrows after that. In contrast, the model of the housing market in Stein (1995) is static and thus cannot capture this dynamic adjustment process. Thus, one clear direction for future research involves building explicitly intertemporal models of house prices that can both accommodate leverage effects and at the same time generate empirically plausible price dynamics.

In this regard, there are two quite different approaches that one might take. On the one hand, one might try to stick within the confines of a fully rational model that—as in Kiyotaki and Moore (1997)—incorporates both intertemporal considerations and collateralized borrowing. It is not yet clear to us how far such a model will be able to go in terms of rationalizing the kinds of impulse responses seen in Figure 2.

Alternatively, one might superimpose leverage effects on top of a ‘‘behavioral’’ model of house-price dynamics. That is, one might begin by accepting the interpretation of Case and Shiller (1989, 1990), Cutler, Poterba, and Summers (1991), and many others who suggest that the short-run momentum and long-run fundamental reversion seen in house prices reflect an irrational speculative phenomenon.[19] The question would

[19] Recent theoretical articles by Barberis, Shleifer, and Vishny (1998), Daniel, Hirshleifer, and Subrahmanyam (1998), and Hong and Stein (forthcoming) build behavioral asset-pricing models that incorporate both short-run momentum and long-run reversals and that could in principle be adapted to study the sort of housing-market issues discussed here.

then be how homeowners' debt positions either temper or amplify these baseline speculative inefficiencies. Interestingly, in this sort of model, there might be cases where leverage actually has a net beneficial effect on housing-market efficiency. For example, if the gradual price adjustment seen in low-leverage cities reflects the fact that market participants systematically underreact in the short run to news about fundamentals, then to the extent that homeowner leverage accelerates the reaction of prices, it might be helping to make the market more efficient.

References

Abraham, J.M. and Hendershott, P.H. ''Bubbles in Metropolitan Housing Markets.'' *Journal of Housing Research,* Vol. 7 (1996), pp. 191–207.

Barberis, N., Shleifer, A., and Vishny, R.W. ''A Model of Investor Sentiment.'' *Journal of Financial Economics,* Vol. 49 (1998), pp. 307–343.

Bernanke, B.S. and Gertler, M. ''Agency Costs, Net Worth, and Business Fluctuations.'' *American Economic Review,* Vol. 79 (1989), pp. 13–31.

Case, K.E. and Shiller, R.J. ''The Efficiency of the Market for Single-Family Homes.'' *American Economic Review,* Vol. 79 (1989), pp. 125–137.

——— and ———. ''Forecasting Prices and Excess Returns in the Housing Market.'' *Real Estate and Urban Economics Journal,* Vol. 18 (1990), pp. 253–273.

Cutler, D.M., Poterba, J.M., and Summers, L.H. ''Speculative Dynamics.'' *Review of Economic Studies,* Vol. 58 (1991), pp. 529–546.

Daniel, K., Hirshleifer, D., and Subrahmanyam, A. ''Investor Psychology and Security Market Under- and Overreactions.'' *Journal of Finance,* Vol. 53 (1998), pp. 1839–1885.

Elias, S., Renauer, A., and Leonard, R. *How to File for Bankruptcy.* 5th ed. Berkeley, Calif.: Nolo Press, 1995.

Fisher, I. ''The Debt-Deflation Theory of Great Depressions.'' *Econometrica,* Vol. 1 (1933), pp. 337–357.

Garbade, K.D. ''Federal Reserve Margin Requirements: A Regulatory Initiative to Inhibit Speculative Bubbles.'' In P. Wachtel, ed., *Crises in the Economic and Financial Structure.* Lexington, Mass.: Lexington Books, 1982.

Goodman, J.L., Jr. and Ittner, J.B. ''The Accuracy of Home Owners' Estimates of House Value.'' *Journal of Housing Economics,* Vol. 2 (1992), pp. 339–357.

Gropp, R., Scholz, J.K., and White, M.J. ''Personal Bankruptcy and Credit Supply and Demand.'' *Quarterly Journal of Economics,* Vol. 112 (1997), pp. 217–251.

Hong, H. and Stein, J.C. ''A Unified Theory of Underreaction, Momentum Trading and Overreaction in Asset Markets.'' *Journal of Finance,* forthcoming.

Hynes, R.M. and Berkowitz, J. ''Bankruptcy Exemptions and the Market for Mortgage Loans.'' Working Paper, Federal Reserve Board, 1998.

Kashyap, A.K., Scharfstein, D.S., and Weil, D. ''The High Price of Land and the Low Cost of Capital: Theory and Evidence from Japan.'' Working paper, MIT, 1990.

Kiyotaki, N. and Moore, J.H. ''Credit Cycles.'' *Journal of Political Economy,* Vol. 105 (1997), pp. 211–248.

Lamont, O. and Stein, J.C. ''Leverage and House-Price Dynamics in U.S. Cities.'' Working Paper no. 5961, NBER, 1997.

Poterba, J.M. ''House Price Dynamics: The Role of Tax Policy and Demography.'' *Brookings Papers on Economic Activity* (1991), pp. 143–183.

Pulvino, T. ''Do Asset Fire Sales Exist? An Empirical Investigation of Commercial Aircraft Transactions.'' *Journal of Finance,* Vol. 53 (1998), pp. 939–978.

Shleifer, A. and Vishny, R.W. ''Liquidation Values and Debt Capacity: A Market Equilibrium Approach.'' *Journal of Finance,* Vol. 47 (1992), pp. 1343–1366.

Stein, J.C. ''Prices and Trading Volume in the Housing Market: A Model With Down-payment Effects.'' *Quarterly Journal of Economics,* Vol. 110 (1995), pp. 379–406.

Topel, R.H. and Rosen, S. ''Housing Investment in the United States.'' *Journal of Political Economy,* Vol. 96 (1988), pp. 718–740.