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A Simple Error Correction Model of House Prices*

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INTRODUCTION

Why does evidence to date suggest that the market for stocks and bonds is fairly efficient, but the market for housing may not be? One way to think about explaining how well the market "works" is to consider how well the supply side responds to effective demand. In a well-functioning market, increases in demand imply increases in the supply of housing. Prices remain stable. In a poorly functioning market increases in demand do not call forth sufficient supply, at least over some reasonable time frame, and instead prices rise.

Thus, a simple measure of how well housing markets work is the ratio of typical prices to typical incomes. Figures 1 through 3 illustrate the wide range of such measures over time, using data from several major metropolitan areas (MSAs). The heavy lines in Figures 1–3 show the ratio of median housing prices for each MSA to per capita incomes, year by year, from 1979 through 1996.[1]

These three charts were chosen to represent different types of patterns discernible in 133 MSAs that were plotted.[2] For comparison, the grand median of this ratio of price to per capita income (over all MSAs and years of data) is 4.5, with

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[1]Price data are from Census, Fannie Mae, and Freddie Mac; income data are from the Bureau of Economic Analysis. These data are described in some detail below, as are the thin lines in the Figures. The thin lines are estimated "equilibrium" house price to income ratios for the corresponding places and times. Again, the construction of these equilibrium measures is discussed in some detail below.

[2]The full set of plots is available at wiscinfo.doit.wisc.edu/realestate.

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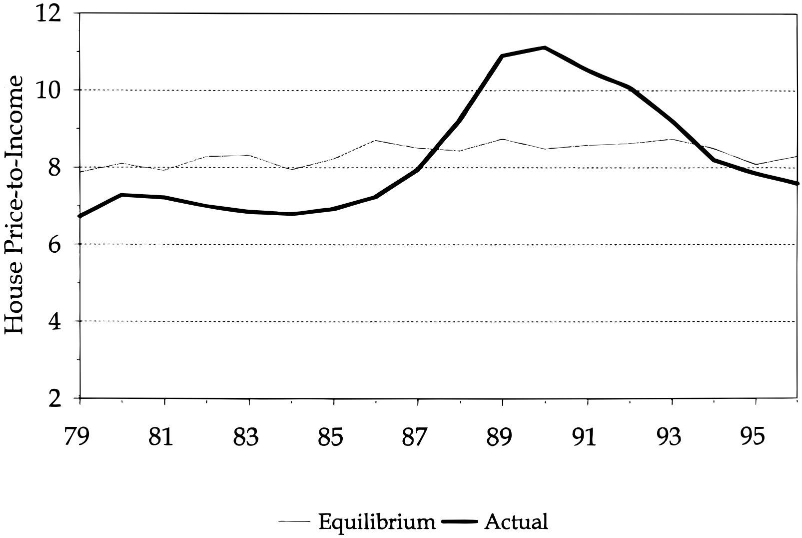

FIG. 1. Los Angeles house-price-to-income ratios: actual, and estimated equilibrium.

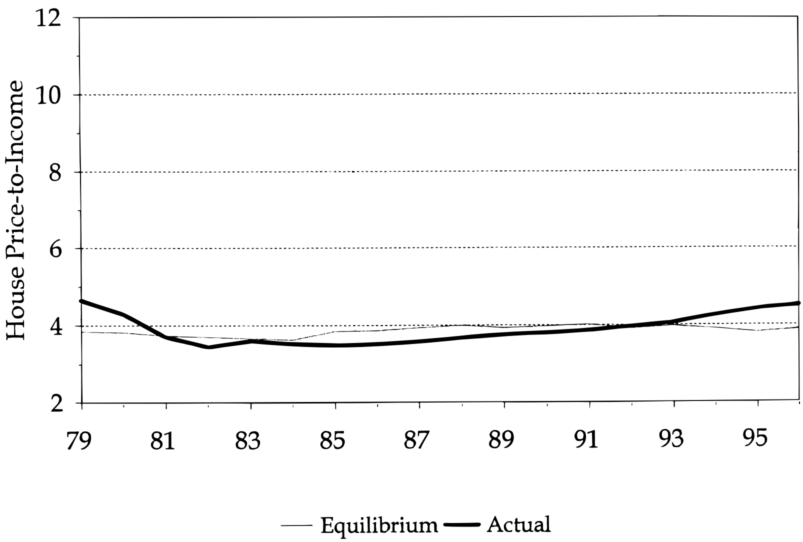

FIG. 2. Houston house-price-to-income ratios: actual, and estimated equilibrium.

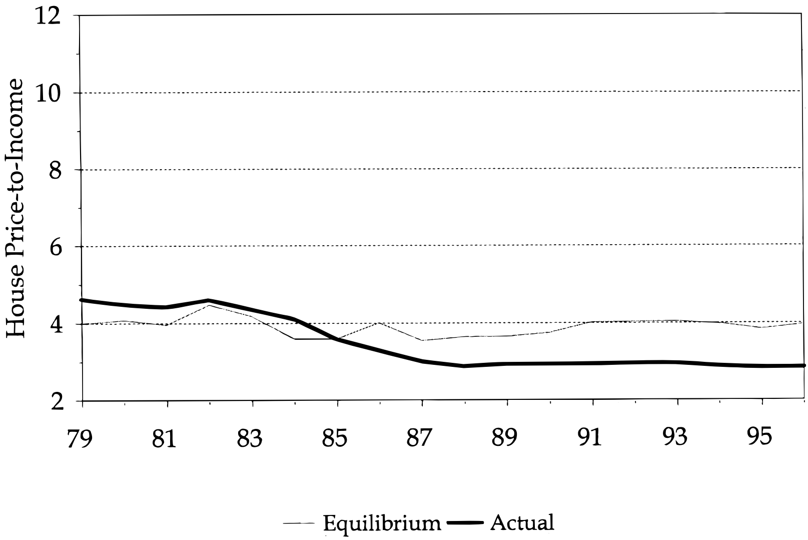

FIG. 3. Milwaukee house-price-to-income ratios: actual, and estimated equilibrium.

a mean of 4.9. Los Angeles (Fig. 1) represents an MSA with high and volatile prices in relation to incomes. Providence, New York, and San Diego are additional examples of volatile MSAs. Figure 2, Houston, is roughly similar to Tulsa and Baton Rouge, to give two other examples of declining markets. Tampa, Nashville, and Kansas City are roughly comparable to Milwaukee (Fig. 3), i.e., stable markets with prices near their estimated equilibrium level.

The approach to studying the forecastability of housing prices taken in this paper is a simple one. First we examine the nature of "equilibrium" housing prices. Next, we construct a simple model that tests whether prices tend to revert to some equilibrium ratio of price to income. Further, we investigate, in a preliminary way, how supply conditions—particularly the regulatory environment—affect both the equilibrium price and the time path of adjustment to equilibrium. Research on the determinants of house prices has demonstrated that government policies are important determinants of the *level* of prices (Malpezzi, 1996; Malpezzi *et al.*, 1998; Rose, 1989, among others).[3] But as Abraham and Hendershott (1996) have pointed out, little explicit empirical work has been done on the effect of regulation on housing price dynamics.

[3]Cross-country analysis has also demonstrated the relationship between policies and house prices (Malpezzi, 1990; Renaud, 1989; Malpezzi and Ball, 1993; Angel and Mayo, 1996).

PREVIOUS RESEARCH ON THE TIME PATH OF HOUSING PRICES

The literature on housing markets *per se* is enormous.[4] Here we restrict ourselves to a selection of empirical papers that focus on the following question: is it possible to consistently forecast the direction of changes in housing prices?[5]

While many points remain to be debated, many studies suggest, and others commonly asume, that markets for (U.S.) stocks and bonds are weak-form efficient.[6] That is, given public information, it is not possible to forecast the future time path of prices, at least consistently and precisely enough to offset the costs of trading on these forecasts. A market is said to be weak-form efficient if profitable trading rules cannot be derived from past prices.[7] A market is said to be semi-strong-form efficient if any such profits are less than the transactions costs of obtaining them. Note that transactions costs in housing are quite large, so the distinction makes a difference. It would be quite plausible to identify apparent opportunities to profit in housing markets where price differences do not exceed the transactions cost of buying and selling.

Housing markets differ from financial markets in important ways. They are illiquid, investments are heterogenous in the extreme (physically but especially with respect to location), transactions costs are large, information is particularly costly, and as a rough approximation every household consumes a housing unit whether its members think housing a good investment or not. Thus it is not surprising that evidence suggests that housing markets are not generally efficient in the sense financial markets are. If so, there may be information to exploit to predict the likely time path of prices. Better expectations about prices of the underlying asset straightforwardly yield better decisions about pricing and underwriting mortgages. Shiller (1993) provides a fascinating review of some of the consequences of incompleteness in housing markets.

Linneman (1986) was among the first papers to directly test the efficiency of this market.[8] Using micro level homeowner data from Philadelphia, Linneman estimated hedonic indices for two time periods, 1975 and 1978. The test showed that units with negative residuals in a hedonic equation in one period (loosely speaking, units that were lower than average in price given their level of characteristics) had higher than average appreciation in the succeeding

[4]See Green and Malpezzi (1998) for recent reviews of the U.S. housing market literature. See Malpezzi (1990) for reviews of housing markets in other countries.

[5]Cho (1996) provides a more comprehensive review.

[6]See Fama (1970) and Malkiel (1990) for cogent reviews. But see DeBondt and Thaler (1985) and Huizinga and Mishkin (1986) for some evidence to the contrary.

[7]See Fama (1970). Profits in this context are "economic" profits or "excess returns," i.e., in excess of the normal profit required to keep sufficient producers in the industry.

[8]Gau (1984, 1985) also tested for efficiency in real estate, but in the commercial market. Many papers which attempt to forecast housing prices implicitly address the efficiency issue, but here we review papers that address the issue explicitly.

period. Linneman found that public information was correlated with actual returns, but that in the great majority of cases possible excess returns were swamped by the costs of buying and selling to take advantage of those opportunities. That is, he found evidence of weak-form inefficiency, but the market was semi-strong-form efficient.

Several papers by Karl Case and Robert Shiller, notably Case and Shiller (1989), extended these results. Case and Shiller obtained appraisal data for Atlanta, Chicago, Dallas, and San Francisco/Oakland for 1970 to 1986. Using the method of repeat sales,[9] Case and Shiller constructed time series housing price indexes and regressed changes in prices against lagged values. If markets are efficient there should be no correlation between current and past returns. In fact there is such correlation, even after Case and Shiller corrected for some possible sources of spurious correlation. However, Case and Shiller note, a la Linneman, that transactions costs are high in housing markets and could eat up much of any potential profit. They do find that, under plausible assumptions about trading costs, some potential for excess profit exists, at least ex post. They note that forecasting individual house prices is so noisy that their aggregate results are swamped.

Some studies have examined the efficiency of the market in a single metropolitan area. Hill *et al.* (1997) is a good example. Using data from Baton Rouge, they applied a test for the presence of a random walk to 347 repeat sales. Hill *et al.* also applied their test to the well-known Case–Shiller data. Their Baton Rouge data permitted the use of a hybrid housing price model that combined hedonic and repeat sales data, which has been suggested in papers such as Case and Quigley (1991) as a more efficient method of price construction.

Hill *et al.* show that if housing prices follow a random walk, heteroskedasdicity will be induced in housing price changes. They test for heteroskedasdicity, as an indirect test for the presence of a random walk in house prices. They rejected in all hypotheses of the random walk in both Baton Rouge and the four cities studied by Case and Shiller.

Several papers have tested for speculative bubbles or inefficiencies using primarily lag variables. Gyourko and Voith (1992) analyzed housing price changes from 56 metropolitan areas over the period 1971 to 1989. They used proprietary median house price data from the WEFA Group. Using some straightforward specifications with trends and lags, they could not reject a hypothesis of equal apreciation over all metropolitan areas. This is a somewhat surprising finding, but may be due to the fact that in that part of their work they were looking at long-run 20-year trends in appreciation. They found that when year-to-year price changes were examined, some metropolitan areas demonstrated significant serial

[9]Basically using regression techniques to construct price indexes of regular periodicity from paired sales which occur at irregular periods. See Bailey *et al.* (1963) for the origins of the method, and Case and Shiller (1989, 1990) for some extensions.

correlation, consistent with the findings of Case and Shiller discussed above, and interpretable as evidence of housing market inefficiency. Gyourko and Voith also found some evidence of convergence in prices; that is, high initial housing price levels were associated with lower price changes in subsequent periods.

Another related literature is that of stock adjustment models fitted with aggregate national data. We can trace these back as far as to Muth (1960); more recent versions are well represented by DiPasquale and Wheaton (1994). DiPasquale and Wheaton solve a stock adjustment model for reduced form that incorporates a lagged adjustment from the annual percentage rate at which prices converge to an equilibrium price. They test their models with a number of specifications and data sources (including the Census median house price series and another aggregate series from Fannie Mae). A representative model regresses house prices against a vector of variables proxying equilibrium price including factor prices, user cost, and housing starts per household. The rate of price adjustment to equilibrium in their national model in the first year ranges from 16 to 29% depending on the model.

Meese and Wallace (1993) tested a disequilibrium model using housing price data from the San Francisco Bay area. They constructed a constant quality housing price series for the Bay area using data from 1970 to 1988. They estimated reduced form models for "fundamental price" and then estimated the price changes as a function of the difference between the quarterly price index and their estimated fundamental price. Meese and Wallace adopted an ingenious approach to deal with the simultaneity problem of estimating fundamental prices and price changes. They specified fundamental cost as a function of income, construction costs, the cost of capital, employment, and income. Rather than estimate fundamental price from the data, they gleaned a series of elasticities from a review of previous studies and use these parameters to estimate fundamental price in each period parametrically. In another variant they estimated the fundamental price directly from the data. In the event, they found that the speed of adjustment of actual to fundamental housing price was about 32% of the disequilibrium in a given year.

Abraham and Hendershott (1996) estimated a model which is in several respects similar in spirit to the model in this paper. They estimated a two-equation model, in which one model estimated equilibrium housing prices and the other adjustments to the equilibrium. Abraham and Hendershott estimated equilibrium prices by regressing contemporaneous prices against construction costs, income and employment growth, and changes in real after-tax interest rates. They then used the predicted value from this regression to proxy equilibrium prices and calculated logarithmic differences with actual prices as a measure of disequilibrium. In a second stage they regressed price changes as a function of this disequilibrium measure as well as construction costs, employment and income changes, after-tax interest rate changes, and a disequilibrium measure constructed from the first stage estimates. They also tested for serial correlation.

Abraham and Hendershott deal with the simultaneity problem by using the following procedure. They estimate their model first without disequilibrium measures to get estimates of equilibrium price changes, then integrate these over time to estimate equilibrium price levels. They found that real house price appreciation was affected as expected by construction cost, income changes, and changes in the real after-tax interest rate. This is true across several specifications and subsets of the data. The disequilibrium model performed well in national regressions using all MSAs with data, and in regressions using a subset of coastal cities. Results were insignificant for a subset of 16 inland cities. Abraham and Hendershott attempted to explain variation in their results across cities using indexes of supply restriction from Godschalk and Hartzell (1992) but found no economically meaningful results. They note that additional research on supply restrictions would be particularly welcome.

Many other papers have been published that yield some insights into the questions asked in this paper. For example, Alm and Follain (1994) estimate a series of models of equilibrium rents and asset prices for housing. But some variants of their model have partial adjustment. The model examines the steady state or equilibrium value of rent as well as how rent can deviate over time from its steady state value. But their simulation model is parameterized using a judgemental survey of literature, and provides no direct estimates itself.

Generally the literature on housing efficiency assumes that an investor will trade by buying and selling individual houses (or at least a small number relative to the market), and that the transactions costs he or she faces are those of buying and selling, e.g., brokerage commissions. But financial institutions are *pari passu* buying “the market” when they make decisions about whether to lend for real estate, and at what terms. Their transaction costs (and potential returns) are different from those of owners of the underlying asset, though the positions of the two are related. Unanswered questions, then, include the following. Do the results of Linneman, and of Case and Shiller, hold up when additional markets and time periods are studied? Given particular results about efficiency, is the information of use to financial institutions in making decisions about whether to lend and about underwriting criteria? Can we develop a forecasting model to improve such decisions?

A SIMPLE ERROR CORRECTION MODEL

As we saw in the brief review of housing price studies above, a number of approaches have been taken in the literature. Our goal here is to devcelop a model simple enough for application to a large number of markets with reasonable data collection and analysis costs, yet grounded in theory and (if possible) with some intuitive appeal.

Several recent papers (Malpezzi, 1990; Renaud, 1989; Malpezzi and Ball, 1993) have focused on the ratio between house prices and incomes. Generally these studies have suggested that in equilibrium, aggregate marketwide house price to income ratios range somewhere between 2 and 3. While discussions of such a ratio rest generally on empirical foundations, a simple model of a market with elastic supply side and unitary long run income and stock-price elasticities would yield a constant ratio.

One obvious shortcoming of the papers just cited for our present purpose is that they focus on international comparisons rather than the U.S., our focus here. However, we want to do more than simply replicate this type of analysis with U.S. data, because these studies are generally static (i.e., examine more or less a single cross-section of markets). For our purposes we prefer a dynamic model which tells us something about the likely time path of prices given an initial house-price-to-income ratio.

We therefore adopt a simple error-correction model (ECM) as our framework. The classic error-correction formulation begins by positing long-run relationships between a dependent variable (here house prices), lagged values of the dependent variable, and one or more independent variables (here income), with lag structures to be empirically determined.[10]

Following Malpezzi (1990) and Renaud (1989) we begin by positing a long-run equilibrium ratio between typical house prices, P, and incomes, Y:

$$\frac{P^e}{Y^e} = k. \tag{1}$$

Now, a number of studies such as Angel and Mayo (1996), Malpezzi (1990), and Renaud (1989) have posited or implied that there is some "natural" or desired k, i.e., that (at least over some range), a low k is a "good thing" and a high k is a "bad thing." More specifically, these papers have argued that a high price-to-income ratio is a symptom of an inelastic housing market and (presumably) one with significant capital misallocation. However, in this paper we take a less normative approach, at least for the moment, and examine equilibrium prices in the traditional sense.

An equilibrium price is defined as one from which there is no systematic tendency to depart, conditional on the values representing market conditions. However, there is no reason to believe that k is the same for all market conditions, or that for any of the usual reasons the relationship above could not be stochastic. Letting t denote the time period under consideration, we can re-represent the equilibrium condition for a representative market as

$$\frac{P_t^e}{Y_t^e} = k_t = Z\delta + \eta_t, \tag{2}$$

[10]Among many good introductions to error-correction models see Kennedy (1992, pp. 250–267). As Stuart Rosenthal has pointed out, our ECM is somewhat different from "the" stylized ECM typically estimated by macroeconomists. We return to this point below.

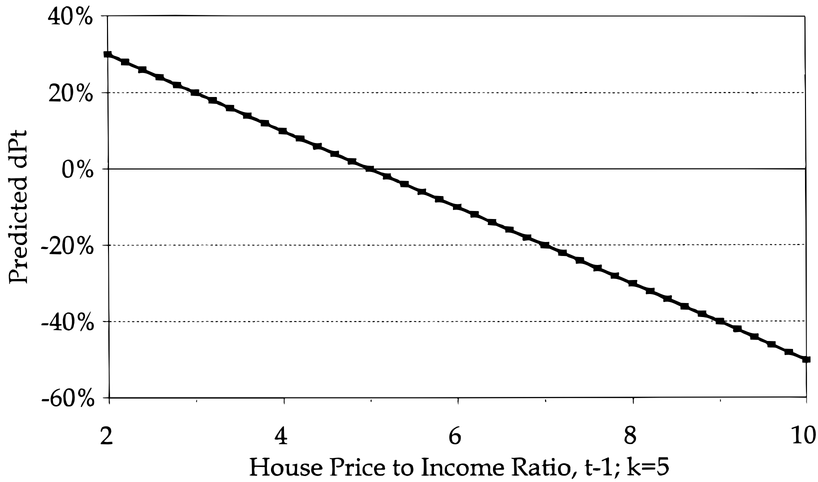

FIG. 4. First year price change from ECM: $k = 5$; $b_1 = -0.1$.

where Z is a vector of market conditions and other determinants of k, such as regulation; δ is the vector of corresponding parameters; and η is a well-behaved error term. Z could, in principle, be contemporaneous with t or could incorporate lagged values.

The equilibrium condition is, of course, only half of the model. The other half describes movement to k if a market has been "shocked" out of equilibrium. Given k, a simple linear ECM can be written,

$$dP_t = \beta_0 + \beta_1\left(\frac{P_{t-1}}{Y_{t-1}} - k\right) + \cdots \beta_n\left(\frac{P_{t-n}}{Y_{t-n}} - k\right) + X\alpha + \varepsilon_t, \tag{3}$$

where each term in brackets measures how far out of equilibrium the ratio is in a particular period, n is the order of lags, and ε is a well-behaved error term. Of course, there may be determinants of price changes other than disequilibrium and random shocks; we represent these as a vector X. Vector X represents market conditions, the regulatory environment, and so on. Our expectations are that β_0 would be zero and that the other β_i would be negative. That is, if the house-price-to-income ratio exceeds the equilibrium k, prices tend to fall; if the ratio is less than k, prices will likely rise.[11]

The simple model just posited implies that the expected change in price is simply proportional to the size of the (estimated) disequilibrium. This is illustrated in Fig. 4 for a one-period model ($n = 1$), where k has been (for now arbitrarily)

[11]We say "tend" and "likely rise" because of course in any given period actual price changes may differ from expected.

set to 5 and β_1 set to -0.1. That is, if the actual house-price-to-income ratio in the preceding period *was* 5 (was k), the expected price change is zero. If the ratio were 3, the expected price change this period would be 20% as prices rose toward their equilibrium value. If the ratio were 7, the expected price change would be a fall of 20% as prices fell toward their equilibrium value.

An alternative assumption is that "small" departures from equilibrium have less effect than "large" departures. This can be easily represented by incorporating cubic terms in the model,

$$dP_t = \beta_0 + \beta_1\left(\frac{P_{t-1}}{Y_{t-1}} - k\right) + \gamma_1\left(\frac{P_{t-1}}{Y_{t-1}} - k\right)^3 + \cdots \beta_n\left(\frac{P_{t-n}}{Y_{t-n}} - k\right)$$
$$+ \gamma_n\left(\frac{P_{t-n}}{Y_{t-n}} - k\right)^3 + X\alpha + \varepsilon_t, \tag{4}$$

where the γ_i are expected to be negative. Figure 5 shows an illustrative result for a one-period model with $\beta_1 = -0.05$ and $\gamma_1 = -0.005$.

The model can be extended in two directions, both related to the speed of adjustment. First, the speed of adjustment could be asymmetric, i.e., different if above the equilibrium level than if below. Second, supply side variables could affect the speed of adjustment and k. In this paper we test for the former, but focus especially on the latter, and in particular on regulation as a key supply side parameter.

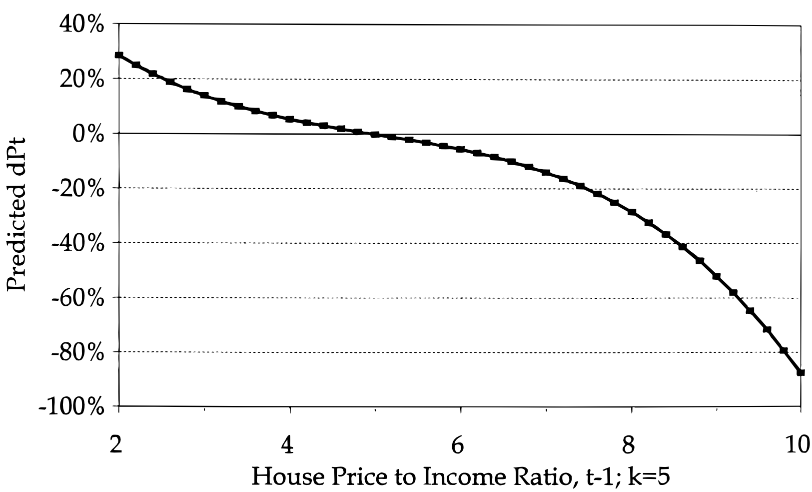

FIG. 5. First year price change from ECM: $k = 5$; $b_1 = -0.05$; $b_2 = -0.005$.

DATA

The housing price data used in this paper are constructed from two constituent parts: from the Fannie Mae/Freddie Mac repeat sales price indexes ("the Agency indexes") and from Census-based hedonic price indexes from Malpezzi *et al.* (1998). Income and population data are from the Bureau of Economic Analysis. Data on the regulatory environment and the physical geography of metropolitan housing markets are taken from Malpezzi (1996) and Malpezzi *et al.* (1998). In this section we discuss the key data series in more detail.

The Agency repeat sales data are used to measure price changes used as dependent variables for the second-stage error correction model and are also used to derive the house-price-to-income ratio used in the first stage of the model, which examines equilibrium prices. The Agency series are available quarterly, but we converted them to annual changes in order to match the periodicity of other data. Prices and all other monetary variables are deflated to their real values ($1992).

In order to calculate price levels for the first-stage estimation we need a benchmark place-to-place price index. We used the Census-based hedonic index for 1990 presented in Malpezzi *et al.* (1998). Price levels for other years were found by calculating forward or backward from 1990 using the Agency price change data. House-price-to-per-capita-income ratios are then calculated straightforwardly as this price level divided by the corresponding BEA per capital income figure for each metropolitan area and year. The data comprise 133 metropolitan areas and span 18 years from 1979 through 1996.

Of course it is well known that other price measures are available. In an earlier version of this paper we relied on housing price data from the National Association of Realtors. Their data on median sales prices from the multiple listing services are often criticized for a lack of quality adjustment. In fact, we estimated (but do not report) the same model presented in this paper, but using the NAR data with an ad hoc quality adjustment (see Hendershott and Thibodeau, 1990) in place of the price data described above. We found that our qualitative results were remarkably robust with respect to choice of the Agency/Malpezzi *et al.* price data versus the NAR data. Since the Agency data are still theoretically preferred, and also since they are available for more metropolitan areas over the period we study, for the rest of the paper we report only results from those data. However, the robustness of the results to choice of price index is certainly gratifying.

Most past discussions of the house price-to-income ratio (see Malpezzi, 1990; Renaud, 1989) have focused on the ratio of housing prices to median *household* incomes, given the natural correspondence between housing units and households. But unfortunately we do not have reliable and independent data on household incomes by MSA by time. Since BEA presents MSA specific data on *per capita* income over time we use this measure as an alternative. Again, each individual price and income datum, and their changes, are deflated using the GDP deflator.

Table I presents summary statistics on real house price changes for each MSA. The overall grand median of 2375 annual real price changes for our panel is 0.4%. While this is a small median change, with such a large sample it is unsurprising that a Wilcoxon sign test rejects the hypothesis that the median is zero. The distribution of price changes is roughly bell-shaped, although normality is rejected. The first quartile of all real annual changes is −3.1% and the third quartile is 3.6%. There are some significant outliers in both directions. The largest observed annual real price increase is an implausible 47% in Fort Worth. The largest annual real price decline, 38%, was observed in New Haven. The presence of such outliers suggests that robust estimation techniques be used, of which more below.[12]

Most past discussions of the house price to income ratio have focused on the ratio of typical prices to typical household incomes, given their natural correspondence. However, we do not have reliable and independent data on *household* incomes by MSA and by time period. The BEA does present MSA-specific data on *per capita* income over time; so we use these data.

Turning next to the ratio of MSA median prices to MSA average incomes per capita, we find that house price to per capita incomes range from 2.4 in Peoria in 1986 to 13.2 in Santa Cruz in 1989 (Table II). Over all periods and markets, the grand median ratio of house price to per capita income is 4.5, with a mean of 4.9. The first and third quartiles of the ratio are 3.8 and 5.5, respectively. The distribution of these ratios is more skewed than the distribution of price changes, but there are no obvious outliers.

Market conditions are represented by income and price variables, population, geographical variables, interest rates, and regulation. Income and price variables have already been discussed, and population is also from BEA. The geographic variables are taken from Malpezzi (1996) and are simple dummy variables for whether a metropolitan area is located on a major coastline (ocean or Great Lake). The adjacent park variable is a dummy variable for metropolitan areas located adjacent to a large national park, a military base, an Indian reservation, or another major constraint on expansion.

The regulatory index is taken from Malpezzi *et al.*(1998), and in turn builds on an index constructed for Malpezzi (1996). In the latter paper we constructed an index of the metropolitan regulatory environment for development based upon survey data collected by Linneman *et al.* (1990). In the former paper we constructed an instrument for this regulatory measure using market conditions and geographic dummy variables to deal with the potential endogeneity of this

[12]Eicholtz and Huisman (1998) find other housing price change series, in the U.S. and in the Netherlands, “thick in the tails.”

TABLE I
Real Annual Housing Price Changes

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Akron, OH	18	0.001	0.054	−0.119	0.086
Albany–Schenectady–Troy, NY	18	0.018	0.093	−0.178	0.205
Albuquerque, NM	18	0.005	0.052	−0.084	0.085
Allentown–Bethlehem, PA–NJ	18	0.024	0.091	−0.145	0.247
Ann Arbor, MI	18	0.011	0.083	−0.260	0.130
Appleton–Oshkosh–Neenah, WI	18	0.000	0.117	−0.350	0.308
Atlanta, GA	17	0.007	0.035	−0.044	0.095
Atlantic City, NJ	14	0.045	0.171	−0.316	0.382
Augusta, GA–SC	18	−0.006	0.036	−0.086	0.043
Austin, TX	18	−0.005	0.078	−0.187	0.111
Bakersfield, CA	15	−0.015	0.038	−0.095	0.061
Baltimore, MD	18	0.012	0.049	−0.075	0.117
Baton Rouge, LA	17	−0.015	0.046	−0.116	0.056
Bellingham, WA	17	0.021	0.105	−0.109	0.327
Bergen–Passaic, NJ	18	0.022	0.103	−0.125	0.236
Birmingham, AL	18	−0.001	0.051	−0.073	0.110
Bloomington–Normal, IL	17	−0.010	0.070	−0.182	0.091
Boston, MA	17	0.026	0.087	−0.118	0.243
Boulder–Longmont, CO	18	0.014	0.057	−0.108	0.122
Buffalo, NY	18	0.009	0.066	−0.126	0.165
Canton, OH	18	0.000	0.049	−0.113	0.061
Cedar Rapids, IA	18	−0.002	0.067	−0.166	0.082
Charleston, SC	16	0.003	0.065	−0.179	0.124
Charlotte–Gastonia–Rock Hill, NC–SC	18	0.013	0.036	−0.064	0.068
Chicago, IL	18	0.015	0.058	−0.117	0.132
Cincinnati, OH–KY–IN	18	0.000	0.042	−0.110	0.061
Cleveland, OH	18	0.001	0.050	−0.120	0.083
Colorado Springs, CO	18	0.004	0.057	−0.083	0.092
Columbia, SC	18	0.000	0.041	−0.127	0.055
Columbus, OH	18	0.007	0.039	−0.112	0.055
Dallas, TX	18	−0.013	0.054	−0.112	0.136
Davenport–Rock Island–Moline, IA–IL	16	−0.011	0.094	−0.279	0.100
Dayton–Springfield, OH	18	0.000	0.061	−0.161	0.066
Daytona Beach, FL	18	−0.010	0.051	−0.125	0.092
Denver, CO	18	0.000	0.047	−0.076	0.085
Des Moines, IA	18	−0.013	0.066	−0.204	0.082
Detroit, MI	17	0.023	0.052	−0.088	0.113
Eugene–Springfield, OR	15	0.027	0.062	−0.091	0.121
Evansville, IN–KY	18	−0.010	0.043	−0.093	0.065
Flint, MI	13	0.034	0.030	0.006	0.091
Fort Collins–Loveland, CO	18	0.006	0.065	−0.125	0.123

TABLE I—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Fort Lauderdale–Hollywood–Pompano Beach, FL	18	−0.004	0.047	−0.108	0.096
Fort Wayne, IN	16	0.024	0.126	−0.126	0.471
Fort Worth–Arlington, TX	18	−0.017	0.043	−0.113	0.074
Fresno, CA	13	−0.008	0.049	−0.063	0.094
Gary–Hammond, IN	16	0.009	0.057	−0.118	0.141
Grand Rapids, MI	18	0.013	0.051	−0.091	0.092
Green Bay, WI	18	−0.001	0.063	−0.205	0.071
Greensboro–Winston-Salem–High Point, NC	18	0.002	0.033	−0.089	0.051
Greenville–Spartanburg, SC	18	0.007	0.060	−0.206	0.089
Hamilton–Middletown, OH	18	−0.008	0.044	−0.108	0.045
Harrisburg–Lebanon–Carlisle, PA	18	0.011	0.045	−0.096	0.109
Hartford, CT	18	0.007	0.094	−0.099	0.231
Houston, TX	16	−0.019	0.056	−0.132	0.078
Huntsville, AL	17	−0.008	0.047	−0.107	0.075
Indianapolis, IN	18	0.003	0.039	−0.113	0.043
Jacksonville, FL	18	−0.003	0.029	−0.092	0.041
Kalamazoo, MI	18	0.005	0.055	−0.099	0.081
Kansas City, MO–KS	18	−0.008	0.039	−0.111	0.045
Knoxville, TN	18	−0.004	0.043	−0.095	0.053
Lancaster, PA	18	0.015	0.091	−0.260	0.175
Lansing–East Lansing, MI	18	0.007	0.060	−0.207	0.076
Las Vegas, NV	17	0.001	0.058	−0.096	0.162
Lexington–Fayette, KY	17	−0.010	0.043	−0.129	0.064
Lincoln, NE	18	−0.004	0.053	−0.132	0.069
Little Rock–North Little Rock, AR	18	−0.007	0.044	−0.098	0.085
Los Angeles–Long Beach, CA	18	0.023	0.125	−0.125	0.387
Louisville, KY–IN	18	0.010	0.068	−0.180	0.122
Madison, WI	18	0.008	0.057	−0.107	0.109
Melbourne–Titusville–Palm Beach, FL	18	−0.007	0.055	−0.105	0.118
Memphis, TN–AR–MS	18	0.000	0.060	−0.145	0.113
Miami–Hialeah, FL	16	0.004	0.045	−0.074	0.100
Middlesex–Somerset–Hunterdon, NJ	18	0.018	0.094	−0.108	0.227
Milwaukee, WI	18	0.001	0.077	−0.240	0.122
Minneapolis–St. Paul, MN–WI	18	−0.004	0.030	−0.073	0.037
Modesto, CA	18	0.005	0.102	−0.152	0.329
Monmouth–Ocean, NJ	18	0.017	0.100	−0.101	0.289
Nashville, TN	18	0.006	0.051	−0.122	0.067
Nassau–Suffolk, NY	17	0.032	0.086	−0.100	0.173
New Haven–Meriden, CT	18	0.016	0.176	−0.379	0.424

TABLE I—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
New Orleans, LA	18	−0.016	0.053	−0.102	0.064
New York, NY	18	0.031	0.092	−0.095	0.199
Newark, NJ	18	0.023	0.090	−0.112	0.228
Norfolk–Virginia Beach–Newport News, VA	18	0.003	0.045	−0.112	0.095
Oakland, CA	18	0.028	0.108	−0.066	0.360
Oklahoma City, OK	16	−0.015	0.074	−0.188	0.095
Omaha, NE–IA	18	−0.005	0.034	−0.084	0.051
Orlando, FL	18	0.004	0.030	−0.037	0.058
Peoria, IL	16	−0.004	0.091	−0.172	0.153
Philadelphia, PA–NJ	18	0.022	0.065	−0.077	0.163
Phoenix, AZ	17	−0.010	0.046	−0.095	0.058
Pittsburgh, PA	18	−0.001	0.068	−0.182	0.084
Portland, ME	17	0.008	0.085	−0.127	0.168
Portland, OR	17	0.017	0.067	−0.107	0.127
Providence, RI	18	0.018	0.100	−0.119	0.266
Provo–Orem, UT	16	0.022	0.071	−0.115	0.144
Racine, WI	17	−0.005	0.094	−0.271	0.166
Raleigh–Durham, NC	17	0.006	0.037	−0.098	0.058
Reading, PA	18	0.022	0.122	−0.314	0.256
Reno, NV	17	−0.001	0.035	−0.079	0.039
Richmond–Petersburg, VA	18	0.004	0.035	−0.072	0.061
Riverside–San Bernardino, CA	17	0.007	0.109	−0.143	0.343
Rochester, NY	18	0.004	0.053	−0.093	0.116
Rockford, IL	13	−0.005	0.083	−0.256	0.083
Sacramento, CA	18	0.009	0.092	−0.083	0.307
Saginaw–Bay City–Midland, MI	17	0.005	0.057	−0.114	0.092
Salem, OR	16	0.020	0.053	−0.092	0.112
Salinas–Seaside–Monterey, CA	18	0.022	0.081	−0.050	0.291
Salt Lake City–Ogden, UT	18	0.007	0.067	−0.096	0.144
San Antonio, TX	18	−0.013	0.100	−0.190	0.253
San Diego, CA	18	0.013	0.099	−0.062	0.332
San Francisco, CA	18	0.038	0.120	−0.130	0.358
San Jose, CA	18	0.047	0.138	−0.106	0.469
Santa Barbara–Santa Maria–Lompoc, CA	18	0.025	0.093	−0.133	0.269
Santa Cruz, CA	16	0.017	0.080	−0.132	0.199
Santa Rosa–Petaluma, CA	18	0.029	0.119	−0.064	0.434
Sarasota, FL	17	0.003	0.039	−0.060	0.085
Seattle, WA	18	0.021	0.094	−0.064	0.362
Springfield, IL	18	−0.007	0.111	−0.296	0.254
Springfield, MA	18	0.022	0.103	−0.121	0.238
St. Louis, MO–IL	18	−0.003	0.039	−0.110	0.048
Stockton, CA	17	0.007	0.081	−0.077	0.252

TABLE I—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Syracuse, NY	18	−0.001	0.073	−0.185	0.111
Tacoma, WA	18	0.017	0.052	−0.061	0.159
Tampa–St. Petersburg–Clearwater, FL	18	−0.006	0.024	−0.043	0.049
Toledo, OH	18	−0.004	0.067	−0.219	0.074
Trenton, NJ	18	0.027	0.152	−0.280	0.328
Tucson, AZ	18	0.001	0.062	−0.116	0.109
Tulsa, OK	17	−0.019	0.049	−0.112	0.083
Vallejo–Fairfield–Napa, CA	18	0.016	0.086	−0.075	0.294
Visalia–Tulare–Porterville, CA	14	−0.003	0.049	−0.067	0.117
Washington, DC—MD–VA	18	0.016	0.071	−0.103	0.182
West Palm Beach–Boca Raton–Delray Beach, FL	17	0.005	0.051	−0.125	0.090
Wichita, KS	18	−0.022	0.046	−0.153	0.062
Wilmington, DE–NJ–MD	18	0.022	0.062	−0.049	0.154
York, PA	18	0.004	0.052	−0.106	0.101

type of regulation. In this paper we use the instrumental variable for the regulatory environment.

PRETESTS OF THE DATA

The analysis below maintains certain classical assumptions, which are often violated with time-series data. In this section we briefly discuss some of the tests undertaken prior to analysis. We began by plotting autocorrelation functions and testing for unit roots for our dependent variables, real housing price changes and the house-price-to-income ratio.[13] Unsurprisingly, we can reject the hypothesis of a unit root (consistent with an assumption of stationarity) for price changes (a differenced variable). However, we could not reject the hypothesis of a unit root in the house-price-to-income ratio (a ratio of levels).

It is well known that two or more series, which are individually nonstationary, may be cointegrated, in which case error terms from a regression equation—a linear combination of the variables—will be stationary.[14] It is stationarity of this error term that is actually required for valid estimation of the model. We are able

[13]Unit root tests for panel data are discussed in Levin and Lin (1992) and Im *et al.* (1996). We use critical tables provided by Levin and Lin for our tests.

[14]More precisely, if the true error terms are stationary, and if we have the correct model, the estimated errors (the residuals) should exhibit stationary behavior.

TABLE II
Housing-Price-to-Income Ratios

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Akron, OH	18	3.68	0.34	3.21	4.30
Albany–Schenectady–Troy, NY	18	4.74	0.99	3.21	5.95
Albuquerque, NM	18	5.58	0.33	5.00	6.20
Allentown–Bethlehem, PA–NJ	18	4.92	0.89	3.75	6.17
Ann Arbor, MI	18	4.25	0.50	3.47	5.10
Appleton–Oshkosh–Neenah, WI	18	3.73	0.24	3.20	4.19
Atlanta, GA	17	4.72	0.21	4.44	5.09
Atlantic City, NJ	14	5.03	0.54	4.12	5.87
Augusta, GA–SC	18	4.28	0.13	3.95	4.52
Austin, TX	18	6.16	0.86	4.87	7.75
Bakersfield, CA	15	4.93	0.36	4.19	5.48
Baltimore, MD	18	5.25	0.57	4.45	5.93
Baton Rouge, LA	17	4.55	0.77	3.72	5.69
Bellingham, WA	17	5.22	0.95	4.02	6.61
Bergen–Passaic, NJ	18	7.91	1.76	5.47	10.77
Birmingham, AL	18	3.80	0.23	3.44	4.23
Bloomington–Normal, IL	17	3.33	0.31	3.07	4.22
Boston, MA	17	8.89	2.27	5.44	11.96
Boulder–Longmont, CO	18	5.50	0.58	4.84	6.83
Buffalo, NY	18	3.92	0.49	3.22	4.50
Canton, OH	18	3.56	0.32	3.24	4.27
Cedar Rapids, IA	18	3.22	0.31	2.89	3.90
Charleston, SC	16	5.23	0.21	4.81	5.53
Charlotte–Gastonia–Rock Hill, NC–SC	18	4.19	0.25	3.76	4.68
Chicago, IL	18	4.71	0.63	3.82	5.64
Cincinnati, OH–KY–IN	18	3.89	0.24	3.53	4.33
Cleveland, OH	18	4.02	0.35	3.57	4.65
Colorado Springs, CO	18	5.59	0.45	4.80	6.31
Columbia, SC	18	4.61	0.13	4.36	4.84
Columbus, OH	18	3.92	0.25	3.55	4.44
Dallas, TX	18	4.39	0.56	3.70	5.12
Davenport–Rock Island–Moline, IA–IL	16	3.24	0.61	2.69	4.54
Dayton–Springfield, OH	18	3.54	0.28	3.09	3.96
Daytona Beach, FL	18	5.06	0.15	4.86	5.42
Denver, CO	18	4.79	0.46	4.05	5.29
Des Moines, IA	18	3.30	0.32	3.05	4.24
Detroit, MI	17	3.32	0.39	2.66	4.03
Eugene–Springfield, OR	15	4.21	0.79	3.34	5.59
Evansville, IN–KY					
Flint, MI	13	2.83	0.27	2.50	3.34
Fort Collins–Loveland, CO	18	5.21	0.51	4.55	6.30
Fort Lauderdale–Hollywood–Pompano Beach, FL	18	5.29	0.25	4.96	5.88

TABLE II—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Fort Wayne, IN	16	3.23	0.26	3.05	4.11
Fort Worth–Arlington, TX	18	4.41	0.58	3.70	5.10
Fresno, CA	13	4.85	0.33	4.55	5.71
Gary–Hammond, IN	16	3.35	0.38	2.89	3.98
Grand Rapids, MI	18	3.77	0.29	3.32	4.41
Green Bay, WI	18	3.71	0.31	3.18	4.36
Greensboro–Winston-Salem–High Point, NC	18	4.28	0.18	3.97	4.57
Greenville–Spartanburg, SC	18	4.06	0.13	3.91	4.36
Hamilton–Middletown, OH	18	3.90	0.28	3.58	4.68
Harrisburg–Lebanon–Carlisle, PA	18	3.93	0.31	3.47	4.35
Hartford, CT	18	7.33	1.49	5.51	10.26
Houston, TX	16	3.44	0.73	2.85	4.62
Huntsville, AL					
Indianapolis, IN	18	3.44	0.18	3.16	3.75
Jacksonville, FL	18	4.21	0.16	3.97	4.47
Kalamazoo, MI	18	3.59	0.28	3.23	4.12
Kansas City, MO–KS	18	3.34	0.25	3.03	3.91
Knoxville, TN	18	3.80	0.21	3.60	4.33
Lancaster, PA	18	4.39	0.54	3.44	4.97
Lansing–East Lansing, MI	18	3.47	0.23	3.12	3.89
Las Vegas, NV	17	4.69	0.27	4.37	5.22
Lexington–Fayette, KY	17	4.55	0.23	4.28	5.22
Lincoln, NE	18	3.67	0.29	3.38	4.32
Little Rock–North Little Rock, AR	18	4.27	0.31	3.82	4.82
Los Angeles–Long Beach, CA	18	8.27	1.52	6.74	11.14
Louisville, KY–IN	18	3.48	0.27	3.11	4.03
Madison, WI	18	3.96	0.35	3.58	4.72
Melbourne–Titusville–Palm Beach, FL	18	4.43	0.22	4.05	4.92
Memphis, TN–AR–MS	18	4.70	0.18	4.39	4.98
Miami–Hialeah, FL	16	5.83	0.32	5.44	6.35
Middlesex–Somerset–Hunterdon, NJ	18	6.71	1.37	4.89	9.08
Milwaukee, WI	18	3.90	0.38	3.45	4.66
Minneapolis–St. Paul, MN–WI	18	4.25	0.17	4.05	4.63
Modesto, CA	18	5.78	0.64	4.96	7.16
Monmouth–Ocean, NJ	18	6.34	1.41	4.58	8.87
Nashville, TN	18	4.33	0.27	3.95	4.92
Nassau–Suffolk, NY	17	6.94	2.00	3.70	9.65
New Haven–Meriden, CT	18	5.78	1.37	3.85	8.27
New Orleans, LA	18	4.67	0.70	3.80	5.77
New York, NY	18	8.63	2.31	5.26	11.96

TABLE II—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Newark, NJ	18	7.75	1.68	5.46	10.51
Norfolk–Virginia Beach–Newport News, VA	18	5.29	0.30	4.72	5.76
Oakland, CA	18	7.69	1.17	6.19	9.81
Oklahoma City, OK	16	3.78	0.84	3.01	5.04
Omaha, NE–IA	18	3.47	0.21	3.18	3.93
Orlando, FL	18	5.05	0.15	4.65	5.29
Peoria, IL	16	2.88	0.57	2.36	4.42
Philadelphia, PA–NJ	18	5.29	0.91	4.08	6.53
Phoenix, AZ	17	5.06	0.44	4.45	5.60
Pittsburgh, PA	18	3.26	0.25	2.95	3.72
Portland, ME					
Portland, OR	17	4.16	0.68	3.39	5.57
Providence, RI	18	6.60	1.55	4.50	8.98
Provo–Orem, UT	16	6.59	1.00	5.58	8.72
Racine, WI	17	3.84	0.39	3.49	4.98
Raleigh–Durham, NC	17	5.07	0.35	4.49	5.63
Reading, PA	18	4.15	0.50	3.41	4.67
Reno, NV	17	4.58	0.24	4.28	5.03
Richmond–Petersburg, VA	18	3.93	0.18	3.64	4.19
Riverside–San Bernardino, CA	17	5.61	0.60	4.92	6.98
Rochester, NY	18	4.14	0.38	3.53	4.67
Rockford, IL	13	3.41	0.33	3.11	4.12
Sacramento, CA	18	5.87	0.73	5.05	7.35
Saginaw–Bay City–Midland, MI	17	2.95	0.33	2.59	3.79
Salem, OR	16	4.20	0.67	3.47	5.40
Salinas–Seaside–Monterey, CA	18	8.96	1.37	7.19	11.08
Salt Lake City–Ogden, UT	18	5.85	0.78	4.84	7.59
San Antonio, TX	18	5.57	0.86	4.48	6.96
San Diego, CA	18	7.75	0.94	6.66	9.57
San Francisco, CA	18	8.48	1.77	6.40	11.42
San Jose, CA	18	9.73	1.82	7.33	12.98
Santa Barbara–Santa Maria–Lompoc, CA	18	9.81	1.45	8.04	12.48
Santa Cruz, CA	16	10.04	1.77	8.19	13.21
Santa Rosa–Petaluma, CA	18	7.61	1.27	6.21	9.72
Sarasota, FL	17	4.89	0.20	4.53	5.22
Seattle, WA	18	5.00	0.75	4.14	5.96
Springfield, IL	18	3.53	0.32	3.18	4.47
Springfield, MA	18	6.15	1.60	3.89	8.53
St. Louis, MO–IL	18	4.03	0.21	3.64	4.42
Stockton, CA	17	5.28	0.62	4.51	6.42
Syracuse, NY	18	4.24	0.46	3.54	4.87
Tacoma, WA	18	4.97	0.56	4.32	5.91

TABLE II—*Continued*

Metropolitan area	Number of observations	Mean	Standard deviation	Minimum	Maximum
Tampa–St. Petersburg–Clearwater, FL	18	4.68	0.17	4.38	4.90
Toledo, OH	18	3.50	0.29	3.15	4.26
Trenton, NJ	18	6.18	1.30	4.24	8.40
Tucson, AZ	18	5.54	0.38	5.01	6.14
Tulsa, OK	17	3.47	0.60	2.89	4.47
Vallejo–Fairfield–Napa, CA	18	7.30	0.92	6.25	8.93
Visalia–Tulare–Porterville, CA	14	4.86	0.24	4.59	5.29
Washington, DC–MD–VA	18	6.40	0.82	5.33	7.66
West Palm Beach–Boca Raton–Delray Beach, FL	17	4.75	0.17	4.55	5.13
Wichita, KS	18	3.47	0.40	3.01	4.28
Wilmington, DE–NJ–MD	18	4.70	0.73	3.71	5.66
York, PA	18	3.79	0.33	3.23	4.11

to reject the null for the estimated error in the house-price-to-income cointegrating vector at the 1% level.

ESTIMATION PROCEDURE FOR EQUILIBRIUM HOUSE PRICE-TO-INCOME

A natural way to estimate k is to examine observations where subsequent changes in prices are approximately zero. That would suggest that the house-price-to-income ratio in the proceeding observation was more or less near equilibrium. Our prior belief that market conditions could also affect k suggests the strategy of selecting observations where subsequent price changes were near zero and regressing the house-price-to-income ratio in the proceeding observation against a vector of potential determinants of k such as regulatory stringency and city size.

An immediate problem surfaces. If we follow this procedure, our estimate of k, appearing in the right-hand side of Eqs. (3) and (4), depends on the subsequent change of price, the left-hand side of Eq. (2). We have a classic simultaneity problem. The regressor in Eq. (3) is correlated with the error in Eq. (2).

We handle this problem in an equally classic way. We use an instrumental variable approach to estimate changes in price purged of the error term in Eq. 2. Instrumental variables include exogenous variables described below, as well as dummy variables for each metropolitan area to pick up any fixed effects. We use this estimated change in price to select observations with price changes near zero (plus or minus 1%). Then we regress this restricted sample's house-price-to-income ratio against a vector of market determinants.[15]

Another natural way to select observations in or near equilibrium would be to select entire markets which did not exhibit any significant real price change over the period, or alternatively, to select markets for which the house-price-to-income ratio was extremely stable over the entire sample. In fact, some metropolitan areas have wide ranges of house-price-to-income ratios over time, while others are stable, as Table II shows. Many markets had house-price-to-income ratios that varied by less than 1 over 18 years; Greenville, South Carolina, Columbia, South Carolina, and Jacksonville, Florida were all below 0.5, for example. At the other extreme Nassau, Boston, and New York all had ranges over 6.

In the event, we combine these two methods in an ad hoc way that does take account of both forms of information. We estimate the equilibrium house-price-to-income ratio using a restricted sample of observations whose subsequent house price changes were less than 1% in absolute value. As noted, we use the instrumental variable for the house price change in this sample selection to deal with possible simultaneity bias. Finally, we weight observations inversely to the range of the house-price-to-income ratio over the data period. Thus, cities such as Greenville and Columbia would have a weight of about 2, while observations from cities like New York and Boston would have a weight of less than 0.2.[16]

Another specification issue relates to the treatment of intercepts in the panel used to estimate the equilibrium house-price-to-income ratio. It is common to include MSA-specific intercepts in such a panel, in the well-known fixed-effects model (Mundlak, 1978; Judge *et al.*, 1980; Kennedy, 1992).[17] However, two related problems arise with the fixed-effects approach in the first stage of our model. First, since we are dealing with a subset of observations presumed near equilibrium, we are examining only a subset of MSAs, and within many of them, only a few observations. For many such observations, MSA intercept-shifters will pick up most or all of the variance because of lack of degrees of freedom. Second, a well-known problem with the fixed-effects model is difficulty in predicting out of sample, i.e., for MSAs not included in the original panel. All inferences and predictions are conditional on the metropolitan areas in the estimation sample. Clearly if we wish to understand disequilibrium in the second stage we need to predict equilibrium house prices for exactly those MSAs not

[15]As it happens, the results are not dramatically different if the procedure is carried out using the original price change rather than the instrumental variable, suggesting that simultaneity is not as large a problem here as might be expected.

[16]There are often a few observations from volatile cities in the equilibrium sample, since even the most volatile cities have small price changes for some years.

[17]If additional information is brought to bear on the specific form of these errors, they can of course be explicitly modeled as random effects, as discussed in the references above. We do not have sufficient information to reliably adopt such an approach here.

included in the first stage sample. Thus, we estimate the first stage model with a single intercept.

RESULTS FOR EQUILIBRIUM HOUSE-PRICE-TO-INCOME RATIO

Table III presents the results of this regression, representing Eq. (2). The dependent variable is the logarithm of the house-price-to-income ratio in a given period if the subsequent period's estimated price change is near zero. The log of this ratio was used after preliminary regressions showed heteroskedasdicity that a logarithmic transformation mitigated. The regression explains about 60% of the variation in the dependent variable. The candidate determinants are the regulatory environment, some geographical variables, per capita income and its growth rate, population and its growth rate, and the nominal mortgage interest rate. The regulatory index and the geographic variables are metropolitan area effects that do not vary over time. The mortgage interest rate is a national one that varies over time but not place.[18] Income, population, and their changes vary with time and place.

An arresting finding from Table III is the very strong effect that the instrumental variable for regulation has on the equilibrium house price to income ratio. The *t*-statistic is 18, and the standardized coefficient is almost twice that of the next largest standardized effect. Markets with more stringent regulatory environments have higher equilibrium house-price-to-income ratios.

Other variables perform as expected. Population has a much stronger (and significant) effect on the regulatory environment than income. The mortgage interest rate has a negative effect, consistent with the notion that in markets with high nominal rates liquidity constraints do moderate housing prices.

Given the results from this regression, we can predict the value of the dependent variable for every observation, whether or not in assumed equilibrium. We can then exponentiate back to get the predicted equilibrium house-price-to-income ratio for that city and that year. This calculation is the source of the thin lines in Figs. 1 through 3, above. Note the difference in estimated equilibria between Los Angeles (a stringently regulated metropolitan area) and Houston and Milwaukee (moderate regulatory environments). Figure 6 plots the average equilibrium house-price-to-equilibrium ratio implied by the regression results, for the observed range of the regulatory index. Other variables are set at sample means. The range of equilibrium ratios is quite remarkable, from about 3 to about 8.

[18]A number of studies, such as Abraham and Hendershott (1996), use an after-tax real interest rate, as suggested by the user cost formulation of housing prices. We tested real versus nominal interest rates and found better results with nominal rates. This result could be interpreted as evidence of liquidity constraints in mortgage lending.

TABLE III
Model Equilibrium House Price–Income Ratio
(Dependent Variable: House-Price-to-Income Ratio)

Regulation (IV)	Coefficient	0.0809
	Standard error	0.0044
	t-statistic	18.3
	Prob > $\|t\|$	0.000
MSA adjacent to water	Coefficient	0.0066
	Standard error	0.0160
	t-statistic	0.4
	Prob > $\|t\|$	0.682
MSA adjacent to park	Coefficient	0.0633
	Standard error	0.0245
	t-statistic	2.6
	Prob > $\|t\|$	0.010
Log real income per capita	Coefficient	−0.0344
	Standard error	0.0588
	t-statistic	−0.6
	Prob > $\|t\|$	0.559
Annual change in real income per capita	Coefficient	−0.8104
	Standard error	0.4577
	t-statistic	1.8
	Prob > $\|t\|$	0.077
Log population	Coefficient	0.1007
	Standard error	0.0099
	t-statistic	10.1
	Prob > $\|t\|$	0.000
Annual change in population	Coefficient	3.7888
	Standard error	0.6435
	t-statistic	5.9
	Prob > $\|t\|$	0.000
Mortgage interest rate	Coefficient	−1.0022
	Standard error	0.4406
	t-statistic	−2.3
	Prob > $\|t\|$	0.023
Intercept	Coefficient	−0.7125
	Standard error	0.5768
	t-statistic	−1.2
	Prob > $\|t\|$	0.217
Degrees of freedom		423
Adjusted R^2		0.61
Sample: Observations with subsequent IV for price change near *z*		

ESTIMATION PROCEDURE FOR HOUSE PRICE CHANGES

Following Eq. (3), armed with our estimates of *k*, we can calculate the disequilibrium measure for each market at each year by taking the difference between the observed house-price-to-income ratio and the predicted equilibrium value.

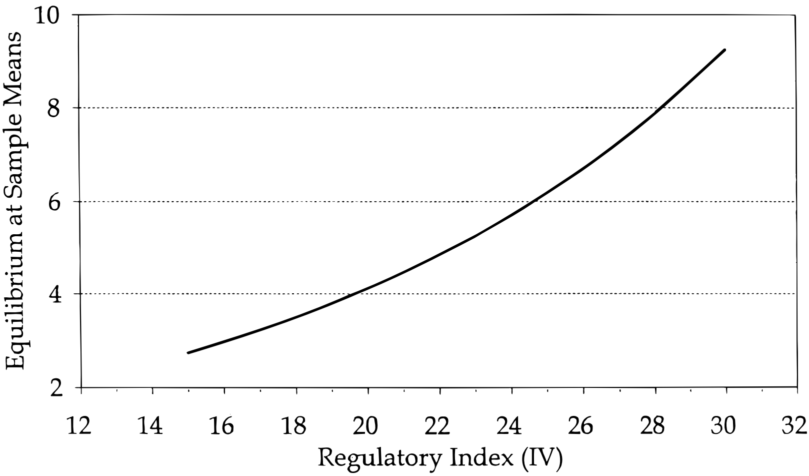

FIG. 6. Equilibrium house-price-to-income ratios by level of regulation.

This is our basic measure of disequilibrium. In Eq. 4, we suggested that if large departures from equilibrium mattered more than small, a cubic functional form might be appropriate. The disequilibrium measure is straightforwardly cubed to obtain this variable.

The median disequilibrium measure is 0.04, with a mean of 0.23; i.e., it is skewed to the right. In fact, there are a few outlying observations which are extraordinarily large. In preliminary results we found that, unsurprisingly, these few observations were extraordinarily influential and were outliers in any model that they were estimated in. We used a very conservative definition of an outlier: three interquartile ranges above the third quartile. The few observations beyond this quite generous cut-off were eliminated from further analysis (see Tukey, 1977).

In addition to our bellwether disequilibrium measure, appropriately lagged, we also included population and real income per capita changes on the right hand side, as well as national mortgage rates and the regulatory index. Our priors would be that disequilibrium measures, both linear and cubic, would have negative coefficients. From the proceeding discussion we also can test for the existence of a cubic effect in disequilibrium. We can examine whether adjustment is symmetric about equilibrium, i.e., whether markets move faster to equilibrium if a price shock is positive as opposed to negative, or vice versa. We can also test for the effects of higher order lags in this equilibrium.

Income and population are expected to have positive effects on price changes (conditional on disequilibrium and the other measures). Mortgage interest rates would depress the price changes, following the argument in the proceeding paragraph, and we would expect higher house price increases in more stringently regulated markets, especially conditional upon income and population changes. In addition to entering regulation directly, we can examine its effect on the speed of adjustment to equilibrium by segmenting the model and testing interaction terms.

In the first stage, we eschewed the fixed-effects model, because we had few observations for some MSAs and none for others. This was a particular problem since we had to use the first stage to predict equilibrium price-to-income ratios out of sample. Such is not the case in the second stage. Thus we estimate a number of fixed effects models in this second stage, permitting intercepts to vary by MSA.

Of course, potential problems remain with the fixed-effects models, even with this larger sample and no immediate need to predict out of sample. Fixed-effects models are often interpreted as a measure of our ignorance; that is, MSA-specific intercepts pick up the effect of omitted or mismeasured MSA-level variables. We note in particular that the regulatory environment varies by MSA, and that we measure this imperfectly when we use the regulatory index from Malpezzi (1996) and even more so when we use the instrumental index from Malpezzi *et al.* (1998). To the extent that regulatory differences will be well captured by such intercept shifters,[19] we may find our imperfect indexes performing less well. However, the focus of our attention in results below will be on disequilibrium measures, which we believe to be better (not perfectly) measured. Also, our prior—consistent with our measurements—is that unlike regulation, disequilibria vary a lot over time within an MSA, as well as across MSAs. Thus fixed effects will probably pick up more errors in our measurement of the regulatory environment than in our measurement of disequilibria.

RESULTS FOR HOUSE PRICE CHANGES

Armed with the disequilibrium measures described above, we estimated several versions of Eqs. (3) and (4). In the event, equilibrium measures were always negative and significant in the linear term, but never significant in the cubic term. Also, *t*-tests showed that we could not reject the hypothesis that adjustment above and below equilibrium proceeded at the same rate. Finally, when higher order lags were introduced results became unstable. Typically a large initial disequilibrium coefficient of one sign would be followed by an almost equally large coefficient of the opposite sign for the subsequent period.

Thus after initial experimentation, including testing for robustness following Belsley *et al.* (1980), we settled on a very simple model with a single disequilibrium measure, lagged one period (i.e., constraining adjustment to be identical

[19]Though such intercept shifters undoubtedly capture other omitted variables and misspecifications, so we would not push the interpretation of these dummies as entirely or even primarily measuring regulation.

above and below equilibrium and neglecting higher order lags). Given the few observations with outliers in house prices changes discussed above, we found qualitatively similar but generally superior results using Welsch's bounded influence estimator. These regressions are the ones reported in the paper. We also found that a fixed-effects model provided superior results, so we concentrate on reporting results from that model here (although some other results are reported).

Table IV presents a summary of results for several estimates of Eq. (3), although the intercepts are suppressed for readability. The first column of Table IV presents the estimate of Eq. (3) for the entire estimation sample. The dependent variable is annual real change in house price, based on the deflated Agency repeat sales indexes, described above. Our disequilibrium measure is constructed by taking the difference between each observation's predicted house-price-to-income ratio, using the results in Table III and the observed ratio. It is lagged one period. Our regulation measure in column 1 is the instrumental variable of Malpezzi *et al.* (1998). Column 1 presents a fixed effects model, i.e., one with a separate intercept for each MSA.

This first version of the model explains 28% of the variation in price changes. With one notable exception, all variables perform as expected. The t-statistics are all high. Probabilities of observing such high t-statistics under nulls range from less than 1 in 100 to 1 in 10,000.

According to these results, a one-unit increase in disequilibrium is associated with a subsequent 2.7% drop in housing prices. Figure 7 presents a graphical representation of the first-year price change predicted by this coefficient for various house-price-to-income ratios, and assuming k is set at the sample median of 4.5. Roughly, if the house-price-to-income ratio in a given ratio is about 3, we can expect an average real price change the next period of about 5%. On the other hand, if the ratio is, say, around 8, we can expect a subsequent first-year real price drop of 9%.

Higher incomes are also associated with higher housing prices, as are faster population growth rates. Higher mortgage rates depress price changes. The unexpected result alluded to above is this: higher levels of regulation, measured with our instrumental variable, are associated with *lower* house price increases, conditional on the rest of the model.

Now, a wide range of literature has demonstrated that higher levels of regulation are associated with higher levels of housing and land prices, including (for example), Angel and Mayo (1996), Black and Hoben (1985), Fischel (1990), Malpezzi and Ball (1993), Rose (1989), Segal and Srinavasan (1985), and Shilling *et al.* (1991). What is more, this very measure has been shown to drive higher housing price levels in Malpezzi *et al.* (1998). What's up with our changes?

The simple correlation of this regulatory index and the dependent variable (real price changes) is $+0.11$ in our sample. While this is not large in absolute terms, the probability of observing such a correlation under the null is less than

TABLE IV
Model House Price Changes (Dependent Variable: Annual Real Change in House Price)

		All observations	All observations	All observations	Low–moderate regulation	Stringent regulation	All observations	All observations
Disequilibrium measure	Coefficient	−0.0272	−0.0257	−0.0089	−0.0312	−0.0265	−0.0689	−0.0497
	Standard error	0.0019	0.0023	0.0015	0.0026	0.0039	0.0178	0.0143
	t-statistic	−14.09	−11.41	−5.79	−11.90	−6.80	−3.88	−3.48
	Prob > $\|t\|$	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0005
Type of regulation index		IV	Actual	IV	IV	IV	IV	Actual
Regulation index	Coefficient	−0.0129	0.0135	0.0034	−0.0114	0.0089	−0.0128	0.0133
	Standard error	0.0048	0.0068	0.0007	0.0061	0.0185	0.0048	0.0068
	t-statistic	−2.66	1.98	5.00	−1.88	0.48	−2.65	1.95
	Prob > $\|t\|$	0.0080	0.0486	0.0001	0.0604	0.6283	0.0082	0.0511
Interact disequilibrium and regulation	Coefficient						0.0018	0.0010
	Standard error						0.0008	0.0006
	t-statistic						2.3610	1.7070
	Prob > $\|t\|$						0.0183	0.0883
Annual change in real income per capita	Coefficient	0.7734	0.8070	0.8964	0.7803	0.8150	0.7772	0.8158
	Standard error	0.0666	0.0885	0.0798	0.0670	0.1887	0.0665	0.0884
	t-statistic	11.61	9.12	11.24	11.65	4.32	11.68	9.23
	Prob > $\|t\|$	0.0001	0.0001	0.0001	0.0001	0.001	0.0001	0.0001
Annual change in population	Coefficient	2.0745	2.4297	0.5677	2.0539	2.3298	2.0779	2.4763
	Standard error	0.1690	0.2473	0.1159	0.1684	0.5014	0.1687	0.2481
	t-statistic	12.28	9.83	4.90	12.20	4.65	12.31	9.98
	Prob > $\|t\|$	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
Mortgage interest rate	Coefficient	−0.5602	−0.5373	−0.4851	−0.4779	−0.8499	−0.5211	−0.5047
	Standard error	0.0532	0.0688	0.0645	0.0538	0.1682	0.0558	0.0710
	t-statistic	−10.53	−7.81	−7.52	−8.88	−5.05	−9.36	−7.11
	Prob > $\|t\|$	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
Fixed effects?		Yes	Yes	No	Yes	Yes	Yes	Yes
Degrees of freedom		1868	803	1991	1450	413	1867	802
Adjusted R^2		0.28	0.35	0.15	0.32	0.19	0.28	0.35

Fixed effects model: Intercepts for each MSA. If no, one intercept. Intercepts available upon request.
Estimator: Bounded influence regression (Welsch, 1980).
Type of regulation: Actual is Malpezzi (1996) index for 56 MSAs. IV is Malpezzi *et al.* (1998) instrumental variable for 242 MSAs.
Regulation: If Malpezzi–Chun–Green index <22.5, low–moderate; else stringent.

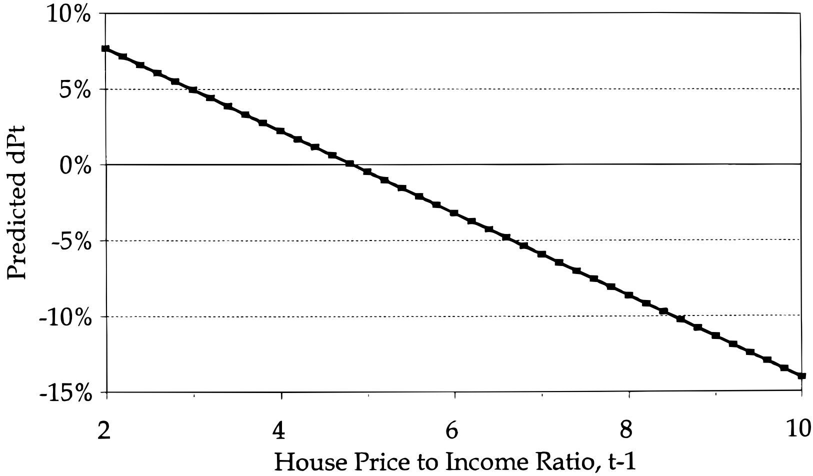

FIG. 7. First year price change from ECM: $k = 4.5$; $b_1 = -0.0272$; $b_2 = 0$.

one in 10,000. Thus the sign of the variable changes in the model. But of course it is the sign in the model that we are interested in.

The second column of Table IV presents an identical model, except that we replace the Malpezzi *et al.* instrumental variable with the original Malpezzi (1996) index. Notice that in this model, regulation has the expected sign and is significant. It would thus be tempting to rely on the original index. But it is quite plausible that the regulatory environment is in turn a function of price changes. Avoiding possible biases due to this endogeneity is exactly why we constructed the instrumental variable in the first place. However, note a very reassuring result: the other results, including the coefficient of disequilibrium, hardly change across columns 1 and 2.[20] Thus, while we may have some difficulty teasing out the effect of regulation on price changes, conditional on other variables, our other results, including the one we are most interested in, are not much affected.

In our discussion of the fixed-effects specification above, we pointed out our prior that the MSA-specific intercepts might pick up the effect of any mismeasurement in the regulatory index. To the extent this is so, we could argue that the "true" measure of regulation is some unknown mixture of our imperfect measure and some of the fixed effect. This seems plausible when we examine column 3 of Table IV where we present a very restrictive model with a single intercept (no fixed effects). In this restrictive model regulation has the expected sign. But other results are generally less impressive; e.g., the *t*-statistics for disequilibrium

[20]This is particularly gratifying since not only has the specification of regulation changed, but the regression is estimated on the much smaller sample of 55 MSAs for which we have the original regulatory index. Thus, results seem robust with respect to choice of MSAs in the sample.

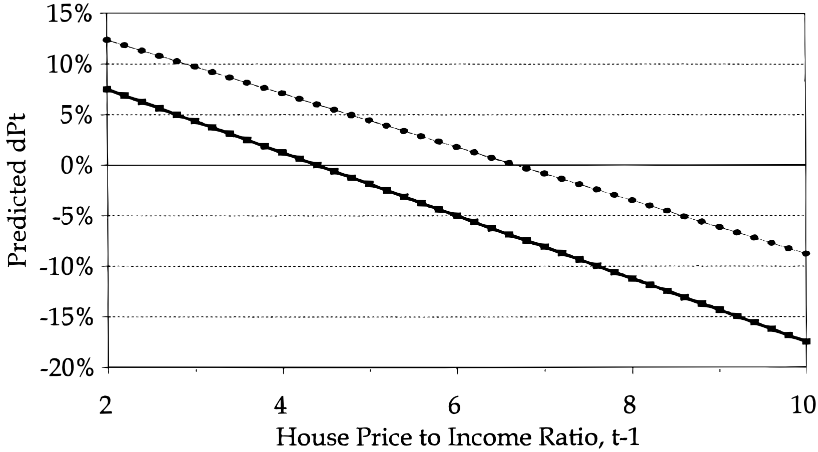

FIG. 8. First year price change from ECM: representative high and low regulation MSAs.

measures, population changes, and interest rates fall substantially. Point estimates for our disequilibrium measure and for population changes also fall quite a bit.

Taking these arguments and results together, we henceforth focus on fixed effects models using the instrumental variable. The downside is that the instrumental regulatory variable is perhaps incomplete as a measure of regulation, and that unobserved true regulation is some combination of this variable and some unknown component of the fixed effects. This leaves us a little less informed than we'd like to be about the effect of regulation on price changes, conditional on other variables. But the results give us apparently robust information on the effects of other variables.

In the past few paragraphs we have been considering in some detail the effect of regulation on housing price increases, conditional on other variables, i.e., even in the absence of disequilibrium. Another way of thinking about the effects of regulation is that markets with higher regulation may be slower to adjust back to equilibrium if shocked away. One natural way to test this is to segment the sample into stringent and low-moderate regulatory environments, to estimate separate equations, and to examine changes in the coefficient of the disequilibrium measure. Columns 5 and 6 of Table IV do so.[21] Clearly the general performance of the equation is inferior in the sample of markets with stringent regulation, as is to be expected. In particular the coefficient of the disequilibrium measure is significantly lower, although still negative and significant.

Another way to present these results is to graph them. Figure 8 presents the simulated one-period price change given departures from equilibrium in either

[21]We broke the sample at a value of 22.5 for the regulatory index. This somewhat arbitrary breakpoint is the same one used in Malpezzi (1996) and Malpezzi *et al.* (1998).

direction, similarly to Fig. 7. The top line in Fig. 8 represents the predicted first-year price change for a stringently regulated MSA, with a subsample median equilibrium house-price-to-income ratio of 6.5. The lower line represents the low–moderate regulation markets, with a subsample median equilibrium of 4.2.

Figures 7 and 8 present first period changes only. A market with an initial shock which pushes house prices out of equilibrium in either direction is followed by a significant price change, according to our estimates. However, this change is rarely sufficient to bring the price back to equilibrium in one period. We can therefore simulate the time path of housing prices given an initial shock in a period-by-period adjustment. Two representative simulations are presented in Fig. 9 (for low–moderate regulation MSAs) and Fig. 10 (for stringently regulated MSAs). Other variables, including the equilibrium house-price-to-income ratio, are set to subsample means.

Clearly these are highly stylized simulations, since over a 10-year period a market that has had an initial shock may well have a second or third. But it does give us a rough sense of how fast these results imply convergence to the long-run equilibrium. For markets with higher regulation the convergence is clearly much slower and to a higher level (ratio).

Another natural specification is to permit continuous interaction between regulation and disequilibrium (rather than a discrete and somewhat arbitrary sample division). For notational convenience let D represent the disequilibrium term from Eq. (3), above, let R represent our index of regulation, and let X represent all other right hand side variables (including intercepts). Rewrite Eq. (3) as

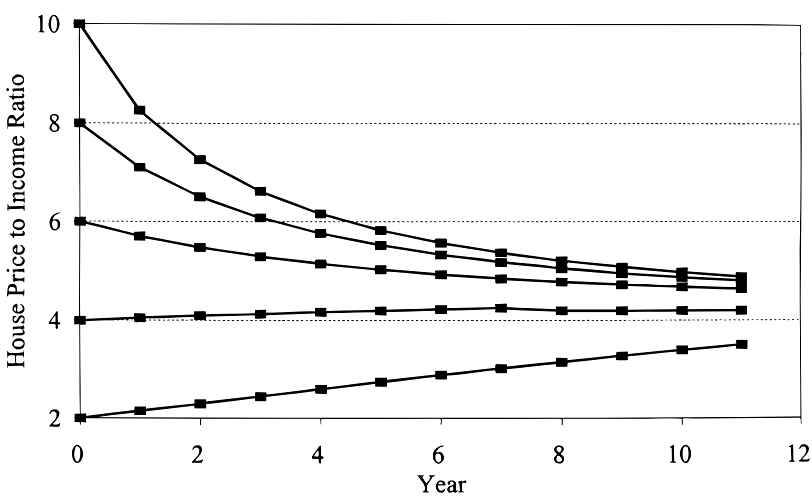

FIG. 9. Time path of HP-to-income ratios for low–moderate regulation MSAs to equilibrium of 4.2.

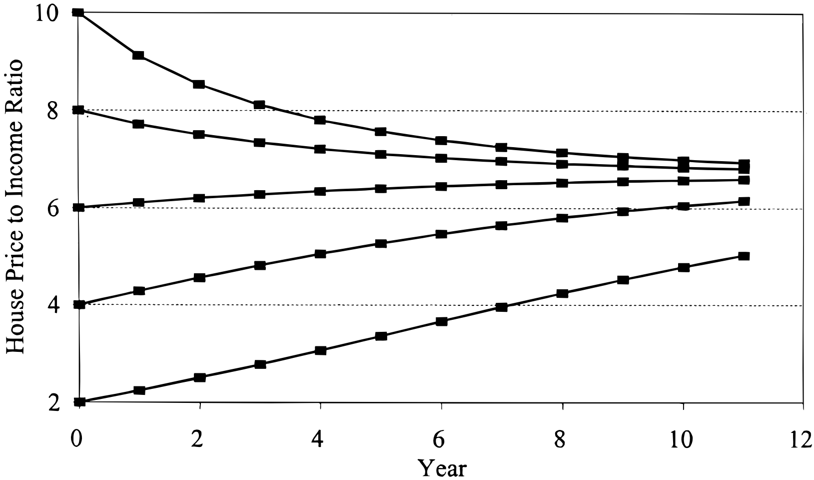

FIG. 10. Time path of HP-to-income ratios for stringent regulation MSAs to equilibrium of 5.8.

$$dP_t = X_t\alpha + \beta_1 D_{t-1} + \beta_2 R_t + \beta_3 D_{t-1} \cdot R_t + \varepsilon_t, \tag{5}$$

where $D \cdot R$ is the interaction of the disequilibrium and regulatory variables. It seems quite possible that the effect of disequilibrium on conditional price changes could vary with the regulatory regime; but we have no particular reason to argue that the effect of the regulatory regime on conditional price changes depends on the level of disequilibrium. Then the total effect of disequilibrium on price changes is actually $(\beta_1 + \beta_3 R)D$.

Column 6 of Table IV presents such a varying parameter model. The estimated effect of disequilibrium is now the sum of the first coefficient, -0.0689, and the level of regulation times $+0.0018$. The range of our regulatory index is from about 15 to 30. Figure 11 plots the resultant total effect of disequilibrium by the stringency of the regulatory environment. The first year effect of a unit of disequilibrium on housing price changes ranges from about -0.04 to -0.015 as the environment becomes more stringent. Figure 12 shows the convergence implied at a low level of regulation, with the index set to a value of 16.

Since we have elaborated our concern about the measurement of the regulatory variable, in the presence of fixed effects intercepts, we must check the robustness of this interaction term. Column 7 shows that, as before, moving to the "actual" regulatory index changes the sign of the coefficient of the regulation variable (from -0.0128 in column 6 to $+0.0133$ in column 7). But the coefficients of the disequilibrium and interaction terms are fairly robust.

We have not yet carried out formal forecasts of future price changes. That would require a little further data collection and some forecasting of exogenous variables. But as a rough indicator, it might be interesting to examine the top

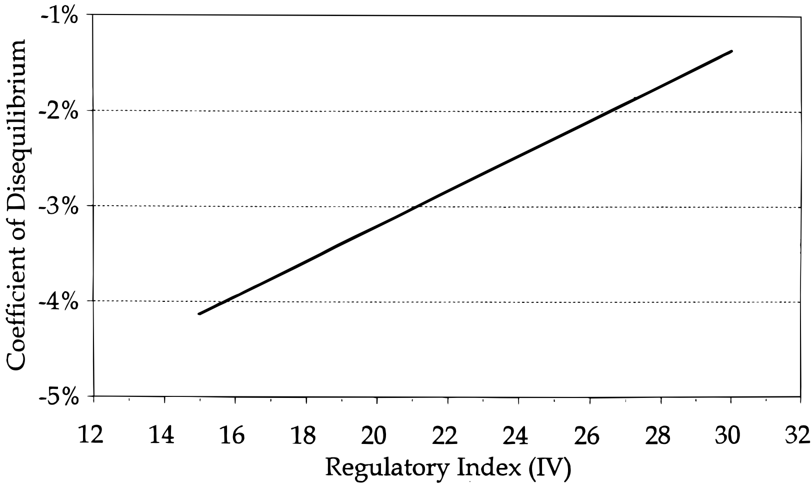

FIG. 11. Net disequilibrium coefficient by level of regulation.

five markets in estimated disequilibrium in the last year of the data in hand (1996). Those are, in descending order, Santa Rosa, San Jose, Santa Barbara, Vallejo, and Salt Lake City. Loosely, we would expect price declines in these markets.[22] The five markets with the "most negative" disequilibrium estimates

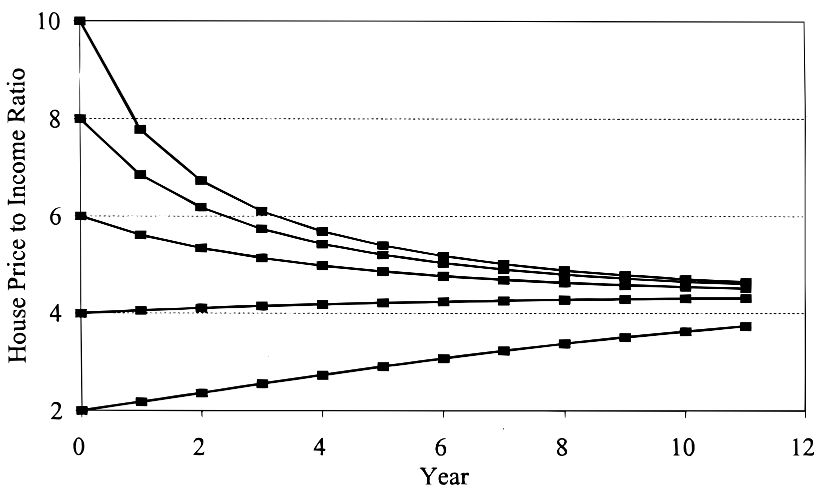

FIG. 12. Time path of HP-to-income ratios for REGHAT = 16 to equilibrium of 4.2.

[22]We say loosely because we saw above that the time path of adjustment to equilibrium depends only partly on disequilibrium. Other market conditions and the regulatory environment also matter.

are Las Vegas, West Palm Beach, Houston, Ann Arbor, and Reno. In three or four years I will report the results obtained by shorting the former five markets and buying into the latter five.

SUMMARY AND CONCLUSIONS

First, our work generally confirms previous work that housing price changes are not random walks and are at least partly forecastable. Several variants of a simple two-equation model of housing prices were formulated and tested.

The equilibrium house-price-to-income ratio was found to be well modeled by a simple reduced form. The stringency of the regulatory environment was a particularly powerful determinant of the equilibrium ratio. Previous literature has demonstrated that more stringent regulations are related to higher housing prices, on average, but this paper is the first to relate regulations to higher equilibria. The distinction is an important one.

Housing price changes were well modeled by a simple error correction formulation. The speed of adjustment was symmetric above and below equilibrium, as far as we could determine. We argued for and tested the possibility that large departures from equilibria could call forth larger proportional changes in price than small departures, but cubic terms were insignificant, so we cannot reject the null of linear adjustment.

While most results were in accord with expectations, the index of regulatory stringency did not perform robustly in the second stage models of the determinants of price changes. This may be due to the fact that a fixed-effects model was estimated, and that MSA-specific dummies in fact pick up the regulatory environment (which does vary by MSA) better than our imperfect index. By and large, other results are robust.

Price changes were determined by measured disequilibria in previous periods, as expected. Faster rates of population growth and of income growth were associated with higher conditional price changes, suggesting a less than perfectly elastic short-run housing supply. Higher mortgage rates lowered price changes, *ceteris paribus*.

While the effect of regulation on conditional price changes was not robust, the effect of regulation on the speed of adjustment to equilibrium was. A simple and arbitrary segmentation by regulatory stringency and a varying-coefficient model where the adjustment parameter varies continuously with regulation both confirm that the speed of adjustment is faster in less stringently regulated environments.

Many future research directions suggest themselves. First, the lack of robustness of the regulatory measures in the price change equations suggests much remains to be done to better measure the regulatory environment. Explicit modeling of the metropolitan area affects in the random effects or error components framework is possible (though perhaps difficult with current data). Finally, our error correction model has relied on a simple, common sense structural specification. It would be possible to model housing prices in the atheoretical error correction model framework commonly found in the macroeconomics literature, for comparison.

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