An empirical study of local dynamics and contagion in real estate markets

By
Paul Anglin
University of Guelph

and

Jian Zhou
University of Guelph

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We offer two hypotheses to substantiate this difference. First, that it is evidence of real-time price competition between segments. The second hypothesis focuses on the effects of expectations based on recent prices. Our analysis suggests that competition between different segments of a housing market may be a countervailing force to the "Greater Fool" Theory of price bubbles.

JEL: C31, C60, D80, R21, R31
Key words: dynamics, contagion, price, real estate, cycle, risk, disaggregation

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The market prices of homes in a city are usually assumed to adjust so that each segments of the market should have the same dynamic characteristics as the entire city. Using nearly 20 years of data, this paper presents evidence that prices in different segments grow at substantially different rates over an extended period of time. More importantly, we show that the lag structure of price changes in a segment differs from the lag structure of changes in the average price in the city: the effects of a change in the average price in a city seem to appear after a longer lag and are smaller when estimated using an aggregate series than if estimated with less aggregation.

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Price dynamics in residential real estate markets have attracted the attention of governments around the world and the media because these dynamics worry property owners and potential buyers. To respond to these worries, researchers have used progressively more sophisticated statistical techniques. Most commonly, aggregated data is used to extrapolate past events into the future using a simple or complex forecasting method. The evidence presented below suggests that "the" price of real estate in a city may not be as well-defined as the statistical methods anticipate: a rising tide does not raise all boats at the same rate.

When seeking to explain patterns in aggregate data, it is commonly argued that the average price does not quickly adjust to its steady state equilibrium value because of adjustment lags (either due to the lengthy construction process or due to government policy) or because of not-necessarily-rational expectations (Capozza, Hendershott and Mack, 2004; Wheaton 1999; Clayton, 1997). To these adjustment processes, we study a contagion effect which creates patterns in local prices that may be obscured by aggregated data. Asymmetric contagion could also change the estimated properties of the aggregated data because the evolving effect of a shock would depend on where the shock originated. Anglin (2006) showed that, in theory, such patterns are possible and can be economically significant. This paper shows that this pattern is statistically significant.

We argue that contagion can exist for two reasons: expectation and competition. Many people understand that expectations affect decision made in the housing market, about when to buy, when to sell and whether to buy a big or small house. At an aggregate level, in the well-known Greater Fool Theory, a higher price leads to expectations of greater appreciation which is sustained for a period of time if more fools rush in. This reason may be relevant at a local level if a location becomes "hot". At the same time, the housing market offers an interesting insight into the competition and the market process because it takes time for buyers and sellers to make a decision. Thus, rather than rushing toward a location that price trends suggest will be hot, buyers may substitute away to find a house for a lower price in a nearby location. The possibility of close substitutes limits the relevance of the Greater Fool Theory.

This paper uses data on the average selling price of residential houses in different districts of Toronto Canada between 1989 and mid-2009. We use these data to study three hypotheses concerning the dynamics of aggregate prices and prices at a disaggregated level. By focusing on the near-contemporaneous relationships within a city, we are able to study price dynamics without needing to specify "fundamental value" by means of the discounted present value of a future flow of utility.

The next section discusses the data informally to demonstrate that local features are interesting in their own right: owning a single property does not guarantee that its price moves with the average. The following section discusses some of the vast literature on price dynamics. Even though we note the range of econometric methods that could be used, we use Vector Auto-Regressive (VAR) method to study these relationships without imposing an a priori specification. After grouping the 86 districts of the Greater Toronto Area into subaggregates, we show that interactions are significant and not symmetric. We also show that the characteristics of the time path of selling prices at a local level differ from those of the overall average price for the city because, at the local level, the strength of the effects of expectations is much weaker than the strength of the effects of competition. A concluding section summarizes the findings.

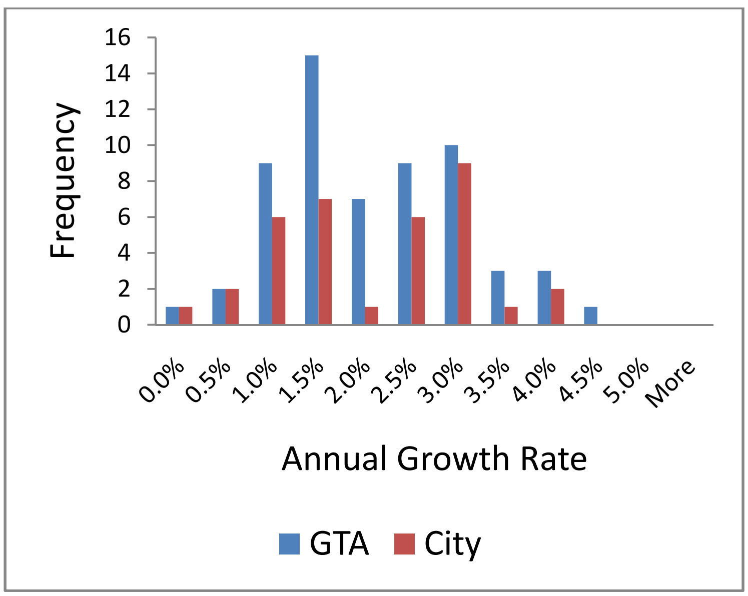

Figure 1: Distribution of Change in Annual Average Price across Districts 1989- 2008

Figure 1 illustrates the dispersion of price changes between different districts.[1] The taller bars indicate the distribution for the whole area, including the faster-growing outlying areas, where more new homes would be added, and the shorter bars indicate how many of those districts are in the smaller area described as the City of Toronto. Figure 1 shows a remarkable range of annual growth rates of the average price and, though there is a difference between the distribution in the GTA and in the City, the difference cannot be described simply. Over the 19 year period, the difference in the growth rates between fast-appreciating and slow-appreciating districts exceeded 3 percentage points per year. Even so, districts in the bottom quartile can be located surprisingly close to districts at the median or even in the top quartile.

These facts challenge the basis of theories of price dynamics which focus on aggregates.

A Selective Literature Review

The simple idea that there is a pattern to the dynamics of price quickly becomes complicated in application. Even if prices in a city can be summarized by a single number, such as the average price, people debate whether the cycle is intrinsic to the market or is being driven by external forces. For this reason, the empirical methodology can be very sophisticated and different methodologies can produce different conclusions (e.g. Zhou, 2010). This section covers some of the issues which have been debated before noting three hypotheses which are relevant to understanding the dynamics at a disaggregated level.

Papers which focus on the role of fundamentals include Zhou (2010), Edelstein and Tsang (2007), Gyourko, Mayer and Sinai (2006), Muellbauer and Murphy (1997), Case and Mayer (1996), Mankiw and Weil (1989) vs. DiPasquale and Wheaton (1994), and Poterba (1991). Some papers test for the significance of restrictions derived from behavioral models (Meese and Wallace, 1994). The general conclusion of this literature is that, in many cities using commonly available information, price changes are predictable, to some degree, but that much of the dynamics are not explained by changes in the fundamental value.

[1] These data do not control for the characteristics of the house or changes in the distribution of characteristics. The conclusion discusses why we think that this omission has little effect on the analysis or conclusions.

A few papers have looked at how to combine variation among segments of a housing market with variation over time. For example, Ho, Ma and Haurin (2008), Gyourko, Mayer and Sinai (2006), Bourassa et al (2006) Seslen Wheaton and Pollakowski (2005) and Smith and Ho (1996) have documented differences in the rate of appreciation within a city. Tirtiroglu (1992), using an approach more familiar to experts in finance, showed that "excess returns" in a neighborhood were related to the returns in nearby neighborhoods. Cannon, Miller and Pandher (2006) estimated the risk-return trade-off implied by differences within the US market and concluded that a 2.48 percentage point increase in annual return was associated with a 10 percent increase in volatility. A few researchers have focused on time series questions even though their data makes it possible to study the interaction between segments (e.g. Goodman and Thibodeau, 2007; Roehner, 1999; Clayton, 1997).

Very recently, some researchers have started to look at part of this problem at different levels of geography. In the U.K., several studies have considered "ripple" effects as the prices of housing in different parts of the country respond to changes in other parts (e.g. Gupta and Miller, 2009; Holly, Pesaran, Yamagata, 2010). Using a variety of definitions of a "segment", Hendershott, MacGregor and Tse (2002), Jones, Lieshman and Watkins (2003), Cook (2003), Wang (2004), Stevenson (2007) and Ho, Ma and Haurin (2008) considered the interaction between different segments. He and Winder (1999) considered the links between nearby submarkets, and included data on income and building permits to measure the degree to which different submarkets are "close". These papers concluded that the segments were related but there was less agreement on the mechanism which created the relationships or whether the effects between any pair of locations are unidirectional or bidirectional.

The work on ripple effects in the U.K. discussed four mechanisms which transmit effects geographically, summarized by Gupta and Miller (2009) as "migration, equity conversion, spatial arbitrage, and exogenous shocks with different timing of spatial effects". While these mechanisms are interesting, they can be very hard to separate, especially in the context of a city. The papers rarely test any one mechanism. The work on "downpayment effects" discussed how something which increases the price of a house makes it easier for a seller to move. Such effects are usually associated with a correlation between prices and the volume of sales, e.g. Ho, Ma and Haurin (2008) which found that the effects go up the quality ladder but not down using data from Hong Kong. Our work has insufficient degrees of freedom although our discussion of the links between different price levels indicates a superficial inconsistency.

We offer three hypotheses. The first motivates the other two and identifies a common maintained assumption. The second and third hypotheses focus on the mechanisms of contagion.

Beyond the obvious hurdle of having appropriate data, we conjecture that few researchers have considered the problem of price dynamics with contagion from other segments of a housing market for two reasons. The first reason is that the statistical technology has not, until recently, been readily available. This fact may explain why much of the existing research considered the "easier" problem of comparing the dynamics of prices in different cities and hoping that the exogenous variables (e.g. property tax rates, demographic variables, regulations, geography, fixed effects) would adequately summarize the differences between cities. In part, this paper is motivated by the issue of whether the statistical errors associated with assuming that a market can be summarized by a single price are more important than the errors that can be removed by refining the estimator. To the extent that omitted variables affect inter-city analyses, these imperfections either do not exist in our study or their severity is reduced.[2]

The second reason why few people may have considered price dynamics with contagion is that Rosen (1974) offered such a strong and useful model for determining prices at a point in time that there appears to be little need to consider how relative prices evolve over time. The most common specification of a hedonic price function, if it recognizes the time dimension at all, measures time using a time trend variable or a series of time-related dummy variables:

$$\ln(p_{it}) = a(t) + b_i X_{it} + u_{it} \quad (1)$$

$$u_{it} \sim N(0, \sigma^2)$$

where i represents an observation, t represents a date and X represents the exogenous

[2] We are indebted to Charles Leung for this insight.

variables describing the house (including location). By taking the difference between observations at two different times, t1 and t2, or by supposing that the type of house does not change over time, $X_{it1}= X_{it2}$, the estimated values of a(t) can be used to construct an index. Later analysis can try to explain patterns within the evolution of the index. This specification imposes the restriction that an event raises or lowers the prices of all houses equally.

Formally, Rosen's model is a static model but, in a dynamic model and regardless of expectations (so long as they are common), an arbitrage argument implies that if the paths of prices in two different segments differed systematically then it should be possible to profit from the difference. These excess profits are not compatible with a weak notion of an equilibrium. Thus,

Hypothesis 1: The time path of prices in each segment displays the same characteristics as displayed by the time path of the average price.

While it is possible to avoid the restrictive specification of equation (1), by introducing interaction variables or non-linear estimation methods, Hypothesis 1 does not rely on any particular specification except that b_i does not vary with time.

If Hypothesis 1 is rejected then we need to investigate its premises. We choose to consider the effects of price competition and of expectations. Instead of assuming that price dynamics depend on a single state variable, denoted by s_t, suppose that there is more than segment. Consider a model with two segments, 1 and 2, with two state variables s_{1t} and s_{2t} that evolve over time. In general, a change in either state variable can shift both excess demand curves, ED_1 and ED_2, at a point in time. Based on the behaviors of buyers and sellers, excess demand in each segment also depends on the current prices in the two segments, p_{1t} and p_{2t}, and, since real estate is a durable good, excess demand at a point in time depends on expectations of the state variables, e_{1t} and e_{2t}. At each point in time, the market clearing prices, p^*_{1t} and p^*_{2t}, satisfy

$$0= ED_1(s_{1t}, s_{2t}, p^*_{1t}, p^*_{2t}, e_{1t}, e_{2t}) \quad (2)$$

$$0= ED_2(s_{1t}, s_{2t}, p^*_{1t}, p^*_{2t}, e_{1t}, e_{2t}).$$

In a housing market, the excess need not be equal to zero for every point in time. The time taken by the search, matching and bargaining processes offers a natural outlet for any temporary deviations from equilibrium. In a steady state equilibrium where the state does not vary, $p^*_{it} = e_{it}$ for i= 1, 2 and, by appropriate scaling of the state variables, if $s_{1t} = s_{2t}$ then $p^*_{1t} = p^*_{2t}$. These two conditions lead to two hypotheses.

A common complaint about research on price dynamics is that data on expectations is needed to compare the user's cost of home ownership with the cost of the alternative but is rarely available. Some researchers (Schulz and Werwatz, 2004) compensate for the lack of data by using a Kalman filter to estimate a portion of a (aggregated) price change that is unexpected. Garino and Sarno (2004) proposed an estimator which tests for the presence of "rational bubbles" (LeRoy, 2004); using data from the U.K., they found that prices have sometimes grown faster than can be justified by rationality. Rather than using data on expectations, Clayton (1997) used a unique data set from Vancouver and showed that expectations were not consistent with "rationality" in the short run. Meese and Wallace (1994) reached the same conclusion and both papers argued that, in the long run, expectations were consistent with the predictions of rational expectations.

In a disaggregated model, suppose that the price in segment 1 rises. If a buyer believes that this rise indicates that this segment is becoming "hot" then they would want to buy soon (i.e. before the price becomes too high to afford or to profit from the future price increase). The decisions by more buyers to look in that segment would increase the bargaining power of sellers and would increase the price in that segment even more. Since many of these buyers were looking in other segments, their substitution would lower the price in those other segments and confirm the expectations of buyers that following the herd was a good idea. Thus, expectations would serve to magnify the differences between segments.

Hypothesis 2: If p_{it} increases, then p_{it+1} increases ***and*** *p_{jt+1} decreases ($j \neq i$).*

Our last hypothesis emphasizes the effects of competition between segments. Controlling for expectations, if the price in segment 1 rises then searchers would find that fewer properties in that segment satisfy their reservation utility criterion. Thus, they would look in another segment (or leave the active market). This shift would increase the number of people looking in segment 2, increase excess demand in segment 2 and, either by direct competition with each other or by seller awareness of the greater number of potential buyers, raise the price on houses which sell. Another mechanism produces the same outcome: if buyers are considering properties in more than one segment then the weaker bargaining position implied by the higher price in period t in segment 1 would make it more likely for them to buy in segment 2 and for a higher price than they would have otherwise. Given the time it takes to buy or sell a house, the prices in these other segments would rise with a lag.

Our hypothesis uses the idea that this argument has two parts: if a high price in segment 1 during period t causes a buyer to look in segment 2 during period t+ 1, then the same search behavior implies that a high price in segment 2 during period t+ 1 and the reduced number of buyers active in segment 1 during period t+ 1 would negotiate lower prices:

Hypothesis 3: If p_{it} *increases, then* p_{it+1} *decreases* **and** p_{jt+1} *increases* $(j \neq i)$.

It may seem as though our hypotheses offer a categorization of all possible outcomes since, except if the results are statistically insignificant, an increase in the price in one segment can only either increase or decrease the price in another segment.[3] Table 1 shows that our hypotheses offer more than a simple categorization. Rejection of both hypotheses is possible and would indicate the need to think more carefully about the micro-level interaction on its own and how its aggregate effects appear in the measured price trends for an entire city. It is also possible for both hypotheses to be relevant but that one dominates the other.

Table 1: Possible Effects of an Increase in p_{it}

On p_{jt+1} \ On p_{it+1}	Decrease	Insignificant	Increase
Increase	Competitive Hypothesis (3)		
Insignificant			
Decrease			Expectations Hypothesis (2)

[3] Ho, Ma and Haurin (2008) noted that a price change can have a wealth effect and a substitution effect on consumer behavior. Our Hypothesis 3 focuses on the behavior of buyers for whom both the wealth and substitution effects of a price increase go in the same direction. Our Hypothesis 2 can be seen as an extension of their analysis, where an increase in the capital gains expected from buying in a location affects wealth.

To be compatible with an equilibrium, the buyer behavior proposed above needs to account for two other aspects of a residential housing market: the behavior of sellers and quantity demanded. Seller behavior is consistent with these hypotheses since their houses are immobile. They also tend to commit to offering their house for sale for some time, longer than one period or one month, and their bargaining position, as indicated by their reservation utility, varies less over time than the distribution of price offered by buyers. Thus, other than to accept the risk to wait for a buyer who is willing to pay the price that the seller wants, a seller has limited ability or incentive to react to a change in short term conditions. Over a longer term, the decision of sellers to enter or exit a market can determine how long the price level in a segment can deviate from its fundamental value.

In a search market, a buyer can raise or lower their reservation utility level according to their experience and expectations but we assert that the equilibrium conditions on the reservation utility of a buyer or seller are relatively stable.[4]

Our data may offer insight into some issues which, though not the subject of a formal test, are related to the hypotheses. First, the standard version of the Greater Fool Theory argues that price increases can be supported because more buyers would enter the market when they expect prices to rise and this entry would confirm the expectations. We cannot test this idea since we have no direct knowledge on the activities of buyers. Our argument concerning Hypothesis 2 uses the idea that if buyers are acting on their expectations then the buyers who are most likely to switch are those who were already looking for a house someplace else. We note that, if inactive buyers becoming active is an important aspect of the market process, then an already-active searcher is more likely to be on the margin and their behavior would offer relevant insights to the behavior of inactive searchers.[5]

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[4] In principle, this discussion offers the outline of an equilibrium model of search and matching in a changing environment. We could be more precise in our specification but we conjecture that, given the data available to us, the formality would offer little extra insight. The key insight is that the equilibrium could be characterized by the reservation utilities and that the criteria vary less than any changes in conditions for a single period. Prices and the level of trading activity are consequences of these criteria and the matching process

[5] There may also be behavioural theories which rationalize the behaviour of a buyer who looks at a price increase in one neighbourhood and infers that the price in that district will fall, or that prices in all districts will rise. The first case is indistinguishable from ordinary competition hypothesis and the second case implies that expectations have a no effect on the choice between districts. We suggest that most learning heuristics or other models of adaptive expectations do not allow for these cases.

If expectations affect where buyers look then this fact is not sufficient to create an aggregate price bubble. Aggregate price trends depend on the interaction amongst all segments and can only be determined by investigating the eigenvalues of the matrix of interactions.

Lastly, we do not offer any hypotheses on the magnitude of the effects. In principle, the magnitude of the expectations effect should be an increasing function of both the duration of the bubble and the deviation between the purchase price and the expected selling price.[6] Whether a small bubble can become bigger depends whether the effects of historic prices on expectation and the effects of expectation on excess demand are big enough. The magnitude of the competitive effect depends on the willingness of a buyer to substitute and that willingness depends on the similarity of the segments.

Econometric Approaches

Studying how prices evolve over time in neighboring segments can use many different methods. Because the question is dynamic, and to build on earlier studies of aggregated data, a time series method seems reasonable. Because the question involves neighboring segments, a spatial method seems reasonable. Because the data is organized as repeated sampling from a given population of segments, a panel data method seems reasonable. It may also be useful to derive an estimator independently which could, more completely, incorporate the behavioral and inferential problems facing a buyer or seller. Though these suggestions are reasonable, it is not reasonable to attempt all of them in the one paper.

We start with some simple tests of the time series properties but our main analysis follows the advice of Giacomini and Granger (2004) who advocated using Vector Auto-Regression for two reasons. First, since our data involves more units of time than units of location, the problem is mostly a time series problem. Second, because VAR is flexible and easy to implement:

[6] We use the idea of "eventual selling price" to note that the buyer may not be aware of the fundamental value used by a researcher to define a bubble and to note that, consistent with the Greater Fool Theory, it can be profitable to be foolish now if, at some later time, a greater fool pays more.

$$\Delta P_t = \Gamma_0 + \Gamma_1\, \Delta P_{t-1} + \Gamma_2\, \Delta P_{t-2} + \ldots + \Gamma_r\, \Delta P_{t-p} + B_0\, X_t + U_t \qquad (3)$$

where ΔP_t is a $K\times 1$ vector of percentage change in price levels from the previous month. X_t is a vector of exogenous variables. Γ_i, and B_0 are vectors of coefficients to be estimated.

Testing the Hypotheses

The data used in this paper are compiled from a monthly publication of the Toronto Real Estate Board (TREB) called *Market Watch*. We use data from January 1989 to June 2009, although, for some districts, the available information covers a shorter period. TREB divides their coverage into more than 80 districts covering nearly all of the "Greater Toronto Area" (known locally as the GTA).[7] The City of Toronto represents only 34 of those districts. The districts have various sizes, shapes and compositions. Though we focus on residential transactions of single family houses, some of the districts are primarily industrial while other districts are primarily rural. The advantage of using such a large area is that the outer ring of districts is mostly rural now and distance reduces the possibility of contagion from cities even further away.

The average price of a single family residential home in the Toronto area rose from C$273,698 during 1989 to C$379,347 during 2009.[8] This increase of only 39 percent during 19 years understates recent events since the average selling price fell by 28 percent between 1989 and 1996. According to the popular press, prices in this city were not widely suspected of being so high as to represent a "bubble". Statistics Canada reported that the population of the City of Toronto (zones 1 to 4) grew from 2.4 million to 2.7 million while the population of the larger "Census Metropolitan Area" of Toronto grew nearly four times faster from 3.9 million to 5.6 million

[7] Our analysis uses the districts shown in the most recent map (July 1997). The boundaries of the districts tend to follow major roads and rivers and they have changed slightly from time to time with the greatest changes being in the outermost districts. The types of data reported for these districts also changed from time to time.

It may be better to define a submarket as a step in the analysis, as shown in Goodman and Thibodeau (2007) or Liu (2010). Unfortunately, the data are not available to us in a form suitable for such analysis; the boundaries were defined by local agents who have used them for many years.

[8] For the reader's information, the foreign exchange rate with respect to the US dollar varied from C$1.20 during 1989 to near parity during 2007 and again during 2009, with a peak of C$1.57 during 2002. Canada's Core Consumer Price Index rose by about 48 percent between 1989 and 2009.

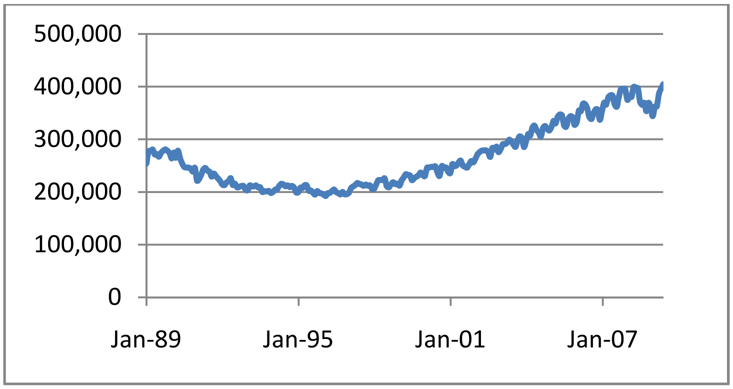

Figure 2: Price Trend

The first bit of evidence that the time series properties of the disaggregated data differ from those of the aggregated data is found using a Variance Ratio Test corrected for heteroscedasticity (Lo and McKinnon, 1988). We estimated whether the percentage change in a district's average price followed a random walk over various horizons. Table 2 reports the average z-score for different time horizons.[9] At the shortest horizon, the hypothesis of a random walk was strongly rejected for all but two districts using a 5 percent significance level: the variance of prices is much less than would be expected from a random walk and the variance ratios imply negative autocorrelation. At the longest horizon, this hypothesis was rejected at the 5 percent level for 60 of 85 districts.

Table 2: Summary of z-scores for Variance Ratio Tests
1989- June 2009, Corrected for Heteroscedasticity

Horizon (#months)	2	4	6	12	24
Average	-4.75	-4.19	-3.76	-3.00	-2.22
Maximum	-1.29	-1.23	-1.25	-1.25	-1.09
Minimum	-6.40	-5.68	-4.85	-3.72	-2.74

The Variance Ratios for aggregated data also display negative z-scores that are statistically significant except for the shortest and longest horizons. The important difference between the aggregate and the districts lies in the magnitude of the variance ratio, as reported in Table 3. (If the data followed a random walk, the variance ratios would be close to 1.)

[9] The test statistics use the full time period for most districts but, because some districts have no sales in a month, the relevant time period was shortened to allow for an uninterrupted time series. Two districts were excluded because this adjustment would imply that not enough data was available to compute the variance ratio for the longest horizon.

The disaggregated data display much lower ratios except at the two longest horizons. In fact, for the 2, 4 and 6 month horizons, the variance ratio for the city is greater than the variance ratio for any district in the city; at the 24 month horizon, the variance ratio for the city is greater than the variance ratio for each individual district for every horizon.

Table 3: Variance Ratios for Districts and for City as an Aggregate
1989- July 2009

Horizon: #months	2	4	6	12	24
Average of Variance Ratios across Districts	0.54	0.31	0.22	0.13	0.11
Variance Ratio for Aggregate	0.93	0.71	0.55	0.44	0.63

Our main analysis analyses data at an intermediate level of disaggregation, to retain a reasonable number of degrees of freedom within the constraints of having no more than 250 months of data and of allowing for unspecified interaction. We choose to work with two types of aggregation that are consistent with competitive closeness: geographic zones and price levels. The 86 districts are aggregated into 12 geographic zones and 12 price levels. The zones were selected to include geographically-contiguous districts radiating out from zone 1 at the central business district. The City of Toronto includes zones 1 to 4 plus a bit of zone 7. To construct the data series for the 12 price levels, we ranked all districts by their average price level during 2004 and 2005 (after deleting two districts in which very few houses were traded in any month). This time period was chosen since the aggregate price is seen as being relatively stable. After dividing the districts into levels of 7 districts each, 4 districts were assigned to the next higher or lower category if there was a natural break point in the price distribution or if the other level had several districts near to the reassigned district. Finally, the district level data are aggregated into a zone price or a level price using a sales-weighted average for that month. Table 4 summarizes these data.

Table 4: Descriptive Statistics: Zones and Levels

Zones	N. Obs.	Percentage Change in Avg. Price per month: Mean	Std. Dev.	Sales/mth	Avg. Price
1	245	0.005	0.071	648	318507
2	245	0.004	0.050	562	249372
3	245	0.004	0.070	416	365113
4	245	0.001	0.032	426	216095
5	245	0.002	0.024	1126	233497
6	245	0.002	0.034	582	324355
7	245	0.002	0.033	340	219392
8	245	0.004	0.071	190	303899
9	245	0.006	0.091	147	285044
10*	221	0.004	0.065	98	256227
11*	221	0.006	0.082	154	208099
12*	221	0.003	0.042	339	187137
Levels					
1	245	0.001	0.032	451	174481
2	245	0.001	0.030	336	192026
3	245	0.002	0.033	362	207739
4	245	0.002	0.031	356	218896
5	245	0.002	0.026	857	227037
6	245	0.002	0.031	436	246912
7	245	0.003	0.038	525	265983
8	245	0.004	0.072	334	273461
9	245	0.002	0.039	424	325752
10	245	0.003	0.046	398	317701
11	245	0.005	0.069	278	320392
12	245	0.006	0.084	308	515688
TREB	245	0.002	0.029	5028	266070

*Data for 1989 and 1990 do not exist for zones 10, 11 and 12.

Table 4 demonstrates that the different zones and levels differ in many ways. The average price in some zones or levels grew more rapidly than in others. The zones which appreciated more quickly also tended to have a higher standard deviation of prices and have fewer sales in a typical month. They were on the outer ring of zones.

The average price in the highest level grew faster than any other level and was more variable. The descriptive statistics also show that, surprisingly, the average price for the entire period in level 9 is very slightly higher than the average price for the entire period in levels 10 and 11. Investigation shows that the standard deviation, across the monthly data and compared to the other levels, is relatively high for levels 10 and 11. Therefore, while the districts in these levels may be high-priced during 2004 and 2005 and were relatively high priced at other times, some of the districts may be categorized differently if the base of comparison were changed.

One concern with using data on the average price of houses sold through a local real estate board is that it would not control for changes in the quality or types of housing being sold. We hope that this issue is not severe since most of our analysis uses sub-aggregates that, in our opinion, are large enough for the law of large numbers to smooth out any month-to-month effects of changes in the mix of houses sold. We will use dummy variables to control for any seasonal effects.[10]

Based on the results of an Augmented Dickey Fuller Test, we can reject the hypothesis that the percentage change in price in any one zone or any one level displays a unit root. We can also reject a unit root hypothesis for the change in the average price.

Although we wish to focus on the interaction between districts, it is also reasonable to include other variables which might affect that the state of the market. The obvious indicators are the inflation rate (measured as year over preceding year change in the national "core" Consumer Price Index, which excludes the more unstable components of the regular Consumer Price Index), the city's unemployment rate (seasonally adjusted) and the real mortgage interest rate (the nominal interest rate on a 5 year term as reported by the Bank of

[10] Finally, we note that an independent agency has estimated quality-adjusted price series for Toronto based on Case and Shiller's methodology (available from housepriceindex.ca). These data cover only the last 10 years of our study and is not available at the kind of disaggregation needed to investigate our hypotheses but it can suggest the importance of quality change relative to trends in the average price of houses sold. Specifically, if we compare the ratio of the average selling price to the quality adjusted price index for the first 12 months of the index to the same ratio for 12 months at the end of our study, we find that that ratio increased by 4.4 percent over those 10 years. When converted into a monthly change, this implied change in the average quality is smaller than the month-to-month changes reported in Table 3 by an order of magnitude or more. If changes in the quality or types of houses sold dominate then buyers should be able to recognize these effects and the month-to-month changes in the reported average selling price should have no statistically significant effect on behavior later in other places.

Canada minus the actual year-over-year inflation rate during the preceding 12 months). Table 5 summarizes these data.

Table 5: Descriptive Statistics for Macroeconomic Variables

Variable	Mean	St. Dev.
Inflation Rate	2.08	0.76
Unemployment Rate	7.62	1.97
Real Mortgage Rate	5.88	1.79

Sources: Statistics Canada, Bank of Canada

As a benchmark, Table 6 reports the coefficients estimated using the percentage change in aggregate price as the dependent variable. Our preferred specification is the leftmost. Since theory offers no compelling direction, an important issue in this analysis is how many lags to include. After exploring various specifications, we focus on the specification which includes the first, second and 12th lags. Using the aggregated data with the independent regressors above, several criteria, including, the Akaike Information Criterion and the Schwartz Bayesian Information criterion recommend using a lag length of two. Adding lagged values of the exogenous variables adds little insight. When estimating the system of disaggregated prices with the independent regressors, these criteria usually recommend relatively short lag lengths of zero, one, or two lags, although there is less agreement amongst the different criteria. Adding the third lag has little effect on the overall performance and, mostly produces individually insignificant coefficients. There is strong evidence that including the 12th lag improves the performance of the estimates. It is worth noting that the 12th lag is significant even when dummy variables are included in the estimation to account for monthly effects: removing them would reduce the R^2 by about 20 percentage points, change the magnitude and statistical significance of the coefficient on the 12th lag. We chose to work with lags 1, 2 and 12 of the dependent variables to make our work comparable to studies using aggregated data. Johansen's test establishes that the rank of the cointegrating vector is 11, which indicates a full rank or level of interaction within the system.

All of the coefficients on lagged price changes are negative, which implies that positive change in one period would be followed by a series of negative changes in later periods, other things being equal. By including exogenous variables in the analysis, we are able to separate the contagion from the catalyst which starts the process. Our empirical results indicate that an increase in the mortgage rate has a negative effect on the growth in prices. The unemployment rate has no effect. An increase in the real interest rate has the expected effect but an increase in the inflation rate has no statistically significant effect.

Table 6: Estimated Model using Aggregated Data (1989- June 2009)
Estimated using VAR
Dependent Variable: Percentage Change per month in Average Price

	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Lag 1.	-0.332	-5.16	-0.287	-4.65	-0.324	-5.10	-0.341	-5.21
Lag 2.	-0.145	-2.25			-0.140	-2.22	-0.162	-2.36
Lag 3.							-0.047	-0.72
Lag 12.	0.161	2.56	0.174	2.77			0.155	2.45
Unempl. Rate	0.000	0.50	0.000	0.56	0.000	-0.39	0.000	0.45
Real Mortgage Rate	-0.003	-3.81	-0.003	-3.34	-0.003	-4.010	-0.003	-3.88
Inflation Rate	-0.003	-1.18	-0.002	-1.03	-0.001	-0.580	-0.003	-1.24
Constant	0.009	1.06	0.007	0.80	-0.005	-0.51	0.011	1.20
12 Monthly dummy variables included in all specifications								
R^2	0.52		0.51		0.49		0.52	
Adj. R^2	0.48		0.47		0.45		0.48	
N. of Obs.	233		233		243		233	
Log-Likelihood	585.758		583.256		605.419		586.014	

To test for the presence and significance of the hidden dynamics, we estimate models based on the 12 zones and the 12 levels. The left hand pair of columns in Table 7 shows that, if each zone or level were estimated independently without interaction, then the explanatory power of the regression decreases for most all but one or two zones relative to the Adjusted R^2 shown in Table 6. This decrease suggests the price path of subaggregates experience some extra shocks. The right hand pair of columns shows that adding lagged values of price changes in *other* zones or levels increases the explanatory power above the level shown in Table 6. Obviously, the increase would be more dramatic with a more focused specification which included only the significant zones or levels, since the adjustment to the R^2 must account for 51 regressors if estimated jointly, compared to only 18 if estimated separately whereas the discussion below shows that the interaction is not complete.

Table 7: Adjusted R^2 of Disaggregated Regressions
Estimated using VAR with lags 1, 2 and 12
Dependent Variable: Percentage Change in Price

	Estimated Separately		Estimated Jointly	
	Zones	Levels	Zones	Levels
1	0.515	0.300	0.569	0.354
2	0.469	0.271	0.494	0.276
3	0.395	0.309	0.369	0.313
4	0.340	0.328	0.341	0.306
5	0.344	0.301	0.435	0.318
6	0.264	0.334	0.370	0.386
7	0.344	0.292	0.370	0.310
8	0.269	0.240	0.316	0.243
9	0.209	0.219	0.319	0.256
10	0.286	0.341	0.375	0.417
11	0.277	0.384	0.335	0.432
12	0.257	0.411	0.275	0.422

Inspecting the coefficients, which are not reported here but are available upon request, shows another important distinction. In all cases, estimating the system increases the magnitude of the coefficient for the first and second lag lengths of a zone or level on itself, often by 50 percent relative to the case of estimates shown in Table 6. The effect of the different estimation on the 12th lag is that it either changes sign or becomes insignificant. The data reject the hypothesis, with a p-value of less than 1 percent, that the magnitude of the first lag effects of a change on itself are equal for all zones but do not reject the same hypothesis using the data on levels (p-value= 0.23 using a Wald test).

The coefficients on the independent variables are also noteworthy because they help to reveal the difference between contagion and the thing(s) which starts the process. Though one might argue that the effects of a change in the unemployment rate might have a greater effect in some locations than in others, changes in the city-wide unemployment rate have little effect. A change in the real mortgage interest rate is significant in all zones. Although differences in the value of the coefficients suggest that the magnitude of the effect appears to differ amongst zones, the data do not reject the hypothesis that the interest rate coefficients in different equations are equal to one another. The data on levels offers a different insight. It remains true that the data do not reject the joint hypothesis that the effects are equal across levels. But, when the coefficients in each equation are inspected separately, the effect of a change in the real mortgage rate is strongly statistically significant for the 6 *lowest* price levels while the p-value for the four of the five *highest* price levels is more than 10 percent. It is tempting to conjecture that the available tests are not powerful to identify this effect.

To emphasize the idea that each zone interacts with a subset of other zones, instead of interacting with all zones equally as might be reasonable if the aggregate price were a sufficient statistic, Table 8 shows whether a change in one zone “Granger causes” a change in another zone. Table 9 shows the same kind of information using the data on Levels. As with the other tables reported here, the analysis is based on a model which use lags of 1, 2 and 12 months.

Table 8: Significant Granger Causality Links between Zones
* implies statistical significance at 10 percent level, ** implies significance at 5% level, *** implies significance at 1% level. An indication of statistical significance shows that Zone j Granger causes Zone i where i denotes a row and j denotes a column.

Cause / Effect	1	2	3	4	5	6	7	8	9	10	11	12	Total Number
Zone 1		***	**	**									3
Zone 2				**									1
Zone 3													0
Zone 4	**		**				*		**				4
Zone 5	***					**		*			*	***	5
Zone 6					*		**		**		**	***	5
Zone 7			*		**								2
Zone 8									*				1
Zone 9			**										1
Zone 10	**	**					**	**			**		4
Zone 11	**				***	**							3
Zone 12		*								*			2
Total Number	4	3	4	2	3	2	3	2	3	1	3	2	

Table 9: Significant Granger Causality Links between Levels
* implies statistical significance at 10 percent level, ** implies significance at 5% level, *** implies significance at 1% level. An indication of statistical significance shows that Level j Granger causes Level i where i denotes a row and j denotes a column.

Cause Effect	1	2	3	4	5	6	7	8	9	10	11	12	Total Number
Level 1			*	***						*			3
Level 2	***							*		*			3
Level 3	**					**		***					3
Level 4													0
Level 5	*					***					***	**	4
Level 6				*	*				***				3
Level 7			**		*					**	*		4
Level 8	***												1
Level 9	*			**	***	*		**		***			6
Level 10					***	**		**			***	***	5
Level 11			***	**	***			**	*	**			6
Level 12				**	***					**	**		4
Total Number	2	2	2	1	7	3	3	3	1	1	1	1	

Both of these tables suggest that the statistically significant links are not symmetric: the fact that one zone has a statistically significant effect on another zone does not imply that the second zone also has a statistically significant effect on the first. The data on Levels indicates that many of the significant effects are not near the diagonal: for example, a month-to-month change in the average prices at a low-to-middle level such as 4 or 5 has a statistically significant effect on the most expensive levels 11 or 12.

The results in Tables 8 and 9, in addition to the results of the variance ratio tests reported in Table 2 and 3, indicate that the dynamic properties of prices at the micro level differ from the properties at the aggregated level: Hypothesis 1 is rejected.

The nature of the interaction between different parts of the system is harder to characterize since it is less narrowly focused. Interaction comes in two forms: the own-effects of one part of the system on itself later and cross-effects of one part on other parts later. All of the coefficients for the first lag and all of coefficients for the second lag coefficients for an own-effect are negative. Many of the t-statistics on the first lag coefficients exceed 5.0. The own-effect impulse response functions for each Zone and each Level are downward sloping. Therefore, after a positive shock, prices within a segment fall.

This finding is inconsistent with Hypothesis 2 on the effects of expectations and consistent with the Hypothesis 3 on the effects of competition.

Table 10: Summary of Cross-Effects on First Lag

Zone	-, Sig.	-	+	+, Sig.	Level	-, Sig.	-	+	+, Sig.
1	1	3	7	0	1	0	4	7	0
2	0	4	5	2	2	0	5	4	2
3	0	4	7	0	3	0	5	6	0
4	0	5	3	3	4	0	3	7	1
5	1	5	2	3	5	1	2	6	2
6	1	6	4	0	6	0	3	5	3
7	0	3	7	1	7	1	4	3	3
8	2	5	3	1	8	0	4	6	1
9	0	4	7	0	9	0	4	5	2
10	1	7	3	0	10	1	4	5	1
11	2	2	6	1	11	2	3	4	2
12	1	3	6	1	12	0	5	5	1
Total	9	51	60	12	Total	5	46	63	18

For each dependent variable, "+, Sig" indicates that the number of coefficients for the first lag of other zones or levels with t-statistics greater than 1.96. Similarly, "-, Sig" indicates the number with t-statistics less than -1.96, "+" indicates the number of coefficients with t-statistics between 0 and 1.96 while "-" indicates the number with t-statistics between 0 and -1.96. Because each equation in the system of twelve endogenous variables includes one "own-effect", either total at the bottom adds up to 132= 12* 11.

The cross effects are harder to summarize since the source of the effect may come from any of 11 alternatives. Table 10 summarizes the cross-effects. This table shows a tendency for the effect to be positive, which is most consistent with Hypothesis 3. This tendency is not overwhelming since the totals may combine the hypothesized interaction effects between related zones or levels with the statistical noise associated with zones or levels which do not interact. Using a Chi-squared test, we can reject the hypothesis that these totals are the result of the coefficients being generated randomly by a Normal distribution with a zero mean ($p< 0.01$) for both levels and zones): i.e. far more than 2.5 percent of coefficients have t-statistics exceed +1.96 than there should be.

We conclude that Hypothesis 2 on the effect of expectations is rejected and Hypothesis 3 on the effect of competition cannot be rejected. The support for Hypothesis 3 is not complete since the effects are, generally, in the direction that is expected but the effects seem to be apparent even when the competing locations do not appear to be *close* substitutes based on geography or price level. Changing the number of lags in the VAR model can change the values of various coefficients but has no effect on these findings.

Conclusion

No individual buys a property that “represents” an entire market. Changes in an aggregate price indicate general trends but this paper shows that the dynamics of prices in different segments differ significantly from the dynamics noticeable at an aggregated level, in terms of statistical significance, in terms of magnitudes of lagged effects and in terms of the determinants. We suggest that these local dynamics can be based on either of two forces: expectations or competition. We suggest that, in this one city, the force of expectations is weaker than the force of competition.

Initially, we thought that the most important path of contagion would be based on competitive closeness in terms of neighboring geographic zones or neighboring price levels. The results suggest that this understanding is incomplete. Since the difference between detached homes and condominiums may be more stable over time, looking for common trends in the prices of these two segments of a real estate market which are often located near to each other may be more fruitful.

This incomplete understanding may affect a popular theory which argues that an increase in low price segments adds equity which makes it easier for the home owners to finance the downpayment needed to move to a more expensive house. This transmission mechanism is usually tested by looking at the correlation between monthly price changes and monthly changes in sales volume. We do not include data on sales volume, due to limited degrees of freedom. Our results suggest that the causation between different price levels do not all flow from low-priced to high-priced segments.

Our use of local data on possible substitutes available to a buyer offers several advantages. First, it allows us to investigate a different aspect of the idea that the prices which people are willing to pay depends on (potentially self-reinforcing) expectations. Though, our research does not answer the larger question of the basis for expectations, it remains relevant since every price bubble must start somewhere and at some time. By studying the role of competition between close substitutes, we note a limitation on the familiar Greater Fool Theory. Second, by disaggregating the housing market in one city, our analysis is able to avoid many of the issues related to whether the "fundamental value" (from which a high price is supposed to deviate) is being measured accurately or precisely.

This analysis confirms the analytical discussion in Anglin (2006) which argued that a deeper understanding at a micro level can have macro-consequences. Instead of a model which supposes that a single variable can summarize the state of the market, where that state variable evolves according to some lagging process, there may be several state variables. Or, the prices in each district may act as a set of state variables that "remember" the price history in more detail than is possible in a model that presents a city as unified. Linking the micro level and the macro level analyses offers two intriguing possibilities. First, if price bubbles in a city are asymmetric, in the sense that the average price grows more slowly when it is growing than the average price falls when it returns to a sustainable level, to what extent can the asymmetry be explained by a change in the correlation between shocks to the different parts of a city? Second, if expectations are relatively unimportant at the local level but extremely important at the aggregate level or if price trends reported at an aggregated level differ from the evidence as it appears at the local level, then how do individuals form their expectations?

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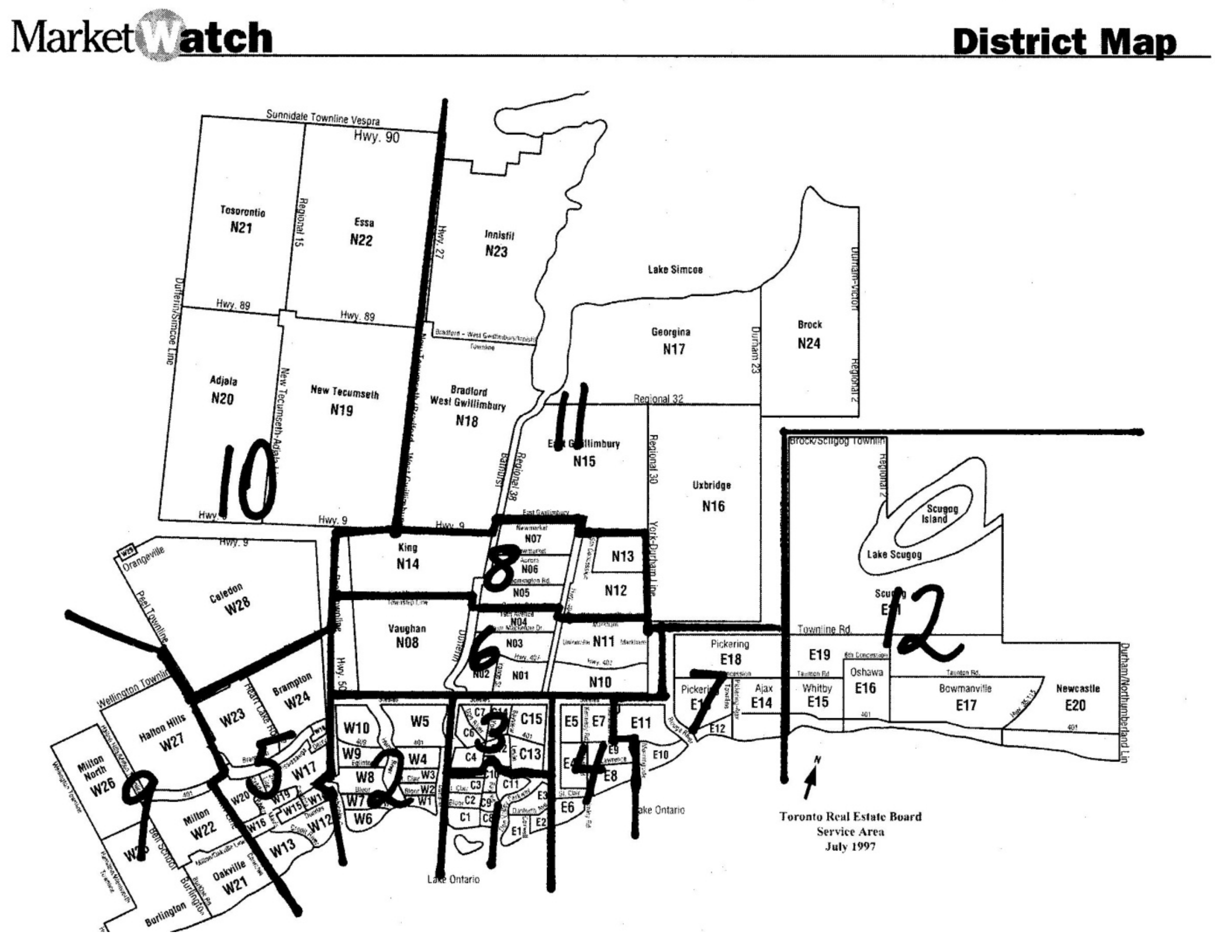

Map of Districts and Zones
(Source of map: *Market Watch*, published by the Toronto Real Estate Board)

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