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Three Essays in Housing Markets

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by

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Abstract

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The 2007 collapse of housing price and subsequent recession highlighted the fundamental role housing plays in the economy. Housing is not only one of the largest single expenditures most consumers have but also has a large impact on both local and national economies. In this dissertation I investigate three aspects of the housing market. The first essay shows the role that government policy can have in impacting housing prices and rents through an examination of the Arizona immigration enforcement legislation of 2010. I show that the implementation of the legislation had a negative impact on Arizona's rents and housing prices, resulting in an estimated loss of $40 billion in lost wealth from owner-occupied properties and $13.8 billion in lost rental income. In my second essay I investigate the dynamic relationship between Real Estate Investment Trusts (REITs) and housing prices. I find that the series are cointegrated and that REITs adjust to changes in their shared cointegrating relationship. I then use this finding to show that the cointegrating residual improves upon one-period ahead out-of-sample forecasts of REIT returns. In my third essay I investigate the role of macroeconomic announcements on high-frequency REIT returns. I use the forecast errors for a AR(1) rolling forecast as proxies for the surprise associated with an announcement. In addition, I use the Quandt-Andrews breakpoint test to determine changes in the announcement effect regimes. I argue that this is better

than defining ad hoc regimes based on states of the business cycle when estimating the effect of macroeconomic announcements. The results indicate that macroeconomic announcements do have a real impact on REIT returns, and thus REITs do reflect market fundamentals.

To my parents and brother for the many years of support, especially in terms of venison.

Table of Contents

List of Figures

List of Tables

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Chapter 1

Dissertation Introduction

In this dissertation I explore the dynamics of housing markets in America in three chapters. My objective is to examine dynamic relationships between housing, government policy and finance in ways that have not been fully investigated to date. While the economics of housing has received some attention following the housing bubble collapse, interest in it still lags below what its importance to the economy calls for. According to the National Association of Home Builders' website, before the crisis two-thirds of homeowners' had more wealth in their housing equity than in the stock market. Homeownership forces people to save through their mortgage payments while investing in other assets does not. Furthermore, before the last recession between 16% and 18% of the U.S. GDP was linked to housing or housing services. While according to the Bureau of Labor Statistics, 2.3 million jobs were lost in construction from January 2007 to January 2011. Given this importance it is surprising that more focus has not been placed on the dynamics of housing markets.

Perhaps one reason for this is that until recently empirical data on housing has been limited and not publicly available. Relatively recent advances in hedonic price indices have begun to change this. First developed and applied in housing by Karl Case, Allan Weiss, and Robert Shiller in the 1980s, hedonic price indices have become a popular way to quantify the health of the housing market. In the last few years state and MSA level hedonic price indices have become available from Freddie Mac, and commercial property indices from Moody's. The availability and popularity of these hedonic price indices opens up a range of possible economic inquiry. This dissertation will examine the housing market from three different perspectives.

In my first section, I will examine how public policy has ramification on housing prices and rents in light of Arizona's 2010 immigration enforcement legislation. Using difference-in-difference across several data sources shows that the passage and implementation of this law caused rents to fall between 7% and 10% and housing prices to fall between 8% and 11%. Subsequent robustness tests indicate that the decline in prices is correlated with the percentage of the population that was Hispanic and uncorrelated with the change in the documented population. Examination of the time-interaction coefficients and a synthetic control model also support these conclusions. Calculations of the welfare effects of these changes suggest a sizable impact. The loss of private wealth in owner-occupied housing could be as high as $40 billion. The loss of rental income could have a present value of $13.8 billion. This chapter offers both a stark reminder of how public policy can have impacts beyond that which it is primarily focused upon. It also illustrative how newly available hedonic price indices can easily be used in light of exogenous shocks to measure housing market changes.

The rising importance of housing and real estate to finance has come to greater attention following the 2007 credit crunch when credit default swaps and mortgage backed securities spread the crisis to banks across the world. One aspect in which housing and finance interact is through the role of Real Estate Investment Trusts (REITs). REITs are companies which return at least 90% of their profits back to shareholders, and hence are exempt from federal income taxes. Though the first legislation permitting REITs in the United States was signed into law by President Eisenhower in 1960, it was not until the early 1990s that REITs became widely traded and that the firm structure began to be widely emulated in other countries. As of 2010, estimated assets held by REITs globally were greater than $1 trillion (Wechsler, 2012). Many REITs are publicly owned and openly traded on major stock exchanges. This makes them an attractive way for people to invest in the housing markets without actually buying physical property.

In the second chapter of my dissertation I focus on the interaction between housing prices and REITs. I examine directly the link between the housing market and finance by investigating the dynamic relationship between REIT stock prices and housing prices. This answers the question as to whether REITs contain information about the future movement of housing prices or simply respond to the changes in the housing market. The results indicate that REITs and housing prices move together in a long-run cointegrated relationship and that short-run disequilibrium is corrected by subsequent movements in REIT. This arises from the fact that transitory shocks are more prevalent in REIT variation, while the majority of shocks to housing prices are permanent. I use the cointegrating residual from the long-run relationship to perform out-of-sample forecasting of REITs. The results show that this cointegrating residual significantly improves the forecasting performance.

These findings led me to question whether house price index announcements had an effect on REIT daily returns. My final chapter uses an event study approach to examine this question in depth. Given that it is often difficult to find announcement effects in equity market returns, see Flannery and Protopapadakis (2002) for example, I find that surprise announcements in terms of the S&P Case-Shiller Home Price Index have very strong effects on daily REIT returns. I found that a one-percent surprise increase in the S&P Case-Shiller announcement has an average effect of around 2.5% increase on Equity REITs. I also find that other macroeconomic announcements are important and that using an empirical breakpoint test as opposed to relying on ad hoc definitions of coefficient regime changes results in more significant announcement effects than would otherwise be expected.

Separately these chapters represent important additions to their respective literature. Together they point towards a future where our empirical understanding of the dynamics of housing markets is much deeper than has been previously possible.

Chapter 2

The Effect of Arizona's Immigration Enforcement Legislation on Housing Prices and Rents

2.1 Abstract

In this first chapter, I examine the effect of the passage and implementation of Arizona's immigration enforcement laws upon local home prices and rents. By using difference-in-difference across several data sources show that the passage of Arizona's law and its subsequent implementation caused rents to fall between 7% and 10% and housing prices to fall between 8% and 11%. Subsequent robustness tests indicate that the decline in prices is correlated with the percentage of the population that was Hispanic and uncorrelated with the change in the documented population. Examination of time-interaction coefficients and a synthetic control model also support these conclusions. Calculations of the welfare effects of these changes suggest a sizable impact. The loss of private wealth in owner-occupied housing could be as high as $40 billion. While the loss of rental income could have a present value of $13.8 billion.

2.2 Introduction

There is much anecdotal evidence to suggest that the passage of recent immigration enforcement legislation has caused hard times for small businesses, the absence of children from schools, and the flight of undocumented families toward states with less restrictive legislation (Gomez, 2010). While some statistics suggests potentially large movements in population in response to this type of legislation, strong empirical evidence is rare.

In this chapter, I attempt to quantify the effect of this legislation upon rents and housing prices in Arizona around the state's passage and implementation of immigration enforcement legislation in 2010. A body of literature has attempted to use natural experiments and a difference-in-difference framework to quantify the impact of immigrants on their host nation.[1] One of the best known is Card's work on the Mariel Boatlift, which measures the impact on wages in Miami after 120,000 Cubans immigrated to southern Florida over a 5-month period in the early 1980's (Card, 1990). In this chapter I propose that the passage of Arizona's immigration enforcement legislation acted as a "boatlift" in reverse. Immigrants with shallow roots found it easier to move to avoid a new and unknown legislative regime rather than risk being caught up in its enactment.[2]

In applying these natural experiment techniques to housing markets, my chapter joins Saiz (2003)'s extension of Card (1990)'s analysis of the Mariel Boatlift. Saiz found that the Boatlift increased Miami's renter population by 9% and associated rents by between 8% and 11%. Saiz (2007) also generalized his findings through a national sample of rents and housing prices to conclude that a one-percent increase in the population, caused by immigrants, causes a one-percent increase in local rents.

[1]Greulich et al. (2004) take a different path by arguing that immigrants' effect on the ratio of rent-to-income remains stable. This indicates that there is limited negative impact of immigrants upon the housing rents paid by native renters. Card (2009) says this implies that the rents are offset by their effect on wages, and that the increase in housing prices is usually small enough to be offset by the impact of average earnings in most cities, assuming some housing supply elasticity. It is unclear if this continues to hold true during times of recession

[2]Myers and Lee (1998) pointed out that immigrants often display a network effect when choosing where they will live. They are more likely to settle in cities where previous immigrants from their community have settled. This implies that effects may magnify over time and be easily observable within relatively small geographic areas. Also, since housing often makes up a large percentage of people's regular expenses and immigrants' impacts upon the price of housing and rents may contribute to a large portion of immigrants' welfare effect on society.

Estimates of immigration's effect on prices are generally larger and less precise.

While the Arizona enforcement laws were never fully implemented, there is evidence that the passage and subsequent publicity had real effects upon the state. The Mexican government recorded that 23,380 Mexican citizens had moved back from Arizona between June 1st, 2010, and September 28th, 2010 (Secretaría de Relaciones Exteriores, 2010). Banco Bilbao Vizcaya Argentaria (BBVA) Research used Consumer Population Survey (CPS) data to estimate that there were 100,000 fewer Hispanics in Arizona from the start of 2010 until November of that year (BBVA Research, 2010). Most convincingly, the official U.S. government numbers published through the Department of Homeland Security (DHS) indicate a drop of 110,000 undocumented immigrants between 2010 and 2011 (Hoefer et al., 2012). While the departure of these immigrants may not have been as visually stunning as the arrival of Cubans during the Mariel Boatlift, the absolute numbers appear to be similar and their impact upon their place of departure should be concentrated enough to create statistically observable effects.

The findings of this chapter indicate that the passage of the Arizona's immigration enforcement legislation led to a large negative impact on rents. It also seems to have contributed to a more rapid decline in housing prices in Arizona during the collapse of its housing bubble. The estimates indicate that the average rent in Arizona fell between 7% and 10% and that the local housing prices declined by between 8% and 11% following the partial enactment of the legislation. These results are stable through the addition of controls covering local economic activity. Robustness checks suggest that the fall in price and rent was greater in areas with larger Hispanic populations and that changes in the documented population do not explain the price changes being observed.

The structure of this chapter is as follows. In "Background" I presents some history on Arizona's immigration enforcement legislation. Under "Data" I highlight the sources benefits and limitations of the data used in this chapter. "Methodology" explains the use of difference-in-difference framework in this context. Finally "Conclusion" includes some welfare analysis, policy implications, and possible extensions.

2.3 Background on Arizona's Legislation

As documented by Morse (2011a), the effects of Arizona's immigration enforcement laws consist of two separate but related bills. The first is Arizona SB 1070, which contains a number of provisions relating to the enforcement of federal immigration laws. It restricts state and local officials from acting to prevent federal officials from enforcing immigration laws. It requires state and local law enforcement to determine a person's immigration status during a lawful stop and allows them to arrest that person if they believe there is probable cause that the person has committed an offense that may make him or her removable from the United States. SB 1070 also creates a state violation for immigrants not carrying an immigration registration document, as well as another violation for unauthorized immigrants who solicit or apply for work. Finally, it creates misdemeanors for hiring unauthorized immigrants from a motor vehicle and for transporting an unauthorized immigrant if the driver is already in violation of a criminal offense. The second piece of legislation is HB 2162, which amends SB 1070 to clarify the language such that law enforcement officials cannot consider race, color, or national origin when implementing the law. Both laws were passed by April 30th, 2010, and were expected to come into full effect July 29th, 2010.

The day before the laws were to come into effect a federal judge placed most of the new regulations on hold, leaving only three provisions to be enacted. These were the provisions that prevent state officials from hindering federal officials' immigration enforcement efforts, the new traffic violation for picking up day laborers, and some new restrictions concerning how employers check employees' eligibility. The decision was eventually appealed to the U.S. Supreme Court. Lam and Morse (2012) documents that in June 2012, the U.S. Supreme Court ruled that of the portions of SB 1070 challenged, only the portion allowing law enforcement officers to determine immigration status during a lawful stop was constitutional. The Supreme Court struck several provisions including those that allowed for warrantless arrest, that made it a state crime to fail to carry a federally issued immigration document, and that made it illegal for undocumented immigrants to solicit, apply for or perform work

According to Morse (2011b), these laws are important in part because of the national interest and similar legislation that they inspired. As of June 2011, over 53 similar 'omnibus' immigration enforcement bills had been introduced in 30 states. Five states passed similar legislation in 2011 including Alabama, Georgia, Indiana, South Carolina and Utah, and portions of each have been challenged in court and prevented from coming into effect.

2.4 Data

In this study I use three different sources for rental and housing data. House price data at the state and MSA level are obtained from the Freddie Mac House Price Index (FMHPI). Data on rents come from the Consumer Price Index (CPI) and the U.S. Department of Housing and Urban Development yearly Fair Market Rate (FMR) estimates. Each of these data sets yields similar results.

2.4.1 CPI Rent of Primary Residence

The Bureau of Labor and Statistics (BLS) calculates two estimates of the cost of shelter through the CPI Housing Survey. The one used in this chapter is the Rent of Primary Residence. This data is derived from the question, "What is the rental charge to your CU for this unit including any extra charges for garage & parking facilities? Do not include direct payments by local, state or federal agencies. What period of time does this cover?" This is superior to the other question concerning rent in the CPI survey, which concerns the Rental Equivalence of Owner-Occupied Housing. Rental Equivalence is an estimate by homeowners of what they believe they could receive if they had rented out their entire house unfurnished. Since the home owners are currently occupying their residence, it seems questionable that they have a complete knowledge as to the market value of renting their current residence.

The sample for the CPI Housing Survey is generated through a biannual survey of selected rental units that are geographically stratified. Newly built housing units are added regularly through a sample of building permits obtained from the Census Bureau. Rental prices are weighted for physical characteristics as well as types of utilities and government provided financial subsidies. Rent of Primary Residence data were extracted from the St. Louis FRED database, which itemizes rent of primary residence for 27 large Metropolitan Statistical Areas (MSA) and Consolidated Metropolitan Statistical Areas (CMSA). The CPI yearly estimates are available for ten years covering 2002 to 2011 and samples were indexed to 2002 for convenience.

2.4.2 Department of Housing and Urban Development: Fair Market Rate

The second source of rent data is from the U.S. Department of Housing and Urban Development (HUD). The Fair Market Rate (FMR) is a prediction of future rent prices for 2-bedroom apartments that cover all 366 MSAs and are used in several government programs to determine levels of rent subsidization. FMR predictions for each geographic area are made and published by October 1st of the previous year with the intent that they represent the best estimate for rents for April 1st of the following year. This requires some care when interpreting the FMR coefficients for time and place interaction. For example, the data in the 2012 FMR reflects surveys obtained sometime prior to October 2011. For the purpose of this chapter, I treat FMR years 2010 and 2011 as lead effects, as they should correspond to forecasts made upon data obtained before the Arizona legislation was passed and enacted. Historical FMR is available from 1983 through 2012, but the overall sample was limited to 2001 to 2012 to simplify comparison and to better match the control variables.[3]

Some sampling issues arise in the construction of the FMR data set. Prior to the 2012 estimate, FMRs were based on the 2000 U.S. Census and adjusted with commissioned surveys of renters who had moved in the last 15 to 24 months. However, the 2012 FMR was changed by using data from the American Community Survey (ACS) 5-year estimates to create a base average for each MSA's rent. This method was then dropped if the survey data from the area were significantly different from the new ACS base estimates. For the purpose of this chapter, it is assumed that these changes included no systemic measurement error related to Arizona or Arizona's MSAs. This assumption does not appear excessive as the new estimation process

[3]Difference-in-difference estimates using the longer series were not notably different than the shorter series estimates.

primarily affects smaller geographic areas such as rural counties with fewer than 100 survey observations rather than MSAs.

It should also be noted that FMR is generally set as the estimated rent for the samples' 40% quantile.[4] Under certain rare circumstances, HUD allows the median estimate of the sample to be substituted. In the FMR estimate for 2012, 28 MSAs were allowed to use the median estimates, these included two Arizona MSAs: Tucson and Phoenix-Mesa-Glendale HUD (2011). The result of this change should be an increase in the reported FMR rate for these two MSAs and should therefore include a positive bias for Arizona rent MSAs during the treatment period. It seems reasonable to conclude that this change was implemented to buffer low-income renters receiving government subsidies from the severity of the fall in rents in Arizona, specifically in Phoenix and Tucson.

2.4.3 Freddie Mac House Price Index

The FMHPI is based on repeated sales and mortgage refinancing data held jointly between the Federal Home Loan Mortgage Corporation (Freddie Mac) and the Federal National Mortgage Association (Fannie Mae) from January 1975 onwards. The index is a repeated transaction hedonic statistic, which means it is a weighted average of repeated sales and refinances of the same property throughout the time period controlling for housing quality, location, and type of sale. The total data set from which the index is derived includes roughly 25 million transaction pairs of single family homes and townhouses. While the data cover all states and MSAs, it is not random since it is based on Fannie Mae and Freddie Mac's total loan portfolio history and not a random selection (Freddie Mac, 2013). Given these restrictions the Freddie

[4]For example, the 50% quantile corresponds to the sample median. So estimating FMR based on 40% quantile (also called the second quintile) corresponds to a slightly lower than average rent.

Mac indices are the most thorough and descriptive time series currently available among publicly available housing price indices.

The second Home Price index of interest consists of Freddie Mac's MSA level House Price Index. This data set will cover 366 MSAs from March 2001 to August 2011. These dates were chosen to minimize the difference in data sets caused by the geographic definitions of MSAs changing between censuses.[5] It should be noted that due to the form of construction of FMHPI, the MSA level data is in effect the unweighted disaggregation of the state level data. This specification of the data may leave out certain rural Freddie Mac observations that occur in areas within Arizona but outside of one of Arizona's six MSAs.[6]

2.4.4 Control Variables

Following Saiz (2007), the two main control variables utilized are the lags of the logs of income and unemployment. Both attempt to control for local economic activity that ought to impact house prices and rents. Unemployment rate data was extracted from the Bureau of Labor Statistics (BLS) in the form of the Local Area Unemployment Statistics (LAUS). As a proxy for per capita income, I used Average Weekly Wage (AWW) available from the Quarterly Census of Employment and Wages (QCEW) provided by the BLS. The advantage of this variable is that it is available quarterly, which is useful when looking at monthly house price data. AWW should have a positive coefficient, as AWW should increase with wealth and disposable income, thus leading to higher prices. Growth in the unemployment rate should have a negative effect on prices as people's demand for habitation is constricted.

[5]As with the other data sets, difference-in-difference regressions of longer time series length without controls have similarly significant results.

[6]Arizona's MSA's are Flagstaff, Lake Havasu City-Kingman, Prescott, Phoenix-Mesa-Scottsdale, Tucson and Yuma

A third control variable was constructed to attempt to control for overall population changes. MSA and state level population estimates were formed through monthly linear approximations of U.S. Census yearly estimates. The U.S. Census estimates population at state and county levels in five parts: base estimates, births, deaths, international migration, and domestic migration. The U.S. Census uses the decennial census as the base population. Changes to the base are estimated through several methods. First, births and deaths are calculated on a county level using data from the National Center for Health Statistics (NCHS). Both estimates are based by matching birth and death rates to each county's percentage of population based on sex, race and Hispanic origin. Estimates of net migration of foreign born, natives, and Puerto Ricans are based on American Community Survey data, while data on population movement for Armed Services personnel is based on data from the Defense Manpower Data Center (DMDC) and the 2000 Census. Finally, the U.S. Census estimates domestic migration based on changes in address of IRS tax returns and Medicare enrollment (U.S. Bureau of the Census, 2011). It is important to note that the U.S. Census' methodology does not account for changes in the undocumented immigrant populations.

2.5 Methodology

This chapter will examine the effect of the Arizona Immigration Enforcement legislation through a difference-in-difference methodology, of the form:

$$Y_{it} = s_i + \tau_t + \beta L_{it} + \epsilon_{it} \tag{2.1}$$

In which Y_{it} is the log of the home price index or rents. The individual geographic fixed effects, s_i, are included to control for non-time varying idiosyncratic variations.

Seasonal time variables, τ_t, are included to control for any nationwide time-specific events such as inflation, national-level economic shocks, etc. The treatment effect, β, will be defined as a location and time-specific interaction term that will be one for locations that are within Arizona during the time after which the immigration enforcement legislation has been passed and zero otherwise. I use clustered standard errors to correct for serial correlation. Bertrand et al. (2004) argue persuasively that clustering standard errors is optimal in difference-in-difference regressions.[7]

Lead effects are added to account for any confounding changes in house prices and rents prior to the laws' enactment. Lengths of the lead and treatment effect vary depending upon the data set. Both the CPI and FMR data on rent is yearly and is thus limited in number of post-legislation observations. The CPI rent data's lead effect is one for Phoenix in 2008 and 2009, while the treatment dummy covers years 2010 and 2011. The FMR housing methodology means that any year's estimate is only influenced from new survey data from the previous year. This implies the lead effects for the purpose of this chapter should include the interaction between Arizona's MSA's and FMR estimates for 2010 and 2011, as the 2011 FMR sampling seems to have taken place prior to the legislation's passing. Thus, the treatment effect for FMR covers only the interaction of FMR 2012 estimate.

One of the benefits of including the home price index is that the monthly estimates allow a greater frequency of observations with which to identify lead and treatment effects. The lead effect for the Freddie Mac House Price Indices are dummy variables that cover the interaction term between Arizona and the four months before SB 1070 was passed; this includes December 2009 to March 2010. The second treatment effect is the four month time period between the passage of the laws in Arizona in April and

[7]It has been noted that clustered standard errors are biased towards zero when dealing with limited numbers of treatment individuals. Running the same regressions with Newey-West standard errors did not lead to a difference in the inference Newey and West (1987).

the implementation of the law at the end of July. The final treatment effect is that which occurs after the law is implemented in August 2010 to the end of the time series in August 2011. This breakdown allows some inference as to the dynamic effects of the process being examined. The second fixed effect regression model attempts to control for time-varying Arizona variations with lagged control variables of the form $X_{i,t-1}$. These regressions have the form:

$$Y_{it} = s_i + \tau_t + \beta L_{it} + \gamma X_{i,t-1} + \epsilon_{it} \tag{2.2}$$

The main controls of AWW and unemployment rate are meant to control for Arizona's level economic variations that could affect home prices and rents. An additional control variable was suggested as the estimate for aggregate population and is included in Table 4. Interpreting the coefficient of the treatment coefficient as an average treatment effect would require some rather heroic assumptions concerning the strength of the control variables in relation to the dependent variable. However, a simpler and commonly held assumption that the supply of housing units is perfectly inelastic and stable would allow these coefficients to be viewed as being generated by movements in the demand curve (Saiz, 2007). Thus, given the controls, the interpretation of the treatment coefficient as having come predominately from the departure of undocumented immigrants does not seem implausible.

In general, difference-in-difference also requires care in terms of potential unobserved linear trends and control group selection. This is a great concern since the treatment group is Arizona after the 2007 housing price collapse. The results are also robust to the addition of Arizona-specific time trend, as well as Arizona-specific time effects. Abadie et al. (2010)'s synthetic control model also seems to support the finding that house prices were negatively impacted by the legislation.

2.6 Empirical Results

Due to the collapse of the Arizona housing bubble, much of the argument that the legislation had a causal effect is based on the differential movements of rent and housing prices during the sample period. Observation of the CPI data shows rents in Phoenix were stable and increasing until 2010, then dropped sharply and stabilized. This differs markedly from the housing market in Arizona which according to the FMHPI reached its maximum in March 2007, then began to decline. The U.S. Census estimates for this time period indicate that the population of Arizona was actually expanding across the entire sample during the same period the Department of Homeland Security estimates of the undocumented immigrant population in Arizona show a sharp drop for the year 2010. Table 2.1 includes the regression using the Rent of Primary Residence data from the CPI.

The first regression estimated in Table 2.1 is equivalent to a difference-in-difference of the Phoenix rental market using a selection of the largest U.S. metropolitan areas as the counterfactual. The lead effects indicate that the two years prior to the legislation's enactment, Phoenix rents had not changed significantly from those of other large U.S. cities. This could be due to a differential impact of the collapse of the housing bubble and foreclosure crisis. Declining house prices should lead to decreases in rents. However, if the percentage of the population who are renters is increasing during this period, the decline in rents could therefore be delayed or non-existent. The CPI Rent of Primary Residence shows a difference-in-difference treatment effect of about 8.5% following the legislation. Including controls in the second regression results in only a slight decline in the size of the drop of rents to about 7.3%. These observations are reinforced through examining the FMR data in regressions III and IV of Table 1. The lead effects for these regressions are likewise insignificant and the coefficients indicate a fall in rents within the range of 8.3% and 10%. These

Table 2.1: Regressions for Rent

	CPI		FMR	
	I	II	I	II
Lead Effect	-0.006	0.000	-0.016	-0.020
	(0.011)	(0.011)	(0.038)	(0.033)
Treatment Effect	-0.085***	-0.073***	-0.083***	-0.100***
	(0.011)	(0.009)	(0.032)	(0.033)
AWW		0.845**		0.522***
		(0.270)		(0.082)
Unemployment		-0.017		0.064***
		(0.027)		(0.015)
Specifications				
Clustered SE	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
R^2	0.80	0.97	0.79	0.78
N	27	27	366	366

Note: Time series is from 2001 to 2012. Lead Effects for CPI are 2008 and 2009. Treatment effects for CPI are 2010 and 2011. Lead Effects for FMR are 2010 and 2011. Treatment Effects or FMR is 2012. Two and three asterisks signify significance at the 5% and 1% levels, respectively. AWW and Unemployment are lagged and in logs.

results are larger regardless to the positive bias inherent in the FMR sampling as mentioned in the data section. Replacing the Arizona lead and lag dummy variables in the regressions with controls with year specific interaction terms allows for a visual interpretation of how these treatment effects are spread over time. Figure 2.1 includes these interaction terms with standard errors for the CPI regression. Figure 2.2 includes the interaction terms for the FMR data.

The effects upon the Arizona housing market using the Freddie Mac state-level index are more complicated since the lead effects indicate that the Arizona housing market began the 2010 year already significantly below the national average. State-level housing prices are examined in Tables 2.2.

Figure 2.1: Estimated Effect on Rent of Primary Residence and CPI in Arizona with 95% Confidence Intervals

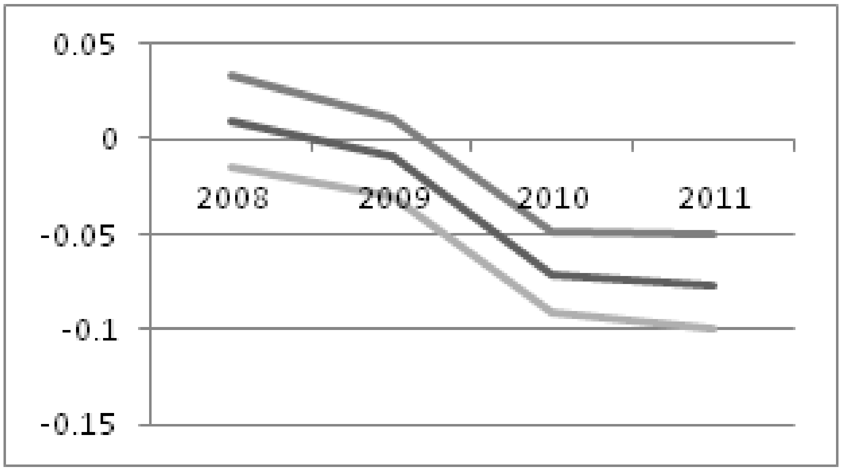


Figure 2.2: Estimated Effect on Fair Market Rate for Arizona MSAs with 95% Confidence Intervals

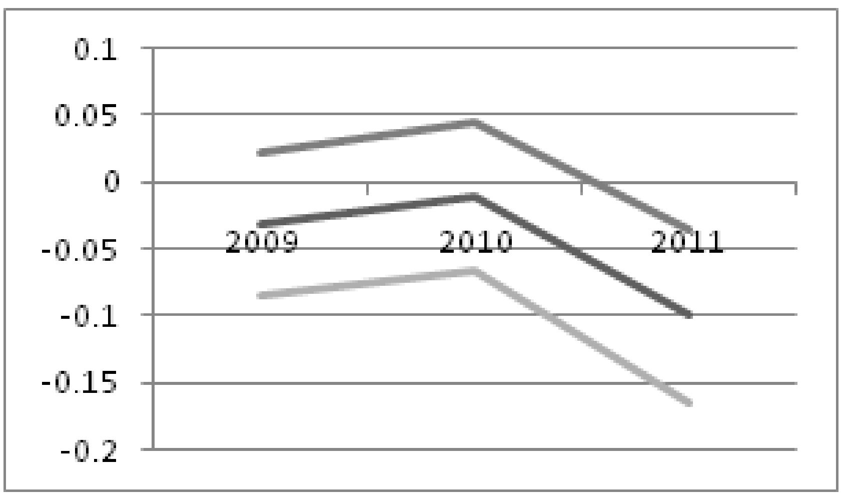


Table 2.2: Regression on Housing Prices

	State Level		MSA Level	
	I	II	III	IV
Lead Effect	-0.193***	-0.146***	-0.121***	-0.101***
	(0.016)	(0.020)	(0.035)	(0.026)
Law Passed	-0.217***	-0.158***	-0.152***	-0.120***
	(0.016)	(0.021)	(0.036)	(0.029)
Law Implemented	-0.313***	-0.269***	-0.228***	-0.211***
	(0.018)	(0.021)	(0.034)	(0.026)
AWW		0.752***		0.786***
		(0.27)		(0.090)
Unemployment		-0.245***		-0.227***
		(0.051)		(0.017)
Specifications				
Clustered SE	Yes	Yes	Yes	Yes
Month and Year Effects	Yes	Yes	Yes	Yes
FE	Yes	Yes	Yes	Yes
R^2	0.63	0.72	0.509	0.599
N	50	50	366	366

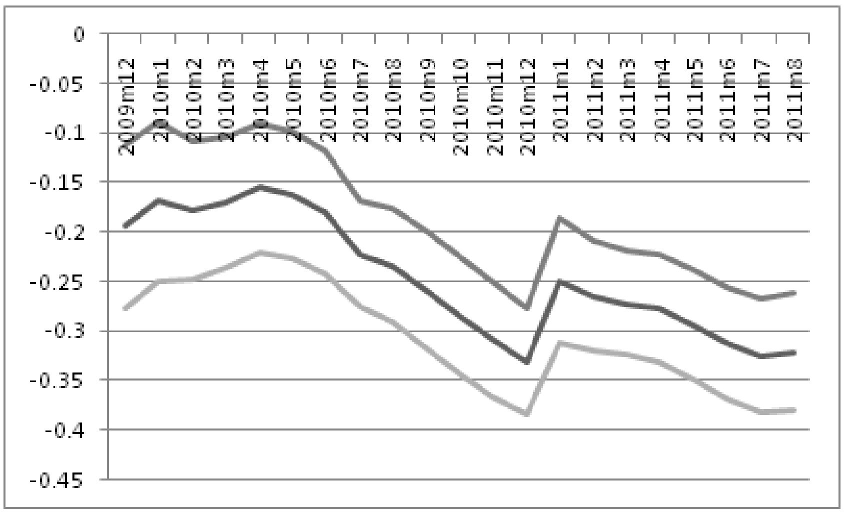
Note: Series are from 2001M3 to 2011M8. Lead Effects are from 2009M12 to 2010M3. Law Passed denotes period from 2010M4 to 2010M7. Law Implemented denotes period from 2010M8 to 2011M8. Two and three asterisks signify significance at the 5% and 1% levels, respectively. AWW and Unemployment are lagged and in logs.

The lead effects in these regressions indicate that the Arizona state housing market was already markedly below that of the counterfactual. The dummy variable indicating the three-month period in which the Arizona immigration law was passed but not yet implemented shows a slight decrease in house prices, though they are not remarkably different than what was observed before the law was passed. However, in the year following the implementation of the law, housing prices dropped precipitously. The difference between the coefficients from when the law was passed and when it was implemented is 9.6% for the first regression and becomes 11.1% when the control variables are included. The results using data at the MSA level are given in regressions III and IV. These suggest a fall in housing prices between 7.6% and 9.1%

depending on the model specification.

Figure 2.3 and Figure 2.4 include the estimated coefficient of month-state interactions. Both the state and MSA level regressions show the main negative impact on housing prices occurred between July 2010 and December 2010. The difference between the Law Implemented dummy and the Lead Effect for housing is in the range of an additional 11% and 12% below the national expectations. These results hold when lagged variables are added to control for the local Arizona economic conditions.

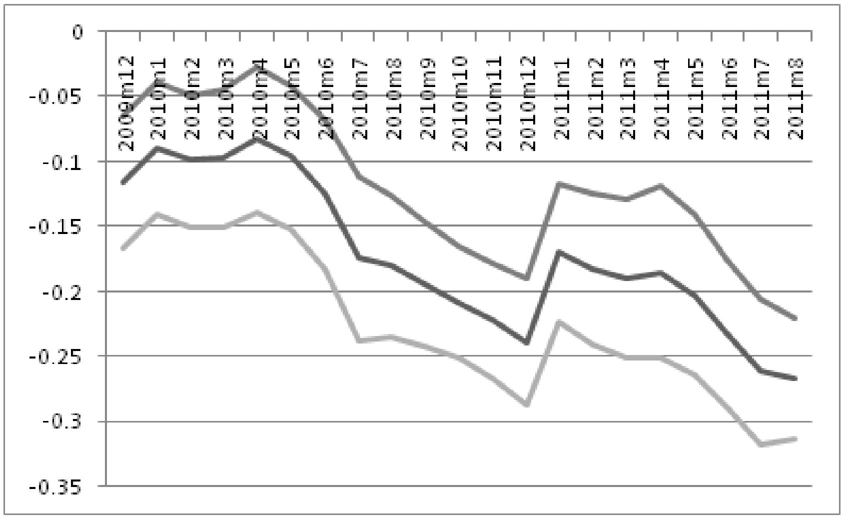
Figure 2.3: Estimated Fixed Effect Coefficients and 95% Confidence Interval on State Level Prices



The Arizona housing markets begin the period below the national average but quickly fall after the passage of the bills and continue to fall until the end of the year. This seems to be consistent with the hypothesis that the legislation caused a large outflow of undocumented workers, leaving a surplus of apartments and houses for rent.

While undocumented immigrants are probably less likely to be homeowners, the

Figure 2.4: Estimated Fixed Effect Coefficients and 95% Confidence Interval on MSA Level Prices



passage of the legislation may have impacted housing prices directly in numerous ways. Some families consist of combinations of both documented and undocumented immigrants, and possibly U.S. citizens. It is therefore possible that some families chose to put their houses on the market in an attempt to leave Arizona before the legislation became effective. Also, the Arizona housing market has one of the largest collections of secondary homes. This sub-market may be impacted by the legislation if owners were renting second homes to undocumented immigrants and were then unable to pay mortgage payments when tenants left over a short period of time.[8]

In comparing the coefficients of the control variables in the housing price regressions to those in the rent models, it should be noted that there is some difference. The lags of the control variables in the FMHPI data regressions are significant and show

[8]An alternative interpretation could center on the negative publicity and boycotts associated with Arizona's legislation. These may have made houses in Arizona less attractive both as a place for people to retire to as well as an investment for more institutional investors. This interpretation seems less likely in light of the Census' estimate of growing Arizona documented population during this time period.

the expected sign across all regressions. However, in the rental data sets the lag of unemployment is either insignificant or significantly positive. This is consistent with the fact that the collapse of the housing bubble was correlated with the decline in the economy during the latest recession. Most likely foreclosure forced former home owners into rental properties. Thus, increased unemployment may have led to increased demand and prices within the rental market even as the economy was in decline.

The absolute size of the treatment effects seems somewhat larger than has been previously presented in the literature. Saiz (2003) noted that a 9% increase in the renter population from the Mariel boatlift resulted in an increase in Miami rents of between 8% and 11%, though potentially more within the lower quintile of rental market. A simple comparison to this study with Census data suggests that the 2010 rental population of Arizona was about 2,439,840 people. Assuming the 110,000 undocumented immigrants who were estimated to have left the state were all renters, this would correspond with a 4.5% decline of the renter population. This suggests that the rental effect measured from the Arizona immigration enforcement legislation was actually larger than the recorded Mariel Boatlift effect. However, in light of the fact that both treatments occurred during a recessions which had previously caused a downturn in the housing and rental markets, the results make sense. The treatments are, in fact, in opposite directions; the Mariel Boatlift adding value to property prices during a weak period resulted in a smaller recorded treatment effect. However, the Arizona treatment was pro-cyclical in the sense of creating further downward pressure on a market already under stress. Given this rationale, these findings fit very well within the general framework that changes in population due to immigration impact both home prices and rents.

The variation across MSAs allow for a check on whether more heavily Hispanic MSAs are differently affected. Table 2.3 contains the coefficients of interaction of the treatment effect term with the percentage of Hispanics in each MSA as provided by the American Community Survey. The coefficient using both sets of MSA level data are significant and negative. The sign and the significance imply that housing prices and rents have dropped more in areas with a large Hispanic population.

Table 2.3: Hispanic Interaction Terms across MSAs

	FMHPI	FMR
Hispanic Interaction	-0.005***	-0.002***
	(0.0003)	(0.001)
Specifications		
Clustered SE	Yes	Yes
Time Effects	Yes	Yes
Individual Fixed Effects	Yes	Yes
R^2	0.598	0.78
N	366	366

Hispanic Interaction term is the interaction between treatment effect and percentage of population which is Hispanic for each MSA. Three asterisks signify significance at the 1%.

The main argument of this chapter is based on the assumption that the chief purpose for houses and apartments is to provide shelter for people, so it should follow that large out flows of population have a significant negative effect on the demand for both commodities. To check whether the change in prices and rents were caused by swings in the aggregate population, a linearization of the U.S. Census' estimated change in population is included in Table 2.4. If aggregate population is a major factor, this control should catch some of the significance observed in the treatment dummies.

However, across all data sets the results tend to indicate that if anything, the inclusion of this variable leads to an increase in the difference between the lead and treatment effects. As mentioned before, when observing the actual state population

Table 2.4: Regressions Including Estimated Population Change

Data Sets:	CPI	FMR	HPI: States	HPI: MSA
	I	II	III	IV
Lead Effect	0.022	-0.017	-0.178***	-0.115***
	(0.016)	(0.027)	(0.037)	(0.029)
Law Passed			-0.18***	-0.131***
			(0.030)	(0.031)
Law Implemented	-0.05***	-0.097***	-0.287***	-0.221***
	(0.016)	(0.028)	(0.028)	(0.027)
AWW	0.932***	0.542***	0.732***	0.742***
	(0.262)	(0.086)	(0.249)	(0.084)
Unemployment	-0.023	0.066	-0.245***	-0.231***
	(0.025)	(0.150)	(0.054)	(0.018)
Population	-0.251	-0.084	0.47	0.405***
	(0.186)	(0.087)	(0.365)	(0.093)
Specifications				
Clustered SE	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
R^2	0.85	0.781	0.724	0.606
N	27	366	50	366

Note: Series lead and treatment effects are the same for each data set as those described in corresponding regressions shown in Tables 1 and 2. Three asterisks signify significance at the 1%. Population, AWW and Unemployment are lagged and in logs.

estimates in their raw form, the reason for this becomes clear. While the number of undocumented immigrants fell by roughly 110,000 in the year 2010 to 2011 according to the Department of Homeland Security, the U.S. Census Bureau estimates for Arizona's population increased. This is probably due to the internal migration portion of the U.S. Census Bureau' estimates relying primarily on changes in IRS tax returns and Medicare recipients' addresses. Thus, the best local population estimates available tend to misestimate actual population change that is caused by the undocumented segment of the population.

One suggestion was to split the treatment group by population to observe any differential impact in terms of large and small MSAs. Given the limited number of

MSAs in Arizona, it was decided to split the MSAs between those with more than and less than 500,000 residents. The MSAs with more than 500,000 residents consist of Phoenix and Tucson while those with fewer include Prescott, Flagstaff, Lake Havasu, and Yuma. These results are included in Table 2.5.

Table 2.5: Regressions on Samples of Differing Populations

	FMHPI		FMR	
	MSA > 500k	MSA < 500k	MSA > 500k	MSA < 500k
Lead Effect	-0.067	-0.111***	-0.039***	-0.01
	(0.037)	(0.033)	(0.010)	(0.041)
Law Passed	-0.079	-0.137***		
	(0.051)	(0.034)		
Law Implemented	-0.212***	-0.208***	-0.022	-0.133***
	(0.042)	(0.029)	(0.042)	(0.036)
AWW	0.724***	0.820***	1.018***	0.437***
	(0.194)	(0.099)	(0.231)	(0.086)
Unemployment	-0.317***	-0.198***	0.138***	0.046***
	(0.042)	(0.017)	(0.036)	(0.016)
Specifications				
Clustered SE	Yes	Yes	Yes	Yes
Month and Year Effects	Yes	Yes	Yes	Yes
R^2	0.62	0.62	0.68	0.82
FE	Yes	Yes	Yes	Yes
N	98	268	98	268

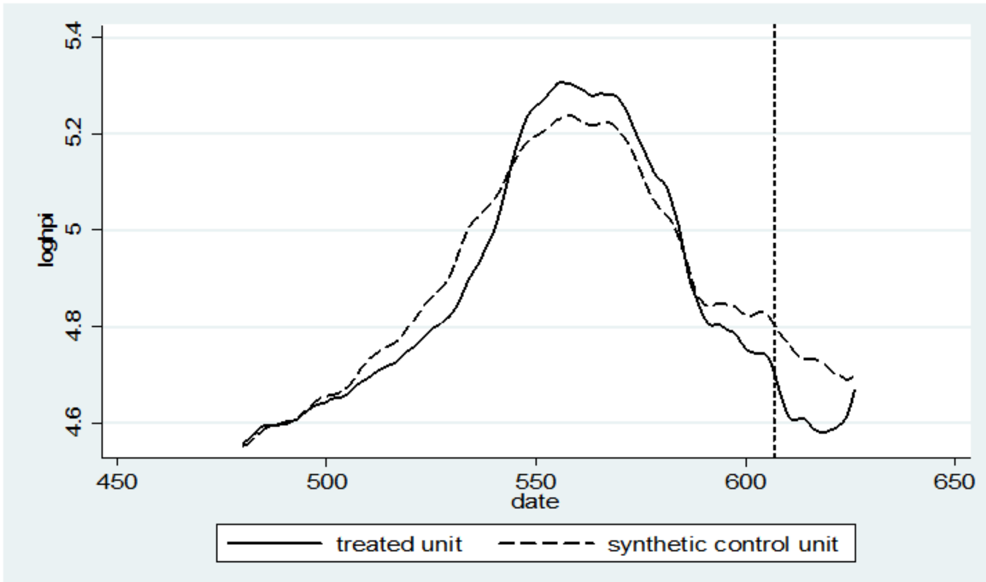
Three asterisks signify significance at 1%. AWW and Unemployment are lagged and in logs.

The housing price regression in Table 2.5 gives some support to the argument that the decline in the Arizona housing market focused in Phoenix and Tucson followed the legislations implementation. However, the overall impact does span both large and small MSAs. Regressions III and IV show that in the FMR data rent prices did not fall significantly in Phoenix and Tucson, but as noted before, both these MSAs had their sampling changed for the 2012 FMR and therefore should be biased towards zero. A further issue within these findings is that they are contradicted by the results from the CPI reported in Table 2.1. For these results, Phoenix rental prices are shown to have fallen dramatically in comparison to the control of other large U.S. cities. Therefore, what I conclude from Table 2.5 is that the impact of the legislation on rents was as widespread outside of metropolitan areas as it was shown to be within Phoenix in Table 2.1.

One more criticism of difference-in-difference methodology is the centrality of the control group in estimating an effect. Abadie et al. (2010) suggest a data-driven process for choosing an optimal control group in creating the synthetic control method. In applying this process to state-level Arizona housing prices, the synthetic control is created by matching on unemployment, AWW, population, and Hispanic population. Results are shown in Figure 2.5.

The optimal synthetic control group following Abadie et al. (2010)'s methodology consists of a composite home price index weighted by 24.4% of California's house prices, 14.5% of Florida's, 23.3% of Nevada's, 30.7% of New Mexico's, and 9% of Utah's. These housing markets give the best match to Arizona's over the time period. While the synthetic control forecast does differ from Arizona's prior to the June 2010 treatment date, the fall in home prices clearly accelerates after the legislation is passed and implemented. While inference using this model is lacking, it does indicate the effect being picked up in the regression models is not due to a sample selection issue.

Figure 2.5: Estimated Synthetic Control Model for Arizona Housing Prices. The solid line is the log of Arizona home price index for the time period 2001 to 2012. The hashed line is the calculated optimal synthetic control group. The vertical dotted line represents the enactment of Arizona's immigration legislation in July 2010.



2.7 Conclusion

In this chapter I have estimated the effect of the passage and implementation of the Arizona immigration enforcement legislation on housing market. The results indicate that the short term effect of the legislation was negative and significant in both markets. Further analysis suggests that the most likely cause of this was a large decline in the population of undocumented immigrants. From a theoretical perspective, observation of both the direction and magnitude of these effects are useful in terms of quantifying the real impact of immigration and the underpinnings of the housing market. The findings support those of Saiz (2007), which argue that immigrants have a very real impact on housing and rental markets. They also suggest that prices in these markets are not as static as they seem, and that changes can occur rapidly if the determinants of demand change significantly over a short period of time.

In terms of welfare and policy recommendations, the fact that legislation can lead to precipitous declines in population, which in turn negatively impacts the demand for housing, should be of much interest. Calculations of the welfare effects of these changes suggest a sizable impact. The loss of private wealth in owner-occupied housing could be as high as $40 billion.[9] Loss of rental income could be as much as $57 million per month, which would have a present value of $13.8 billion.[10]

While these effects are large, they do not contain the total impact of this legislation on the Arizona economy. For example, hedonic house price indices do not contain the sales that result from foreclosure. Also, any lost welfare from an increase in Arizona foreclosures following the enactment is therefore not being captured in this study. In addition, secondary macroeconomic effects are not being captured. Due to the important role the housing industry has played in the economy and consumers' response to losses in wealth, the final economic impact may be much more substantial.[11]

[9]An estimation of lead and treatment effects suggests that Arizona's immigration enforcement legislation led to a 10% decline in home prices. According to the American Community Survey (2013), Arizona has 1,877,387 owner-occupied houses, with a 2010 5-year estimated median value of $215,000. This would give an estimated value for owner-occupied housing units in Arizona of a little more than $400 billion, so a 12% change would be a cost of $40 billion in private wealth.

[10]Given 33% of occupied homes are rental units, Arizona in 2010 had about 785,727 rented units (American Community Survey, 2013). The average fall in FMR rate for 2-bedroom apartments in Arizona was $73.15 per month. This implies a loss to monthly rental income of about $57 million. Assuming yearly leases, this implies a potential loss of almost $690 million over the course of a year. Net present value is estimated as this quantity times the inverse of the interest rate, here assumed to be 5%.

[11]According to the National Association of Home Builders (NAHB) these two areas made up between 17% and 18% of U.S. GDP between 2007 and 2009. According to the Bureau of Labor Statistics at its nadir during the Great Recession the U.S. economy had shed 8.8 million jobs Goodman and Mance (2011). Over 17% of this or 1.5 million jobs lost were from the construction industry Hadi (2011). In addition, housing has a large impact on consumption since 66% of households have more wealth in their houses than in stocks. Homeownership compels households to save through mortgage payments from which the principal has traditionally been recouped by the sale of the

The findings in this chapter support those of other work on the impact of immigration on housing. They are also noteworthy from a theoretical perspective as they show the effects of a decrease in the immigrant population, as opposed to previous research, which has focused primarily on increases in the population from immigration. This raises the possibility of future research as to whether positive and negative changes in population on home values and rents have symmetric effects. In addition, this chapter does not look into the effect of other states' attempts at immigration enforcement legislation. While some similarities exist between these laws and their implementations, the centrality and attention paid to Arizona' may be unique. Future research may reveal whether this is true.

property in the future (Siniavskai, 2005). As Kishor (2007) has shown, consumption responds more strongly to home price fluctuations than to stock market fluctuations.

Chapter 3

Understanding the Dynamic Relationship Between Real Estate Investment Trusts and Housing Prices

3.1 Abstract

In this chapter, I investigate the dynamic relationship between Real Estate Investment Trust (REIT) stock prices and housing prices. I examine whether REITs contain information about the future movement of housing prices or simply respond to the changes in the housing market. The results indicate that REITs and housing prices move together in the long-run and any short-run disequilibrium in that relationship is corrected by the subsequent movements in REITs. This arises from the fact that transitory shocks are more prevalent in REIT variation, while the majority of shocks to housing prices are permanent. I use the cointegrating residual from the long-run relationship to perform out-of-sample forecasting of REITs and the results show that this cointegrating residual significantly improves the forecasting performance of out-of-sample REIT returns.

3.2 Introduction

Real Estate Investment Trusts (REITs) are companies that own and operate income producing real estate and do not have to pay federal corporate income tax if they distribute at least 90% of their taxable income as dividends to shareholders. Though the first legislation permitting REITs in the United States was signed into law by President Eisenhower in 1960, it was not until the early 1990s that REITs became widely traded and that the firm structure began to be widely emulated in other countries. As of 2010, estimated assets held by REITs globally were greater than $1 trillion (Wechsler, 2012). Many REITs are publicly owned and are openly traded on major stock exchanges. This makes them an attractive way for people to invest in the housing markets without actually buying physical property.

However, a debate exists on the exact relationship between REITs and housing prices. Beyond investments, REITs are also frequently used as proxies for housing prices in financial models, as they are traded in real time while other housing price measures are often available only monthly or quarterly. Ross and Zisler (1991) were among the earliest to examine the informational content of REITs. They argued that REITs are better than hedonic price indices as proxies for housing prices since REIT returns come from a continuous market and do not have additional factors such as appraisals in their construction. Some researchers have gone so far as to say that REITs are the best measure of housing prices (Cotter and Roll, 2011).

On the other hand, much of the earlier work on REITs found that the majority of variation inherent in REITs was linked to stocks and bonds with little left over in order to explain housing price fluctuations (Peterson and Hsieh, 1997). From this perspective, REIT proxies would add little information to any model that could not be controlled for by equity or interest rate variables. Conversely, Clayton and MacKinnon (2001) have noted that while REITs are highly volatile in the short-term, long-term movements in REITs are less correlated with the stock and bond market, and thus may contain information concerning housing markets. They argue that as REITs became more institutionalized and widely traded during the 1990s, they may have become more highly correlated with housing prices.

In this chapter, I examine what dynamic relationship REITs and housing prices have. One would expect that REITs contain information about future movements in house prices since they are traded actively in the stock market and have features of financial asset-prices.[1] However, the frequent booms and busts in REIT prices leads one to believe that REITs simply respond to movements in house prices. To examine this question I test whether REITs and house prices move together in the long-run. The results suggest the they are cointegrated and do move together in the long-run. Contrary to the forward looking nature view point of the REIT, I find that if there is a short-run disequilibrium in the cointegrating relationship, REIT returns adjust to correct for the disequilibrium.

I utilize the error-correction property of the REITs and house prices to measure the magnitude of the cyclical component in both the series using the Beveridge-Nelson decomposition(Beveridge and Nelson, 1981). The results from the Beveridge-Nelson decomposition show that REITs have a much larger cyclical component than do house prices. I also use the structural variance decomposition proposed by Gonzalo and Ng (2001) to estimate the relative importance of the permanent and transitory components of the dynamic relationship. Furthermore, the sign of the cycle is consistent with the recent booms and busts in REITs. These results are also consistent with the Vector Error Correction Model (VECM) result, where I find that REITs move to correct for the short-run disequilibrium. The Gonzalo-Ng results show that at differ-

[1]The forward-looking nature of prices is termed price discovery and has been explored in great detail in macroeconomic and finance literature. The conventional findings indicates that derived financial asset prices would contain information about the future movements of the asset they are derived from. Some related work has been done in describing the relationship between spot and futures markets for commodities, such as Garbade and Silber (1983) and Figuerola-Ferretti and Gonzalo (2010). However, much greater focus has been given to the price discovery relationships within the stock and bond markets. Garbade and Silber (1979) examined the central role of the New York Stock Exchange in pricing of stocks as opposed to other 'satellite' exchanges. Lien and Shrestha (2012) show that the market of credit default swaps often contain information concerning their attached bond markets. Thus derived assets often include forward looking information or signal market changes prior to those impacting their associated assets. As it applies to this chapter, changes within real estate markets could be predicted by returns to REITs today.

ent horizons between 23% and 38% of the variation in Equity REITs are permanent, whereas between 47% and 61% of the variation from the house price is permanent.[2] This is also in line with the expectation that REITs move in response to a temporary shock in the cointegrated system.

I then use the cointegrating residuals from the cointegrating relationship between REITs and house prices to examine whether the inclusion of the cointegrating residual in a baseline model significantly improves the forecasting performance of the one-step ahead REIT returns forecast. Using the error-correction property of the cointegrating model, I perform in-sample and out-of-sample forecasting exercises on REIT returns. First, I examine the long-horizon predictive power of the cointegrating residual for both REIT returns and house price growth. I also find that the predictive power of this cointegrating residual increases until the sixth or eighth horizon for REIT returns. I then perform out-of-sample forecasting analysis in which I add the cointegrating residual as a predictor to a series of recursive forecasts. The evidence shows that the inclusion of cointegrating residual significantly improve the one-step ahead forecasting performance of the model.

The structure of this chapter is as follows. The "Data" section presents a brief explanation of REITs and the hedonic price indices that I use. In the "The Dynamic Relationship between REIT and Housing Prices" section, I present the methodology and results of how REITs and house prices are related. This includes descriptions and results for the unit root tests, Vector Error Correction Models, variance decompositions, long-horizon regressions and out-of-sample forecasts. In "Conclusion" I include concluding remarks and extensions.

[2]The Beveridge-Nelson methodology corresponds to this paradigm in that the impact of the permanent component is limited to one future period ahead while the Gonzalo-Ng methodology allows for observation of different forecast horizons.

3.3 Data

3.3.1 REITs

In theory the prices of REIT should be comprised of either the present value of future returns from housing sales or the flow of rents from managed properties. Since these rents generally fluctuate over time in a constant ratio with house prices, it is reasonable to expect that the current price of a REIT stock should be related to the price level of the real estate that comprise its assets. While these assets may be different from those in the housing market, it is not gratuitous to assume that both groups of assets reflect some shared market or at least reflect market fundamentals similarly. However, much analysis of REITs has come to mixed conclusions as to what degree REITs can explain housing price fluctuations.

The main source of REIT data for this chapter was obtained from the National Association of Real Estate Investment Trusts (NAREIT, 2013). The monthly FTSE NAREIT REIT Index series includes data from December 1971 to December 2012. These series were first created to help in the construction of index tracking funds and as a performance benchmark for other assets (FTSE, 2013). REIT stocks included in this index are screened quarterly to insure that they are liquid and freely tradable.[3] While three types of REIT Indices are examined, the main interest is in the Equity REIT Index series which covers the REITs that are most commonly used when examining relationships between housing and REITs. Breakdowns of the Equity REIT Index constituents are given in Table 3.1 and 3.2.

As Table 3.1 shows, Equity REITs are primarily focused on commercial property which is linked to rental income. This is somewhat different from the All Equity and

[3] From NAREITs website, accessed 12 March 2013, the total number of REITs used in its monthly indices has fallen from 203 to 176 in 14 years, which means adjustments occur at an average of two per year since the index was created.

Table 3.1: Equity REIT Top 5 Constituents

Constituent	Property Subsector	NMCap (USDm)	Weight
Simon Property Group	Regional Malls	48,547	10.61
HCP	Health Care	20,461	4.47
Ventas Inc	Health Care	19,042	4.16
Public Storage	Self Storage	18,501	4.04
Equity Residential	Apartments	18,391	4.02
Totals		124,943	27.3

All REIT indices which are used for comparison later. While these indices give similar conclusions to the Equity REITs they include REITs whose future returns may not be directly in line with housing rents. For example, the All Equity series includes REITs associated with infrastructure and timber land while the All REIT Index includes mortgage REITs which invest in residential mortgages and other mortgage backed securities.

Table 3.2: Equity REIT Subsector Breakdown

Property Subsector	Number of Constituents	Net Mcap(USDm)	Weight%
Apartments	15	76,497	16.72
Diversified	16	38,218	8.35
Free Standing	6	11,982	2.62
Health Care	11	70,002	15.3
Industrial	7	22,709	4.96
Lodging/Resorts	14	28,078	6.14
Manufactured Homes	3	4,139	0.9
Mixed	5	10,014	2.19
Office	19	53,265	11.64
Regional Malls	8	77,072	16.84
Self Storage	4	26,261	5.74
Shopping Centers	18	39,417	8.61
Totals	128	457,654	100

From examining Table 3.2, it is evident that Equity REITs in general have a very limited exposure to residential housing of any kind. While the apartment sector is the second largest subsector still covers only 16.7% of the overall weight in the index. The commercial property aspect is plainly in the majority. Therefore, it is not surprising that Equity REITs would exhibit significantly different volatility than that which is observed in the private housing market.

3.3.2 Housing Price Data

The housing price data for this chapter are hedonic price indices obtained from Moody's, S&P, and Freddie Mac. In general, hedonic price indices are create from the weighted summation of the repeated sale, property appraisals, and refinancing of the same property over a number of years. I focus mostly on the Freddie Mac House Price Index (FMHPI) due to its length and sample breadth. Similar examinations, such as Cotter and Roll (2011), often prefer the S&P Case-Shiller Indices, but for my purpose this would impose a limit to the length of time series that I can examine. Moreover, the S&P Case-Shiller indices are limited to an aggregation of the largest 10 or 20 American cities while the FMHPI includes data across all United States regions. Thus, the S&P Case-Shiller Home Price Index should be biased towards the higher price end of the housing market. Similarly, while the price indices available from Moody's do give broader real estate sector breakdowns, such as the Apartment and Core Commercials indices that I examine below, they have an even shorter time series than that available from the S&P Case-Shiller indices.[4]

The FMHPI is based on repeated sales and mortgage refinancing data held jointly between the Federal Home Loan Mortgage Corporation (Freddie Mac) and the Federal National Mortgage Association (Fannie Mae) from January 1975 onwards. The index is a repeated transaction hedonic statistic, which means it is a weighted average

[4]To ensure that any relationship between the variables of interest is not caused by seasonal fluctuations in housing prices, the property price indices were seasonally adjusted using the Census X-12 program.

of repeated sales and refinances of the same property throughout the time period controlling for housing quality, location, and type of sale. According to Freddie Mac (2013), the total data set from which the index is derived includes roughly 25 million transaction pairs of single family homes. While the data cover all states in the U.S., it is not random since it is based on Fannie Mae and Freddie Macs total loan portfolio history instead of random selection. Given these restrictions, the Freddie Mac indices are currently the most thorough and descriptive time series currently available among publicly available housing price indices.

3.4 The Dynamic Relationship Between REITs and Housing Prices

The examination of the dynamic relationship between REITs and housing uses methodology based on the shared long-run relationship of cointegration. This methodology has several benefits in terms of super-consistency and forecasting implications, see Lettau and Ludvigson (2004) for example. However, it requires that the assumption that the system under investigation is in fact cointegrated is correct.[5] For cointegration to exist the series in question must be non-stationary in levels. The test statistics and p-values for both the REIT and house price indices are in Table 3.3. The Augmented Dickey-Fuller unit root test fails to reject the null of a unit root for all the series (Dickey and Fuller, 1979). These findings are in line with the expectation that both housing prices and the levels of equities contain a unit root.

Following Stock and Watson (1993), I use a dynamic ordinary least squares

[5] Engle and Granger (1987) explain cointegration as the property such that two non-stationary variables have a linear combination where the two variables are stationary. Examination of statistical relationships before acknowledging the cointegration of the variables in the system can result in spurious regression results.

Table 3.3: Unit Root Tests on Levels

	ADF Test Statistic
FMHPI	-1.92
	(0.32)
S&P Case-Shiller 10-City HPI	-1.11
	(0.71)
Moody's Apartment Price	-2.32
	(0.17)
Moody's Core Commercial	-2.24
	(0.19)
NAREIT Equity REIT Index	-1.09
	(0.72)
NAREIT All REIT Index	-2.20
	(0.21)
NAREIT All Equity REIT Index	-1.09
	(0.72)

P-values are in parentheses. Two asterisks denote coefficients significant at the five-percent level. Ng-Perron and Phillips-Perron unit root tests support these findings.

(DOLS) regression to estimate the long-run relationship. This has the advantage over the static OLS proposed by Engle and Granger (1987) in that it controls for serial correlation in both variables and exhibits better small sample characteristics. The DOLS equation takes the form:

$$r_t = c + \beta h_t + \sum b_{a,i} \Delta r_{t-i} + \sum b_{y,i} \Delta h_{t-i} + \epsilon_t \tag{3.1}$$

Where r_t is the Equity REIT index, h_t is the house price index, Δr_{t-i} and Δh_{t-i} are the lagged first differences of the respective series. Leads and lags were chosen conforming to SIC criteria. In Table 3.4 I present the coefficient of price index from the DOLS equation. The estimated coefficients of cointegrated series are known to be super consistent and converge to their asymptotic quicker than non-cointegrated estimation. As both variables are in logs I can interpret the coefficient as the long-run elasticity of the REIT type with respect to the given price index. There are some indications that movements in commercial prices have greater impact on REITs than housing prices. The FMHPI also has a greater elasticity than the S&P Case-Shiller Index though the time-series for these regressions are not the same. Also, the elasticity is larger for Equity and All Equity than for the All REIT classification. This is most likely due to the inclusion of mortgage based REITs in the All REIT category. Mortgage REITs primarily purchase mortgages and mortgage backed securities and have dynamics that are much different that the other REIT classifications.

Table 3.4: DOLS Estimates of Cointegrating Vector

	Coefficient	Std. Error	P-Value
FMHPI and Equity REIT	0.969	0.027	0.00
FMHPI and All REIT	0.474	0.042	0.00
FMHPI and ALL Equity	0.969	0.027	0.00
Moody's Apartment and Equity REIT	1.448	0.060	0.00
Moody's Core Commercial and Equity REIT	1.590	0.055	0.00
S&P Case-Shiller 10-City and Equity REIT	0.719	0.046	0.00

Standard errors are Newey-West HAC errors

Table 3.4 includes the DOLS estimated coefficients of price indices on REIT. Since both REITs and the indices are in logs, I can interpret these as the long-run elasticities of REIT with respect to each price index. The All REIT index has the lowest elasticity, which is most likely due to its inclusion of mortgage REITs as stated above. The Moody's Apartment and Core Commercial indices have much larger elasticities than the housing price indices, which may be due to their closeness to REITs fundamental asset or simply because their series span only the years 2001 to 2011. Shortened S&P or FMHPI variables, that correspond with the Moody's time series, have estimated elasticities which are also larger. This is not inconsistent with the belief that REITs and housing prices move together in the long-term, though it does highlight that the relationship may vary noticeably in the short-term. From these results I could form an estimate of the long-run relationship in terms of REITs in the form of Equation 3.2.

$$r_t = \hat{c} + \hat{\beta} h_t + v_t \tag{3.2}$$

Where, $\hat{c}$ and $\hat{\beta}$ are the estimated regression coefficient of house prices on REIT from Equation 3.1. The cointegrating residual can then be formed by solving Equation 3.2 in terms of v_t and using the actual REIT such that:

$$\hat{v}_t = r_t - \hat{c} - \hat{\beta} h_t \tag{3.3}$$

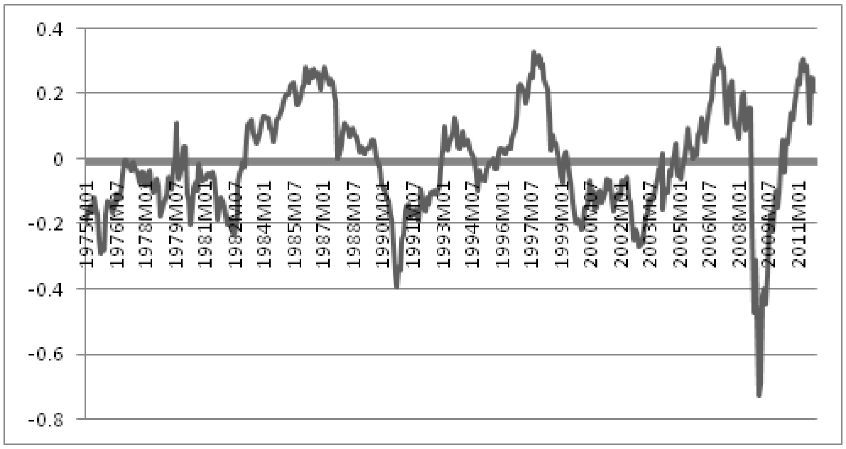
Where, the cointegrating residual, v_t, is the deviation of REITs from the common stochastic trend that they share with housing. For the FMHPI this residual is formed as:

$$\hat{v}_t = r_t - 0.013 - \hat{\beta} h_t \tag{3.4}$$

In Figure 3.1, the cointegrating residual for Equity REIT and FMHPI implies that any movement away from the long-run value for REITs will eventually be corrected in one of two ways. Either home prices must increase or REIT prices must fall. For example, the REIT movements from 2005 to 2008 indicate that valuations of REITs were higher than their long-run relationship with house prices; suggesting that either houses had been undervalued or REITs were overvalued. Similarly, after the financial crisis REITs seem to have expanded well above what their long-run relationship with housing prices would suggest is normal.

Assuring that REIT and house prices are cointegrated implies that their cointegrating residual is stationary or lacking in a unit root. I examine these residuals

Figure 3.1: Cointegrating Residuals for Equity REIT and House Prices



in Table 3.5 using the Augmented Dickey-Fuller unit root test on six REIT-housing price combinations. The test rejects the null of the series having a unit root in each of the specifications.

3.4.1 Vector Error Correction Model

An explicit examination of the relationship of REITs and housing prices is possible through the modeling of a Vector Error Correction Model (VECM) which I perform for each of the six REIT-housing price combinations. Intuitively, as the two variables move together over a long period of time, then it must be the case that one of the variables is adjusting to correct for shocks and to maintain their long-term equilibrium. This system can be estimated with a VAR system of the first-differences including the lagged cointegrating residuals as an exogenous variable such that:

$$\Delta x_t = c + \gamma \hat{v}_{t-1} + \Gamma(L)\Delta x_{t-i} + e_t \tag{3.5}$$

Table 3.5: ADF Test Statistic on Cointegrating Residuals

	ADF Unit Root Statistic
FMHPI and Equity	-3.90***
	(0.00)
FMHPI and All	-3.88**
	(0.01)
FMHPI and All Equity	-3.90**
	(0.00)
Moody's Apartment Index and Equity	-3.57**
	(0.01)
Moody's Core Commercial Index and Equity	-3.17**
	(0.02)
S&P Case-Shiller HPI and Equity	-3.57**
	(0.01)

P-values in parentheses, double asterisks denote coefficients significant at the five-percent level. Ng-Perron and Phillips-Perron unit root tests support these findings.

Where Δx_t is the vector of log first differences including REITs and housing prices, c is a (2 x 1) vector of constants, $\hat{v}_{t-1}$ is the lagged cointegrating residuals defined from DOLS, $\Gamma(L)$ is a finite-order distributed lag operator whose lags are chosen by SIC criteria, and e_t are the residuals. The results of the VECM for housing prices and the Equity REIT are given in Table 3.6.

An unexpected increase in either REITs or housing prices must be adjusted for in the future by the other in order for the series to continue to move together over time.[6] The VECM estimates this adjustment parameter as the coefficient associate with $\hat{v}_{t-1}$. The adjustment parameter is negative and significant for the lagged change in REIT variables which indicates that Equity REITs adjust to changes in the housing prices over time.[7] Summary results of the adjustment parameters for all of the remaining

[6]This is more formally described as the Granger Representation Theorem: if a vector or two variables, x_t, is cointegrated one of the estimated adjustment parameters must be different from zero in the equation for Δx_t of the VECM representation.

[7]This is also consistent with the idea that price discovery is being captured by the FMHPI and then impacting REIT.

Table 3.6: Coefficients from a Cointegrated VECM

Dependent Variable	Δh_t	Δr_t
Δh_{t-1}	1.220**	1.655
	(26.534)	(0.942)
Δh_{t-2}	-0.528**	1.834
	(7.647)	(0.696)
Δh_{t-3}	0.264**	-2.461
	(5.816)	(1.421)
Δr_{t-1}	0.004**	0.153**
	(2.963)	(3.190)
Δr_{t-2}	0.003**	-0.133**
	(2.733)	(2.751)
Δr_{t-3}	-0.003**	0.089
	(2.379)	(1.810)
$\hat{v}_{t-1}$	-0.001	-0.048**
	(1.794)	(3.193)
R-squared	0.929	0.084

T-values in parentheses, double asterisks denote coefficients significant at the five-percent level in bold.

five combinations are given in Table 3.7.

For each combination, the significant negative coefficient indicates that REITs move in response to changes first detected in the housing price indices. The coefficients on Moody's Apartment and Core Commercial indices seem to be larger than those of residential price indices, which are in line with expectations that housing price change represent a form of expectation of future dividend payment in REIT. Alternatively, this could be simply due the the smaller size of the Moody's time series.

Table 3.7: Summary of Coefficients of Lagged Residuals from VECMs

	Price Index	REIT Index
FMHPI and Equity REIT	-0.000	-0.048**
	(0.000)	(0.015)
FMHPI and All REIT	-0.001	-0.057**
	(0.000)	(0.014)
FMHPI and ALL Equity	-0.001	-0.048**
	(0.000)	(0.015)
Moody's Apartment HPI and Equity REIT	0.005	-0.169**
	(0.003)	(0.062)
Moody's Core Commercial and Equity REIT	-0.002	-0.079**
	(0.002)	(0.030)
S&P/Case-Shiller and Equity REIT	-0.001	-0.056**
	(0.001)	(0.018)

Newey-West HAC standard errors in parentheses. Two asterisks denote coefficients significant at the five-percent level in bold.

3.4.2 Beveridge-Nelson Decomposition

In a cointegrated framework with the above characteristics, I would expect to find that the cyclical component of REITs is much larger than the cyclical component of housing prices.[8] In order to estimate the size of these components, I utilize a Beveridge-Nelson decomposition to decompose the system into trend and cycle components. The execution of this uses a multivariate state-space method developed by Morley (2002). The objective of this exercise is to observe the relative sizes of the cyclical components for both REIT and housing. In simplified matrix notation, the

[8]Figuerola-Ferretti and Gonzalo (2010) have pointed out that the price discovery process as described by Garbade and Silber (1983) coincide exactly with the permanent-temporary components described by are Gonzalo and Granger (1995). The cyclical components correspond to the non-price discovery portion of variation within each series.

decomposition for a cointegrated system in state-space can be represented as:

$$\Delta X_t = F X_{t-1} + v_t \tag{3.6}$$

Where X_t is the cointegrated system including the first-differences of Equity REITs and housing prices, the F matrix is constructed from parameters from the VECM. Then, the Beveridge-Nelson trend will be the long-run forecast of the series:

$$\tau_t = y_t + [1\ 0\ 0]F(I-F)^{-1}X_{t|t} \tag{3.7}$$

While the Beveridge-Nelson cycle will be the deviation from the trend:

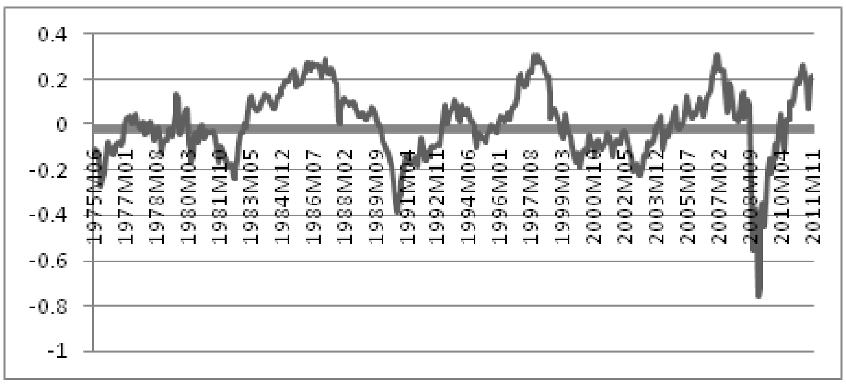
$$c_t = -[1\ 0\ 0]F(I-F)^{-1}X_{t|t} \tag{3.8}$$

The model's derived cyclical components are shown in Figure 3.2 and Figure 3.3. The difference in scale is indicative of the degree in which REITs and housing prices are influenced by non-permanent factors. While the REIT cycle scale is an order of magnitude larger that housing cycle, this varies notably across the sample. Clayton and MacKinnon (2001) have noted that their Equity REITs are about five times more volatile than the S&P Case-Shiller Home Price Index. This seems to be very much in line with the scale of volatility being displayed in the Beveridge-Nelson decomposition, and is consistent with the argument that REITs respond to changes in housing prices.

These results are robust to different sub samples of the time series. The estimated cycles and their scale are not affected by truncating the time series to time prior to 1991, nor is it changed by removing the housing market collapse post-2006. This implies that the cointegrating relationship between REITs and housing prices is not simply caused by a single regime, such as the housing bubble, but appears to be a fundamental property of the dynamic relationship of the two variables.

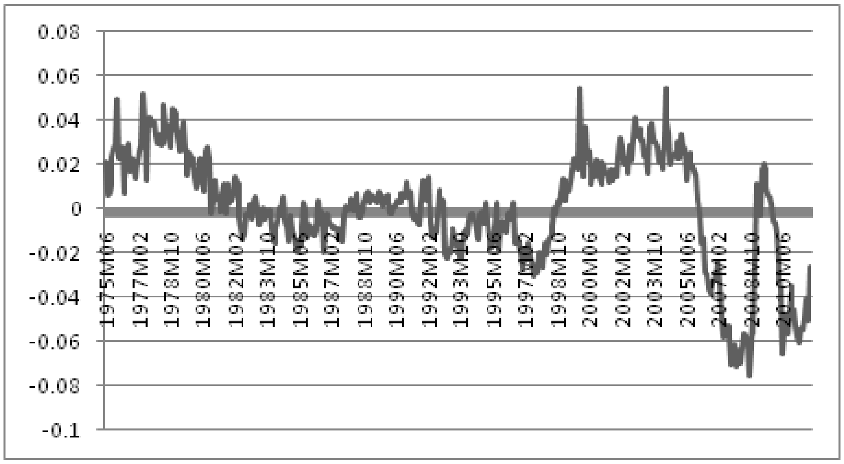
The cyclical deviation from the mean for both variables should be interpreted as deviations from the long-run trend. Thus, during the housing bubble from 1998 to 2006, housing prices were between 4-6% above their long-run trend. While REITs were above that trend for only half the time, the deviation of REITs from the trend during the housing bubble was much greater at around 30%. For housing prices, most of the variation is due to the trend. This reinforces the argument that most of the changes in housing prices are permanent and are also the source of movements in REITs.

Figure 3.2: REIT Beveridge-Nelson Cycle



The Beveridge-Nelson decomposition also gives an explanation for the differing observations concerning the portion of variation in REITs which is explained by stocks and bonds. For example, Clayton and MacKinnon (2001) find increased correlation between REITs and housing prices in the 1990s. The cyclical portion of both variables in the 1990s is less volatile than the previous and following portions of the series. It

Figure 3.3: Housing Price Beveridge-Nelson Cycle



seems reasonable that the increase in correlation between REITs and housing prices that Clayton and MacKinnon (2001) observed was conditional on this time period, and not necessarily caused by increased institutionalization or depth of the REIT market.

3.4.3 Gonzalo-Ng Permanent and Temporary Components

The Beveridge-Nelson methodology has some limitations since the trend component of the Beverdige-Nelson decomposition is defined as a random walk. Gonzalo and Granger (1995) and Gonzalo and Ng (2001) lay out a generalized framework that uses the error correction property of the cointegrated series to calculate the importance of permanent and transitory shocks at different forecast horizons. The Granger Representation Theorem provides an explicit link between the VECM form of a cointegrated VAR and the Wold moving average representation (Engle and Granger, 1987).[9]

[9] Also see King et al. (1991)

Gonzalo and Granger (1995) build off of this by defining the permanent shocks as those having the effect on the level of X_t such that:

$$\lim \frac{\delta E_t(X_{t+h})}{\delta \eta_{pt}} \neq 0 \tag{3.9}$$

While transitory shocks are those defined where Equation 3.10 holds true:

$$\lim \frac{\delta E_t(X_{t+h})}{\delta \eta_{pt}} = 0 \tag{3.10}$$

This implies that permanent component are those whose shocks that have led to enduring changes to the system at the measured horizon while the temporary component does not. Using these definitions, the Gonzalo-Ng decomposition uses a two-step process to decompose the series into permanent and temporary components. The first step estimates the VECM as I have done above and uses its parameters to create the matrix:

$$G = \begin{bmatrix} \hat{\gamma}_{\perp}{}' \\ \alpha' \end{bmatrix} \tag{3.11}$$

Where $\hat{\gamma}$ are the estimated adjustment coefficients from the VECM, and α are the number of cointegrating vectors. Given a Wold moving-average representation of $\Delta X_t = C(L)e_t$, the Choleski decomposition allows the definition of the system as:

$$X_t = C(L)G^{-1}e_t = \begin{bmatrix} D_{11}(L)D_{21}(L) \\ D_{12}(L)D_{22}(L) \end{bmatrix} \begin{bmatrix} u_t^p \\ u_t^T \end{bmatrix} \tag{3.12}$$

The results for the Equity REIT Index and the housing prices are given in Table 3.8. The results suggest that a much larger proportion of the shock to the Equity

REITs is temporary as compared to housing. For REITs the permanent component explains 38% of the variation in a one-step ahead forecast and decline to 23% at longer horizons. On the other hand, housing prices count 61% of their variation as permanent at the one-step ahead forecast, and this falls to 47% at the longer horizons.[10] The Gonzalo-Ng decomposition clearly supports the previous results from the VECM which suggest that a larger portion of variation in REITs is temporary.

Table 3.8: Gonzalo-Ng Variance Decomposition

	REIT		FMHPI	
Horizon	P	T	P	T
1	0.38	0.62	0.61	0.39
2	0.36	0.64	0.56	0.44
5	0.29	0.72	0.48	0.52
10	0.24	0.76	0.47	0.53
∞	0.23	0.77	0.47	0.53

3.4.4 Long-Horizon Regressions

Another way to show that REITs move in response to changes in housing prices is to examine the long-horizon predictability of REITs with respect to the cointegrating residual. Following Lettau and Ludvigson (2001), cointegrating residuals should have long-horizon forecasting power for REITs if REITs are responding to permanent changes in the housing market. Long-horizon regressions use the difference of REITs over an assortment of horizons to compare how well a variable can predict long-term, in-sample changes. This gives a framework for comparing how well the calculated cointegrating residual predicts REITs at various horizons in contrast to other potential control variables. The dependent variable for H-period and f-horizons for these

[10]This suggests from the price discovery framework that real estate prices are much better captured by the housing price indices than REITs.

regressions are constructed in the form of:

$$\Delta y_{f,t} = y_{f,t+1} - y_{t+1} + \cdots + y_{f,t+H} - y_{t+H} \tag{3.13}$$

Then the long-horizon regressions presented in Table 3.9 and 3.10 have the OLS form of:

$$\Delta y_{f,t} = \beta_1 \hat{v}_{t-1} + \beta_2 \Delta x_{t-1} + \epsilon_t \tag{3.14}$$

In Equation 3.14, $\hat{v}_{t-1}$ are the lagged cointegrating residuals, and Δx_{t-1} are assortments of other potential control variables. In Table 3.9 and Table 3.10, long-horizon regressions are sorted from the shortest to longest horizons, with the one-horizon being synonymous to a simple first-difference of the Equity REIT Index, and twenty-four being the difference between the index two years in the future and the present value at time t. Standard errors are Newey-West HAC and in parentheses, while the R^2 for each regression is in brackets. The value of R^2 for long-horizon regressions is interesting as it shows the amount of variation which is explained by the independent variables. In terms of regressions with the cointegrating residual, a larger R^2 shows which variable is adjusting over time. Large R^2 over longer horizons implies the dependent value is adjusting and mean-reverting.

Table 3.9: Long-Horizon Regressions without Controls

		Forecast Horizon H						
Row	Lagged Regressors	1	2	4	6	8	12	24
Panel A: House Price Growth								
1	$\hat{v}_{t-1}$	-0.003	-0.008	-0.019	-0.03	-0.04	-0.063	-0.124
		(0.004)	(0.007)	(0.013)	(0.010)	(0.027)	(0.042)	(0.097)
		[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Panel B: REIT Growth								
2	$\hat{v}_{t-1}$	-0.049**	-0.108**	-0.231**	-0.350**	-0.470**	-0.678**	-1.204**
		(0.018)	(0.030)	(0.050)	(0.071)	(0.085)	(0.098)	(0.152)
		[0.02]	[0.04]	[0.10]	[0.14]	[0.18]	[0.25]	[0.39]
3	S&P	0.224**	0.096	0.274	0.239	0.192	0.17	-0.266
		(0.110)	(0.118)	(0.195)	(0.198)	(0.235)	(0.329)	(0.517)
		[0.02]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
4	HousePrice	-0.003	-0.008	-0.019	-0.03	-0.04	-0.063	-0.124
		(0.004)	(0.007)	(0.013)	(0.010)	(0.027)	(0.042)	(0.097)
		[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
5	HouseStarts	-0.009	0.133	0.138	0.167	0.163**	0.209**	0.194
		(0.025)	(0.069)	(0.058)	(0.088)	(0.074)	(0.101)	(0.158)
		[0.00]	[0.01]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
6	YieldSpread	0.003	0.006	0.014	0.021	0.029**	0.045**	0.09
		(0.003)	(0.005)	(0.009)	(0.012)	(0.014)	(0.017)	(0.023)
		[0.00]	[0.00]	[0.01]	[0.02]	[0.02]	[0.04]	[0.08]

Standard errors are Newey-West and given in parentheses. R^2 values are in brackets. Two asterisks signify coefficients significant at 5% level. Each row represents a set of seven regressions, one for each horizon examined. The independent variable for each regression is a single lag of a potential control variable. HousePrice is growth rate in Freddie Mac House Price Index. HouseStart is the growth rate of New Residential Construction from the U.S. Census. S&P are S&P 500 returns. YieldSpread is the difference between interest paid AAA and BAA corporate bonds.

Panel A of Table 3.9, are the long-horizon regression of housing prices with the cointegrated residual. The findings show that the cointegrating residual has no significant power in forecasting changes in housing prices. The coefficients of the regressions are insignificant and the R^2 values for the regressions are essentially zero. This is in line with the assertion that housing prices are not responding to changes in REITs. Conversely, Panel B shows that the cointegrating residual does explain the long-term changes in REIT. The R^2 of the longer-horizon regressions including the cointegrating residual explain almost 40% of the total variation. Across all horizons the cointegrating residual is significant and negative, implying that growth in the difference between REITs and housing prices predicts future adjustment of REITs in the direction of housing prices.

In comparison the other selected long-horizon controls do not perform as well as the calculated cointegrating residual, as they are significant for only a few horizons and markedly lower R^2 values. The S&P 500 returns are significant only at the first-period ahead forecast regression. This is consistent with the assertion that a large portion of the short-term variation in REITs is associated with other equity market movements. The yield spread and growth rate of housing starts do appear to have some predictive power over the longer horizons but the amount of total variation they explain as calculated by R^2 is much smaller than the cointegrating residual. The growth rate of housing prices is not a significant predictor of long-term changes in REITs. This has two potential implications. In line with the previous assertions, is that the relationship between REITs and housing prices is based off of levels and not the differences. Therefore, previous changes in the housing prices do not contain a large amount of information on long-horizon changes in REITs.

I also perform a series of more general long-horizon regression models that take into account other control variables in addition to the cointegrating residual. The results from these regression models are presented in Table 10 have the form:

$$\Delta y_{f,t} = \Delta y_{f,t-1} + \beta_1 \hat{v}_{t-1} + \beta_i \Delta x_{i,t-1} + \epsilon_t \tag{3.15}$$

In Row 1 of Table 3.10, I include a lag of the dependent variable. The R^2 of the longer forecast horizons for this model indicate that the cointegrating residual and AR(1) term capture upwards of 80% of the variation in the higher order horizon regressions. Even though controlling for the autoregressive properties within REITs, I find that the cointegrating residuals are significant at all except the longest horizon.

In Row 2 of Table 3.10, I present the long-horizon regressions with a full array of potential control variables. The cointegrating residuals seem to have the largest coefficient between six and eight horizons, and do not lose significance except for the extremely long twenty-four horizon regressions. While the S&P 500 returns and the growth rate of housing starts do have predictive power for REIT returns, the cointegrating residual between HPI contains much more consistent coefficients across multiple regression horizons. These findings indicate that the cointegrating residual is the best predictor of REIT returns over the short and medium-terms.

In comparing R^2 it is useful to compare the values for Row 1 and 2. While for the first horizon of the second row the variables capture 9.6% of the total variance as opposed to 4% under the simpler model, this advantage disappears quickly as I move to higher-order horizons. This illustrates that the additional variables are not that beneficial in explaining the long-horizon REIT returns. This is in line with Peterson and Hsieh (1997) assessment that REITs share much short-term variation with stocks and bonds.

Table 3.10: Long-Horizon Regressions with Controls

		Forecast Horizon H						
Row	Lagged Regressors	1	2	4	6	8	12	24
Panel C: REIT Growth With Controls								
1	$\hat{v}_{t-1}$	-0.055***	-0.055**	-0.065**	-0.067**	-0.071**	-0.067**	-0.044
		(0.017)	(0.022)	(0.026)	(0.032)	(0.031)	(0.029)	(0.029)
	AR(1)	0.151**	0.481***	0.783***	0.816***	0.876***	0.913***	0.950***
		(0.061)	(0.057)	(0.051)	(0.061)	(0.048)	(0.030)	(0.019)
		[0.04]	[0.26]	[0.66]	[0.71]	[0.82]	[0.88]	[0.93]
2		-0.065***	-0.049***	-0.053**	-0.059**	-0.051**	-0.049**	-0.023
		(0.020)	(0.025)	(0.021)	(0.028)	(0.022)	(0.025)	(0.032)
	AR(1)	0.082	0.913***	0.797***	0.811***	0.882***	0.913***	0.947***
		(0.937)	(0.021)	(0.053)	(0.054)	(0.042)	(0.021)	(0.020)
	S&P	0.224*	-0.005	-0.196*	-0.149	-0.309**	-0.307***	-0.312***
		(0.124)	(0.105)	(0.105)	(0.120)	(0.151)	(0.094)	(0.103)
	HousePrice	-1.57	1.059	0.046	0.042	0.119	0.147	-1.166
		(0.936)	(1.006)	(1.03)	(1.449)	(1.484)	(0.667)	(0.738)
	HouseStarts	-0.040*	0.096**	0.051	0.042	0.06	0.088**	0.094**
		(0.936)	(1.006)	(1.030)	(1.449)	(1.484)	(0.667)	(0.738)
	YieldSpread	-0.065	0.000	0.004	0.004	0.006	0.006**	0.011***
		(0.020)	(0.023)	(0.006)	(0.009)	(0.022)	(0.003)	(0.032)
		[0.096]	[0.29]	[0.68]	[0.72]	[0.83]	[0.89]	[0.93]

Standard errors are Newey-West and given in parentheses. R^2 values are in brackets. One, two, and three asterisks signify coefficients significant at 10%, 5%, and 1% level respectively. Each row represents a set of seven regressions of the long-horizon change in Equity REIT with a set of lagged regressors. HousePrice is growth rate in Freddie Mac House Price Index. HouseStart is the growth rate of New Residential Construction from the U.S. Census. S&P are S&P 500 returns. YieldSpread is the difference between interest paid AAA and BAA corporate bonds.

3.4.5 Out-of-Sample Forecasts

In this section, I show that the estimated cointegrating residuals perform well in one-period ahead forecasts in comparison to other potential predictors. The one-period ahead recursive methodology recalculates each forecast using all available past data for each periods forecast. This methodology should make it more difficult for the cointegrating residual to show forecasting power, as early results should contain larger forecast errors from forming predictions on limited numbers of initial observations (Lettau and Ludvigson, 2001).

Forecasting relies on a close relationship between the variable being forecasted and variables chosen as predictors of the future expected value of interest. In principle any variable may include information about future movements useful in a forecast. However, the cointegrating relationship should be particularly well suited to be exploited for forecasting purposes. The cointegrating residual itself should be a good predictor of changes for whichever variable that is moving to correct for the short-run disequilibrium in the system. Earlier, I have shown that the cointegrating residual has a direct influence on the future movements of REITs in the constructed VECMs. Hence, the lagged cointegrating residuals could be good at forecasting future REIT movements.

To determine the forecast power, I compare the Mean Squared Prediction Errors (MSPE) from the forecast of Equity REIT Index with the derived cointegrating residuals and another base forecast variables. First, a potential control is used to forecast REIT returns using the one-period ahead recursive methodology. Next, a second model with both the cointegrating residuals and the control is run to predict Equity REIT Index and MSPE is calculated. If the MSPE decreases between the first and second model, I can conclude that the variable of interest has added additional information which is beneficial in forecasting the future path of the REITs. Inference is provided through comparing residuals through the Clark-West nested comparison test (Clark and West, 2007).

An array of control variables were chosen to test the cointegrating residuals power. To begin with the first two lags of the cointegrating residuals are compared to AR(1) model and to a model with only a constant term. Then the cointegrating residuals are compared to the growth rate of important indicators of housing markets which may contain predictive power. Finally, since REITs are known to closely follow other financial assets, the cointegrating residuals are compared to S&P 500 returns and the yield spread. Clark-West statistics were computed for each in Table 3.11.

Table 3.11: Out-of-Sample Comparison Tests

		Clark-West	
Comparison	MSEu/MSEr	T-Stat	P-value
$\hat{\upsilon}_{t-1}$ vs AR	0.978	2.56	0.010
$\hat{\upsilon}_{t-2}$ vs AR	0.979	2.52	0.012
$\hat{\upsilon}_{t-1}$ vs Constant	0.975	2.49	0.013
$\hat{\upsilon}_{t-2}$ vs Constant	0.982	2.82	0.005
$\hat{\upsilon}_{t-1}$ vs grHouse(-1)	0.976	2.38	0.018
$\hat{\upsilon}_{t-2}$ vs grHouse(-1)	0.983	2.66	0.008
$\hat{\upsilon}_{t-1}$ vs grHstart(-1)	0.976	2.49	0.013
$\hat{\upsilon}_{t-2}$ vs grHstart(-1)	0.976	2.79	0.006
$\hat{\upsilon}_{t-1}$ vs grS&P(-1)	0.978	2.53	0.012
$\hat{\upsilon}_{t-2}$ vs grS&P(-1)	0.975	2.57	0.011
$\hat{\upsilon}_{t-1}$ vs Yield Spread(-1)	0.985	2.15	0.032
$\hat{\upsilon}_{t-2}$ vs Yield Spread(-1)	0.977	2.70	0.007

HousePrice is growth rate in Freddie Mac House Price Index. HouseStart is the growth rate of New Residential Construction from the U.S. Census. S&P are S&P500 returns. YieldSpread is the difference between interest paid AAA and BAA corporate bonds.

As Table 3.11 shows, the computed cointegrating residuals decrease MSPE across all of the proposed nested models. The Clark-West statistics confirms that in each model that I can reject the null that the addition of the cointegrating residuals does nt lead to reduction of MSE. This implies that the relationship between REITs and housing prices are potentially important predictors of future REIT returns. It also supports the previous findings that REITs are adjusting to permanent shocks to housing prices, since as REIT prices deviate from the long-run relationship they become more likely to readjust back to it in the future.[11]

3.5 Conclusion

The dynamic relationship between REITs and housing prices is a much debated issue in financial and housing economics. In this chapter, I have shown that Equity REITs move in response to movements in housing price indices. Furthermore, I have shown that the cointegrating residual significantly improves forecasting performance for REITs in both in-sample and out-of-sample forecasts. In summary, REIT prices are affected by the level of housing prices.

These findings have certain implications in terms of empirical modeling. My results indicate that price indices, regardless of their inherent drawbacks, capture the permanent variation in housing markets to a greater extent than do REITs. This casts doubt on the usefulness of daily REIT prices or returns as proxies to changes in housing markets in general. While over the long-term investing in REITs does

[11]In Brennan and Xia (2005) criticism of Lettau and Ludvigson (2001), they argue that the out-of-sample forecast of the type used here, are not truly out-of-sample, since the cointegrating residual itself is estimated across the entire sample. They form a cointegrating residual using only a time trend in place of a variable of interest and find that its out-of-sample results are as good as one derived by Lettau and Ludvigson (2001). Lettau and Ludvigson (2005) argue that this criticism is invalid since the variable Brennan and Xia (2005) developed is non-stationary, and that their cointegrating variable does have economic significance. From my perspective there are reasons to believe that the methodology used here may succeed in face of Brennan and Xia (2005) criticism. While Lettau and Ludvigson (2001) focused on macro variables such as consumption and wealth the relationship of REITs and housing may be more simple and direct in terms of economic theory. A possible extension which would manage this criticism would be to estimate the cointegrating residual in a recursive method before including it in the forecast, or to compare it to residuals constructed by replacing house prices with a trend line.

seem to offer exposure to housing markets, my study indicates that the temporary volatility of REITs is much greater than that of housing prices.

From a policy perspective, this chapter implies that policymakers should attach more importance to housing price fluctuations than REIT fluctuations when considering macroeconomic conditions. It raises questions about the Bank of Japans recent policy to purchase J-REITs (Shirakawa, 2012). If the Bank of Japans purpose of this policy is to support property prices, actually purchasing property, or helping to finance others to do so, may have a much stronger economic impact than investing directly in a REIT stock.

Several potential extensions of this work are possible. First, since I have documented the long-term relationship between housing prices and REITs, it may be worth looking at the role of housing price announcements upon REIT returns in the short run. Secondly, while this chapter has focused on REIT indices, a more detailed breakdown of how REIT subcategories are influenced by different housing markets may be possible in with more highly disaggregated data. One limitation to this methodology, pointed out in Kishor (2007), is that the cointegrating vector is not allowed to vary over time. While the econometric framework needed to analyze time-varying parameters in a non-stationary manner has not yet been developed it would also be useful in this case.

Chapter 4

The Response of High-Frequency Real Estate Investment Trusts Prices to Macroeconomic Announcements

4.1 Abstract

In this chapter I use an event-study approach to examine the impact of surprise from macroeconomic announcements on daily Real Estate Investment Trusts (REITs) returns. The surprise is estimated as the errors from a rolling autoregressive forecast model. I also use the Quandt-Andrews breakpoint test to take into account potential regime switching in the announcement effect across different states of the business cycle (Andrews, 1993). The findings shows that there is instability in the response of REIT returns to macroeconomic announcements across different time periods. The results suggest that surprise news is incorporated quickly into REIT prices, as would be anticipated by rational expectations theory. For example, a one-percent unexpected increase in the S&P Case-Shiller Home Price Index has an estimated increase of over 2.5% for equity REIT returns on the day it is announced.

4.2 Introduction

Rational expectation theory suggests that the current price of an asset should be equivalent to the discounted payouts from that asset. If agents are rational, they should use all available information when valuing the price of an asset. In this framework, any surprise news that could change the expected value of the asset should be incorporated into the market price quickly. One way of measuring this change is through event-study methodology, in which the estimated surprise is calculated and used to calculate the effect of the new information on asset prices, returns, volatility or trading volume.

Many papers have investigated the role of macroeconomic announcements on varying types of asset classes[e.g. Faust et al. (2007) and Andersen et al. (2002)], but the use of event-study approach in measuring the impact of announcements on Real Estate Investment Trusts (REITs) has not been studied in depth. Bredin et al. (2007) find that REIT returns and volatility respond to monetary shocks but does not investigate the response to any other macroeconomic announcements. From a microeconomic level, Chen et al. (2013) find that forecasting errors of REIT profits influence excess returns for REITs.[1] Ewing and Payne (2005) does investigate how macroeconomic 'shocks' impact REITs but only in a VAR-impulse response framework that is completely different from the event-study framework used in this chapter.[2]

In this paper I calculate the announcement surprise as the forecast errors from rolling sample autoregressive process. Other papers, such as Conrad and Kaul (1988),

[1]While they do not use the same terminology, in some ways their framework is similar to this paper's in that I also use forecasting errors as proxies for surprise. However, their analysis looks primarily at quarterly REIT profit announcements and uses analysts' forecasts from the Institutional Brokers' Estimate System to conclude that forecast errors of REIT profits do impact excess REIT returns.

[2]Ewing and Payne (2005) find that monetary policy, economic growth, and inflation all lead to lower than expected returns in REITs, while default risk premium shocks are associated with higher future returns. The authors argue that REITs are viewed as safe-havens in times of economic downturn, hence their somewhat counter-cyclical behavior.

have used a similar process when investigating stock returns. Flannery and Protopapadakis (2002) states that econometric modeling has successfully found announcement effects on Treasury yields and foreign exchange markets. For the purpose of this paper, this has several advantages over the use of survey data from market participants. First, survey data is difficult and expensive to obtain. It also may contain systemic errors if the participants have incentive to lie about their expectations of future announcements.[3] Finally, survey data that is available may not be of the same quality, frequency, and period for all the variables of interest. Estimating the announcement surprise through a rolling forecast method circumvents these problems. This allows the examination of a greater variety of types of announcements with a greater number of observations which should allow for a greater precision in our estimates.

The findings suggest that there is evidence that surprise announcements of the CPI, Industrial Production, and Case-Shiller HPI do impact Equity REIT returns across the entire sample. Empirically these findings are not out of line with, Flannery and Protopapadakis (2002)'s overview of macroeconomic announcement effects. They also found that about one-third of the macroeconomic announcements resulted in significant coefficients. The findings support the belief that stock prices reflect market fundamentals. While the estimated magnitude of the announcements effects range widely, this is not out of line with respect to those found in by Wongswan (2006).

Previous studies have argued that finding macroeconomic announcements effects is difficult because the impact of announcements is unpredictable and time-varying. This line of reasoning dates back to at least Dornbusch (1976)'s model on how monetary policy announcements can cause overshooting within exchange rate markets. More recently, McQueen and Roley (1993) argue that previous research has failed to find any impact of news on stock prices because these impacts varied in sign and

[3]An example of this is the recent Libor rate setting scandal.

magnitude across the business cycle.[4]

While it is possible to define regimes in an ad hoc method based on the state of the business cycle this introduces researchers' a priori beliefs into the study potentially biasing results. Andrews (1993) proposes the Quandt-Andrews breakpoint test which estimates potential break points in the time series without having to specify a potential date. Using this test to determine the regime switching results in evidence that all the macroeconomic announcements seem to cause changes in REIT returns at least during some states of the business cycle. There also appears to be some evidence that the sign of the announcement changes across findings of this chapter support the idea that the strength of the announcement effect does vary by time with respect to REITs. In support of Boyd et al. (2005), the sign of the effect of surprises in unemployment rate, nonfarm payrolls, and consumer confidence index announcements switch signs for All Equity and Equity REIT returns between regimes.[5] However, the results do

[4]Two recent papers have generalized these announcement effects using real time data focusing on exchange rates. Faust et al. (2007) examine how the unexpected component of U.S. macro announcements impacts exchange rates and U.S. and foreign interest rates. Andersen et al. (2002), also include in their analysis equity returns. Both papers find that a wide variety of announcements create significant impacts on a wide array of assets during a small twenty-minute window following the announcement. They illustrate some of the difficulties of analyzing regression results across stocks, bonds and exchange rates. While bonds and exchange rates behaved as one would expect to positive news, their coefficients for stocks were negative. Therefore, "good" announcements about a stock resulted in a negative return for the period following the announcement. ABDV rationalized this as the difference between a "cash flow" effect and a "interest rate"effect. Since the price of an asset can be thought of as the discounted sum of future cash flows any news can effect an asset's price through either the future cash flow from dividends or through a change in the discount rate. They argue that announcement effects on stocks vary in a positive relationship during a recession, but in a negative relationship during economic expansion, as the news changes market participants'expectations of future monetary policy. However, Faust et al. (2007) argue that this is in no way a simple natural experiment, as any particular announcement could affect a given asset in a number of ways. For example if s_t was a GDP announcement that came in lower than expected, the impact could be that expectations of monetary policy will be looser in the future, that future corporate profits will be lower than previously thought, or that the likelihood of government stimulus is now more likely. Therefore, interpreting the estimated coefficient is much more involved than a causal inference from difference-in-difference than one would imagine in this circumstance.

[5]Boyd et al. (2005) find that unexpectedly high unemployment announcements, on average, decreased stock prices during recessions, but also decreased them during expansions. The authors conclude that the business cycle is very important in determining the degree and sign of macroeconomic announcements' effect on asset prices.

not find any example of a significant coefficient in the first period switching to become a significant coefficient of the opposite sign in the following period. Therefore, the results contradict the notion that announcement sign effects are simply determined by macroeconomic conditions such as expansions or contractions.

This chapter follows the following format. In "Data" I review the announcement and REIT data I use. In "Methodology" the proposed method for uncovering the announcement effect and for measuring its significance on REIT returns is presented. In "Results" the regression and breakpoint test results are reviewed along with their implications. In "Conclusion" some possible future extensions are presented.

4.3 Data

Table 4.1 includes a breakdown of the macroeconomic announcements that I will investigate in this chapter. The macroeconomic announcements includes both variables which have been used before, such as housing starts, the Consumer Price Index (CPI), nonfarm payrolls, unemployment, and industrial production, and two which have not been used in previous event studies; the S&P Case-Shiller Index and Nielsen's Consumer Confidence Index (CCI). A limiting factor on macroeconomic variables included in this study was the availability of historical announcement dates. Of those previously studied in Faust et al. (2007), found housing starts, nonfarm payrolls and unemployment were found to have a significant impact on U.S. exchange rates while CPI did not. Housing starts are the New Residential Construction variable calculated by the U.S. Census. Nonfarm payrolls and the unemployment rate are both published monthly by the Bureau of Labor Statistics. Housing starts and the S&P Case-Shiller Home Price Index should be closely related to the housing market which REITs are believed to represent.

Announcement days for unemployment rate and nonfarm payrolls are the first Friday of every month while the CCI and S&P Case-Shiller Home Price Index are both released on the last Tuesday of every month. In the sample, when these days did not correspond to active trading days, for example due to a holiday, that months observation was dropped. Industrial production, CPI, and housing starts announcement days are more variable and were extracted from their respective sources websites. For housing starts, monthly announcement dates were publicly available from only 2010 onwards, so our analysis is limited to quarterly announcements. For housing starts, the Federal Reserve Bank of Philadelphia has aggregated professional forecasters predictions available, which is used for the robustness test at the end of the chapter.

Table 4.1: Macroeconomic Announcements

Data Release	Frequency	Source	First Day	Last Day	Time
Case-Shiller HPI	Monthly	S&P	1/26/1999	3/26/2013	9:00
CPI	Monthly	BLS	1/14/1999	3/15/2013	8:30
Consumer Confidence Index	Monthly	Nielsen	1/26/1999	3/26/2013	10:30
Housing Starts	Quarterly	U.S. Census	5/18/1999	2/16/2013	8:30
Industrial Production	Monthly	Fed	1/15/1999	3/15/2013	9:15
Nonfarm Payrolls	Monthly	BLS	1/8/1999	3/8/2013	8:30
Unemployment Rate	Monthly	BLS	1/8/1999	3/8/2013	8:30

Acronyms for sources are as follows: BLS (Bureau of Labor Statistics), Census (Bureau of the Census), Fed (Federal Board of Governors), S&P (Standard and Poor).

Table 4.2: Real Estate Investment Trust Data

Series	Ticker Symbol	Data Start	Data End
All REIT	FN17C	2/26/2009	3/28/2013
All Equity REIT	FNER	1/4/1999	3/28/2013
Equity REIT	FN19	3/7/2006	3/28/2013
Mortgage REIT	FN43	3/7/2006	3/28/2013

REIT daily prices extracted from Bloomberg

The data on REIT daily returns is extracted from Bloomberg is summarized in

Table 4.2. The beginning dates for each REIT series are important to note, since it is possible the sign and significance of the OLS coefficients may vary across the business cycle. All Equity REITs have the longest data set. This implies that the ALL Equity series is exposed to more variation in the macroeconomic environment. This may make significant OLS based fixed-coefficients difficult to observe. The Equity and Mortgage REIT series cover 2006 to 2013. The All REITs series is available only from 2009 onwards. Therefore, the coefficient from the series should not be directly compared across the REIT series.[6]

The REITs presented in Table 4.2 also contain compositional differences that may be associated with very different market dynamics. Mortgage REITs consist mainly of REITs which purchase mortgages and mortgage backed derivatives, while Equity REITs include REITs whose primary focus is in owning commercial property such as apartments, health care facilities, and self-storage units. All Equity REITs include Equity REITs as well as REITs that focus on infrastructure and timber lands. Therefore, they have price dynamics very different than that seen in more commercial based REITs. Finally, the All REIT category is the most inclusive, and includes commercial as well as mortgage, infrastructure, and timber REITs. It has been observed that Mortgage REITs have been declining in value and volume for many years, while infrastructure and timber REITs have been growing. Since Equity REITs contain neither they are the primary focus of research when using REITs as proxies for real estate.

[6]Matching the proper announcement surprise with the proper REIT return is critical for this type of analysis. The Case-Shiller HPI is lagged two-periods to account for the fact the announcement is for a date two months previous. CCI is contemporary for the month that it is announced. All other variables are for the previous month and should be lagged one period. In data set housing starts were adjusted prior to time-series analysis.

4.4 Methodology

Papers attempting to measure the relative size of a surprise in a given announcement follow one of two paths. The first is to use some form of econometric modeling to estimate the expected return and then subtract that from the actual return. An example of this is Conrad and Kaul (1988)'s investigation into weekly stock returns employing a simple ARMA(1,1) model. Flannery and Protopapadakis (2002) argue that while econometric modeling has been successful with announcement effects in Treasury Yields and exchange rate volatility it has not been successful in equity markets since increased forecast error noise should bias coefficients towards zero. Therefore, many papers, see Chen et al. (2013) or Faust et al. (2007) as examples, use various surveys to calculate their announcement surprise variables. This methodology suffers from two drawbacks. The first is measurement error as surveys can be biased systematically if the same agents are continually surveyed for the same variable. An extreme example of this would be the recent Libor rate setting scandal. Since major market players often have incentive to lie it is in no way certain that they are offering their true expectation of future announcements as the survey itself will probably change market expectations. Secondly, survey data is often expensive and difficult to obtain. For example, publicly available survey data available from the Federal Reserve Bank of Philadelphia is limited in quantity and frequency. Thus modeling the shock allows for a much greater depth in the time series as well as in the scope of variables investigated.[7]

In this chapter, announcement shocks are modeled using a simple AR(1) model. As Flannery and Protopapadakis (2002) argue this method should bias the regression coefficients towards zero, which gives more weight to the results of significant coeffi-

[7] In the case of Faust et al. (2007), their private data sample is limited to the major macroeconomic announcements that their surveys cover, such as: CPI, the Federal Funds Rate, GDP, Housing Starts etc.

cients that are found. Econometric modeling also has the benefit over survey data in consistency. While it is not clear that survey data is of consistent quality across all the different macroeconomic variables studied, the simple econometric model employed implies that the all calculated announcement surprises are comparable in terms of quality. Finally, the modeling of the perceived shock is preferred here as it allows the expansion of both the depth of the number of announcement series in terms of number of observations as well as the breadth of the number of variables being investigated.

The creation of the announcement surprise index begins with estimating a AR(1) model with a constant, equivalent to:

$$r_{t|p} = c_{t|p} + \psi r_{t-1|p} + u_t \tag{4.1}$$

For a sample size p of ten-months, $c_{t|p}$ is the constant conditional on the sample, and ψ is the coefficient of the AR(1) term conditional on the sample. The estimated coefficients are then used to create a one-period ahead forecast for the next time period. From this forecast I calculate a forecast error in the form:

$$\hat{s}_{t+1} = r_{t+1} - \hat{c}_{t|p} - \hat{\psi}_t r_{t|p} \tag{4.2}$$

Here, I define $\hat{s}_{t+1}$ as the announcement surprise. In terms of modeling, I could think of this as the surprise investors would perceive if they only viewed the last ten-months of announcements for a given variable. To estimate the impact of announcements on REIT returns, I will run the regression:

$$r_t = c + \beta \hat{s}_t + \epsilon_t \tag{4.3}$$

I define Normal REIT Returns as the difference between the daily closing values of the REIT indices for the day before and the day of the announcement. Failing to reject that $\hat{\beta} = 0$ implies I should conclude that the announcement has had no effect on REIT returns for that day. I also check the impact on market adjusted returns and mean adjusted returns. Market adjusted returns are defined as:

$$x_t = r_t - \zeta_t \tag{4.4}$$

Here, x_t are market adjusted or excess returns, r_t are REIT daily returns, and ζ_t are the daily returns for the S&P 500. Many studies have indicated that REITs share a substantial amount of their variation in common with stocks or bond markets, see Cotter and Roll (2011) as an example. Therefore, market adjusted returns may give some indication as to whether the influence of a macroeconomic announcement influences REITs primarily through the shared component with other stock market announcements or through a component which belongs to REITs alone. I also examine whether the results vary when using REIT mean adjusted returns as the dependent variable. Here, I define mean adjusted returns as:

$$a_t = r_t - \bar{r}_m \tag{4.5}$$

Where r_t are the REIT daily returns, and $\bar{r}_m$ are the averaged monthly REIT returns for that particular series. Brown and Warner (1985) argue that mean adjusted daily stock returns are usually more normally distributed than normal or market adjusted returns. This implies that mean adjusted returns generally have lower power towards rejecting the null of a coefficient equal to zero.

4.5 Empirical Results

In Table 4.3, shows a summary of the announcement surprise regressions for the monthly frequency variables across all three types of returns, the dynamics do differ across REIT types. While the size of the announcement effect range widely, they are similar to those found by Wongswan (2006). Equity REITs show the most impact by announcement effects across the types of variables tested. This is most likely for two reasons. First, Equity REITs are mostly made up of REITs which invest in commercial real estate. Adams et al. (1999) find that smaller volume stocks have less impact from PPI and CPI announcements. Therefore, the inclusion of smaller timber and infrastructure REITs may temper announcement effects that mainly fall on REITs with larger market capitalization. Secondly, Equity and Mortgage REITs' time series runs from 2006 to 2013. It may be that this is a period when macroeconomic announcements have fairly stable effects on REIT returns. Normal returns of Equity REITs do appear to be affected by the surprise announcements of CPI and industrial production. In the case of CPI, this finding is not repeated across the other return types I investigated. The surprise in the announcement of nonfarm payrolls is significant on Mortgage REIT returns and Equity but only at the 10% level. By far, the most significant results are found in the Case-Shiller HPI. Even the All REIT series, whose sample does not include burst of the housing price bubble, shows significant positive coefficient on the S&P Case-Shiller HPI surprise. This finding leads much credence to the argument that REITs reflect real changes in the housing market.

There is little evidence that the announcements of CCI and the unemployment rate impact any of the REIT series. Also, the lack of significance in CPI announcements is not surprising given similar finding in previous papers, see Adams et al. (1999) for example. However, the fact that nonfarm payrolls and unemployment do not show similar results is surprising, since they are both published at the same time and should contain similar information. The failure of the regressions containing the unemployment surprise to pick up significant variation may be caused by two factors. It may be inherent in the way unemployment is forecasted in the model or it could be due to the information that unemployment rate itself represents. Unemployment rates can vary for several reasons, and may not be as clear a market signal as monthly nonfarm payrolls are. Particularly, unemployment can decrease as employees leave the workforce, and may increase during recoveries as discouraged workers return to the labor market.

A drawback to examining the data in this format is that this methodology makes no effort to account for possible regime shifting within the relationships being examined. There are several reasons to believe that announcement effects should not have fixed-coefficients. Even a simple asset pricing model, as Andersen et al. (2002) exploit, has the potential of being impacted by news through two means. First, economic news may change the expected future cash-flows. In this way positive economic news should result in a positive price jump for the affected asset. Secondly, economic news may change future expected interest rates through changes in Federal Reserve policy. In this case positive news increases the likelihood of increased future interest rates and therefore lowers stock prices. Even with this simple model it is not clear that these influences of these two effects should be stable across different states of the business cycle. One approach that has some empirical backing is to control through assuming the relative influence of these two effects vary in predictable way with respect to the state of the business cycle, see Andersen et al. (2002) or Boyd et al. (2005) as examples. More complicated theoretical models, such as proposed by Faust et al. (2007), seem to indicate that any kind a priori belief is likely to be wrong. These studies often look at stock volatility instead, therefore removing the temptation to interpret the sign of the coefficient being estimated.

Table 4.3: Coefficients of Regression of Announcement Surprise on REIT Returns

Panel A: Consumer Confidence Index, Unemployment Rate, Consumer Price Index

	CCI			Unemployment Rate			CPI		
	Returns	Market Adj.	Mean Adj.	Returns	Market Adj.	Mean Adj.	Returns	Market Adj.	Mean Adj.
All	0.030	0.019	0.026*	0.034	0.041	0.050	0.255	-0.189	0.332
	(0.018)	(0.014)	(0.015)	(0.211)	(0.073)	(0.120)	(0.880)	(0.701)	(0.885)
All Equity	-0.025	-0.009	-0.033	-0.030	-0.002	-0.016	-1.572	-1.177	-1.441
	(0.029)	(0.014)	(0.030)	(0.056)	(0.041)	(0.067)	(1.524)	(0.839)	(1.459)
Equity	-0.011	0.012	-0.014	0.025	0.053	0.050	-3.374**	-1.896	-2.926
	(0.026)	(0.012)	(0.027)	(0.104)	(0.062)	(0.126)	(1.305)	(1.571)	(2.842)
Mortgage	0.001	0.024	-0.016	-0.036	-0.009	0.097	-4.921	-3.443*	-4.368
	(0.022)	(0.019)	(0.025)	(0.116)	(0.072)	(0.141)	(3.341)	(1.968)	(3.114)

Panel B: Case-Shiller Housing Price Index, Industrial Production, and Nonfarm Payrolls

	Case-Shiller HPI			Industrial Production			Nonfarm Payrolls		
	Returns	Market Adj.	Mean Adj.	Returns	Market Adj.	Mean Adj.	Returns	Market Adj.	Mean Adj.
All	2.791**	2.002***	2.865***	0.295	0.287	0.238	8.829	1.190	8.166
	(1.067)	(0.574)	(1.064)	(0.348)	(0.234)	(0.327)	(6.836)	(4.350)	(6.632)
All Equity	2.378***	1.389**	2.542***	-0.102	0.047	-0.072	2.356	-0.043	2.187
	(0.773)	(0.617)	(0.835)	(0.329)	(0.198)	(0.313)	(2.235)	(1.738)	(2.453)
Equity	2.492***	1.404**	2.686***	0.732**	0.518**	0.778*	12.703*	4.764	12.477
	(0.874)	(0.546)	(0.880)	(0.360)	(0.202)	(0.399)	(7.361)	(4.665)	(7.887)
Mortgage	2.880*	1.792*	2.780*	0.677	0.464	0.690	17.628**	9.689	11.210
	(1.541)	(1.069)	(1.539)	(0.526)	(0.388)	(0.559)	(8.582)	(6.37)	(8.629)

Standard errors are Newey-West. One, two and three asterisks signify significance at the 10%, 5% and 1% levels, respectively. Time series vary by REIT type: All consists of 2009m3 to 2013m3, Equity and Mortgage REITs are 2006m3 to 2013m3, and All Equity is from 1999m1 to 2013m3.

In order to examine the time varying properties of these coefficients in more detail, I employ the Quandt-Andrews breakpoint test to discover if the estimated announcement effect coefficients show any evidence of breaks across the sample. This test was proposed by Andrews (1993) as an extension of Quandt (1960). The benefit of this approach over other breakpoint tests, see Chow (1960) for example, is that it does not require the researcher to propose an a priori date at which the break should occur. Rather the Quandt-Andrews tests for the most likely break date simultaneously to testing whether the break exists. Removing the researchers' belief from biasing the choice of breakpoint date is important as the economic factors that potentially could impact the sign of an announcement effect are many and varied. This is potentially more true when dealing with equity prices since there is the potential that announcement effects may differ markedly across different equity types, see Adams et al. (1999) for example.

Results for the Quandt-Andrews breakpoint tests are given in Table 4.4. All estimated breakpoints in the coefficients appear to occur between mid-2008 and mid-2010. This is in line with the belief that major changes in the state of the business cycle are important causes of changes in announcement effect coefficients. Mortgage REITs show the least evidence of breaks in their coefficient, while All Equity shows the most. These findings are consistent with the opinion that Mortgage REITs behave differently than the rest of the REIT sample. Also, the extended length of the All Equity time series may give the test greater power. Given the evidence of breaks in the coefficient, I show the estimated coefficients of the regression Normal REIT returns with samples split at the date suggested by the Quandt-Andrews breakpoint test in Table 4.5 through Table 4.8.

Table 4.4: Quandt-Andrews Breakpoint Test on Normal Returns

	Housing Starts	CCI	CPI	Unemp.	Case-Shiller	Nonfarm	Ind. Prod.
All	2010Q1 0.11	**2010m9** **0.10**	**2009m12** **0.00**	2010m4 0.21	**2009m11** **0.03**	2010m4 0.37	2009m12 0.19
All Equity	2008Q2 0.34	**2009m3** **0.00**	**2008m09** **0.03**	2008m6 0.19	**2008m5** **0.05**	**2008m10** **0.02**	2008m12 0.16
Equity	2008Q3 0.85	**2009m3** **0.01**	**2009m10** **0.02**	2008m6 0.35	**2008m5** **0.09**	2008m10 0.55	2009m1 0.47
Mortgage	2008Q3 0.19	2008m11 0.25	2008m9 0.17	2008m06 0.73	**2007m11** **0.02**	2009m05 0.70	2009m2 0.33

Quandt-Andrews suggested break point date above with p-values of test below. The null hypothesis is that there is no break in the coefficient. Bold indicates rejection of the null at the 10% level.

Table 4.5: Regression of Announcement Surprise on REIT Returns: Panel A

	CCI				CPI			
	BP Date	Full	Before	After	BP Date	Full	Before	After
All	2010m9	0.030	0.071***	-0.013	2009m12	0.255	-6.360	0.369
		(0.018)	(0.019)	(0.010)		(0.880)	(4.151)	(0.519)
All Equity	2009m3	-0.025	-0.067*	0.033*	2008m09	-1.572	-0.230	-5.660
		(0.029)	(0.040)	(0.019)		(1.524)	(0.408)	(4.406)
Equity	2009m3	-0.011	-0.064**	0.036	2009m10	-3.374	-5.782***	0.325
		(0.026)	(0.028)	(0.022)		(1.305)	(1.914)	(1.207)
Mortgage	2008m11	0.001	-0.005	0.019	2008m9	-4.921	-0.633	-8.726*
		(0.022)	(0.076)	(0.030)		(3.341)	(0.871)	(5.077)

Standard errors are Newey-West. One, two and three asterisks signify significance at the 10%, 5% and 1% levels, respectively.

In Table 4.5, I find that CCI and CPI do have detectable announcement effects for Equity REITs when the sample is split at least during some portion of the business cycle. CCI also shows a significant coefficient in relation to All REITs. Furthermore, in all the CCI regressions and two of the CPI ones we see a shift in sign of the announcement effect as observed by others, such as in Boyd et al. (2005). It has been proposed by Andersen et al. (2002) that the negative effect of some announcements, such as for CCI, is created by increased expectation of tighter monetary policy. This effect may change during the business cycle as market participants focus shifts between whether the cash flow or discount rate of asset prices is more important. Both CCI and CPI appear to exhibit this behavior. However, in no case of sign changes are both the before and after coefficients statistically significant at the five-percent level. In terms of magnitude, the coefficients of CCI and CPI are quite different. While for CCI a one-percent unexpected increase is associated with an estimated increase of only -0.06% for Equity REITs, while for CPI the same increase would be associate with a -5.8% on Equity REITs. This illustrates that CPI seem to have a much greater effect on REITs than CCI does.

Table 4.6: Regression of Announcement Surprise on REIT Returns: Panel B

	Unemployment Rate				Nonfarm Payrolls			
	BP Date	Full	Bef	After	BP Date	Full	Bef	After
All	2010m4	0.034	0.404	0.097	2010m4	8.829	0.490	7.434
		(0.211)	(0.995)	(0.131)		(6.836)	(20.730)	(6.117)
All Equity	2008m6	-0.030	-0.087**	0.295	2008m10	2.356	-0.942	20.132**
		(0.056)	(0.037)	(0.203)		(2.215)	(1.705)	(8.488)
Equity	2008m6	0.025	-0.117	0.335*	2008m10	12.703*	-2.778	18.463*
		(0.104)	(0.092)	(0.186)		(7.361)	(12.042)	(10.664)
Mortgage	2008m06	-0.036	-0.139	0.225	2009m05	17.628**	29.518*	9.875**
		(0.116)	(0.086)	(0.210)		(8.582)	(15.600)	(4.785)

Standard errors are Newey-West. One, two and three asterisks signify significance at the 10%, 5% and 1% levels, respectively.

In Table 4.6 shows the regressions for unemployment and nonfarm payrolls after breaking the sample at the point indicated by the Quandt-Andrews breakpoint test. While none of the regressions in terms of unemployment were significant for the full sample, splitting the sample shows that surprise unemployment announcements did have a significant negative impact on All Equity REIT returns prior to the break. This negative coefficient makes sense in that increased unemployment should imply decreased economic activity in the long-run. For the unemployment rate, the coefficients for All Equity, Equity, and Mortgage REITs all exhibit the same breakpoint as computed by the Quandt-Andrews breakpoint test. This is noteworthy in that it is the only case where the Quandt-Andrews test picks the same breakpoint for different REIT definitions. The most likely reason for this is the infrequency that change is measured within the unemployment rate as compared to the other announcements.

The majority of the estimated coefficients for both nonfarm payrolls and the unemployment rate seem to show a difference in sign before and after the breakpoint.

This seems to support the findings of Boyd et al. (2005) which shows pronounced and regular regime switching of unemployment announcements upon aggregate equity returns. The dynamics for the nonfarm payroll surprise still appear to be quite different from those of unemployment rate, especially in respect to Mortgage REITs. They also exhibit some of the largest recorded effects across the sample of announcements in this study. Before 2009m5, a one-percent increase in the surprise is associated with almost a thirty-percent increase in REIT returns. Ideally one would find that the calculated surprise for nonfarm payrolls and the unemployment rate would have similar levels of significance and estimated magnitudes of effects. It is unclear as to whether this is a particularity of the surprise modeling or simply caused by the greater variability inherent in nonfarm payrolls numbers.

Table 4.7: Regression of Announcement Surprise on REIT Returns: Panel C

	Case-Shiller				Housing Starts			
	BP Date	Full	Bef	After	BP Date	Full	Bef	After
All	2009m11	2.791**	3.518**	2.471**	2010Q1	0.057	0.225**	-0.189
		(1.067)	(1.217)	(1.000)		(0.134)	(0.051)	(0.122)
All Equity	2008m5	2.378***	1.113	3.420***	2008Q2	0.018	-0.015	0.152
		(0.773)	(0.818)	(1.215)		(0.033)	(0.023)	(0.119)
Equity	2008m5	2.492***	3.611***	3.136***	2008Q3	0.107	0.034	0.218*
		(0.874)	(0.811)	(1.111)		(0.065)	(0.066)	(0.117)
Mortgage	2007m11	2.880*	10.682**	2.846*	2008Q3	0.134	0.054	0.320**
		(1.541)	(4.911)	(1.480)		(0.119)	(0.146)	(0.149)

Standard errors are Newey-West. One, two and three asterisks signify significance at the 10%, 5% and 1% levels, respectively.

In Table 4.7 are the coefficients for housing starts and the S&P Case-Shiller Index announcement surprise regressed across breakpoint samples. It should be stressed that the housing starts regressions are using quarterly data. This means that estimated coefficients are more imprecise for two reasons. Not only are the sample sizes decreased because of the breakpoint but also because of the infrequency in the housing starts data. While the full sample of housing starts does not show any significant coefficients across any REIT series, the All and Mortgage REITs do both have significant positive coefficients on at least one post break subsample. For the S&P Case-Shiller Index I do find a greater level of significance in relation to Mortgage REITs before 2007m11. This differential effect is not surprising since Mortgage REIT returns were directly effected by the fall in home prices and increased rates of mortgage default. The S&P Case-Shiller results in this table are significant in all except the pre-2008m5 All Equity sample. This may signify that the importance of the S&P Case-Shiller Index has increased since its purchase by S&P in 2002. In terms of magnitude, the coefficients suggest that the S&P Case-Shiller announcements have a greater impact than housing starts. A one-percent surprise increase in S&P Case-Shiller causes over a three-percent increase in Equity REIT returns for both the before and after splits, while the corresponding effect for housing starts is only 0.03% to 0.22%.

Table 4.8: Regression of Announcement Surprise on REIT Returns: Panel D

	Industrial Production			
	BP Date	Full	Bef	After
All	2009m12	0.295	2.321*	-0.335
		(0.348)	(1.074)	(0.266)
All Equity	2008m12	-0.102	-0.201	0.369
		(0.329)	(0.428)	(0.319)
Equity	2009m1	0.732**	1.317*	0.191
		(0.360)	(0.739)	(0.355)
Mortgage	2009m2	0.677	1.521	-0.065
		(0.526)	(0.936)	(0.317)

Standard errors are Newey-West. One, two and three asterisks signify significance at the 10%, 5% and 1% levels, respectively.

The final Table 4.8 includes the regressions for industrial production. Industrial production announcement effects are the only one that are no longer significant after separating the series at the estimated breakpoint. This may be due to the loss of precision from halving the number of observations for each regression. The is likely since the Quandt-Andrews tests did not estimate a significant breakpoint for the series. In light of these findings and the other breakpoint results it seems that the announcement effect is far richer than the simpler regime switching model proposed by Boyd et al. (2005). Likewise, simple models to explain equity behavior, like Andersen et al. (2002) use, do not appear to be applicable when moving away from aggregate equity markets towards more disaggregated subsectors. Announcements with a direct impact on the future cash flows of an equity class appear to have stronger and more consistent behavior than broader macroeconomic announcements.

In summary, attempting to correct for the time-varying nature of announcements generally seems to reveal that macroeconomic announcements do impact daily REIT returns. This impact does seem to be linked to other macroeconomic conditions or the business cycle. Macroeconomic announcements of variables that are closely linked to the subsector being investigated, such as the S&P Case-Shiller Index, seem to have stronger more consistent effects than other macroeconomic announcements.

4.5.1 Robustness Check

As a robustness check in Table 4.9, I provide side-to-side comparison of the results of regressions of daily REIT returns on both my announcement surprise variable and the forecast errors obtained from the Federal Reserve Bank of Philadelphia Real Time Data Research Center. The Federal Reserve provided errors are from aggregated forecasts of professional forecasters, and represent the best possible forecast. However, these forecasts are available for only a subsection of macroeconomic variables tested and with only quarterly frequency.

Table 4.9: Regressions of REIT Returns on Housing Starts Announcement Surprise

	Normal Returns		Market Adj.		Mean Adj.	
	Fed	AR(1)	Fed	AR(1)	Fed	AR(1)
All	-0.013	0.057	0.022	0.082	-0.016	0.050
	(0.058)	(0.134)	(0.043)	(0.077)	(0.053)	(0.120)
All Equity	0.018	0.018	0.008	0.010	0.014	0.017
	(0.034)	(0.033)	(0.020)	(0.025)	(0.036)	(0.040)
Equity	0.080	0.107	0.072**	0.099***	0.067	0.110
	(0.057)	(0.065)	(0.026)	(0.032)	(0.044)	(0.071)
Mortgage	0.102	0.134	0.171**	0.188	0.086	0.124
	(0.076)	(0.119)	(0.081)	(0.11)	(0.076)	(0.114)

Observations are quarterly. Standard errors are Newey-West. Fed Forecast Errors obtained from Federal Reserve Bank of Philadelphia's Real Time Data Research Center. AR(1) signifies announcement surprise calculated through the forecasting method. Two and three asterisks signify significance at the 5% and 1% levels.

The results in Table 4.9 show that the methodology approximates what would be obtained from professional forecasters very closely, at least for the longer REIT series. In particular, the results for the All Equity series are almost identical. The highest level of discrepancies occurs in the All REIT series and are probably due to the shortness of the time series. Since these observations are quarterly in frequency, the All REIT series represents only twelve observations. Across types of returns, the mean adjusted returns seem to have the greatest variation between the Federal Reserves forecast errors and my own. In terms of the significance of coefficients, both methods find that Equity REITs responses to housing start announcements are significant for market adjusted returns. The professional forecasters error also finds that Mortgage REITs have a significant coefficient. Even in this last case the coefficients between the two methods are similar, though my method results in some increase in the variance of the coefficients. These results support the argument that the simple forecasting method yields results which are consistent with other more complicated forecasts.

4.6 Conclusion

I have presented evidence that an unexpected increase from macroeconomic announcements do have detectable effects on different daily REIT returns. Equity REITs appear to be most strongly and predictably influenced among REIT types examined. This may be due to either the composition of Equity REITs or due to the time sample used. The magnitude of these effects seems to vary unevenly across REITs and across the time series. This variation was explored in greater detail through the use of the Quandt-Andrews breakpoint test and shown some evidence of sign reversal during different portions of the business cycle (Andrews, 1993). Splitting the series at the point suggested by the test result in much stronger results. This supports previous assertions, such as Flannery and Protopapadakis (2002), that the reason past literature has been unable to find significant announcement effects is due to the time-varying nature of the coefficients.

Several extensions to this chapter are possible. Literature has noted that not only do returns reflect new information, but so does the volatility and volume. The framework explored here could therefore be extended into viewing how announcements impact this volatility, perhaps in an ARCH or GARCH type model. Since there appears to be a large degree of changes in parameters over time the use of a time-varying coefficient model could be informative. Extending these results to other means of calculating announcement surprise would strengthen their results. For example, it would be beneficial to compare the results to those obtained with survey level data. However, given the difficulty in obtaining survey data, extending the use of the Philadelphia Fed's aggregated professional forecasters prediction errors to non-farm payrolls, unemployment, and CPI is more feasible. Finally, the addition of real time price data would be useful in being able to differentiate announcement effects that occur on the same day but at different times.

Chapter 5

Dissertation Conclusion

In this dissertation I have used established empirical methods to describe dynamics inherent in the modern housing markets. I have also shown that recently available house price and REIT data have opened up numerous possible avenues for future economic research with wide ranging theoretical and practical implications. First, in terms of policy analysis the increasing disaggregation of housing data opens up many previously unexplored avenues for difference-in-difference studies of exogenous shocks. Secondly, the increased length of house price data can augment the usefulness of time series econometrics techniques in analyzing long-term relationships. Finally, even in terms of macroeconomic announcements, acknowledging the importance of housing creates meaningful results.

In my first chapter I examined the effect of the Arizona immigration enforcement legislation on the housing market using in part new hedonic price indices that allowed me to look at state and MSA-level effects. My results showed that both home prices and rents fell precipitously following the legislation's enactment and that the most likely reason for this was the mass emigration of over 100,000 people to other states and locales. While I did not cover all possible welfare implication of this, the $40 billion impact on owner-occupied home prices and $13.8 billion loss in rental income hint at a wide ranging negative economic impact of this policy. My findings support those of the theoretical literature, such as Saiz (2007), and indicate that policy makers should consider broader impacts and timing of legislature prior to its passage.

Extensions of this examination are possible in a number of directions. First, hedonic indices do not contain the sales that result from foreclosures. Though this is outside of the scope of this data set, examination of the economic impact of the legislation on foreclosures could have interesting results. Several states passed and enacted laws based on the Arizona legislation in 2011. My chapter did not look at housing impact in these states due to constraints on the time series at the time of writing, but an extension of the findings would cover Alabama, Kansas, Mississippi, Missouri, Rhode Island and West Virginia. To my knowledge data on the population of undocumented immigrants are not available for those particular states. If the undocumented population is relatively small the enactment of the legislation may not create observable effects. However, the implementation of the difference-in-difference should be straight forward. Other difference-in-difference studies are also possible such as studying the ramifications of city level policy, natural disaster, or other exogenous shocks such as resources booms.

In terms of time series analysis, the greater disaggregation of pricing data should allow for increasingly precise examination of dynamic relationships between housing and other macroeconomic variables over time. In chapter two I have shown how Equity REITs move in response to movements in housing price indices. These findings have implications in diverse uses of empirical modeling and in terms of macroeconomic policy objectives such as with the Bank of Japan's recent purchase of J-REITs. As more detailed housing price indices become available it should be possible not only to look at macro-level movements of price indices and selected stocks but also to examine the role of real estate prices in particular geographic locations or to look internationally as to how announcements effect REIT returns in other countries.

The final chapter of the dissertation documents the impact of macroeconomic announcements on REIT data. I find that many macroeconomic announcements do have detectable effects on daily REIT returns. An important innovation proposed here was the use of the Quandt-Andrews breakpoint test (Andrews, 1993). In place of ad hoc regimes, much stronger results were found by defining breakpoints through empirical tests. These findings support the idea that markets do incorporate information and that housing is viewed as an important source of information. Several extensions to this announcement literature are possible. Previous literature has suggested that not only do prices reflect the new information but so does the volatility and volume on the days of announcement. Extending the analysis in this direction should be relatively straight forward. Since our results appear to indicate some level of change in the coefficients associated with the surprise, extending the analysis to some form of time-varying coefficient model may also be informative.

The overall results of these three chapters highlight that housing has a real and potentially volatile impact on the economy. This is both in terms of real population adjustment and financial markets. Moreover, the increasing availability of more detailed housing data should allow ever more opportunities for researchers to expand our knowledge of housing market dynamics in the near future.

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Curriculum Vitae

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Working Papers:	1. The Effect of Arizonas Immigration Enforcement Legislation on Housing Prices and Rents
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