Essays on Housing Wealth, Credit Constraints and Intertemporal Consumption

by

Chadi S. Abdallah

(Under the direction of William D. Lastrapes)

Abstract

This dissertation considers how housing wealth affects household behavior. Three essays focus on the importance of credit constraints and their role in explaining the spillovers from housing markets on the real economy. In particular, they examine the extent to which house values affect non-housing consumption demand by serving as collateral for households to borrow against to smooth their spending.

Chapter 2 of the dissertation, entitled "Cross-state Variation in the Response of Consumption to Aggregate Housing Market Shocks in the US: The Role of Collateralized Constraints," estimates the effects of aggregate US housing market shocks on state-wide consumer spending and home prices. Most studies rely on aggregate data or impose inappropriate restrictions on the behavior of state-level variables. I estimate a latent factor vector autoregression (FAVAR) model and identify housing demand and supply shocks using a signs-restriction bayesian approach. I find that the effects of aggregate housing market shocks are heterogeneous across states and that modifying the assumption about the driving forces behind the run-up in real house prices largely affect the results. The heterogeneity of the effects is partly explained by different levels of mortgage market development across states, which is consistent with an important role for collateral or liquidity constraints as an explanation for the observed correlation between house prices and non-housing expenditures.

Chapter 3, entitled "Home Equity Borrowing and Households' Spending: Case Study of a Credit Reform in Texas," investigates whether household spending is affected by an increase in the availability of credit provided by a 1997 Texas legislative change that relaxed severe constraints on the ability of homeowners to use home equity as collateral for consumption loans. Using difference-in-differences methods, I find that the legislative change had sizeable and significant effects on overall spending in Texas, and that the magnitude of the effects is correlated with both the level of income and the amount of equity released by the amendment. The results are consistent with claims that credit constraints are important.

Chapter 4, entitled "Credit Constraints and Intertemporal Consumption: Theory and Evidence from a Credit Reform in Texas," investigates the extent to which a general equilibrium model with financial frictions can explain the aggregate spending effects of the 1997 Texas credit reform. I develop a simple two-agent dynamic general equilibrium model in which home values affect the debt capacity and consumption possibilities of a subset of households. I derive an aggregate consumption Euler equation and estimate its structural parameters. I find that the fraction of total consumption accruing to constrained households has decreased after the passage of the reform, and that this smaller fraction is more than offset by an increase in the loan-to-value ratio. The estimates suggest that the credit reform has induced a strong feedback from collateral values to consumption dynamics through the effect that it has generated on borrowing.

INDEX WORDS: Collateral constraints, Consumption, Credit, Difference-in-differences, Housing wealth, Vector autoregression

Essays on Housing Wealth, Credit Constraints and Intertemporal Consumption

by

Chadi S. Abdallah

B.S., Lebanese American University, Lebanon, 2003

M.S., University of North Carolina at Charlotte, 2005

A Dissertation Submitted to the Graduate Faculty

of The University of Georgia in Partial Fulfillment

of the

Requirements for the Degree

Doctor of Philosophy

Athens, Georgia

2011

Essays on Housing Wealth, Credit Constraints and Intertemporal Consumption

by

Chadi S. Abdallah

Approved:

Major Professor: William D. Lastrapes

Committee: George A. Selgin
Ronald S. Warren, Jr.

Electronic Version Approved:

Maureen Grasso
Dean of the Graduate School
The University of Georgia
August 2011

Dedication

This dissertation is dedicated to my parents Souheil and Jihane Abdallah, and my sisters Diala and Sarah. To my friends – both here and back home – thank you for all your moral support.

Acknowledgments

I thank my committee members - William D. Lastrapes, George A. Selgin, and Ronald S. Warren, Jr. - for their support and guidance. I am particularly indebted to my chair, William D. Lastrapes, for starting me thinking about housing wealth and consumer behavior shortly after I came to University of Georgia. I appreciate all his support, suggestions and guidance during my time here.

Table of Contents

Page

Acknowledgments v

List of Figures viii

List of Tables ix

Chapter

1 Introduction 1

2 Cross-state Variation in the Response of Consumption to Aggregate Housing Market Shocks in the US: The Role of Collateralized Constraints 5

2.1 Introduction 5

2.2 Empirical methods 8

2.3 Empirical results 20

2.4 The role of collateral constraints 27

2.5 Summary and conclusion 33

3 Home Equity Borrowing and Households' Spending: Case Study of a Credit Reform in Texas 57

3.1 Introduction 57

3.2 The Framework 60

3.3 Empirical methods 63

3.4 Empirical results 71

3.5 Summary and conclusion 79

3.6 Appendix. The 1997 Texas Constitutional Amendment . . 100

4 Credit Constraints and Intertemporal Consumption: Theory and Evidence from a Credit Reform in Texas 102

4.1 Introduction 102

4.2 The Theoretical model 104

4.3 Empirical methods 111

4.4 Empirical results 112

4.5 Summary and conclusion 118

5 Conclusion 123

Bibliography 126

List of Figures

2.1 Growth in Real House Prices and Real Consumption Expenditures 35
2.2 Latent Common factor and US aggregate variables 36
2.3 Responses of aggregate variables to housing demand shocks 37
2.4 Responses of state-level retail sales to Aggregate Housing Demand Shocks . . 38
2.4 (cont'd) 39
2.4 (cont'd) 40
2.5 Responses of state-level house prices to Aggregate Housing Demand Shocks . 41
2.5 (cont'd) 42
2.5 (cont'd) 43
2.6 Responses of aggregate variables to housing supply shocks 44
2.7 Responses of state-level retail sales to Aggregate Housing Demand Shocks . . 45
2.7 (cont'd) 46
2.7 (cont'd) 47
2.8 Responses of state-level house prices to Aggregate Housing Demand Shocks . 48
2.8 (cont'd) 49
2.8 (cont'd) 50
2.9 Cross-state averages of dynamic responses 51
3.1 Imputed state-level Real retail sales data 81
3.1 (cont'd) 82
3.1 (cont'd) 83
3.2 State-level Real per capita retail sales for Texas and the controls 84
3.3 Percent of homeowners with home equity loans 85
3.4 State-level Real House Prices 86

List of Tables

2.1 Dynamic responses of retail sales to aggregate housing demand shocks 52

2.2 Dynamic responses of house prices to aggregate housing demand shocks 53

2.3 Mortgage Market Index (MMI) and Components 54

2.4 Cross-state regressions: retail sales 55

2.5 Cross-state regressions: house prices 56

3.1 Descriptive statistics: retail sales 87

3.2 State-level treatment effects, all other states as control 88

3.3 State-level treatment effects, all other states as control 89

3.4 State-level treatment effects, matched states as control 90

3.5 State-level treatment effects, matched states as control 91

3.6 State-level treatment effects, placebo tests 92

3.7 County-level treatment effects, all other states as control 93

3.8 County-level treatment effects, all other states as control 94

3.9 County-level treatment effects, all other states as control 95

3.10 County-level treatment effects, matched states as control 96

3.11 County-level treatment effects, matched states as control 97

3.12 County-level treatment effects, matched states as control 98

3.13 Test for mean house price growth 99

4.1 Full Estimates of the consumption Euler equation 119

4.2 Subpsample estimates of the consumption Euler equation 120

4.3 Robustness analysis 121

4.4 Spending effects of the credit reform 122

Chapter 1

Introduction

Housing wealth, the market value of all residential capital, represents about one half of households' net worth in the US, while consumption accounts for about two thirds of the nation's Gross Domestic Product. Fluctuations in housing wealth are positively correlated with fluctuations in consumption in the data. The size and persistence of these fluctuations make them important candidates for understanding aggregate consumption behavior. This has led economists to investigate why and how housing wealth and consumption are related.

The conventional view is that households feel richer (poorer) as home prices rise (fall), and so they adjust their consumption accordingly. The latter implies that home values affect spending behavior through a direct *wealth effect*. However, such *wealth effects* are theoretically not plausible if homeowners plan to continuously occupy houses that increase in value: while their total wealth changes with house prices, their net wealth does not since the implicit cost of living in them changes as well (See, Buiter 2008, Iacoviello 2011). Another potential weakness of the direct *wealth effect* is that fluctuations in house prices transfer wealth between renters or potential home buyers and current homeowners, so that any resulting net or direct *wealth effect* on spending is not plausible on aggregate (See, Sinai and Souleles 2005). On the other hand, recent theoretical research suggests that house prices can matter for overall spending if households face binding borrowing constraints (See, Iacoviello 2004, Iacoviello 2005, and Iocoviello and Neri 2010). credit constrained households spend less on current consumption than is optimal given their lifetime budget constraints. For such agents, an increase in house prices relaxes their borrowing constraints and allows them to use the excess equity in their homes as collateral for consumption loans.

A number of empirical studies have examined the effects of changes in housing wealth on non-housing consumption demand. However, most of these studies pay insufficient attention to the endogeneity between housing wealth and non-housing consumption; they implicitly make the extreme assumption that changes in housing wealth are exogenous. Clearly, at the aggregate level, housing wealth and consumption are jointly related to a common set of underlying economic shocks. Changes in house prices might reflect shifts in household preferences, changes in availability of residential land, movements in interest rates, or technological shocks in the housing sector. Clearly, the source of the changes in house prices is likely to be very important in uncovering their effects on consumption demand. Simply regressing the time series of consumption on housing wealth, a common procedure in most existing studies, is highly misleading.

More careful empirical work on the linkage between house prices and consumption is thus well motivated and contribute to the existence literature. This dissertation re-evaluates the empirical foundation for estimates of the housing price-consumption link. It carefully considers how aggregate consumption and house prices *jointly* respond to appropriately identified, exogenous shocks. Understanding the sources of changes in consumption due to house price fluctuations is important in determining how volatility in real estate markets spills over into the real economy, and is surely relevant for policy analysis. I estimate strong feedbacks from collateral values to consumption dynamics through the effect that they generate on borrowing. This is interpreted as evidence for the importance of credit constraints; the spillovers from the housing market to non-housing consumption demand is most likely due to *collateral effects* rather than direct *wealth effects* of changing house prices.

Chapter 2 estimates the effects of aggregate US housing market shocks on state-wide consumer spending and home prices. Most studies rely on aggregate data or impose inappropriate restrictions on the behavior of state-level variables. I estimate a latent factor vector autoregression (FAVAR) model and identify housing demand and supply shocks using a signs-restriction bayesian approach. I find that the effects of aggregate housing market shocks are heterogeneous across states and that modifying the assumption about the driving forces behind the run-up in real house prices largely affect the results. The heterogeneity of the effects is partly explained by different levels of mortgage market development across states. This provides support for the argument that direct housing wealth effects are not plausible on aggregate, and suggests that housing wealth affect non-housing consumption mainly through the housing collateral channel.

Chapter 3 investigates whether household spending is affected by an increase in the availability of credit. I study a 1997 Texas legislative change that relaxed severe constraints on the ability of homeowners to use home equity as collateral for consumption loans. Using difference-in-differences methods, I find that the legislative change had sizeable and significant effects on overall spending in Texas. At the state-level, I find that spending increases by up to a 15%, from before to after the amendment, relative to the change in spending by the control group. Evidence from the county level data further suggests that the magnitude of the effect is correlated with the amount of equity released by the reform and that the effect is strongest for low income households: spending increases by up to 4.2% for low income counties and 3.6% for high house price counties. My findings suggest that credit constraints are important and matter, at least for aggregate spending in Texas. This is interpreted as further evidence that the link between house prices and non-housing expenditures is most likely due to collateral effects rather than direct wealth effects.

Chapter 4 investigates the extent to which a general equilibrium model with financial frictions can explain the aggregate spending effects of the 1997 Texas credit reform. I develop a simple two-agent dynamic general equilibrium model in which home values affect the debt capacity and consumption possibilities of a subset of households. I then derives an aggregate consumption Euler equation and estimate its structural parameters. I find compelling evidence for an effect of the Texas credit reform. The effect is the combined result of two findings: I estimate a larger share of constrained households in the Euler equation in the earlier period (between 24 and 30 percent of total consumption), compared to the later period (between 13 and 14 percent of total consumption); second, I estimate a strong feedback from collateral values to consumption dynamics, through the effect that the credit reform has generated on borrowing.

Chapter 2

Cross-state Variation in the Response of Consumption to Aggregate Housing Market Shocks in the US: The Role of Collateralized Constraints

2.1 Introduction

How do consumers respond to fluctuations in house prices? This key question has been the focus of a substantial body of literature and continues to generate much current research, especially in light of the presumed role of the housing sector in the financial crisis and recession of 2007. And the answer may seem easy at first. Figure 2.1 plots the (annualized) quarterly changes in aggregate consumption against changes in housing wealth for the 'US' from 1975 to 2008. It is somehow striking how rises (falls) in housing wealth are followed by rises (falls) in consumption. Alternatively, simple correlations between consumption and real house prices for all 'US' states in my sample suggests that consumption growth tend to be higher in states where house price growth is higher. The case is closed. Or is it? 'Eyeball Econometrics' or simple statistics can be very deceptive as many things are going on simultaneously in the economy so that one may want to be careful to consider just a single cause-and-effect story. For instance, it may be that other types of fundamental shocks coincide with the housing shocks, either reinforcing their effects on consumption or dampening them.

The consensus on this empirical question appears to be that changes in house prices, and thus housing wealth, have important effects on aggregate consumption in the "US" (e.g., Mishkin 2007, Carroll, Otsuka, and Slacalek 2006).[1] Estimates for the conventional marginal

[1] The stock of new housing is large relative to its flow; therefore, fluctuations in housing wealth mainly reflect fluctuations in the price of new and existing houses, rather than changes in the housing stock itself.

propensity to consume MPC out of housing wealth range from 2 to 9 cents-to-the dollar, depending on the specification. Moreover, such *wealth effects* are often found to be more important than changes in other forms of financial wealth, such as equity market wealth (e.g., Case, Quigley and Shiller 2005). Economic theory provides a setting for us to better think about this question and understand what it means after all. The conventional view is that consumers increase spending as house prices rise because of a pure *wealth effect* and a *collateral effect*. The former is best put in the context of the predictions of the permanent income hypothesis, which predicts that aggregate consumption is proportional to the sum of aggregate human and non-human wealth, and where non-human wealth includes the value of the housing stock. The *collateral effect* simply follows from the fact that when the homeowner experiences an increase in current or expected housing wealth, the value of the (housing) collateral she can offer is higher, and thus she can borrow more to finance extra spending. The housing *collateral effects* of house prices on consumer spending are theoretically established and well understood (e.g., Aoki et al. 2004, Iacoviello 2004, Iacoviello 2005, Iacoviello and Neri 2010). However, there are good theoretical reason to doubt the importance of a pure *wealth effect* in an aggregate sense, given that homeowners are hedged against fluctuations in the cost of living as house prices change (e.g., Sinai and Souleles 2005, Buiter 2008).

There are at least two shortcomings with much of the empirical work in this area. First, it is not clear that the literature has adequately addressed all the identification issues involved with deriving their estimates. Existing empirical studies have implicitly made the extreme assumption that a large fraction of fluctuations of housing wealth is exogenous and its dynamics have not been largely affected by consumption decisions, a point emphasized by, for example, Calomiris, Longhofer, and Miles (2009). While it is reasonable to assume exogeneity at the *individual* level, consumption and wealth will be endogenous in any sensible model of the macroeconomy, and simply regressing the time series of consumption on housing wealth will be misleading because the housing wealth variable is not exogenous or even pre-determined with respect to the consumption function parameters under any reasonable general equilibrium model of the economy. A more adequate approach, and one I follow here, is to consider how aggregate consumption demand and housing market variables respond to appropriately identified, exogenous shocks. The source of these shocks is likely to be a key in uncovering the dynamic effects of interest. A second issue is that most studies have only relied on aggregate data in deriving their estimates. The problem with aggregate data is that it does not rule out alternative explanations for the time series correlation between consumption and house prices: either indirect wealth effects or reverse causality running from changes in household savings to changes in wealth (Iacoviello (2009)). Moreover, using aggregate time series data also ignores potentially informative cross-sectional information embedded in more disaggregated data. While pooling the data appears to exploit both dimensions, it denies the heterogeneity between cross-sections and potentially masks important differences in behavior. I am particularly skeptical of the adequacy of panel data approaches because they do not account for the fact that any potential effect of house prices should be expected to vary considerably across sections, given substantial institutional differences in the structure of their financial systems and distributions of income and assets across households.

In this paper, I attempt to fill the void in the literature by re-evaluating the empirical foundation for estimates of the housing wealth-consumption link. I estimate the effects of aggregate housing market shocks on consumer spending across the states in the 'US'. Compared to existing empirical research on the link between house prices and consumption, I impose less structure and exploit the potential heterogeneity that characterizes the set of variables across states. In particular, I start from some weak, yet informative, prior assumptions on the effects of some shocks and use a dynamic model that carefully distinguishes the exogenous sources of these shocks to the demand and supply of housing. Then, I exploit information on common factors driving the fluctuations in a large set of state-wide variables and examine how the identified aggregate housing market shocks affect those variables. The empirical strategy explained in the next section employs the factor-augmented vector-autoregression (FAVAR) model (e.g., Bernanke, Boivin and Eliasz 2005). It provides a parsimonious representation of the co-movements in the large set of state-wide variables [2].

I find that there is considerable variation in the responses of state-wide consumer spending and home prices to aggregate housing demand shocks, and that the results are consistent with a collateral effect of housing rather than a conventional pure housing wealth effect. I then attempt to explain the cross-state variation by regressing the estimated responses from the FAVAR model on a mortgage market index (MMI) that measures the degree of flexibility and development of housing finance in each state. The regression results reinforces my support for collateral effects as a key factor in explaining the spillovers from the housing market to the rest of the economy.

2.2 Empirical methods

2.2.1 Empirical model

The empirical model is a FAVAR, which combines a dynamic factor model (e.g., Bernanke, Boivin and Eliasz 2005, Stock and Watson 2005, and Forni et al 2009) and a standard structural VAR. The approach allows combining a relatively small number of aggregate variables with a wide selection of states' variables without imposing too much structure – such as the homogeneity assumption inherent in panel methods – and without loosing too many degrees of freedom. This is particularly well suited for the issue at hand. Below, I explain the model in details.

Let $\mathbf{x}_{it}$ denote the joint stochastic process of the variables in state i. With n states in the sample, each represented by k economic variables, I can collect all state-wide variables in a process $\mathbf{x}_t$ of dimension $nk \times 1$. I want to examine how the state-wide variables in $\mathbf{x}_t$

[2] Del Negro and Otrok (2007) use a FAVAR model to examine the effects of aggregate and local shocks on state-wide house prices. However, they focus on money supply shocks and do not study the response of consumption expenditures. Moench and Ng (2010) also rely on a FAVAR to estimate the effects of aggregate housing market shocks on consumer spending, but disaggregate only to a regional level.

respond to aggregate housing market shocks over time. clearly, if nk is large, the system is over-parameterized and cannot be estimated as an unconstrained vector autoregression (VAR). To address this dimensionality problem, I parsimoniously represent the data generating process by imposing reasonable restrictions on the system. First, I introduce a small set of aggregate variables that are assumed to be independent of the variables in $\mathbf{x}_t$, and may either be observable or latent. Second, I make the assumption that contemporaneous and lagged correlations of the state-specific variables in $\mathbf{x}_t$ are due *solely* to the joint dependance on the aggregates. The restrictions significantly reduces the parameters to be estimated and makes it possible to precisely define and estimate the aggregate housing shocks and ultimately study their dynamic effects on the state-wide variables in $\mathbf{x}_t$ [3].

Let $\mathbf{z}_t$ denote the $h \times 1$ vector of aggregate variables, or common factors, with $h < nk$. I further assume that the joint stochastic process in $\mathbf{x}_t$ and $\mathbf{z}_t$ is generated by the following linear structural model with p lags:

$$\begin{bmatrix} A^0_{xx} & A^0_{xz} \\ 0 & A^0_{zz} \end{bmatrix} \begin{bmatrix} x_t \\ z_t \end{bmatrix} = \sum_{j=1}^{p} \begin{bmatrix} A^j_{xx} & A^j_{xz} \\ 0 & A^j_{zz} \end{bmatrix} \begin{bmatrix} x_{t-j} \\ z_{t-j} \end{bmatrix} + \begin{bmatrix} u_{xt} \\ u_{zt} \end{bmatrix} \tag{2.1}$$

where $u_t = \begin{bmatrix} u_{xt} \\ u_{zt} \end{bmatrix}$ is a white noise vector process normalized so that $Eu_tu_t' = I$. The assumption of mutual independence restricts the $nk \times nk$ sub-matrix A^j_{xx} to be diagonal while block exogeneity restricts the $nk \times nk$ matrix A^j_{zx} to be 0 for $j = 0, 1, ..., p$.

The restrictions imply the following reduced form VAR:

$$\begin{bmatrix} x_t \\ z_t \end{bmatrix} = \sum_{j=1}^{p} \begin{bmatrix} B^j_{xx} & B^j_{xz} \\ 0 & B^j_{zz} \end{bmatrix} \begin{bmatrix} x_{t-j} \\ z_{t-j} \end{bmatrix} + \begin{bmatrix} \epsilon_{xt} \\ \epsilon_{zt} \end{bmatrix} \tag{2.2}$$

[3] An alternative approach to deal with the dimensionality problem is to apply Bayesian (shrinkage) priors in order to increase the number of variables included in the VAR (Leeper, Sims, and Zha (1996)).

where $B^j_{xx} = (A^0_{xx})^{-1}A^j_{xx}$ is diagonal, for $j = 0, 1, ..., p$. The covariance matrix of the reduced form errors is $\Omega = \begin{bmatrix} \Omega_{xx} & \Omega_{xz} \\ \Omega_{xz} & \Omega_{zz} \end{bmatrix}$. Taking advantage of block exogeneity restrictions allows me to re-parameterize (2) and separate it into independent parts (Hamilton (1994), pp. 309-13, and Lastrapes 2005):

$$x_t = \sum_{j=1}^{p} B^j_{xx}x_{t-j} + \sum_{j=0}^{p} G^j z_{t-j} + v_t \tag{2.3a}$$

$$z_t = \sum_{j=1}^{p} B^j_{zz}z_{t-j} + \epsilon_{zt} \tag{2.3b}$$

where $G^0 = \Omega_{xz}(\Omega_{zz})^{-1}$ and $G^j = B^j_{xz} - G^0B^j_{zz}$ are of dimension $nk \times h$, for $j = 1, ..., p$. The errors in (3) are uncorrelated and $Ev_tv_t' = \Omega_v = \Omega_{xx} - \Omega_{xz}(\Omega_{zz})^{-1}\Omega'_{zz}$ is block diagonal (Lastrapes (2005)). Finally, we can use the restrictions to rewrite (3a) as

$$x_{it} = \sum_{j=1}^{p} B^{ij}_{xx}x_{it-j} + \sum_{j=0}^{p} G^{ij} z_{it-j} + v_{it} \tag{2.4}$$

where B^{ij}_{xx} is the diagonal block for state i ($k \times k$ for all i) for $j = 1, ..., p$, and G^{ij} is a conformable $k \times h$ sub-matrix of G^j for $j = 0, ..., p$, and $Ev_{it}v'_{it} = \Omega^i_v$.

Note that the diagonality and block exogeneity restrictions imposed on (1) collapses the system's unrestricted nk-dimensional reduced form VAR into n separate k-dimensional VARs, each conditional on contemporaneous and lagged aggregate variables (i.e. common factors). For each state, the system in (4) takes the form of a dynamic factor model. If the aggregates in z are observable, then each of the n VARs in (4) can be efficiently estimated by ordinary least squares methods, equation by equation, since Ω_v is block diagonal (See Lastrapes 2005).

Here, I allow the set of aggregates in z to contain unobservable variables, following much of the recent literature on dynamic factor models (e.g., Bernanke, Boivin and Eliasz 2005, Stock and Watson 2005, and Forni et al 2009). Such unobservable common factors can be extracted from the entire set of data series in my sample. This approach has the virtue of allowing for cross-state relationships in the data in a parsimonious way. Let $z_t = \begin{bmatrix} f_t \\ y_t \end{bmatrix}$, where

f is a set of h_1 latent common factors and y is the set of h_2 observable common factors, with $h = h_1 + h_2$. I further simplify the model by making the common assumption that the dynamics that x_t depend solely on those of z_t. In other words, for each state i, I set $B^{ij}_{xx} = 0$, and G^{ij}=0 for $j = 1, ..., p$. I also restrict Ω^i_v to be diagonal $\forall$ i, thus fully accounting for correlations within states by their joint dependence on the common factors in f and y. Under these assumptions, I can rewrite equations (2.3b) and (2.4) as follows:

$$\begin{bmatrix} f_t \\ y_t \end{bmatrix} = B(L) \begin{bmatrix} f_{t-1} \\ y_{t-1} \end{bmatrix} + \begin{bmatrix} \epsilon_{ft} \\ \epsilon_{yt} \end{bmatrix} \tag{2.5}$$

$$x_{it} = G^i_f f_t + G^i_y y_t + v_{it} \tag{2.6}$$

where $G^i_0 = [G^i_f G^i_y]$, G^i_f is a $k \times h_1$ and G^i_y is a $k \times h_2$. The covariance matrix of the errors in (6) is Ω_{zz}. Equations (2.5) and (2.6) are identical to equations (2.1) and (2.2) in Bernanke, Boivin and Eliasz (2005). However, note that there is lack of identification in the above system, since the equation in (2.6) can in principle be replaced by:

$$x_{it} = G^i_f C' C f_t + G^i_y y_t + v_{it}$$

Where C is any orthonormal matrix. The above equation is observationally equivalent to a model with factors Cf_t and factor loadings $G^i_f C'$. I follow the literature (e.g., Bernanke, Boivin and Eliasz 2005) and normalize the upper-left $h_1 \times h_1$ block of G^i_f to the identity matrix and restrict the $h_1 \times h_2$ block of G^i_y to contain only zeros [4].

The aggregate variables in (5) follow a reduced form VAR. The reduced form errors are linear combinations of the structural errors u_{zt} in (1). Later, I discuss how I identify aggregate housing market shocks from these reduced form errors. The dynamic responses to these shocks of the variables in x_{it} for each state i ultimately depend on the dynamic responses of the aggregate variables in f and y, and on the factor loadings, G^i_f and G^i_y.

An approximation of the dynamic factor model would be a regression model where f_t is replaced by principal components estimates. Here, I proceed differently and avoid making

[4]See Geweke and Zhou (1996) for alternative restrictions and normalizations.

too many a priori assumptions on the drivers of common fluctuations in x_t. I let the common factors in f be latent, instead of pre-specifying a number of regressors. To this end, I estimate my statistical model fully via Bayesian methods, which correctly treat f_t as a vector of unobserved *latent* variables. Note that replacing the factors in f_t by principal components is particularly appealing and easier to implement, but I do not pursue it in this paper as it would only provide me with static factors without taking into account the dynamics in the factors' equations [5].

In order to estimate the system, I follow closely the Bayesian Likelihood-based Gibbs sampling approach of Bernanke, Boivin and Eliasz (2005), which treats the model's parameters as random variables. The FAVAR's state equation is given by equation (2.5). The FAVAR's observation equation is given by (2.6) and can be re-written as follows:

$$Y_t = Gz_t + \tilde{v}_t \tag{2.7}$$

where $Y_t = \begin{bmatrix} x_t \\ z_t \end{bmatrix}$, $G = \begin{bmatrix} G_f^i \\ G_y^i \end{bmatrix}$, and $\tilde{v}_t = \begin{bmatrix} v_t \\ 0 \end{bmatrix}$.

I define the parameter space to be estimated by $\phi = [G \; B(L) \; \Omega_v \; \Omega_{zz}]$. I assume that the state-space system is normal and linear and proceed with Likelihood estimation using multi-move Gibbs sampling and Markov Chain Monte Carlo (MCMC) methods (Carter and Kohn (1994), Kim and Nelson (2000)), which alternately samples the parameters in ϕ and the latent common factors f_t. Let $\tilde{Y}_T = (Y_1, Y_2, ..., Y_T)$ be the history of Y from period 1 through T, and likewise define $\tilde{F}_T = (F_1, F_2, ..., F_T)$. We need to characterize the marginal posterior density of $\tilde{F}_T$ and ϕ. Then, estimates of $\tilde{F}_T$ and ϕ are simply the medians of their respective densities. The task is to approximate these densities.

Estimation is by multi-move, likelihood-based Gibbs sampling of the state-space model in (6) and (7), where the factors are now treated as unobserved.[6] I first sample the parameters

[5]Using principal components estimates would also ignore the uncertainty in the estimation of the factors.

[6]The appendix in Bernanke, Boivin and Eliasz (2004) provides details of the procedure.

conditional on the factors, then proceed to sample the factors conditional on the parameters. This is done until the iterations converge to the joint posterior distribution of the parameters of interest and the latent factor. The algorithm can be summarized as follows:

1. Choose initial values for the reduced form parameters, ϕ_0. I set the initial value of the factor to be the principal component estimated from the set of state-level variables, and the initial values of the transition equation parameters to be the OLS estimates of the system using on the principal components as observed factors.

2. Draw an entire history, over the entire sample period, of the latent factor from its joint density conditional on ϕ_0 and the observable variables. Given the Gaussian structure of the state-space model, the density conditional on the observable is normal and one can generate it using the Kalman filter. Given a draw from this density, work backwards through the sample, again using the Kalman filter, to generate an entire history of draws.

3. Given the history of the draws, all variables in the model are now 'observable'. One can use standard conjugate priors – Normal and Inverse Gamma – for G and Ω_v, respectively. The system in (7) is now simply a VAR in the latent factor and the observable aggregates; draw a new reduced form vector of parameters ϕ_1, using the (non-informative) diffuse prior, based on the OLS quantities.

4. Using ϕ_1, I impose a set of restrictions in order to identify the shocks of interest.

5. Return to step (2) above and again draw an entire history, over the entire sample period, of the latent factor from its joint density, now conditional on ϕ_1 and the observable variables. Iterate on steps (2) through (4) until the sampler converges to generate an empirical distribution for the desired structural response functions. I perform 30,000 iterations with a burn-in of 10,000, so the final 20,000 draws are used to characterize the posterior density. This distribution captures variation over the distributions of the latent factors as well as sampling error.

2.2.2 Identification strategy

Here, I focus on the dynamic responses of the variables in x_t to aggregate housing demand and supply shocks. And since I am only interested in responses to these two shocks, there is a priori no reason to identify the other $(h1+h2)-2$ aggregate shocks. I also ignore the role of all state-specific shocks. The housing demand shocks reflect favorable shifts in preferences that raise the marginal utility of housing, or capture changes in the ability to produce housing services induced by fluctuations in home production technology (Iacoviello and Neri (2009)). Demand shocks can also reflect money supply shocks that alter nation-wide mortgage rates [7]. The housing supply shocks alter the cost of producing houses and developing real estate, and may reflect current (or future) changes in productivity in the housing sector, changes in the adjustment costs of housing, changes in input prices, and so on. I emphasize that I aim at identifying *aggregate* shocks, as in Del Negro and Otrok (2007).

A main objective of this paper is to characterize the cross-state heterogeneity in state-wide consumption response to exogenous movements in real house prices. To this point, the FAVAR framework has solved the estimation problem, but it is not sufficient to identify the economic structure. Further restrictions are needed to achieve identification of the two aggregate shocks of interest. One identifying restriction that may be chosen is the conventional approach as first introduced by Sims (1980), which uses the Choleski decomposition of Ω_{zz} – implying a recursive ordering of the (h=h_1 + h_2) aggregate variables in (7). The recursive structure is however potentially controversial as there is no clear cut theoretical rationale motivating the restrictions. The recursive structure also entails imposing restrictions on the responses of the h_1 factors to the identified shocks in the system, which may not be desirable

[7]While a housing demand shock does not rule out a monetary policy shock, I do not pursue identification of the latter here, for two main reasons. First, recent research suggests only a limited role for monetary policy in driving real house prices across the "US" states (see, Del Negro and Otrok (2007). Second, I chose to focus on housing (demand and supply) as a source of shocks driving real house prices, as opposed to housing as part of the transmission mechanism of other shocks.

given that it is not always possible to provide an exact economic interpretation of the latent factors (see Bernanke, Boivin and Eliasz (2005)).

Here, I proceed differently. I achieve identification by means of robust sign restrictions (e.g., Faust(1998), Uhlig(2005)). As noted earlier, I assume that the housing markets and economies across the n states are driven by the two aggregate housing market shocks. I invoke these two shocks based on the signs of the ex-post impulse responses for a subset of the h aggregate variables: for a given house price elasticity, and uncorrelated demand and supply shocks, I can estimate the different economic primitives and draw impulse responses. The housing demand shock is identified as one that upon occurring, moves the quantity and price of housing in the same direction. The shock that moves the quantity and price in the opposite direction is interpreted as a housing supply shock. Finally, my assumptions, the responses of the $i = 1, .., nk$ state-wide variables in x_t, the focus of this paper, depend only on the degree of exposure to the h aggregate variables in z_t, and are simply computed as follows:

$$\frac{\partial x_{it+j}}{\partial u_{zt}} = \sum_{s=1}^{h} G_s^i \frac{\partial z_{s,t+j}}{\partial u_{zt}} \tag{2.8}$$

where $\frac{\partial z_{s,t+j}}{\partial u_{zt}}$ is the j-period ahead response of common factor s to a shock to the elements in u_{zt} corresponding to the aggregate housing demand and supply shocks, for $s = 1, 2, ..., h$.

As argued earlier, identification of the two housing market shocks is achieved using a signs-restriction approach. The latter define each shock solely based on its ex-post impulse responses for the two aggregate housing variables (i.e. price and quantity). At the risk of belaboring the obvious, I note that, for each identified shock, there are no restrictions imposed on the responses of the remaining aggregate variables in (5), or on the responses of any of the state-wide variables in x_t [8]. I use the Rubio-Ramirez, Waggoner and Zha (2007) algorithm for small-sample estimation and inference of structural VARs identified by signs restrictions. Here, I give a brief description of this algorithm. First, I obtain draws of the reduce-form

[8]The dynamic responses of those variables are left *agnostically* open by the identification procedure. The data will determine such response

parameters – the coefficients and covariance matrix of the VAR in (5). Second, I use the coefficients and the Choleski decomposition of the covariance matrix to obtain A_0 and A_1, the unrestricted structural parameters. More specifically, for a given draw – which gives values for the unrestricted structural parameters, I proceed by drawing an independent standard normal $(h_1 + h_2) \times (h_1 + h_2)$ matrix $\tilde{\Gamma}$, and let $\tilde{\Gamma} = \tilde{Q}\tilde{R}$ be the QR decomposition of $\tilde{\Gamma}$ with the diagonal of $\tilde{R}$ normalized to be positive. I then let $P = \tilde{Q}$ and generate impulse responses from $A_0 P$ and $A_1 P$. If such impulse responses do not satisfy the sign restrictions, I redraw a new independent standard normal matrix $\tilde{\Gamma}$. The estimation and the identification of the VAR in (5) is done at each step of the MCMC algorithm used to estimate the dynamic factors. The iteration generates a distribution for all the impulse vectors. Based on this distribution, I evaluate statistics and report the median responses, together with one standard deviation error bands.

A potential problem of the sign restrictions approach has been emphasized in Fry and Pagan (2007). The authors argue that, in general, sign restrictions contain weak information, so that there will be many impulse responses that can satisfy each sign restriction. When a series of impulse responses are compatible with a particular restriction, identification will then not be exact. They and Paustian (2007) argue that for the sign restrictions approach to *unambiguously* pin down the correct sign of the unrestricted responses, the variance of the underlying shock must be sufficiently large. Their argument is in line with the critique of Fry and Pagan (2007), since it essentially implies that if the shock is *dominant*, data have lots of information on the shock so that one may recover the correct parameters. Iacoviello and Neri (2010) show that housing market shocks account for a non-negligible part of the variation in other macroeconomic variables in the "US", thus providing some justification for my identification strategy.

2.2.3 Data and sample design

I estimate the model using quarterly data over the period 1976:II to 2008:IV [9]. I include in $\mathbf{x}_t$ three sets of state-wide variables (k=3) and use them to extract the common latent factor: real personal disposable income, real house prices, and real consumption expenditures. I measure disposable income as state personal income net of personal taxes, which are available from the Bureau of Economic Analysis (BEA). For real house prices, I use the repeat sales house price indices available from the Office of Federal Housing Enterprise Oversight (OFHEO). All real values denote nominal values deflated by the GDP deflator (GDPDEF) which I obtain from the database of the Federal Reserve Bank of Saint Louis (FRED). To get per capita values, I divide by state population estimates from the Current Population Survey (CPS).

A shortcoming in analyzing the consumption-housing wealth link at the state-level in the "US" is the unavailability of measures for consumption expenditures by households. A proxy for state-level consumption has been proposed in Rapach and Strauss (2006) as the difference between personal disposable income and personal savings income, on the premise that permanent changes in households saving will lead to permanent changes in the flow of income derived from accumulated savings [10]. Rapach and Strauss (2006) show that their proxy has a stable long-run relationship with actual aggregate "US" consumption, but they emphasize that they view it *only* as a long-run proxy as it is not necessarily informative with respect to short-run dynamics.

On the other hand, retail sales are a common proxy as they are an important subset of consumer expenditures, and account for roughly half of aggregate consumption in the "US". However, they do not include expenditures on health services, education, entertainment, and housing. Given my focus on the response of *non-housing* consumption spending, the failure

[9] The sample period is dictated by state-level availability for the house price data.

[10] State-level personal saving data are not available. Using the difference between disposable income and savings income is essentially akin to assuming that labor income is a proxy for consumption.

to account for expenditures on housing gives retail sales an important advantage as a proxy over other measures. Studies that have studied consumption at the state-level have used retail sales as proxy; see for example, Case, Quigley and Shiller (2005), Garrett, Hernàndez-Murillo and Owyang (2005), and Calomiris, Longhofer, and Miles (2009). Unfortunately, retail sales are also not *directly* observable at the state-level [11]. However, they can be imputed using data on quarterly states sales tax revenues and sales tax rates, as in Garrett, Hernàndez-Murillo and Owyang (2005). In my analysis, I use the state-wide retail sales data from Garrett, Hernàndez-Murillo and Owyang (2005) and extend it to cover my sample range [12]. Note that my sample is limited to 43 states (i.e. n=43), because five states have no sales tax (Alaska, Delaware, Montana, Oregon, and New Hampshire) and two states (Nevada and Utah) have incomplete tax revenues records.

The key to my analysis is the validity of the imputed retail sales series as proxies for consumption. Obviously, I am not able to test for such validity for each state. However, evidence of a strong relationship between my proxy and actual aggregate consumption for the "US", is potentially suggestive that such relationship may also exist at the state-level. Therefore, I sum over the (n=43) individual states to obtain an aggregate retail sales series, and compare with the "US" aggregate Personal Consumption Expenditures (PCE, net of housing services). Regressing the quarterly growth rate of my state-wide aggregated retail sales series on the growth rate of the reported PCE, yields a slope coefficient of 1.15, which is small and statistically insignificant from unity, and a negligible intercept term; to wit, a one percent increase in *actual* aggregate Personal Consumption Expenditures (PCE) results in roughly one percent increase in my state-wide aggregated retail sales series. The latter indicates that movements in actual "US" consumption expenditures seem to be sufficiently

[11]The data used in Case, Quigley and Shiller (2005) and Calomiris, Longhofer, and Miles (2009) were constructed by *Regional Financial Associates* (RFA) using county sales tax data. For states with no sales taxes, retail sales were imputed by exploiting the historical relationship between retail sales and retail wages and employment. Detailed information on the methodology used in constructing the dataset is not readily and explicitly available.

[12]I obtain sales tax revenues from the *State Government Collections* database (Census Bureau), and sales tax rates from the Tax Foundation's *Facts and Figures on Government Finances.*

reflected in the proxy adopted in this paper. Moreover, we test for the presence of a cointegrating relation between my aggregate and the reported aggregate in levels. Results (not shown) suggest the presence of a cointegrating vector[13]. This increases my confidence that using the imputed retail sales measures as proxies for consumption at the state-level will be informative about the parameters of interest. Overall, the choice of variables is dictated primarily by data availability but is reasonable in light of my objectives and the need for consistency across states.

I estimate the model over the sample period, as described in the previous section. For the baseline specification, the VAR in (5) consists of seven variables – one latent dynamic common factor (h_1=1) and six aggregate observable variables (h_2=6). My choice to include only one latent common factor is in light of the assumption that the number of observable factors included in (5) is large and sufficient to identify the aggregate shocks of interest [14]. The vector of observable aggregate variables, $\mathbf{y}_t$, contains real GDP (FRED series GDP), real personal consumption expenditures (FRED series PCEC), real house prices (OFHEO HPI all transactions index), real private residential fixed investment (FRED series PRFI), the GDP implicit price deflator (FRED series GDPDEF), and the 5-year Treasury bond rate (FRED series GS5) – where all real values denote nominal values deflated by the GDP deflator. The common factor VAR in (5) is estimated with four lags (i.e. p=4). All series are demeaned, so no intercept is estimated as in Bernanke, Boivin and Eliasz (2005). From a Bayesian perspective, it would be natural to estimate the full model in levels. However, the dynamic factor methods used in the literature are based on the modeling assumption that the errors are independent over time. The latter is not a reasonable assumption in our case

[13] I assume that both variables are first-order integrated $I(1)$, an assumption confirmed by unit root test results (not shown). The cointegration test is carried without a deterministic trend in the static regressions, allowing for trends in the raw data.

[14] Bai and Ng (2002) provide a criterion to determine the optimal number of factors. However, as argued in Bernanke, Boivin and Eliasz (2005), this does not necessarily address the question of how many factors should be included in the VAR.

since the variables in $\mathbf{x}_t$ are clearly non-stationary in levels [15]. I follow common practices in the literature, and compute growth rates using log-differences (in percent) of the series in $\mathbf{x}_t$. I also transform the aggregate observable variables in $\mathbf{y}_t$ into growth rates (except for the interest rate variable which I only first-difference), to maintain symmetry in the model. The (identifying) sign restrictions for all aggregate shocks are imposed for two quarters, with the first quarter corresponding to the period of the shock. Hence, the restrictions are imposed for one quarter after the initial shock. Later, I consider how sensitive my results are to alternative specifications.

2.3 Empirical results

It is potentially useful to provide an interpretation of the latent factor. However, such interpretation is not always possible if the data used to extract the factor are heterogenous in terms of the concepts they represent. It is mainly for this reason that Bernanke, Boivin and Eliasz (2005) do not interpret the factors they extract and use in their analysis [16]. Although a proper interpretation of the factor does not motivate my analysis, I take a look at the estimated latent factor and attempt to briefly interpret the co-movements it captures. Figure 2.2 plots my estimate of the latent factor against four of the aggregate observable variables in y_t. All series are shown in (log) levels. Results suggest that fluctuations in the estimated factor seem to better reflect fluctuations in output and consumption, rather than fluctuations in the housing sector variables [17]. However, it is fair to say that the latent factor exhibit slightly dif-

[15] It would be reasonable to make such assumption if there is cointegration among the variables in $\mathbf{x}_t$, where the non-stationarity in the different variables cancel out or where it makes sense to have a non-stationary latent factor which would pick up the non-stationarity (thus leaving the idiosyncratic errors approximately stationary).

[16] Del Negro and Otrok (2007) were able to interpret their estimated dynamic factor as a national housing factor because the panel of data used to extract the factor consisted of (only) state-level house prices series.

[17] An exact name for the factor is possible only if the data is organized into blocks. Such strategy would provide a natural way to name the factors estimated from each block of data. The latter has been done in the literature (e.g., Ludvigson and Ng (2009), Moench and Ng (2010)) but lies outside the scope of this paper.

ferent dynamics than output and consumption over time. The latter suggests that including the factor in the VAR analysis is potentially informative and valuable to my analysis. In what follows, I present the results for the dynamic responses of the variables in the system to the two aggregate housing shocks. Because my baseline specification uses first-differences of the logs of the data, I accumulate all the responses to obtain the corresponding estimates in the levels of the variables.

2.3.1 Dynamic responses to aggregate housing demand shocks

Figures 2.3 through 2.5 report the estimated impulse response functions of the levels of the variables to positive aggregate housing demand shocks. All responses (solid) are plotted up to a horizon of 40 quarters, and are shown with the 16^{th} and 84^{th} (dashes) percentiles from the posterior distribution [18]. Consider first the dynamic responses of the aggregate variables. According to Figure 2.3, a one standard deviation housing demand shock leads to a persistent increase in aggregate real house prices. Real house prices rise on impact by about 0.23 percent with a peak response of around 1.36 percent. The increase in residential investment in response to the shock is less persistent than that of real house prices. Residential investment rise by about 0.83 percent on impact with a peak response of roughly 3.94 percent two years after the shock. Real GDP rises in response to the shock, peaking by roughly 0.45 percent after about three years. The dynamic response of real consumption tracks well that of output but is somewhat smaller at its peak (0.31 percent). Translating the results into a 1 percent (peak) increase in house prices leads to a 0.23 percent (peak) increase in consumption, which is a somewhat higher than that estimated in the DSGE model of the US economy presented in Iacoviello and Neri (2010). However, such response is only intertemporal as consumption tend to return to its original steady-state level after about three years. The shock is associated with a strong and persisting tightening of the monetary policy stance, which in turn may be associated with the permanent response of the price level: the interest rate rises by about

[18] Assuming a normal distribution, the 16^{th} and 84^{th} percentiles correspond to conventional one standard deviation error bands.

50 basis points, which is consistent with a stabilizing role for monetary policy in response to the shock [19].

I now turn to the analysis of the effects of the aggregate housing demand shocks on the state-wide variables, the focus of this paper. Figure 2.4 and 2.5 report the impulse responses functions for the levels of state-wide consumption and real house prices, respectively. Consider first the dynamic responses of consumption, shown in Figure 2.4. The responses displayed by California, Florida, Massachusetts and South Carolina are very large and precisely estimated [20]. States such as Alabama, Arkansas, and Georgia show dynamic responses that are similar to the mean response across states. Overall, the results indicate that, for most of the states in my sample, consumption rises in the short-run then decreases thereafter [21]. This pattern for the consumption responses potentially suggests an important role for the housing collateral channel; to wit, an increase in real house prices raises the amount that constrained households can borrow and spend today. However, this is not a net *wealth effect*, as households will have to reduce their spending in the future when they service the debt. Hence, at market interest rates, the present value of current and future consumption is not affected by the housing collateral channel. It is not clear that the empirical literature has emphasized the latter, at least not explicitly. The housing collateral channel is analyzed in Iacoviello (2005) and Iacoviello and Neri (2010), and is consistent with the fact that fluctuations in consumption are amplified by endogenous variations in collateral constraints that are tied to the value of the housing asset. Consumption in Connecticut, North Dakota, New York, and Texas *decreases* when housing demand rises. The theoretical models in Iacoviello (2005) and Iacoviello and Neri (2010) show that such negative effects on consumption are

[19] This finding is consistent with the results of Jarociński and Smets (2008).

[20] The consumption response for California is particularly very large. The average response ,over the first five years, is around 2.27 percent compared to the cross-state mean of 0.32 percent

[21] California and New Mexico are the only two states for which consumption appear to rise permanently in response to the shock. Florida and Oklahoma also seem to display permanent effects on consumption but their long-horizon responses are not precisely estimated.

implied in the absence of housing collateral effects [22]. The negative effects on consumption are also plausible in the absence of collateral effects, and are mainly due to substitution effects between housing and non-housing consumption (Iacoviello, 2005). The responses of state-level real house prices are reported in figure 2.5. The responses match the overall pattern of the aggregate house price response. However, I note the state-wide heterogeneity in the responses. Across states, the mean of the maximum increase in real house prices is around 1.23 percent, with a standard deviation of 0.81 percent. I find the largest responses in California, Arizona, Florida, Hawaii, and Maine. There are also notable differences in the persistence of the responses to the shock. For instance, while responses are permanent for each of the states mentioned above, they are less pronounced and tend to fade over time for the other states.

2.3.2 Dynamic responses to aggregate housing supply shocks

Figures 2.6 through 2.8 report the estimated impulse response functions of the levels of the variables to aggregate housing supply shocks, which are normalized to reflect a *negative* shock. As in the previous section, the figures include the $50th$ (solid), $84th$ and $16th$ (dashes) percentiles from the posterior distribution. Consider first the dynamic responses of the aggregate variables. According to Figure 2.6, a one standard deviation negative housing supply shock increases aggregate real house prices by about 0.22 percent on impact percent. The shock permanently decreases residential investment by about 2.30 percent, after an initial decline of 0.95 percent. In response to the shock, real GDP and real consumption fall permanently by about 0.24 and 0.20 percent, respectively. Housing supply shocks seem to have a transmission mechanism that is distinct from that of housing demand shocks. There are few things to note. First, while housing demand shocks have a hump-shaped impact on aggregate real house prices and residential investment, the effects in the case of housing supply

[22] Note that Texas, which experiences a negative effect on consumption from increases in house prices, did not have a market for home equity loans over most of my sample. Until the late nineties, availability of home equity loans in Texas was strictly limited to home improvement loans and loans to pay outstanding taxes – rather than to spend on non-housing goods and services.

shocks are rather monotonic. Second, the effects of housing demand shocks on house prices are larger in magnitude, more persistent, and more significant than those of housing supply shocks [23]. The latter is not surprising, as a simple demand-supply diagram of the housing market in the "US" support the idea that movements in house prices are the consequence of a shift in housing demand, rather than housing supply (Iacoviello 2009). Third, while housing demand shocks exhibit temporary effects on consumption (and output), housing supply shocks have effects that are permanent. The permanent responses indicate wealth effects. Indeed, as noted, consumption *falls* when house prices *rise*. Supply side shocks in housing reflect changes in the production set or changes in the distance between the market allocation and the production frontier, which involve wealth effects by any definition (Mulligan (2009)). Evidently, a *negative* housing supply shock increases costs of construction, raising the real price of housing and thereby taking away resources for other consumption. This really affects real consumer wealth [24]. Yoshida (2008) also argues that the response of consumption to housing supply shocks (temporary or permanent) is generated by the covariation between consumption and rent. In particular, his model predicts that a negative housing supply shock leads to an increase in housing rent, and under certain parameter values [25], consumption decreases because of the complementarity of the non-housing consumption goods and housing services. The negative response of consumption to *negative* housing supply shocks is consistent with the findings in Cardarelli et. al (2009). Finally, unlike in the case of housing demand shocks, the interest rate and price level hardly react to housing supply shocks. The latter is also consistent with the findings in Carderelli et. al (2009).

I now turn to the analysis of the effects of the aggregate housing supply shocks on the state-wide variables. Figure 2.7 and 2.8 report the impulse responses functions for the levels

[23]While Real house prices rise permanently in the case of housing supply shocks, their responses are not precisely estimated over the long run so that I cannot rule out a return to the original steady state.

[24]A natural disaster that leads to the destruction or damage of the housing stock also acts as a negative housing supply shock, exerting *adverse* wealth effects on consumption demand.

[25]Particularly, when the parameter for inter-temporal substitution is greater than the elasticity of inter-temporal substitution between the housing and the non-housing good.

of state-wide real house prices and consumption, respectively. Consider first the dynamic responses of consumption, shown in Figure 2.7. I find that consumption in almost every state decreases in the response to the negative housing supply shock. The effects are generally persistent though imprecisely estimated over long horizons. Few states such as California, Colorado, Ohio, Virginia, and Washington are an exception, as the negative effects on consumption are statistically significant at all horizons. The responses of state-level real house prices are reported in figure 2.8. Across states, the mean (peak) increase in real house prices is around 0.31 percent, about one fourth of the mean increase in the case of housing demand shocks. However, in contrast to my findings in the case of housing demand shocks, there is no apparent heterogeneity in the responses of real house prices to the aggregate housing supply shocks across states. The responses are remarkably uniform and track very closely the response of aggregate house prices.

2.3.3 Robustness checks

Here I consider the robustness of my analysis along several dimensions. First, I perform tests to confirm that the Gibbs sampler has converged instead of getting stuck in some local mode. This is important because the validity of my estimates rests on such convergence. Second, I check whether the magnitude of the estimated dynamic responses are robust to reasonable variations in the statistical model. Finally, I check to see if averages of the estimated dynamic state-level responses are similar to the responses of their corresponding US aggregates; my empirical analysis will be more robust if the latter hold in the data.

First, I perform a battery of tests that is widespread in the MCMC literature. I first checked the precision of the sampler by plotting the associated error bands, and found that the latent factor is very tightly estimated. Second, I evaluate the convergence of the sampler by plotting the latent factor calculated from the first half of the kept draws, against that derived from the second half of the kept draws. Results (not shown) suggest that there is almost no visible deviation. This may suggest that the sampler went through the whole distribution and has converged to the target distribution. While such diagnostics are not a guarantee, they reduce uncertainty and potentially suggest that convergence of the sampler is not an issue.

Second, It is important that the magnitude of the estimated dynamic responses be robust to reasonable variations in the statistical model. The question is – have I misinterpreted the identified shocks and have found pervasive elasticities because of misspecification of the statistical model and inappropriate identifying restrictions? I consider this question along several dimensions. I first make straightforward changes to the model. Increasing the common lag length of the endogenous variables in (5) from four to five mainly yields impulse response functions that are relatively less smooth, without significantly altering the shape or the magnitude of the dynamic responses. Also, the identifying signs-restriction approach is appealingly clean as I only make use of a priori consensual views about the effects of changes in the demand and supply of housing. I however consider how sensitive my point estimates are to changes in the specified horizon over which the sign restrictions are imposed. Increasing the horizon from 2 quarters to 3 quarters does not significantly alter the distribution of point estimates across states or the degree of uncertainty about the point estimates, but significantly adds to the estimation procedure in terms of computational costs [26].

Finally, my empirical analysis will be more robust if I can show that the cross-state averages of the estimated dynamic responses are similar to the responses of their corresponding aggregates. Figure 2.9 plots the cross-state average of the median responses for house prices and consumption, and the responses of the corresponding aggregates. The average median response of sales is larger at all horizons than the response of aggregate consumption for housing demand shocks, but the opposite holds for housing supply shocks. The aggregate house price response and the average of the median state house price responses are reasonably similar, especially so for supply shocks.

[26]Estimating the dynamic factor model itself is computationally costly. Combining this with estimating and identifying the VAR using sign restrictions, at each step of the MCMC algorithm, makes the procedure very costly. Further increasing the horizons over which I impose the sign restrictions potentially requires more draws of potential impulse response functions and is infeasible.

2.4 The role of collateral constraints

So far, I have quantified the spill-over from housing market shocks. In this section, I will attempt to explain the observed variation in state-wide consumption and house prices responses to the shocks. I focus on the role of housing finance in explaining this variation. Mortgage market developments, especially those resulting from the financial reforms of the 1980s, greatly affected the housing markets across "US" states. In particular, they have enhanced the ability of households to borrow against the value of their (housing) assets. The latter suggests a potential role for the collateral effects of house prices – higher house prices increase the value of house as collateral, relaxing borrowing constraints on households. If such collateral effects are important, then states with more developed mortgage markets – where it is relatively easy for households to tap into their home equity to finance spending – would be expected to experience a greater response to housing demand shocks that increase house prices. Of course, the increase debt will have to be serviced, and eventually future consumption will have to decrease. In other words, the positive collateral effects of increasing house prices should only manifest themselves *inter-temporally*. Over long horizons, consumption should *decrease* so that we would expect a negative relationship between the consumption responses to positive housing demand shocks and the level of mortgage markets development. At market interest rates, if the conventional wealth effect is important, the present value of current and future consumption spending will be positively affected by the housing demand shocks that increase house prices. In other words, if the wealth effect is important, there would be no negative relationship at long-horizons.

In order to test the above hypothesis, I now turn to analyzing the cross-state variation in the elasticities of consumption with respect to house prices. In my analysis, I will focus only on the incidence of housing demand shocks on the state-wide variables. My choice is not arbitrary. Housing demand shocks are particularly thought to play an important role in the transmission mechanism of the effects of house prices on consumption demand in an upturn marked by an expansion in housing collateral values and equity extraction. The evidence that I present in this paper suggests that negative housing supply shocks that raise real house prices have negative (rather than positive) effects on consumption demand. Moreover, such shocks are thought to limit the extent to which the housing stock can be adjusted, thus potentially limiting the expansion of new *collateralisable* debt. Liu, Wang and Zha (2009) argue that, in general, credit constraints do not amplify non-financial shocks or financial shocks that shift the *supply* of the housing asset (e.g., housing supply or technology shocks)[27].

I investigate the extent to which my devised measures of the consumption elasticities are related to the level of mortgage market developments. I must first come up with measures that precisely quantify the dynamic responses of consumption at different horizons. I consider seven different measures: the maximum response (r_m),the average response over the first four quarters (r_4),the average response over the first eight quarters (r_8),the average response over the first twenty quarters (r_{20}), the average response over the first twenty-eight quarters (r_{28}),the average response over the first forty quarters (r_{40}), and the average response seven to ten years after the shock (r_l). The measures exploit differences in the persistence and magnitude of the elasticities and thus shed light on responses in the short-run or long-run. This helps in capturing the importance of housing collateral factors in the transmission mechanism of housing demand shocks on consumption demand. Tables 2.1 and 2.2 present the measures of the dynamic responses of consumption and house prices, respectively, along with the measures' rank across states (where a rank of "1" denotes the largest estimate). The state-wide heterogeneity of the responses – displayed in Figures 2.4 and 2.5 and discussed earlier – is evident in the Tables as well.

I must also come up with an index that captures the depth and flexibility of mortgage markets and housing finance across the "US" states. Such index would summarize the extent to which fluctuations in housing collateral values can be translated into greater borrowing by households, and thus greater spending. I construct such index using information from six

[27] In the model of Liu, Wang and Zha (2009)– an economy where producers are credit-constrained – housing demand shocks account for over 90 % of the observed fluctuation in housing prices.

financial variables: the initial fees and charges on a home mortgage (as a percent of loan value), the term to maturity on mortgages (in years), the housing purchase price, the loan-to-value ratio, the percent of all mortgage loans that are adjustable rate, and the percent of homeowners with home equity loans. Lower initial fees and charges makes mortgages loans less costly and thus encourage households to tap into their home equity. A longer term to maturity and a larger loan-to-value ratio allow for greater mortgage debt. The housing purchase price measures the value of housing collateral in a state. The percent of all mortgage loans that are adjustable rate potentially proxies for the number of home equity lines of credit in a state, since most of these lines of credit are adjustable rate. The percent of homeowners with home equity loans is a direct measure of the extent to which households use home equity loans to finance spending. The first five variables are from the Finance board's *Monthly Survey of Rates and Terms on Conventional Single-Family Non-Farm mortgage loans*, available from the Federal Housing Finance Agency (FHFA). The last variable comes from the American Community Survey (ACS) of the Census Bureau. I take time averages for all variables across states [28]. I normalize each of the six variables by its corresponding maximum value across states so that the variables are on a scale of 0 to 1. For each state in my sample, the mortgage market index MMI is constructed as follows:

$$MMI_i = \frac{1}{m}\sum_{r=1}^{m}\chi_m$$

for $i = 1, 2, ..., 43$ and where, for each state i, χ is a sample space containing the $m = 6$ financial variables discussed above (after weighting "initial fees and charges" by -1). I assume that the larger MMI is, the greater the ability for using the value of the housing asset as collateral. I summarize the index and its components in Table 2.3. The mean across states is 0.475 with a standard deviation of 0.082, which implies that the variation across states in the sample is non-trivial. To the extent that such variation is indicative of differences

[28] All variables are available yearly from 1978-2008, except the home equity loan variable, which is available only since 2002.

in mortgage market developments and in institutions for housing finance across states, my index provides useful information that allows me to test for housing collateral effects.

As mentioned earlier, I posit that if collateral effects are important then states with a higher value of the mortgage market index MMI will experience a greater response to positive housing demand shocks – that alter house prices – and thus will spend more. In order to test this hypothesis, I estimate cross-state regressions of the following form:

$$y_i = \beta_0 + \beta_1 MMI_i + \gamma q_i + \epsilon_i, i = 1, ..., 43, \tag{2.9}$$

where y is the estimated elasticity of consumption (or house prices) with respect to the identified housing demand shocks (r_m,r_4,r_{20}, r_{28}, r_{40} or r_l), MMI is the mortgage market index, and q is a vector of control variables. OLS is acceptable as an estimation strategy and inference is correct as long as the mortgage market index MMI is not correlated with other variables representing other causes of variation in the consumption responses [29]. Standard errors used to report tests of statistical significance are robust to heteroscedasticity and are computed from White's consistent estimator of the covariance matrix. One thing to note – because my point estimates of the elasticities of consumption with respect to housing demand shocks are used as the dependant variables in the cross-sectional regressions, specification and identification errors in the FAVAR estimation will bias cross-sectional inference only if these errors are correlated across states with the regressors in those regressions. But if such errors are random, they will most likely only add independent noise to the linear regression error, making it more difficult to uncover statistically significant results.

Panel A of Table 2.4 reports the results from regressing each of the seven measures of the dynamic responses of consumption on the mortgage market index MMI. The results are consistent with my hypothesis; to wit, the estimated coefficient on the mortgage market index is positive and statistically significant in explaining the cross-state variation in the

[29] The assumption that the errors in (7) are orthogonal to the constructed mortgage market index MMI is reasonable given that the components of the index are generally affected by exogenous states' laws and regulations.

short-run measures of the elasticities of consumption (r_4 and r_8). While statistical significance is important in understanding the extent to which sampling error affects inference, a coefficient estimate that is statistically significant is not necessarily economically meaningful. One way to determine the economic importance of the estimated housing collateral effects is to consider how much the short-run measures of the elasticity of consumption would change for a typical change in our constructed mortgage market index. For example, the average state in my sample has an elasticity of consumption over the first four quarters (r_4) of 0.201 % and a mortgage market index MMI of 0.475. According to the estimates in Panel A of Table 2.4, a state with MMI of 0.557, or one standard deviation above the mean, will have a consumption elasticity r_4 of 0.402%, 0.201% *larger* than average ($0.201 = 2.447 \times 0.082$). This quantity corresponds to around 47% of r_4's cross-state standard deviation of 0.431%. For r_8, a one standard deviation increase in MMI increases the mean response of consumption over the first 8 quarters by 0.274, which is about 54 % of the sample standard deviation of r_8. The explanatory power of the mortgage market index declines with the horizon and there is a *negative* and statistically significant relationship between the long-run consumption response r_l and MMI. Such negative relationship suggests that states with higher a higher value of the mortgage market index MMI experience elasticities that *decrease more* going into the next period. For instance, for r_l, a one standard deviation increase in MMI *decreases* the mean long-run response of consumption by 0.048 (from -0.144 to -0.192, which is about 33 % of the sample standard deviation of r_l. The latter result is intuitive and potentially shed light on the fact that when households who are borrowing constrained spend the larger fraction of the increase in house prices today, they will have to reduce their spending in the future as they service the debt. Clearly, the mortgage market index exhibits non-trivial effects on the consumption elasticities and, as hypothesized, the effects are positive over the short-run and negative over the long-run so that, at market interest rates, the present value of current and future consumption will not be affected by the housing collateral channel.

These findings are typically consistent with the presence of collateral effects from aggregate housing demand shocks and are inconsistent with pure wealth effects.

Panel B of Table 2.4 addresses the robustness of the results. Here, I control for regional effects in the regressions. The addition of the regional dummies strengthens the estimated collateral effects of MMI; to wit, in the case of r_4 the estimate increases from 2.447 to 2.978, while in the case of r_8 it increases from 3.340 to 3.853. Table 2.6 reports the Spearman's rank-correlation coefficients (Siegel, 1956) for the consumption response measures and the mortgage market index. Correlations based on rank are less sensitive to extreme point estimates than correlations estimated from the regression models [30]. Panel C shows that, at the 5 % significance level, the null hypothesis of no correlation can be rejected for the same variables that are significantly non-zero in the regression analysis.

As a final exercise, I repeat the above regressions, but replace the retail sales responses to housing demand shocks with those of real house prices as the dependent variables. In the model of Liu, Wang and Zha (2009), housing demand shocks in an economy where producers are credit-constrained have large effects on house prices through an accelerator mechanism: higher land prices increase the value of producer collateral, which further increase the demand for real assets as inputs to production.[31] My cross-state response functions provide a simple test of this prediction. If valid, states with fewer constraints on mortgage borrowing will exhibit a greater real price sensitivity to housing demand shocks. Thus, there should be a positive relationship between my summary response measures and MMI. Table 2.5 contains the results, which weakly support the hypothesis: the estimated coefficients, except on the long-run response, are positive, statistically significant, and are generally large. The rank correlations are positive and large. However, the coefficients estimates decline when regional dummies are included, and the state of California does seem to drive some of the results.

[30] For instance, the extreme sensitivity of spending and house prices in California is evident and may affect the regression results.

[31] Their model predicts that housing *supply* shocks will *not* have similar effects.

In summary, the effects that changes in housing prices have on consumption demand rely on (and are reinforced by) the presence of collateralized constraints. My results provide support for the argument that pure housing wealth effects are not plausible on aggregate (Buiter, 2008), and suggest that the main channel through which housing wealth affect non-housing consumption is through affecting collateral or liquidity constraints. My estimates are consistent with, for example, the estimates in Iacoviello and Neri (2010) who find that collateral effects increase the elasticity of consumption with respect to housing wealth from about 10 to 12.3 percent.

2.5 Summary and conclusion

I have estimated the dynamic responses of state-wide consumption and house prices to aggregate US housing demand and supply shocks. Unlike other studies, I impose less structure and allow for heterogeneity of the responses across states. I use a latent-factor model to ensure a parsimonious relationship among the state variables, and plausible signs-restrictions that allow to distinguish the exogenous sources of shocks to the demand and supply of housing. Overall, my results suggest that house price shocks have helped to underpin strength in consumption spending. First, I find that the spill-overs from housing supply shocks have a different transmission mechanism than those from housing demand shocks. While both types of shocks can have significant effects on consumption demand, only increases in house prices that are due to shifts in the demand for housing seem to explain the observed positive correlation between consumption and housing wealth. Second, I show that there is great dispersion in the response of state-wide spending and house prices to housing demand shocks. I find that such dispersion is associated with different levels of financial innovation across states, and thus is most likely due to collateral effects rather than pure wealth effects. My conclusion is, of course, conditional on my identification strategy. Other models may yield different results, but I believe that my approach has taken a step forward in bringing empirical support that bears on the importance of liquidity constraints in explaining the observed correlation between house prices and consumption.

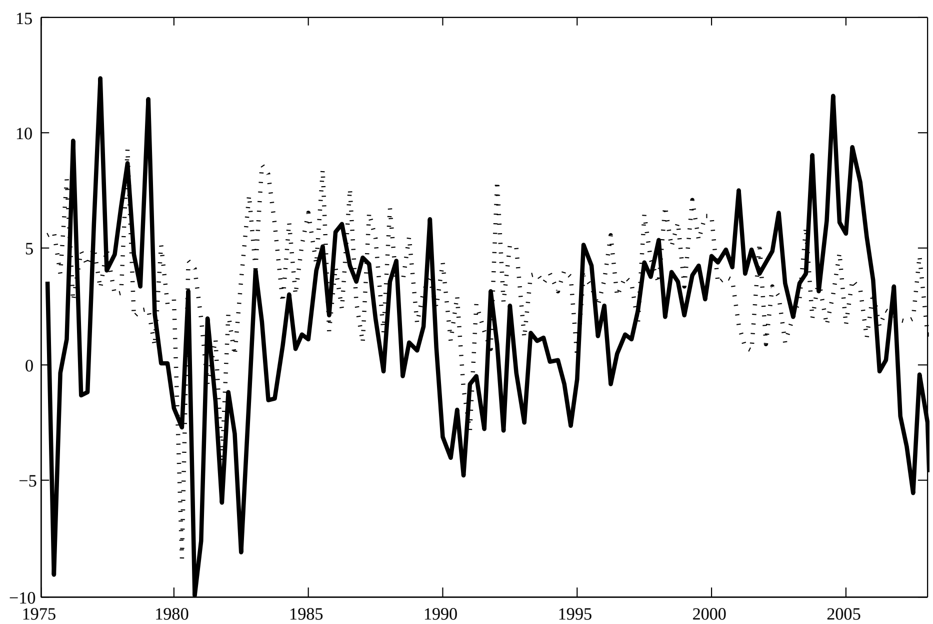


Figure 2.1: Growth in Real House Prices and Real Consumption Expenditures : Annualized quarterly growth rates of real aggregate consumption expenditures (dots) and real house prices (solid) for the "US" from 1975 to 2008.

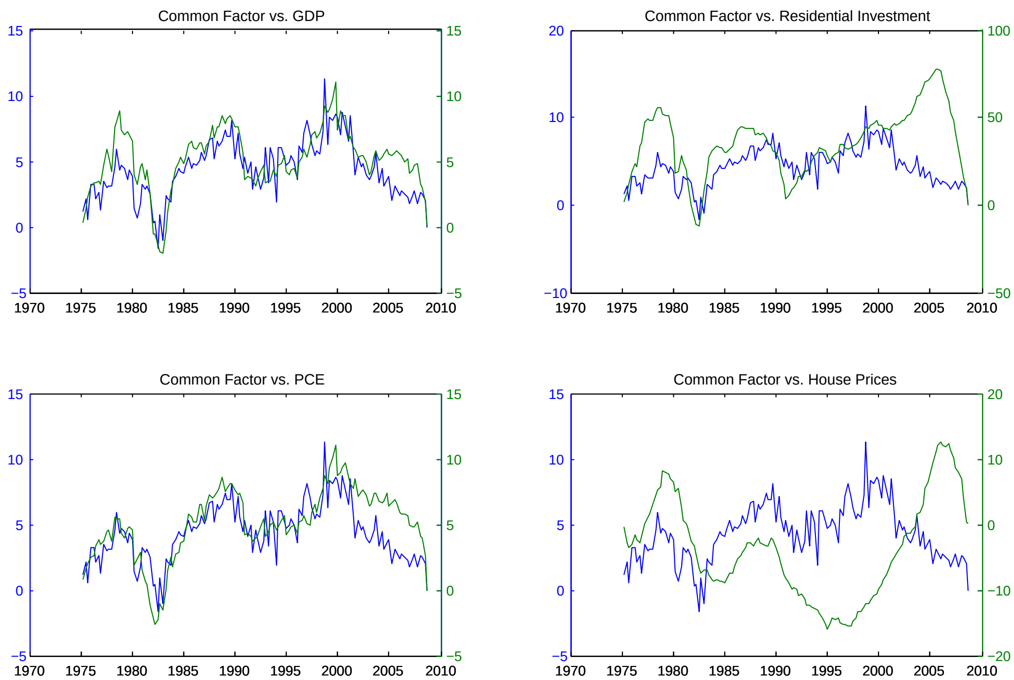


Figure 2.2: Latent Common factor and US aggregate variables: Latent common factor in blue, left scale. All observable aggregates are in logs and their growth rates have been de-meaned.

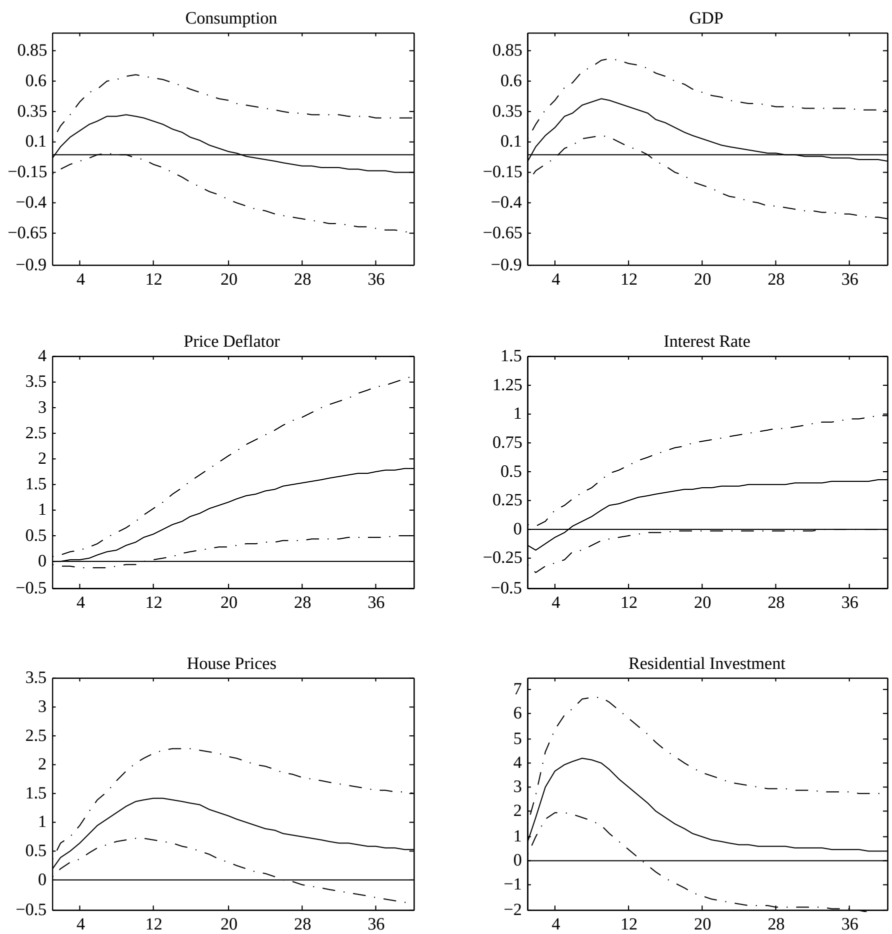
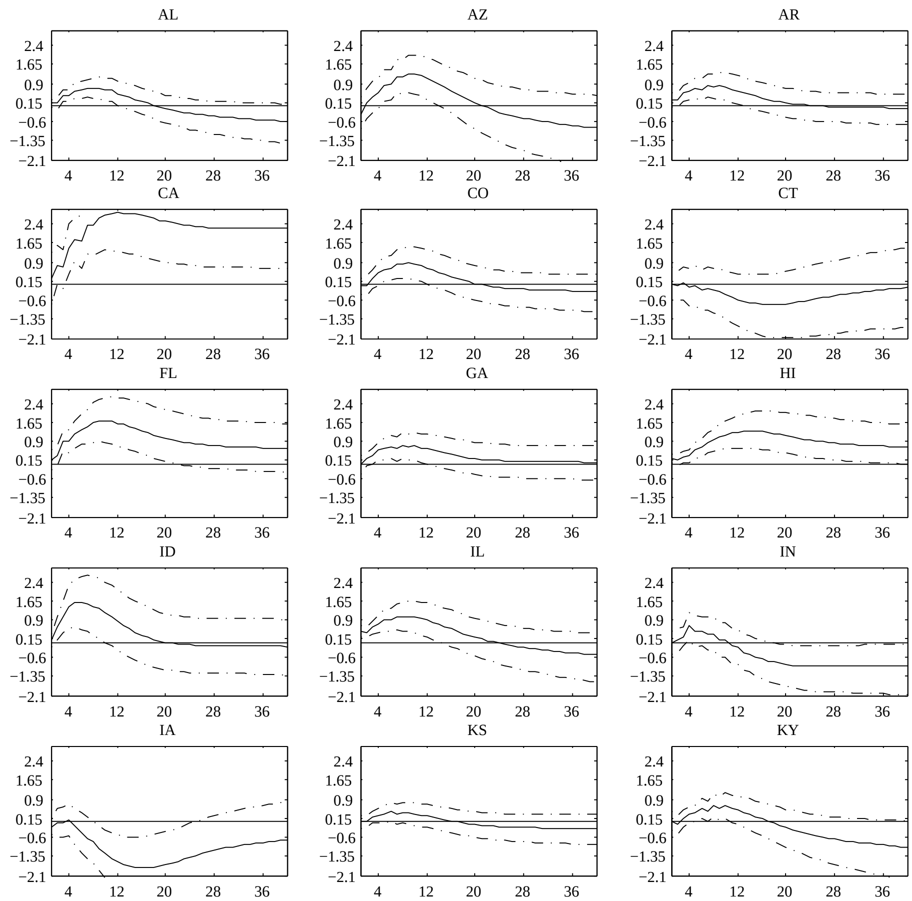
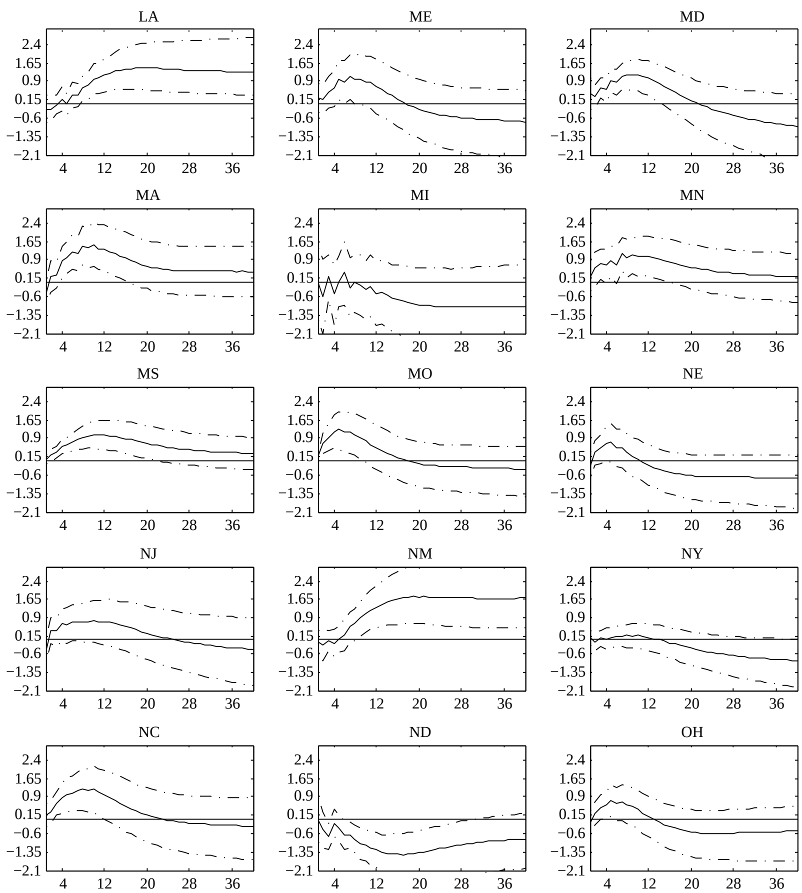


Figure 2.3: Dynamic responses of aggregate variables to housing demand shocks: Solid curve is the median response over the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile bands.



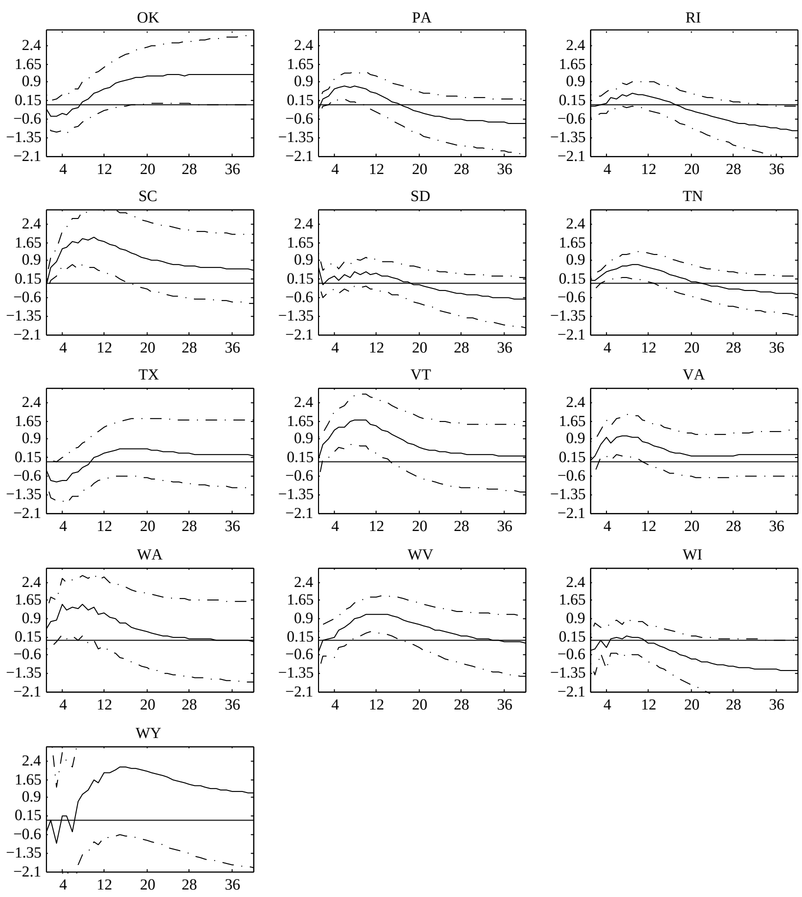
(a)

Figure 2.4: Dynamic responses of state-level retail sales to Aggregate Housing Demand Shocks: Solid curve is the median of the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile.



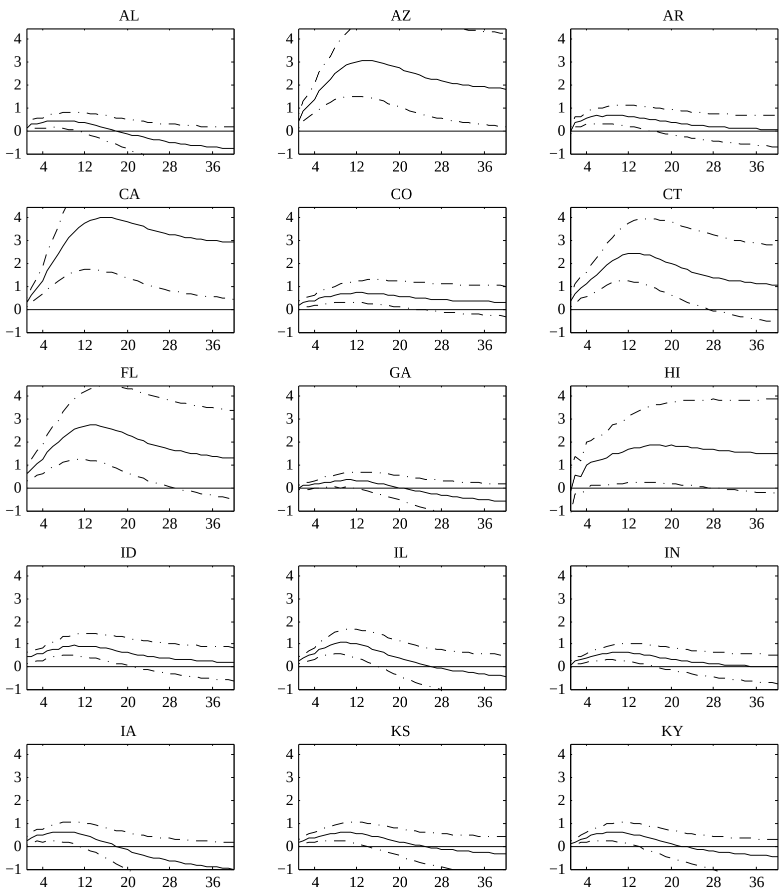
(b)

Figure 2.4: (cont'd)



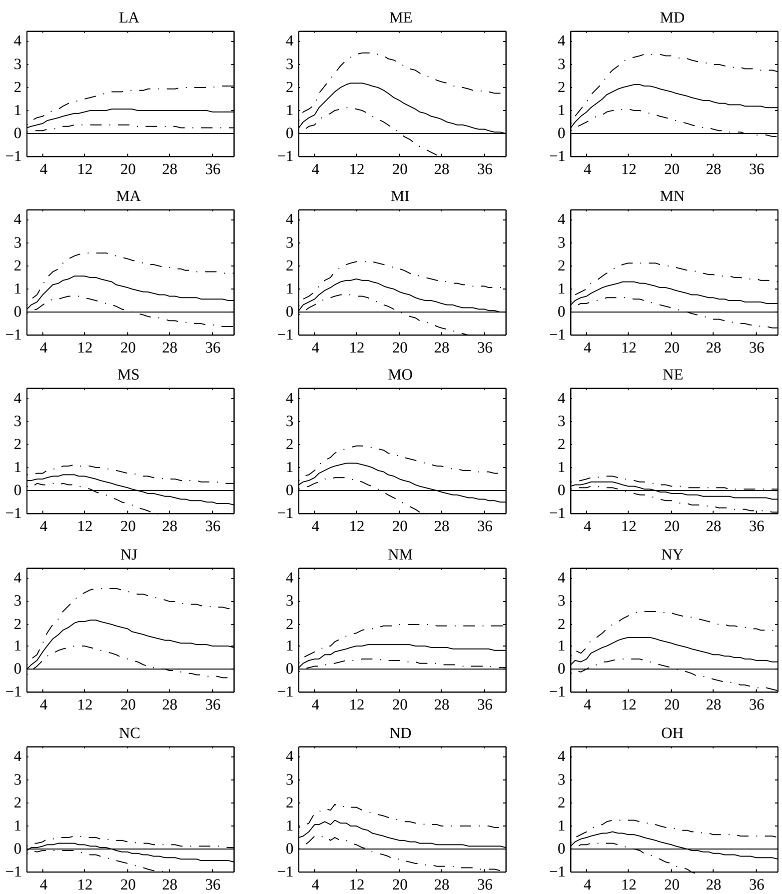
(c)

Figure 2.4: (cont'd)



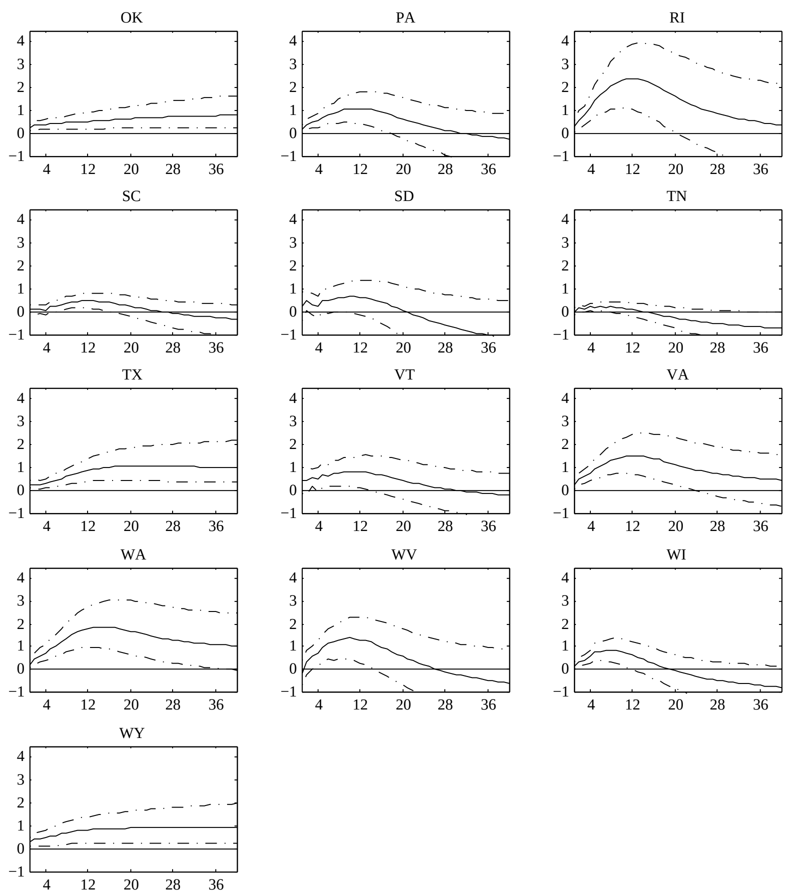
(a)

Figure 2.5: Dynamic responses of state-level house prices to Aggregate Housing Demand Shocks: Solid curve is the median of the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile bands.



(b)

Figure 2.5: (cont'd)



(c)

Figure 2.5: (cont'd)

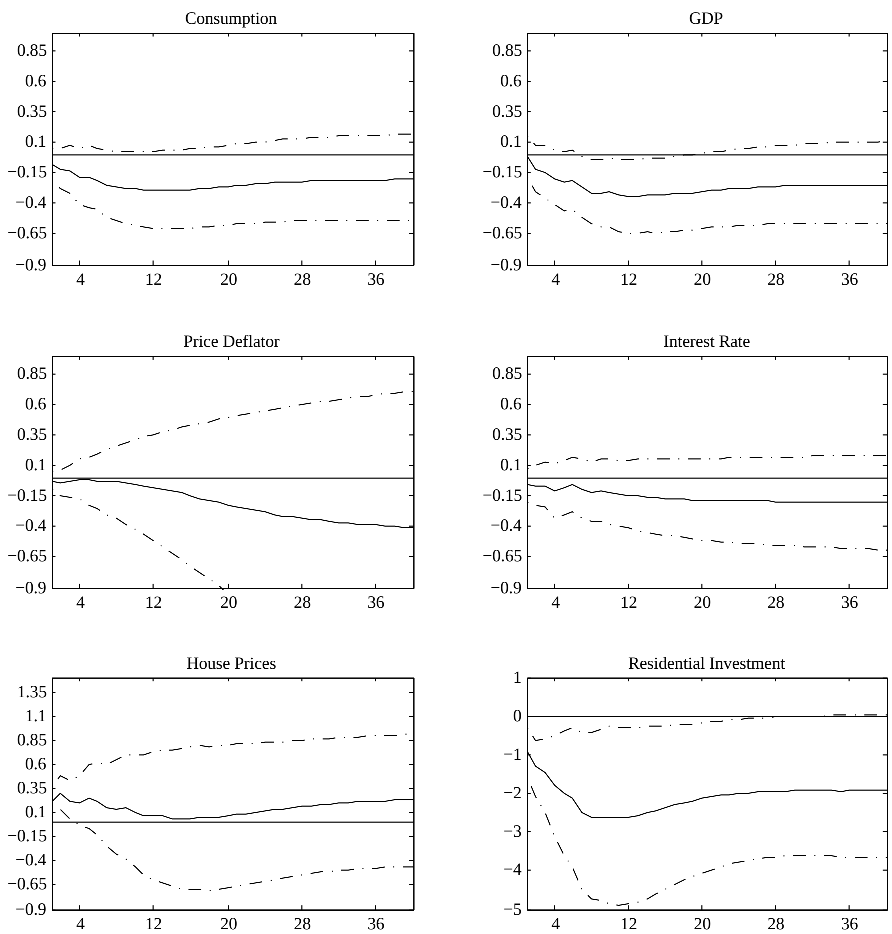
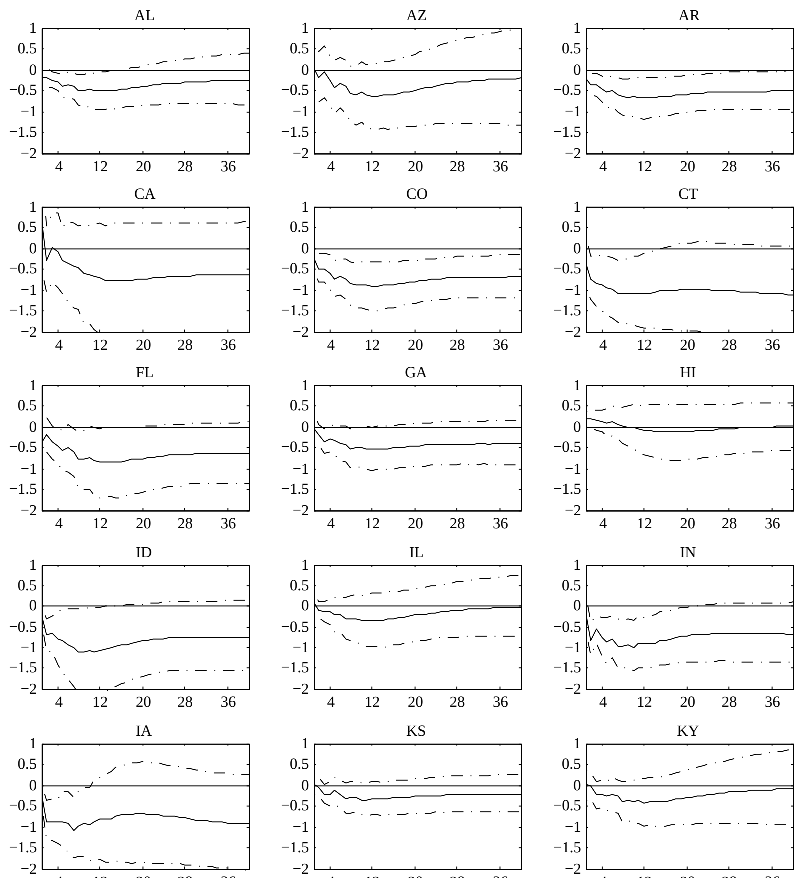
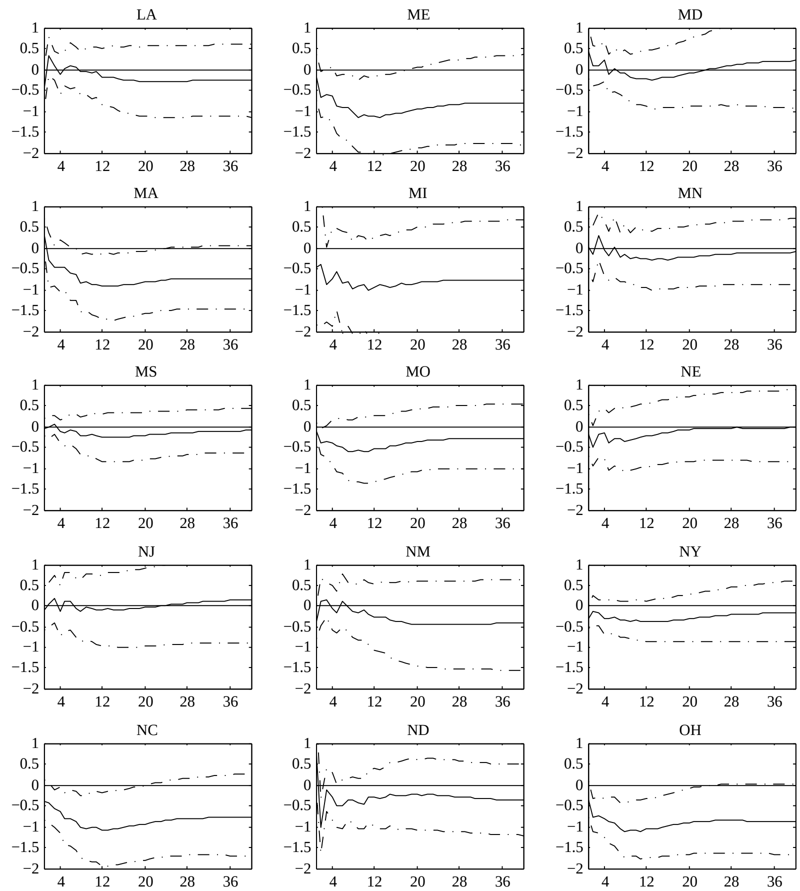


Figure 2.6: DYNAMIC RESPONSES OF AGGREGATE VARIABLES TO HOUSING SUPPLY SHOCKS: Solid curve is the median response over the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile bands.



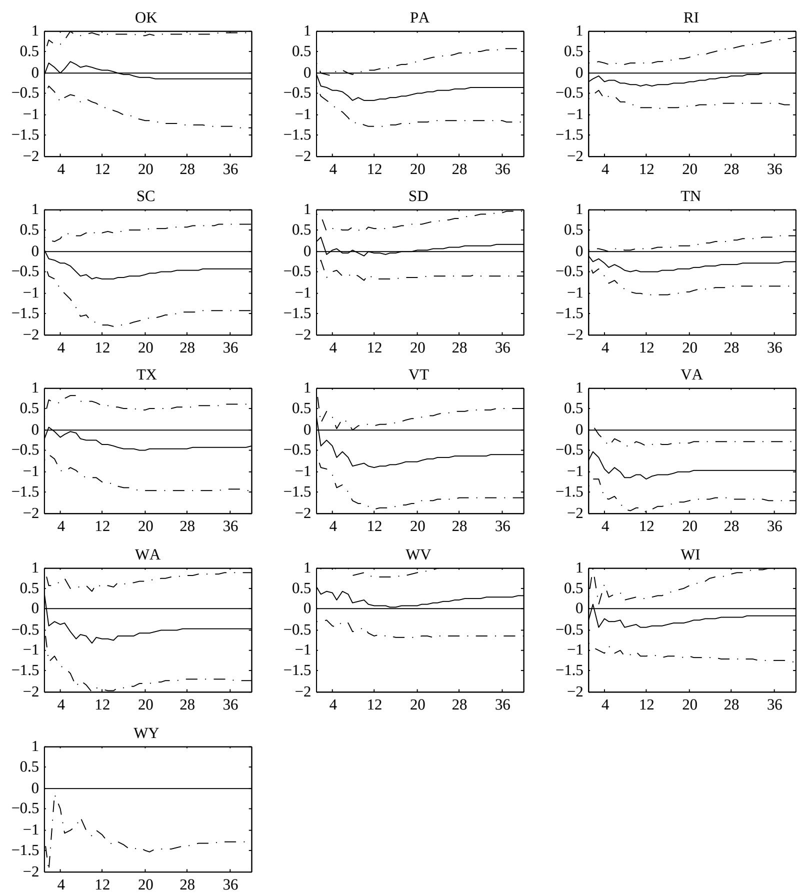
(a)

Figure 2.7: Dynamic responses of state-level retail sales to Aggregate Housing Demand Shocks: Solid curve is the median of the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile.



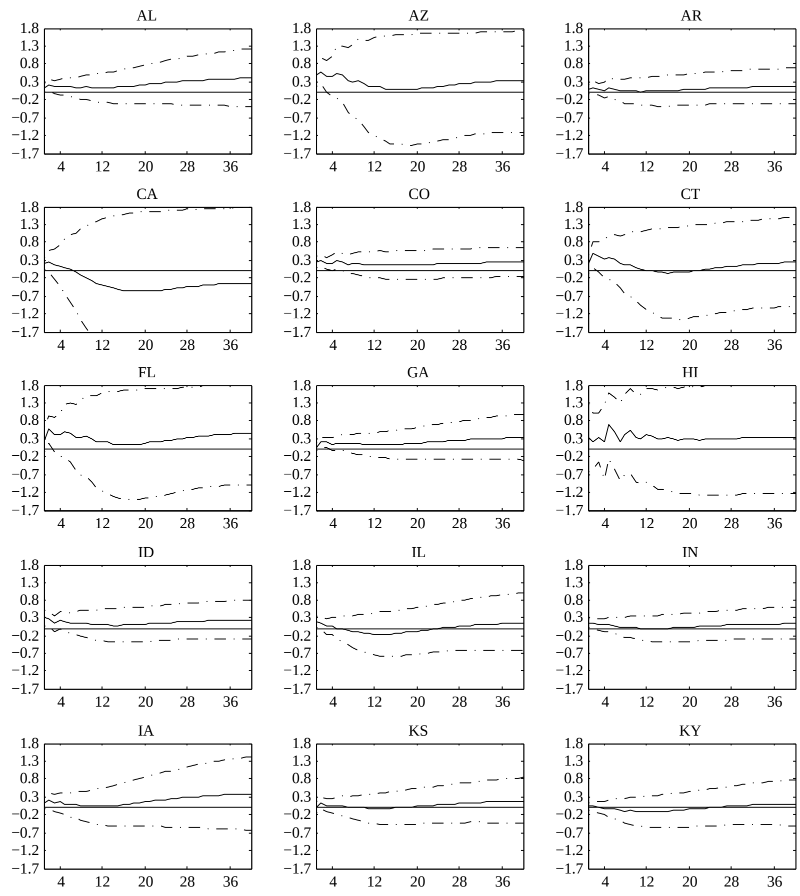
(b)

Figure 2.7: (cont'd)



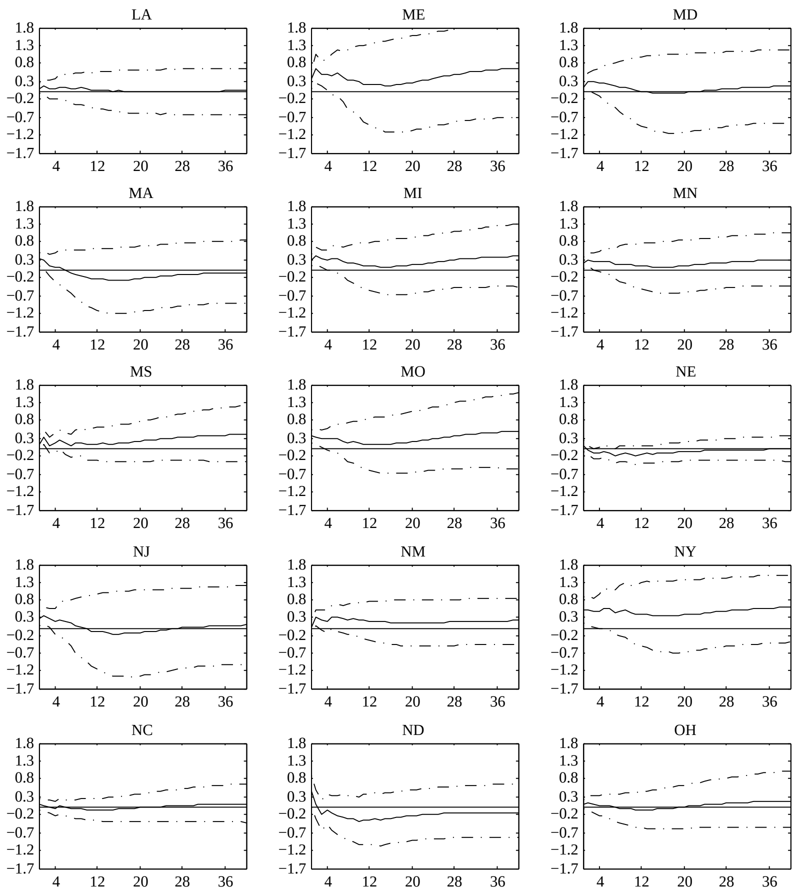
(c)

Figure 2.7: (cont'd)



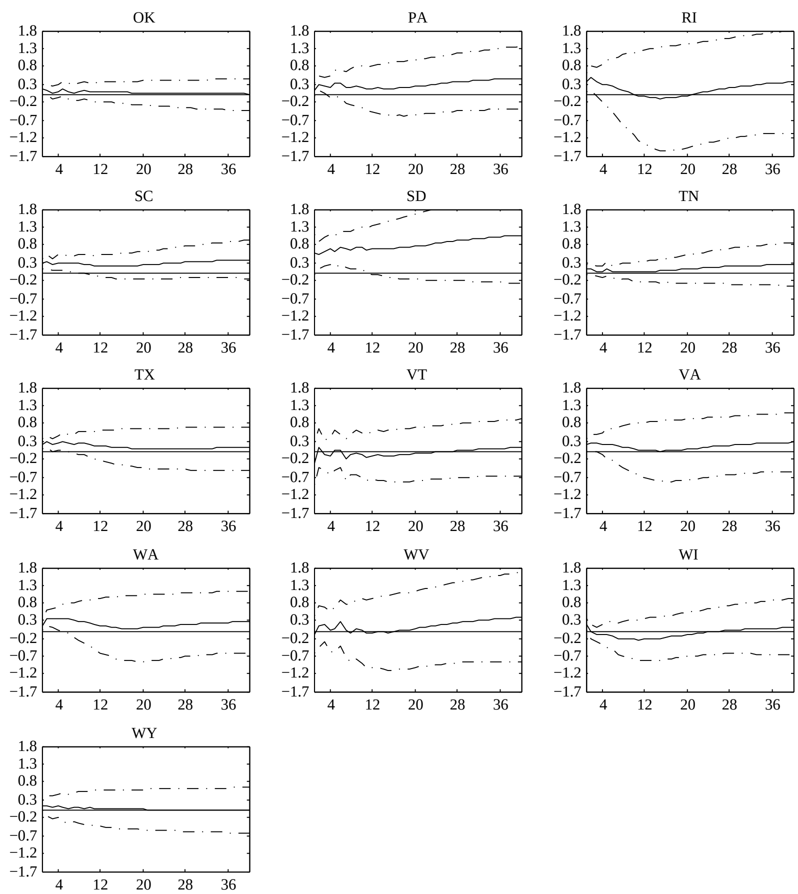
(a)

Figure 2.8: Dynamic responses of state-level house prices to Aggregate Housing Demand Shocks: Solid curve is the median of the posterior distribution. Dashed curves are 84^{th} and 16^{th} percentile bands.



(b)

Figure 2.8: (cont'd)



(c)

Figure 2.8: (cont'd)

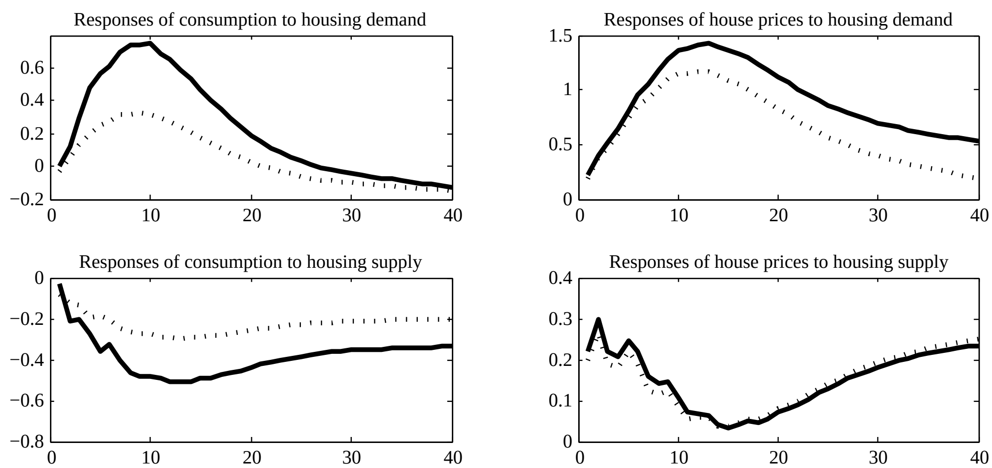


Figure 2.9: Cross-state averages of dynamic responses: Solid curve is the average across states of the dynamic responses, and dotted line represents the dynamic response of the corresponding aggregate. Results are presented for responses to both, aggregate housing demand shocks (top panel) and aggregate housing supply shocks (bottom panel).

Table 2.1: Dynamic responses of retail sales to aggregate housing demand shocks

	State	$r_m(rank)$	$r_4(rank)$	$r_8(rank)$	$r_{20}(rank)$	$r_{28}(rank)$	$r_{40}(rank)$	$r_{40-28}(rank)$
1	Alabama	0.508 (32)	0.216 (23)	0.350 (24)	0.266 (26)	0.143 (26)	0.019 (23)	-0.125 (23)
2	Arizona	0.939 (16)	0.060 (32)	0.410 (20)	0.447 (18)	0.229 (21)	-0.026 (26)	-0.254 (32)
3	Arkansas	0.710 (20)	0.287 (19)	0.478 (15)	0.375 (19)	0.237 (20)	0.116 (19)	-0.121 (22)
4	California	2.975 (1)	0.770 (2)	1.426 (1)	2.267 (1)	2.317 (1)	2.302 (1)	-0.015 (7)
5	Colorado	0.556 (30)	0.069 (31)	0.277 (27)	0.280 (24)	0.187 (23)	0.103 (20)	-0.084 (14)
6	Connecticut	0.151 (40)	-0.024 (34)	0.018 (36)	-0.314 (38)	-0.406 (37)	-0.401 (35)	0.005 (6)
7	Florida	1.366 (7)	0.590 (8)	0.900 (7)	0.976 (4)	0.883 (3)	0.800 (5)	-0.083 (13)
8	Georgia	0.501 (33)	0.282 (20)	0.382 (23)	0.276 (25)	0.140 (27)	-0.011 (25)	-0.151 (24)
9	Hawaii	1.124 (15)	0.250 (21)	0.473 (17)	0.828 (6)	0.841 (4)	0.804 (4)	-0.037 (9)
10	Idaho	1.173 (10)	0.717 (4)	0.936 (5)	0.651 (12)	0.434 (12)	0.247 (15)	-0.187 (27)
11	Illinois	1.148 (12)	0.531 (10)	0.795 (11)	0.755 (8)	0.543 (10)	0.283 (14)	-0.260 (35)
12	Indiana	0.603 (29)	0.312 (18)	0.340 (25)	-0.416 (39)	-0.783 (41)	-1.103 (42)	-0.320 (40)
13	Iowa	0.346 (38)	0.209 (24)	0.088 (34)	-0.667 (42)	-0.878 (42)	-0.970 (41)	-0.092 (16)
14	Kansas	0.657 (24)	0.330 (15)	0.474 (16)	0.478 (17)	0.402 (14)	0.316 (13)	-0.086 (15)
15	Kentucky	0.328 (39)	0.076 (30)	0.169 (31)	-0.104 (35)	-0.398 (36)	-0.749 (39)	-0.351 (41)
16	Louisiana	0.611 (28)	-0.081 (35)	0.074 (35)	0.364 (20)	0.389 (15)	0.363 (12)	-0.026 (8)
17	Maine	0.628 (26)	0.233 (22)	0.395 (21)	0.144 (31)	-0.076 (34)	-0.281 (32)	-0.205 (28)
18	Maryland	1.314 (8)	0.696 (6)	0.951 (4)	0.869 (5)	0.648 (9)	0.392 (10)	-0.256 (33)
19	Massachusetts	1.572 (5)	0.413 (12)	0.839 (9)	1.048 (3)	0.825 (6)	0.519 (8)	-0.305 (38)
20	Michigan	0.703 (21)	0.047 (33)	-0.011 (37)	-0.124 (36)	-0.033 (30)	0.140 (17)	0.174 (2)
21	Minnesota	1.135 (13)	0.577 (9)	0.819 (10)	0.783 (7)	0.655 (8)	0.544 (6)	-0.111 (19)
22	Mississippi	0.847 (18)	0.200 (26)	0.442 (19)	0.566 (14)	0.476 (11)	0.367 (11)	-0.109 (18)
23	Missouri	1.130 (14)	0.729 (3)	0.910 (6)	0.522 (16)	0.264 (19)	0.023 (22)	-0.241 (31)
24	Nebraska	0.454 (34)	0.330 (14)	0.195 (29)	-0.517 (41)	-0.726 (40)	-0.846 (40)	-0.120 (21)
25	New Jersey	1.205 (9)	0.323 (16)	0.664 (12)	0.713 (10)	0.404 (13)	-0.002 (24)	-0.406 (42)
26	New Mexico	1.419 (6)	0.151 (27)	0.318 (26)	0.557 (15)	0.692 (7)	0.877 (3)	0.186 (1)
27	New York	-0.065 (41)	-0.199 (38)	-0.146 (38)	-0.300 (37)	-0.459 (38)	-0.646 (38)	-0.187 (26)
28	North Carolina	0.864 (17)	0.479 (11)	0.650 (13)	0.229 (28)	-0.063 (32)	-0.328 (34)	-0.265 (36)
29	North Dakota	-0.782 (43)	-0.938 (43)	-1.207 (43)	-1.621 (43)	-1.629 (43)	-1.576 (43)	0.053 (4)
30	Ohio	0.677 (23)	0.323 (17)	0.465 (18)	0.106 (32)	-0.074 (33)	-0.188 (30)	-0.114 (20)
31	Oklahoma	0.686 (22)	-0.400 (39)	-0.260 (40)	0.208 (30)	0.337 (17)	0.417 (9)	0.080 (3)
32	Pennsylvania	0.766 (19)	0.405 (13)	0.577 (14)	0.318 (22)	0.034 (29)	-0.282 (33)	-0.316 (39)
33	Rhode Island	0.653 (25)	-0.175 (37)	0.090 (33)	0.319 (21)	0.176 (25)	-0.055 (27)	-0.231 (30)
34	South Carolina	1.581 (4)	0.710 (5)	1.081 (3)	1.089 (2)	0.839 (5)	0.537 (7)	-0.302 (37)
35	South Dakota	0.440 (36)	0.134 (28)	0.174 (30)	0.300 (23)	0.297 (18)	0.240 (16)	-0.056 (11)
36	Tennessee	0.452 (35)	0.130 (29)	0.271 (28)	0.223 (29)	0.087 (28)	-0.072 (28)	-0.159 (25)
37	Texas	-0.379 (42)	-0.651 (41)	-0.647 (42)	-0.510 (40)	-0.515 (39)	-0.558 (37)	-0.043 (10)
38	Vermont	1.168 (11)	0.617 (7)	0.856 (8)	0.590 (13)	0.338 (16)	0.123 (18)	-0.216 (29)
39	Virginia	0.611 (27)	0.207 (25)	0.391 (22)	0.090 (33)	-0.051 (31)	-0.125 (29)	-0.074 (12)
40	Washington	1.604 (3)	1.181 (1)	1.293 (2)	0.664 (11)	0.204 (22)	-0.268 (31)	-0.472 (43)
41	West Virginia	0.515 (31)	-0.081 (36)	0.096 (32)	0.259 (27)	0.185 (24)	0.091 (21)	-0.094 (17)
42	Wisconsin	0.366 (37)	-0.501 (40)	-0.244 (39)	-0.033 (34)	-0.197 (35)	-0.454 (36)	-0.257 (34)
43	Wyoming	1.908 (2)	-0.877 (42)	-0.509 (41)	0.743 (9)	0.988 (2)	1.027 (2)	0.039 (5)
	μ	0.818	0.201	0.373	0.319	0.184	0.040	-0.144
	σ	0.622	0.431	0.508	0.603	0.625	0.647	0.144

Notes: r_m is the maximum response over the first 10 years, r_4 is the average response over the first year, r_8 is the average response over the first 2 years, r_{20} is the average response over the first 5 years, r_{28} is the average response over the first 7 years, and r_{40} is the average response over the first 10 years. r_{40-28} is the average response over years 7 to 10. μ is the sample mean and σ is the sample standard deviation.

Table 2.2: DYNAMIC RESPONSES OF HOUSE PRICES TO AGGREGATE HOUSING DEMAND SHOCKS

	State	$r_m(rank)$	$r_4(rank)$	$r_8(rank)$	$r_{20}(rank)$	$r_{28}(rank)$	$r_{40}(rank)$	$r_{40-28}(rank)$
1	Alabama	0.445 (39)	0.282 (33)	0.350 (39)	0.141 (40)	-0.091 (42)	-0.381 (42)	-0.290 (41)
2	Arizona	3.074 (2)	0.939 (3)	1.527 (3)	2.366 (3)	2.372 (3)	2.251 (3)	-0.121 (11)
3	Arkansas	0.615 (34)	0.278 (34)	0.412 (32)	0.414 (34)	0.277 (33)	0.094 (32)	-0.183 (21)
4	California	3.583 (1)	0.974 (2)	1.592 (1)	2.682 (1)	2.868 (1)	2.954 (1)	0.086 (1)
5	Colorado	0.937 (26)	0.312 (30)	0.479 (28)	0.719 (25)	0.705 (24)	0.647 (21)	-0.058 (8)
6	Connecticut	2.087 (6)	0.792 (6)	1.209 (5)	1.617 (6)	1.500 (6)	1.303 (6)	-0.197 (28)
7	Florida	3.035 (3)	1.035 (1)	1.578 (2)	2.367 (2)	2.374 (2)	2.261 (2)	-0.113 (10)
8	Georgia	0.203 (43)	0.092 (41)	0.139 (41)	0.059 (42)	-0.090 (41)	-0.294 (40)	-0.204 (31)
9	Hawaii	2.559 (5)	0.639 (9)	1.187 (7)	1.935 (5)	1.946 (5)	1.809 (5)	-0.137 (14)
10	Idaho	0.982 (24)	0.576 (11)	0.720 (17)	0.807 (20)	0.721 (21)	0.594 (22)	-0.127 (13)
11	Illinois	0.894 (28)	0.397 (21)	0.580 (22)	0.640 (28)	0.496 (27)	0.295 (27)	-0.201 (29)
12	Indiana	0.535 (36)	0.246 (38)	0.356 (38)	0.368 (35)	0.258 (34)	0.111 (31)	-0.147 (16)
13	Iowa	0.556 (35)	0.397 (22)	0.468 (30)	0.355 (36)	0.189 (36)	-0.020 (35)	-0.209 (32)
14	Kansas	0.474 (38)	0.291 (31)	0.373 (35)	0.282 (38)	0.134 (37)	-0.056 (36)	-0.190 (25)
15	Kentucky	0.498 (37)	0.248 (37)	0.357 (37)	0.283 (37)	0.132 (38)	-0.058 (38)	-0.190 (26)
16	Louisiana	1.038 (23)	0.336 (26)	0.498 (26)	0.793 (21)	0.845 (16)	0.863 (13)	0.019 (4)
17	Maine	3.020 (4)	0.711 (7)	1.270 (4)	2.214 (4)	2.186 (4)	1.982 (4)	-0.204 (30)
18	Maryland	1.682 (9)	0.539 (12)	0.853 (10)	1.302 (8)	1.297 (8)	1.223 (7)	-0.074 (9)
19	Massachusetts	1.395 (13)	0.491 (14)	0.771 (13)	1.051 (12)	0.948 (12)	0.770 (17)	-0.177 (20)
20	Michigan	1.362 (14)	0.359 (23)	0.666 (20)	0.985 (15)	0.873 (15)	0.679 (20)	-0.194 (27)
21	Minnesota	1.284 (15)	0.511 (13)	0.759 (14)	1.002 (14)	0.922 (13)	0.785 (16)	-0.138 (15)
22	Mississippi	1.049 (21)	0.614 (10)	0.801 (12)	0.777 (22)	0.572 (26)	0.291 (28)	-0.282 (39)
23	Missouri	1.160 (18)	0.487 (15)	0.714 (18)	0.869 (17)	0.715 (23)	0.485 (24)	-0.230 (33)
24	Nebraska	0.631 (33)	0.274 (36)	0.418 (31)	0.427 (33)	0.309 (31)	0.158 (29)	-0.151 (17)
25	New Jersey	1.780 (8)	0.437 (18)	0.809 (11)	1.302 (9)	1.219 (9)	1.032 (10)	-0.187 (23)
26	New Mexico	1.059 (20)	0.315 (29)	0.508 (25)	0.811 (19)	0.828 (18)	0.795 (14)	-0.033 (6)
27	New York	1.490 (12)	0.320 (28)	0.595 (21)	1.053 (11)	0.975 (11)	0.790 (15)	-0.185 (22)
28	North Carolina	0.273 (41)	0.026 (43)	0.118 (43)	0.116 (41)	-0.006 (39)	-0.174 (39)	-0.168 (19)
29	North Dakota	0.956 (25)	0.831 (5)	0.855 (9)	0.486 (31)	0.228 (35)	-0.058 (37)	-0.286 (40)
30	Ohio	0.698 (32)	0.331 (27)	0.481 (27)	0.476 (32)	0.331 (30)	0.141 (30)	-0.190 (24)
31	Oklahoma	0.859 (29)	0.345 (25)	0.476 (29)	0.690 (26)	0.721 (22)	0.723 (18)	0.001 (5)
32	Pennsylvania	1.190 (17)	0.410 (20)	0.676 (19)	0.855 (18)	0.690 (25)	0.444 (25)	-0.246 (36)
33	Rhode Island	1.929 (7)	0.709 (8)	1.093 (8)	1.471 (7)	1.317 (7)	1.060 (9)	-0.257 (37)
34	South Carolina	0.729 (31)	0.214 (39)	0.360 (36)	0.527 (29)	0.459 (28)	0.337 (26)	-0.122 (12)
35	South Dakota	0.926 (27)	0.412 (19)	0.562 (23)	0.641 (27)	0.425 (29)	0.084 (33)	-0.341 (43)
36	Tennessee	0.220 (42)	0.063 (42)	0.133 (42)	0.038 (43)	-0.146 (43)	-0.382 (43)	-0.236 (34)
37	Texas	1.099 (19)	0.285 (32)	0.404 (33)	0.742 (23)	0.838 (17)	0.894 (11)	0.056 (3)
38	Vermont	0.403 (40)	0.152 (40)	0.265 (40)	0.158 (39)	-0.069 (40)	-0.370 (41)	-0.300 (42)
39	Virginia	1.281 (16)	0.476 (16)	0.733 (16)	0.976 (16)	0.879 (14)	0.721 (19)	-0.159 (18)
40	Washington	1.540 (11)	0.456 (17)	0.734 (15)	1.172 (10)	1.177 (10)	1.121 (8)	-0.056 (7)
41	West Virginia	1.580 (10)	0.922 (4)	1.209 (6)	1.043 (13)	0.794 (20)	0.535 (23)	-0.259 (38)
42	Wisconsin	0.779 (30)	0.349 (24)	0.530 (24)	0.488 (30)	0.297 (32)	0.058 (34)	-0.239 (35)
43	Wyoming	1.045 (22)	0.277 (35)	0.403 (34)	0.722 (24)	0.810 (19)	0.867 (12)	0.056 (2)
	μ	1.231	0.445	0.675	0.889	0.796	0.636	-0.160
	σ	0.813	0.246	0.377	0.647	0.708	0.756	0.100

Notes: r_m is the maximum response over the first 10 years, r_4 is the average response over the first year, r_8 is the average response over the first 2 years, r_{20} is the average response over the first 5 years, r_{28} is the average response over the first 7 years, and r_{40} is the average response over the first 10 years. r_{40-28} is the average response over years 7 to 10. μ is the sample mean and σ is the sample standard deviation.

Table 2.3: Mortgage Market Index (MMI) and Components

	State	MMI	rank	FC	$TERM$	HP	LTV	ARM	MEW
1	Alabama	0.405	33	0.881	0.916	0.480	0.995	0.378	0.542
2	Arizona	0.486	19	0.932	0.982	0.591	0.953	0.570	0.753
3	Arkansas	0.382	37	0.859	0.912	0.410	0.984	0.580	0.267
4	California	0.694	1	0.656	1.000	0.988	0.925	1.000	0.909
5	Colorado	0.570	5	0.732	0.964	0.676	0.942	0.708	0.863
6	Connecticut	0.611	3	0.743	0.977	0.810	0.900	0.728	0.995
7	Florida	0.499	17	0.836	0.959	0.553	0.944	0.684	0.691
8	Georgia	0.465	26	0.971	0.946	0.557	0.988	0.562	0.706
9	Hawaii	0.588	4	0.962	0.982	1.000	0.921	0.827	0.760
10	Idaho	0.450	28	0.828	0.944	0.520	0.933	0.386	0.744
11	Illinois	0.527	12	0.737	0.911	0.608	0.921	0.673	0.786
12	Indiana	0.470	24	0.742	0.853	0.399	0.950	0.594	0.768
13	Iowa	0.435	30	0.688	0.854	0.381	0.958	0.512	0.594
14	Kansas	0.472	22	0.707	0.941	0.456	0.992	0.616	0.536
15	Kentucky	0.472	23	0.716	0.884	0.437	0.954	0.601	0.673
16	Louisiana	0.346	41	1.000	0.937	0.484	0.989	0.296	0.368
17	Maine	0.486	20	0.793	0.921	0.502	0.916	0.514	0.854
18	Maryland	0.549	8	0.773	0.930	0.787	0.933	0.463	0.951
19	Massachusetts	0.622	2	0.626	0.961	0.779	0.916	0.699	1.000
20	Michigan	0.492	18	0.773	0.910	0.483	0.943	0.485	0.902
21	Minnesota	0.503	16	0.916	0.961	0.586	0.962	0.486	0.938
22	Mississippi	0.358	40	0.855	0.860	0.403	0.990	0.434	0.317
23	Missouri	0.484	21	0.632	0.909	0.450	0.963	0.642	0.572
24	Nebraska	0.400	35	0.934	0.934	0.405	1.000	0.398	0.595
25	New Jersey	0.560	6	0.794	0.950	0.819	0.882	0.541	0.962
26	New Mexico	0.390	36	0.869	0.949	0.480	0.993	0.390	0.394
27	New York	0.523	13	0.690	0.914	0.718	0.895	0.540	0.762
28	North Carolina	0.514	15	0.708	0.917	0.538	0.963	0.479	0.897
29	North Dakota	0.361	39	0.924	0.889	0.410	0.948	0.406	0.435
30	Ohio	0.461	27	0.948	0.919	0.495	0.948	0.423	0.928
31	Oklahoma	0.370	38	0.837	0.918	0.417	0.995	0.406	0.321
32	Pennsylvania	0.436	29	0.900	0.885	0.521	0.938	0.389	0.780
33	Rhode Island	0.549	7	0.698	0.949	0.648	0.941	0.498	0.954
34	South Carolina	0.468	25	0.816	0.936	0.498	0.982	0.533	0.674
35	South Dakota	0.423	32	0.786	0.864	0.425	0.949	0.549	0.535
36	Tennessee	0.425	31	0.792	0.878	0.491	0.968	0.472	0.533
37	Texas	0.343	42	0.930	0.933	0.508	0.998	0.318	0.228
38	Vermont	0.516	14	0.614	0.942	0.493	0.938	0.468	0.869
39	Virginia	0.536	10	0.934	0.966	0.764	0.953	0.530	0.939
40	Washington	0.540	9	0.837	0.963	0.694	0.933	0.634	0.850
41	West Virginia	0.403	34	0.709	0.810	0.429	0.939	0.526	0.422
42	Wisconsin	0.531	11	0.629	0.902	0.466	0.948	0.604	0.893
43	Wyoming	0.327	43	0.801	0.811	0.456	0.908	0.172	0.418
	μ	0.475		0.803	0.922	0.558	0.951	0.528	0.695
	σ	0.082		0.107	0.043	0.157	0.030	0.147	0.224

Notes: Our constructed Mortgage Market Index (MMI), the explanatory variable in the cross-state regressions. The components are as follows. FC denotes initial fees and charges associated with a home mortgage loan. $TERM$ refers to the term of the mortgage loan. HP refers to the purchase price of a home. LTV is the loan to value ratio. ARM denotes the percentage of adjustable rate home loans. The housing collateral variable MEW refers to the percentage of households with home equity loans. All variables are time averages. We divide variables by the corresponding maximum values across states, so that they are on a scale of 0 to 1. The index (MMI) is a simple arithmetic average of all the components. μ is the mean and σ is the standard deviation. Data sources are found in the Appendix.

Table 2.4: Cross-state regressions: retail sales

y	$\beta_0(tstat)$	$\beta_1(tstat)$	$\beta_2(tstat)$	$\beta_3(tstat)$	$\beta_4(tstat)$	R^2
Panel A: all states; no regional dummies						
r_m	-0.662 (-0.82)	3.114 (1.84)				0.149
r_4	-0.961 (-2.58)	2.445 (3.21)				0.199
r_8	-1.213 (-2.85)	3.338 (3.80)				0.274
r_{20}	-1.216 (-1.74)	3.228 (2.21)				0.174
r_{28}	-1.079 (-1.36)	2.658 (1.59)				0.101
r_{40}	-0.944 (-1.09)	2.070 (1.14)				0.047
r_{40-28}	0.136 (1.01)	-0.588 (-1.99)				0.092
Panel B: all states; regional dummies						
r_m	-0.799 (-1.13)	2.963 (2.03)	0.209 (1.28)	0.764 (2.92)	-0.034 (-0.16)	0.316
r_4	-1.206 (-2.81)	2.979 (3.39)	0.086 (0.63)	-0.010 (-0.05)	-0.197 (-1.05)	0.183
r_8	-1.551 (-3.39)	3.853 (4.29)	0.228 (1.41)	0.181 (0.917)	-0.109 (-0.524)	0.288
r_{20}	-1.646 (-2.62)	3.476 (2.82)	0.477 (2.42)	0.693 (3.22)	0.092 (0.42)	0.334
r_{28}	-1.507 (-2.15)	2.866 (2.04)	0.481 (2.34)	0.795(3.09)	0.071 (0.33)	0.294
r_{40}	-1.361 (-1.82)	2.294 (1.51)	0.446 (2.10)	0.835 (2.82)	-0.000 (-0.00)	0.248
r_{40-28}	0.146 (1.11)	-0.572 (-2.03)	-0.035 (-0.81)	0.040 (0.55)	-0.071 (-1.20)	0.094
Panel C: California omitted; no regional dummies						
r_m	-0.036 (-0.05)	1.708 (1.19)				0.036
r_4	-0.946 (-2.16)	2.413 (2.63)				0.163
r_8	-1.077 (-2.23)	3.03 (3.01)				0.203
r_{20}	-0.689 (-1.11)	2.044 (1.62)				0.063
r_{28}	-0.421 (-0.64)	0.180 (0.89)				0.004
r_{40}	-0.177 (-0.27)	0.346 (0.26)				-0.023
r_{40-28}	0.245 (1.99)	-0.834 (-3.05)				0.173
Panel D: California omitted: regional dummies						
r_m	-0.269 (-0.39)	1.818 (1.29)	0.178 (1.02)	0.645 (2.71)	0.051 (0.24)	0.154
r_4	-1.252 (-2.48)	3.077 (2.94)	0.089 (0.64)	0.001 (0.00)	-0.204 (-1.05)	0.146
r_8	-1.484 (-2.70)	3.708 (3.37)	0.224 (1.37)	0.166 (0.79)	-0.098 (-0.47)	0.205
r_{20}	-1.197 (-1.87)	2.507 (2.05)	0.450 (2.19)	0.593 (2.96)	0.164 (0.78)	0.193
r_{28}	-0.929 (-1.39)	1.618 (1.27)	0.447 (2.08)	0.666 (2.88)	0.164 (0.79)	0.154
r_{40}	-0.674 (-1.02)	0.810 (0.63)	0.405 (1.84)	0.681 (2.58)	0.110 (0.53)	0.118
r_{40-28}	0.255 (1.89)	-0.807 (-2.76)	-0.042 (-0.98)	0.016 (0.21)	-0.053 (-0.88)	0.144

Panel E: Spearman rank correlation

y	ρ	p value
r_m	0.277	0.073
r_4	0.363	0.017
r_8	0.432	0.004
r_{20}	0.313	0.042
r_{28}	0.160	0.306
r_{40}	0.059	0.707
r_{40-28}	-0.340	0.026

Notes: The regression in Panels A through D is $y_i = \beta_0 + \beta_1 MMI_i + \beta_2 south_i + \beta_3 west_i + \beta_4 ne_i + \epsilon_i$ where y is the estimated retail sales response to housing demand shocks, MMI is the mortgage market index, and *south*, *west*, and *northeast* are regional dummies according to the census classification (with the midwest omitted). The adjusted R^2 is reported for each regression. The OLS t-statistics are computed from White's consistent estimator of the covariance matrix allowing for heteroscedasticity.

Table 2.5: Cross-state regressions: house prices

y	$\beta_0(tstat)$	$\beta_1(tstat)$	$\beta_2(tstat)$	$\beta_3(tstat)$	$\beta_4(tstat)$	R^2
Panel A: all states; no regional dummies						
r_m	-1.037 (-1.77)	4.772 (3.70)				0.214
r_4	0.042 (0.20)	0.850 (2.00)				0.058
r_8	-0.253 (-0.97)	1.953 (3.47)				0.162
r_{20}	-0.950 (-2.15)	3.869 (3.98)				0.224
r_{28}	-1.141 (-2.07)	4.074 (3.41)				0.205
r_{40}	-1.304 (-1.95)	4.083 (2.84)				0.178
r_{40-28}	-0.164 (-1.16)	0.009 (0.03)				-0.024
Panel B: all states; regional dummies						
r_m	-0.793 (-1.23)	3.558 (2.59)	0.217 (1.01)	0.842 (3.00)	0.542 (1.77)	0.293
r_4	0.107 (0.43)	0.647 (1.32)	-0.005 (-0.06)	0.127 (1.45)	0.048 (0.56)	0.025
r_8	-0.147 (-0.47)	1.534 (2.35)	0.040 (0.33)	0.265 (1.99)	0.158 (1.22)	0.163
r_{20}	-0.775 (-1.64)	2.935 (2.89)	0.180 (1.04)	0.692 (3.28)	0.411 (1.72)	0.309
r_{28}	-0.997 (-1.89)	3.085 (2.70)	0.244 (1.31)	0.866 (3.92)	0.434 (1.67)	0.327
r_{40}	-1.219 (-2.09)	3.110 (2.46)	0.307 (1.54)	1.026 (4.58)	0.422 (1.56)	0.338
r_{40-28}	-0.221 (-2.21)	0.025 (0.12)	0.063 (2.12)	0.160 (5.06)	-0.011 (-0.45)	0.311
Panel C: California omitted; no regional dummies						
r_m	-0.482 (-1.17)	3.526 (3.75)				0.111
r_4	0.187 (0.97)	0.523 (1.30)				0.004
r_8	-0.046 (-0.20)	1.486 (2.93)				0.079
r_{20}	-0.549 (-1.65)	2.968 (3.94)				0.124
r_{28}	-0.640 (-1.59)	2.950 (3.33)				0.101
r_{40}	-0.700 (-1.44)	2.726 (2.62)				0.072
r_{40-28}	-0.060 (-0.49)	-0.224 (-0.93)				0.008
Panel D: California omitted; regional dummies						
r_m	-0.201 (-0.34)	2.280 (1.79)	0.182 (0.85)	0.709 (2.46)	0.637 (2.10)	0.189
r_4	0.269 (1.06)	0.297 (0.59)	-0.015 (-0.16)	0.091 (1.00)	0.074 (0.88)	-0.043
r_8	0.081 (0.25)	1.043 (1.55)	0.027 (0.22)	0.214 (1.53)	0.195 (1.50)	0.070
r_{20}	-0.371 (-0.80)	2.063 (2.06)	0.156 (0.90)	0.602 (2.74)	0.476 (1.99)	0.205
r_{28}	-0.520 (-1.05)	2.055 (1.91)	0.215 (1.15)	0.759 (3.39)	0.511 (1.96)	0.216
r_{40}	-0.669 (-1.27)	1.924 (1.68)	0.275 (1.37)	0.903 (4.14)	0.511 (1.89)	0.221
r_{40-28}	-0.149 (-1.50)	-0.131 (-0.64)	0.059 (1.97)	0.144 (5.21)	0.000 (0.01)	0.264

Panel E: Spearman rank correlations

y	ρ	p $value$
r_m	0.438	0.004
r_4	0.323	0.035
r_8	0.412	0.006
r_{20}	0.475	0.001
r_{28}	0.460	0.002
r_{40}	0.373	0.014
r_{40-28}	0.067	0.670

Notes: The regression in Panels A through D is $y_i = \beta_0 + \beta_1 MMI_i + \beta_2 south_i + \beta_3 west_i + \beta_4 ne_i + \epsilon_i$ where y is the estimated house price response to housing demand shocks, MMI is the mortgage market index, and *south*, *west*, and *northeast* are regional dummies according to the census classification (with the midwest omitted). The adjusted R^2 is reported for each regression. The OLS t-statistics are computed from White's consistent estimator of the covariance matrix allowing for heteroscedasticity.

Chapter 3

Home Equity Borrowing and Households' Spending: Case Study of a Credit Reform in Texas

3.1 Introduction

Issues concerning credit and consumption continue to generate much current research, and are highlighted by the most recent financial crisis and recession of 2007 through 2009. Several commentators have argued that the availability of credit played an important role and has at least partly caused the drop in consumption observed in the data. When markets are incomplete and households face binding constraints on their ability to borrow on future income, welfare losses arise because of the reduced capacity to smooth consumption in the face of income shocks.

Given that the shadow value of the credit constraint is not observed, testing for credit constraints is inherently difficult, and in many cases empirical tests have low power because they usually confuse credit demand and supply (Leth-Petersen 2010). Ideally, testing for the presence of credit constraints require data on a key variable (e.g., household spending) before and after a truly exogenous increase (decrease) in the availability of credit caused, for example, by a policy intervention that expands (contracts) access to credit, and thus relaxes (tightens) credit constraints. A home equity loan is a form of collateralized debt in the sense that it allows a household to secure debt based on the value of the equity in its housing asset.[1] Changes in the legal codes on the ability of households to tap into their

[1]In most economies, housing equity represents the major form of collateral. In the "US", when house prices were at their peak in 2005, home mortgage borrowing by households was \$ 1,033.4 billion, and residential investment was \$ 681.9 billion. At a 74.7 % loan-to-value ratio, the implied borrowing on the construction amount is \$ 509.4, leaving \$ 524 billion of the mortgage borrowing

home equity is potentially informative: adding (removing) restrictions in such legal codes tightens (relaxes) credit constraints of households and thus (reduces) increases their desired borrowing.

A major obstacle confronting studies of housing collateralized constraints and their effects on household spending is, at least for the US, the near-universality of lax home equity lending laws. In one state only, however, homeowners once faced severe legal restrictions on their ability to borrow using home equity as collateral for consumption loans. Prior to 1998, the constitution in Texas prohibited the use of home equity as collateral on loans except for expenditures related primarily to home improvement and tax payments. But in November 1997, a constitutional amendment in Texas passed, allowing home equity lending for general spending purposes. When the Texas amendment passed, no other US state imposed severe restrictions on the access to home equity for discretionary household spending.

In this paper, I exploit the passage of this constitutional amendment, which made it easier for Texans to borrow against the equity in their homes and use the proceeds to finance non-housing expenditures, to quantify the importance of credit constraints. If credit constraints bind, then the increase in credit availability induced by the amendment will increase household spending. As in a conventional 'natural experiment' trends in other US states can be used to infer what would have happened to household spending in Texas in the absence of the change in the home equity lending law. Comparisons of spending in Texas and control states – where there was no changes in home equity laws – provide simple estimates of the importance of collateralized credit constraints.

Theoretical arguments underlying the existence of credit or borrowing constraints are simple and compelling, but there is continuing controversy over their importance for consumption behavior, despite numerous studies on the subject. Existing work on the importance of credit constraints has mainly focused on individual-level analysis. Gross and Souleles

available to be spent on other things, that is, a "cash-out" or withdrawal of home equity. Recent surveys show that a sizeable portion of this equity extraction has been used to finance expenditures on non-housing goods and services (Greenspan and Kennedy 2005).

(2002) use data on credit card limits and debt and find that agents respond to an increase in the availability of credit by spending more. Along similar lines, Alessie, Hochguertel and Weber (2005) use data on credit card debt, and exploit a law in Italy that introduced a cap on the allowable interest rates charged on consumer credit. They find that credit card holders respond by spending more. Agarwal,Liu and Souleles (2007) study the spending responses of households to tax rebates. Stephens (2008) finds that spending of consumers react to anticipated changes in income following the final payment of automobile loans. Along similar lines, Attanasio, Goldberg, and Kyriazidou (2008) study the significance of borrowing constraints in the market for automobile loans, and find results that are consistent with the presence of binding credit constraints. The study by Leth-Petersen (2010) is perhaps the closest one to my work in terms of the nature of the experiment. He estimates the consumption response to a 1992 credit market reform in Denmark that relaxed stringent constraints on the ability of homeowners to borrow using home equity as collateral for non-housing consumption loans. He finds evidence for the existence of credit constraints, but the magnitude of the effects is generally small. While all of the above studies find evidence that is consistent with the hypothesis that agents were facing binding credit constraints, they have different assessment of the importance of such constraints and how extensive they might be.

My paper differs from the above studies in two important ways. First, I study a novel event – the Texas legislative change – which has not yet been examined in the context of credit constraints. Second, I estimate the effects of an increase in credit availability on *aggregate* spending, at the county and state level. Existing studies use individual-level data to derive their estimates, which leaves the relevance of the results at the aggregate level open.[2] To the best of my knowledge, no other study has used a macro approach. While causality and identification issues are more easily dealt with using individual-level data, one main advantage of my macro approach is that it allows me to directly estimate aggregate spending effects that are ultimately the most relevant for policy analysis.

[2]Most studies using individual-level data speculate on the relevance of their results at the aggregate level (e.g., Gross and Souleles 2002, Leth-Petersen 2010)

I find strong support for the importance of credit constraints in Texas: the response of spending in Texas to the lifting of the borrowing constraints, at both the county and state level, is substantial and suggests that the credit elements of the legislative change had a large aggregate impact. The average effects for the state level variation are large: for the preferred specification, spending in Texas rises by around 16%, from before to after the amendment, relative to the change in spending of the control group. While the average effects for the county-level variation are negligible and less precisely estimated, the magnitude of the spending response is correlated with the level of income and the amount of equity released by the legislative change: spending rises by around 4% after the amendment for both the low income counties and the high house price counties.

3.2 The Framework

3.2.1 Collateralized credit constraints

In the Permanent-Income-Hypothesis model, agents are not credit-constrained and any increase in the availability of credit has no real effects on spending. The alternative hypothesis is that agents face binding constraints and thus spend less on current consumption than is optimal given their lifetime budget constraints. For such constrained agents, an increase in the availability of credit leads to the lifting of their borrowing constraints and in turn allows them to increase their spending.

Testing for the presence and importance of credit constraints can then be directly achieved by measuring the change in spending from before to after an exogenous increase in the access to credit. Most studies on the importance of credit constraints make assumptions regarding which households are more likely to face binding constraints and thus will be affected by the lifting of the constraints (e.g., Gross and Souleles 2002, Leth-Petersen 2010). By the nature of the Texas experiment, I can directly observe the 'credit-constrained' group (Texas households) and the 'credit-unconstrained' group (non-Texas households). To test for the importance of collateralized credit constraints, I then compare the change in spending for both groups, from before to after the legislative change.

The legislative change is crucial for the test. Ideally, it would *unexpectedly* expand access to credit for households; to wit, the increase in credit availability would be exogenous. It is hard to make the claim about exogeneity here, since it is possible that the legislative change came at the behest of the residents and the legislators in Texas. For instance, it is possible that the change in home equity lending laws reflected changes in the demand for credit, which in turn were the results of changes in consumption. If true, any causality claim for the issue at hand would be difficult to solve, and my estimates would be upwardly biased relative to a purely randomized experiment. To check when the legislative change first became public knowledge in Texas, I have searched extensively through *Lexis Nexis Academic* for the period 1980-1997. This is a place where one would expect to find mention of such plans. I find that the first time the legislative change is mentioned is in 1981. However, contemporary accounts suggest there remained strong political opposition to the legislative change until 1997, and thus there was a great deal of uncertainty surrounding the likelihood and timing of its passage.[3] This supports the notion that the introduction of the legislative change in 1997 really was a surprise to households in Texas, and in this sense was *unexpected.* Thus, to some extent, the legislative change can be interpreted as an exogenous shock. In any case, even with improper identification, I could still reject the hypothesis that stochastically varying borrowing constraints had no effect on overall spending in Texas.

3.2.2 The Texas legislative change

For more than a century, access to the home equity that owners had built up in their homes was largely untapped in Texas. The Texas Homestead Act of 1839 passed in response to the Banking Panic of 1837 and aimed at protecting citizens from forced sales of their homes. It

[3] For many years, the legislative change faced strong opposition from consumer advocacy groups who were worried about borrowers being potentially vulnerable to the aggressive sales tactics of lenders. Such opposition reinforced the prevailing uncertainty.

was later incorporated into the state's original constitution. Article XVI, Section 50 of the Texas Constitution effectively limited the extent to which households can borrow against the value of their homes. Access to home equity was not possible except for home improvement loans, loans to pay outstanding taxes and loans allowing a spouse to 'buy-out' another in the case of divorce; to wit, homeowners were not able to use the equity in their homes as collateral for general spending purposes. Outside of Texas, there were such restrictions on using home equity as collateral for consumption loans.

On November 4, 1997, the voters of Texas approved a constitutional amendment proposed by the 75th Legislature in *House Joint Resolution 31*[4], which significantly modified the constitutional provisions regarding liens on a homestead and created two additional categories of authorized liens, effective January 1, 1998: a lien for a home equity loan (but not for a home equity line of credit), and a lien for a reverse mortgage. The approved amendment effectively allowed borrowing using a homestead as collateral, up to a total loan-to-fair-market-value of 80% without any restriction on how the proceeds were to be used. However, uncertainties in the constitution resulted in many lenders electing to not make home equity loans or to make them only in certain circumstances. For instance, the constitution's original definition of a homestead limited urban households to no more than one acre of land. A substantial amount of households were living on more than one acre of land, and thus were unable to benefit from the amendment. Additionally, the minimum age to obtain a reverse mortgage was set at 55 years, which was inconsistent with federal law. On November 2, 1999, and effective that date, Texas voters approved two corrective constitutional amendments proposed by the 76th Legislature to address these two uncertainties. First, *Senate Joint Resolution 12*[5] redefined a reverse mortgage consistent with federal law, setting the minimum age to obtain one at 62. Second, *Senate Joint Resolution 22*[6] increased the maximum size of an urban homestead to

[4]http://www.capitol.state.tx.us/tlodocs/75R/billtext/html/HJ00031F.htm
[5]http://www.capitol.state.tx.us/tlodocs/76R/billtext/html/SJ00012F.htm
[6]http://www.capitol.state.tx.us/tlodocs/76R/billtext/html/SJ00022F.htm

10 acres.[7] These corrections allowed wider use of home equity loans, as households in Texas were able to rely more on these loans. Appendix A presents an extract of the 1997 home equity amendment of the Texas Constitution.

3.3 Empirical methods

3.3.1 Data and sample design

My ultimate objective is to estimate the effect of the Texas legislative change on *non-housing* consumption expenditures. One shortcoming here is the unavailability of measures for consumption expenditures at both the county level and the state level. Retail sales are, however, available and are used here as proxy. Retail sales are an important subset of consumer expenditures, and account for roughly half of aggregate consumption spending in the US. However, they do not include expenditures on some services, including housing. Given my focus on the response of *non-housing* consumption spending, the failure to account for expenditures on housing makes retail sales a reasonable proxy for the issue at hand.[8] In what follows, I describe my county-level and state-level data.

Unfortunately, retail sales are not *directly* observable at the state level. The retail sales data used in most previous studies (e.g., Case, Quigley and Shiller 2005, Calomiris, Longhofer, and Miles 2009) were constructed by *Regional Financial Associates* (RFA) using county sales tax data. However, I choose to not use these data because they are problematic, [9] and only span the time period ending in 1999. Other annual retail sales data by state are compiled and published by the *The Survey of Buying Power Data Service: Market*

[7] Two other amendments (*Senate Joint Resolution 42* and *House Joint Resolution 23*) followed in September 2003, allowing lenders to also offer Home Equity Line of Credits. I leave considerations for the effects of the 2003 amendment for future research.

[8] Other studies of consumption also use retail sales as proxy for consumption spending; see for example, Case, Quigley and Shiller (2005), Garrett, Hernàndez-Murillo and Owyang (2005).

[9] For instance, for states with no sales taxes, retail sales were imputed by exploiting the historical relationship between retail sales and retail wages and employment. Detailed information on the methodology used in constructing the dataset is also not readily available.

Statistics.[10] However, these data are not adjusted for industry re-classification, and are thus susceptible to measurement error and bias.[11]

In this study, I impute state level retail sales using data on quarterly states sales tax revenues and sales tax rates as in Garrett, Hernàndez-Murillo and Owyang (2005). Dividing the former by the latter yields estimates of retail sales by state.[12] I obtain sales tax revenue figures from the *State Government Collections* database (*Census Bureau*), and sales tax rates from the Tax Foundation's *Facts and Figures on Government Finances*. This sample is limited to 43 states, because five states have no sales tax (Alaska, Delaware, Montana, Oregon, and New Hampshire) and two states (Nevada and Utah) have incomplete tax-revenue records. I restrict the sample period to begin in 1992 and end in 2002. Hence, I exclude the high oil price volatility of the 1980's experienced by some states (including Texas), the Tax Reform Act of 1986,[13] the 2003 Texas legislative changes allowing home-equity lines of credit, and the housing price bubble of the early to mid 2000s. Excluding these episodes makes it more likely that the economy in Texas economy followed similar trends to those in other states.[14] For each state, I average across quarters to obtain annual values. Finally, I deflate to real values using the Consumer Price Index (all urban consumers, 1982-84=100), available from the *Bureau of Labor Statistics*. Per capita values are obtained using population measures available from the *Bureau of Labor Statistics*. Figure 3.1 plots the resulting imputed annual (log) real retail sales per capita for all states in the sample from 1992 to 2002.

[10]These data are reported in *Statistical Abstract of the United States*.

[11]In 1997, the North American Industry Classification System (NAICS) replaced the Standard Industrial Classification (SIC) system. The change in classification redefines extant industries (including the retail sales industry), and identifies new ones.

[12]Note that the imputed sales figures include only goods and services that are subject to state sales tax. Consumption expenditures usually include other forms of consumption that are not subject to state sales tax.

[13]Tax Reform Act of 1986 (TRA'86) allowed taxpayers to deduct interest on any home mortgage loan (including a home equity loan) secured by a principal residence. This altered the relative price of home-equity loans in all states except Texas.

[14]Recall that the parallelism in trends is essential to the difference-in-differences approach as it mirrors the counterfactual aspect of the model, allowing one to control for how the treated group (i.e., Texas) would have behaved in the absence of the policy change.

I obtain state level disposable income from the *Bureau of Economic Analysis*. In some specifications, I use oil prices[15], oil production by state.[16], and mortgage rates and house prices by state[17] Where applicable, I deflate and convert nominal figures to real per capita values using the same price index and population estimates noted above.

The county level sample comprises yearly retail sales data. I have collected these data from the *Economic Census*, Retail Trade Series, for establishments with paid employees. The *Economic Census* is published at 5-year intervals;, starting in 1977. These retail sales are net of deductions for refunds and allowances for merchandise returned by customers. Sales do not include carrying or other credit charges, sales (or other) taxes collected from customers and forwarded to taxing authorities, gross sales and receipts of departments or concessions operated by other companies, and commissions or receipts from the sale of government lottery tickets. Sales figures do not include retail sales made by manufacturers, wholesalers, service establishments, or other businesses whose primary activity is other than retail trade. However, they do include service receipts and sales to other retailers by establishments primarily engaged in retail trade.

As note earlier, beginning in 1997 the North American Industry Classification System (NAICS) replaced the Standard Industrial Classification (SIC) system as the official means for classifying business establishments. To be consistent and to avoid the measurement error and bias resulting from this change in the industry classification system, I use only the NAICS-based measures of sales. This restricts my sample to begin in 1997, since the 1997 *Economic Census* is the first to classify businesses according to the NAICS.[18] Aside from the change in the industry classification system, the restrictions on the sample period reflect the considerations discussed in the description of the state-level data.

[15]I obtain data on oil prices (West Texas Intermediate) from the *St. Louis Fed* Database)

[16]These data are from the *Basic Petroleum Data Book* (1999) published by the *American Petroleum Institute*

[17]These data are from *Monthly Survey of Rates and Terms on Conventional single-family Non farm mortgage loans*, available from the *Federal Housing Finance Agency.*

[18]For 1992 and earlier censuses, the Standard Industrial Classification (SIC) is used.

Data on county level personal taxes are not available, so I can not measure disposable income at the county level. I use data on personal income from the *Census Bureau*, as originally compiled by the *Bureau of Economic Analysis*. As with the state-level data, I deflate all nominal values by the Consumer Price Index, and obtain per capita values using population measures from the *Current Population Survey*. In some specifications, I use county-level median housing price data.[19]

There are 3143 counties in the US during this period (out of which 254 counties are in Texas). I include in my sample 3006 counties (including 244 counties in Texas) because these have complete records for both sales and income in 1997 and 2002. The final sample includes 6012 county-level observations in a balanced panel.

3.3.2 Empirical model

For investigating the effects of the legislative change, it is tempting to just consider spending data for the treated group *alone* (i.e., Texas) before and after the legislative change. However, spending data most likely exhibit a time trend, making such an approach highly misleading as one would confound the time trend with the treatment effect. I use a difference-in-differences approach to address this issue. The advantage of this approach is that it allows for a counterfactual analysis. For the issue at hand, it estimates a simple difference between actual spending and spending that would have occurred in the post-treatment period in Texas had the legislative change not passed. This, of course, is based on the assumption that an adequate control group for Texas exists and is able to reflect the likely behavior of Texas in the absence of any legislative change.

This difference-in-differences approach is particularly appealing for the issue at hand and is popular in estimating causal relationships. A similar 'treatment and control group' technique was used in many early studies of employment (Card and Krueger 1994), worker's

[19] I obtain the county-level housing price data from the *Decennial Census of Housing*.

compensation (Meyer 1995), schooling outcomes (Cornwell, Lee and Mustard 2005), and consumption (Gross and Souleles 2002, Leth-Petersen 2010).

My general difference-in-differences specification is

$$c_{it} = \delta_0 + \delta_1 D_i + \delta_2 L_t + \delta_3 D_i L_t + \delta_4 D_i L_t z_{it} + \gamma x_{it} + \alpha_i + \epsilon_{it} \tag{3.1}$$

where D_i, a dummy variable taking on the value 1 if a unit of observation i is in Texas and 0 otherwise, distinguishes the 'treatment group' (Texas) from the 'control group' (other, matched control states). L_t is a dummy variable taking on the value 1 if time t belongs to the treatment period (i.e., the period on or after the effective date of the Texas legislative change and 0 otherwise, x_{it} is a vector of exogenous control variables, and f_i is an unobserved individual fixed effect.

The difference-in-differences coefficient is defined as the difference in average spending in Texas from before to after the legislative change *minus* the difference in average spending for a set of control states over the same time period.

It is straightforward to derive the difference-in-differences coefficient using the conditional means implied by the model:

$$C_{it}^{00} = E(c_{it}|D_i = 0, L_t = 0, z_{it}, x_{it}) = \delta_0 + \gamma x_{it} \tag{3.2}$$

$$C_{it}^{10} = E(c_{it}|D_i = 1, L_t = 0, z_{it}, x_{it}) = \delta_0 + \delta_1 + \gamma x_{it} \tag{3.3}$$

$$C_{it}^{01} = E(c_{it}|D_i = 0, L_t = 1, z_{it}, x_{it}) = \delta_0 + \delta_2 + \gamma x_{it} \tag{3.4}$$

$$C_{it}^{11} = E(c_{it}|D_i = 1, L_t = 1, z_{it}, x_{it}) = \delta_0 + \delta_1 + \delta_2 + \delta_3 + \delta_4 z_{it} + \gamma x_{it}. \tag{3.5}$$

where, C_{it}^{11} in equation (3.5) represents expected retail sales in Texas after the amendment, conditional on z and x. Hence, the difference-in-differences coefficient can be written as follows:

$$(C_{it}^{11} - C_{it}^{10}) - (C_{it}^{01} - C_{it}^{00}) = \delta_3 + \delta_4 z_{it}, \tag{3.6}$$

Note that the above formulation of the difference-in-differences coefficient allows for treatment effects to be heterogenous: the treatment coefficient is equal to $\delta_3 + \delta_4 z_{it}$, and so it varies according to the exogenous variables in z_{it}. For the case of a constant treatment effect (i.e., $\delta_4 = 0$), this coefficient is simply δ_3, and measures the *average* treatment effect of the legislative change. Ordinary least squares gives an unbiased estimator of the treatment effect if both the error term ϵ_{it} and the unobserved fixed effect α_i are, on average, zero and are uncorrelated with the treatment (i.e., the passage of the legislative change). If the unobserved fixed effect is correlated with the treatment or the explanatory variables in equation (3.1), estimates of the parameters will be biased. I can eliminate the unobserved effect by taking the first difference of equation (3.1). The difference-in-differences specification is

$$\Delta c_{it} = \delta_2 \Delta L_t + \delta_3 D_i \Delta L_t + \delta_4 D_i (L_t z_{it} - L_{t-1} z_{it-1}) + \gamma \Delta x_{it} + \Delta \epsilon_{it} \tag{3.7}$$

where δ_3 and δ_4 have the same interpretation as in equation (3.1). For the county-level data, $T = 2$ (as described in the earlier), and so the first-difference estimator is identical to the fixed-effect estimator. For the state level variation where $T = 11$, the first-difference estimator is more efficient than the fixed-effect estimator in the presence of autocorrelation.

I present estimates from both the specification in levels (i.e., equation (3.1)) and the specification in differences (i.e., equation (3.2)). For the purpose of inference, I use standard errors that are clustered at the state level and are robust to heteroskedasticity and autocorrelation, based on the non-parametric block bootstrap procedure suggested by Bertrand *et al.* (2004).[20]

3.3.3 Preliminary analysis

The choice of the control group is important in a difference-in-differences framework. As I mentioned earlier, the properties of the difference-in-differences estimator hinge crucially on the assumption that the error term ϵ_{it} in equation (3.1) is uncorrelated with the explanatory variables in the equation. The latter assumption can essentially be satisfied by finding a

[20]For the county-level variation, clustering at the state level allows for common group (state) effects (see Moulton (1990) for a discussion of adjusting standard errors in regression models with mixtures of individual and grouped data). For the state-level variation, clustering at the state level addresses autocorrelation.

control group that is similar to the treatment group in every way except the receipt of the treatment. This can be difficult to do, but a weaker assumption is that, in the absence of the treatment, differences between the control group and the treatment group is constant over time. If it turns out that the growth paths of the outcome variable for the treatment and the control group are different, this information can be always be incorporated into the analysis. For instance, one can add to the model other variables that influence both the outcome variable (i.e., retail sales) and are correlated with treatment status.

I consider two alternative control groups: a general group which includes all states other than Texas, and a group of states that are very similar to Texas. For the latter, I match nine states to Texas along three dimensions. First, I focus on states that share a common border with Texas (i.e., Louisiana, Arkansas, New Mexico, and Oklahoma). Second, I examine the distributions of state-level annual per capita oil production and real house prices in the year of the passage of the Texas legislative change (i.e., in 1997). In each case, I choose states that fall within the same percentile of the distribution as Texas: Wyoming, Louisiana, North Dakota, New Mexico and Oklahoma match along the oil production dimension, while the last three plus Arkansas, Arizona, Alabama and Nebraska match along the housing price dimension.[21] The matched-state control group is more likely to reflect the expected behavior of retail sales in Texas had the amendment not passed than the general control group.

Table 3.1 presents descriptive statistics for both the state-level and county-level samples. Means are presented separately for Texas and each control group. The Table shows that, in the county-level data, average real per capita retail sales in Texas grew by around 5% from before to after the passage of the amendment, while average sales in *each* of the control groups grew by 4.5%, a difference of 0.5%. The state-level data suggests a much bigger average effect: from before to after the amendment, retail sales in Texas grew by around 7% relative to the changes in spending of all other states, and by 11% relative to the change in spending of the matched states. The relatively large effect observed in the state-level data

[21] In 1997, Texas falls in the upper 25th percentile of annual per capita oil production, and in the lower 25th percentile of real house prices.

is evident in Figure 3.2 which plots real per capita retail spending for Texas and the two control groups. The series were normalized on their respective values in 1994. The figure is informative and shows an apparent break in the spending data for Texas after the passage of the legislative change. The break in the data is most striking in 1999 and 2000. This suggests a potentially large effect of the legislative change on retail sales at the state level. The figure shows that most of the increase in retail sales in Texas occurs between 1999 and 2000. By 2002, the effect is more muted. The large changes in spending are not evident in the county-level data because the five-year horizon misses the important response of retail sales in years 1999 and 2000; to wit, this may explain the relatively *smaller* magnitude of the effect observed in the county-level data.

To what extent does the relative increase in retail spending in Texas reflect a potential relaxation of credit constraints brought by the legislative change of 1997? Figure 3.3 shows that only 2.5% of homeowners in Texas held home equity loans in 1997 – i.e., before the legislative change.[22] In 1999, the proportion of homeowners with home equity loans in Texas rose to 4.5%, which was still slightly lower than the national rate of 5% for that year. By 2001, the proportion of homeowners with home equity loans became 6.4%, *exceeding* the national rate of 5.7%. This provides additional preliminary evidence that households in Texas were most likely credit constrained due to the legal restrictions on home equity lending that existed until 1997. However, the preliminary evidence discussed in this section does not allow for other sources of variation in retail spending (e.g., such as differences in income, among other things). In what follows, I present evidence from a formal analysis that accounts for such differences and for the uncertainty in the effect of the treatment, using a difference-in-differences framework.

[22] Note that proceeds from such loans could have *only* been used to finance home improvement or pay outstanding taxes, under the laws prevailing at the time.

3.4 Empirical results

This section presents results from estimating the effect of the 1997 legislative change on Texas households. For each sample, I present results from estimating the models in both levels and differences (i.e., equations (3.1) and (3.7)). I consider the model in differences to be preferred to the model in levels, since it is not subject to bias from excluding unobserved county-level effects. As noted earlier, I estimate the regressions using ordinary least squares. . The t-statistics reported in the Tables and used for inference are computed from White's consistent estimator of the covariance matrix allowing for heteroscedasticity. The t-statistics reported in all Tables and used for inference are heteroscedasticity consistent and (where applicable) account for group effects (i.e., intra-state (cluster) correlation in the county-level data).

3.4.1 State level analysis

Here, I consider the effects of the Texas legislative change on retail spending using the state-level data. As I've noted earlier, I have *annual* data for Texas and the 42 other states, from 1992 to 2002. Obviously, the nature of this state-level data does not allow me to analyze the heterogeneity of the treatment effect *within* Texas; to wit, here I can only present evidence of an *average* treatment effect, so the difference-in-differences estimates are assumed to be constant over time and states.[23] First, the main results from using all other 42 states as a control group are presented. Next, I re-estimate the models using the matched states as controls to confirm that the results are not sensitive to the choice of the control group. Finally, a series of robustness checks are conducted to confirm that the results found are likely to be related to the relaxation of credit constraints in Texas.

Table 3.2 presents the difference-in-differences estimates of the average treatment effect, assuming that 1998 is the initial treatment year in the regressions. All variables are in logs, so that the treatment coefficient is interpreted as a percentage change; t-statistics are in

[23] This imposes the natural restriction that $\delta_4 = 0$ in all specifications of this state-level analysis.

parentheses. Column (i) presents the simple difference-in-differences estimate, suggesting a sizable and statistically significant increase in retail spending in Texas from before to after the legislative change, relative to the change in spending by the 42-state control group. Columns (ii) and (iii) add to the model variables that may have independent effects on the variation in retail spending. In column (ii), I consider a model with state effects, a time trend, and real per capita disposable income; in column (iii), I add these, plus real housing prices, mortgage interest rates and real oil prices. I find convincing evidence of positive treatment effects for retail sales in Texas, with estimates ranging from 1.6% (column (iii), differenced model with the full set of control variables) to 4.6% (column (iii), levels model with the full set of control variables).[24] However, while the estimate in column (i) is highly statistically significant, those in column (ii) and (iii) are only marginally significant. These smaller and marginally significant treatment effects are likely due to the fact that I assume 1998 is the initial treatment year in the regressions. The argument for a different timing of the treatment is plausible here. As noted above in the text, uncertainties surrounding the amendment in 1998 resulted in many lenders electing to not make home equity loans or to make them only in certain circumstances. The subsequent amendment passed in 1999 addressed the uncertainties and allowed wider use of home equity loans. This suggests that the main effects of the legislative change occurred after the uncertainties with the initial amendment were resolved (i.e., after 1999).[25] To consider this possibility, Table 3.3 replicates the analysis assuming instead that 1999 is the treatment year in the regressions. The results in Table 3.3 now suggest that, across all model specifications, treatment-effect estimates range from 8.9% (column (iii), levels model with the full set of control variables) to 14.9% (column (iii), differenced model with the full set of control variables). These estimates are also highly statistically significant. My estimates suggest that Texas households who were credit constrained were able to increase their spending from before to after the passage

[24]The levels model picks up a persistent increase in the *level* of retail sales, while the model in differences picks up a one-time, temporary effect on the *growth rate* of retail sales; hence, both models tell the same story.

[25]This was also evident in my earlier preliminary look at the data and Figure 3.2.

of the home equity amendment. My estimates provide insight on the importance of credit constraints and their role in explaining the spillover from housing markets to consumption demand.[26]

Next, I consider the sensitivity of my results to the choice of the control group. I discussed earlier how the control group consisting of the nine matched states is more likely than the general control group to represent the projected behavior of retail sales in Texas had there been no amendment. Tables 3.4 and 3.5 replicate the analysis using the nine matched states as a control group. Again, I find convincing evidence in both tables of positive treatment effects for retail sales in Texas. Once again, the largest effect occurs in 1999, with estimates ranging from 10% (column (iii),the levels model with the full set of control variables) to 16.5% (column (iii), the differenced model with the full set of control variables). The estimates are also statistically significant, although at levels of significance that are lower than when the control group consists of all other 42 states; This is likely due to the reduced degrees of freedom resulting from using a relatively smaller control group. Using estimates from my preferred specification, the differenced model with a full set of control variables, the treatment-effect estimates are generally robust across both control groups, ranging from around 15% (column (iii) of Table 3.3, using all non-Texas states as a control group) to 16.5% (column (iii) of Table 3.5, using the matched states as a control group). This suggests that my estimates of the spending effect of the legislative change do not rest on the choice of the control group, and gives credibility to my findings.

Aside from the choice of the control group, other factors may render the results presented above sensitive to the design of my natural experiment. To confirm that the effects I find are, in fact, related to the passage of the legislative change, two consistency checks are carried out. First, the estimation exercise will be performed separately for each of the nine matched states, assuming that each of these states is a treated state. If the access to housing equity

[26]Estimates from counterfactuals using aggregate US data in Iacoviello and Neri (2009) also suggest that collateral factors increase the elasticity of consumption with respect to housing wealth from about 10 to 12.3 %. The importance of credit constraints captured by the Texas natural experiment is essential in explaining these collateral factors.

provided by the Texas legislative change is really the reason for the estimated effects on retail spending that I found for Texas, then no positive spending effects should be found for any of the nine matched states. Second, there could be a wealth effect following from the treated group (i.e., Texas) having experienced a more rapid growth in wealth than the control group, due to increasing house prices.

My estimates of the spending effects for Texas will be more credible if I can show evidence that none of the nine matched states experienced a positive spending effect. Such an exercise, of course, rests on my assumption that these nine matched states are similar to Texas except for the home equity treatment. To this end, I perform 'placebo' tests by repeating my analysis, considering each of the nine matched states as the treated state and the remaining eight states (excluding Texas) as the control group. The results are shown in Table 3.6 where the treatment year is assumed to be 1999. The evidence for the matched states does not contradict my results: using estimates from my preferred specification, the differenced model with a full set of control variables, I find that none of the nine matched sample states shows a statistically significant positive treatment effect. This reinforces the validity of my test of the amendment's treatment effects for Texas, and my conclusion confirms that the spending effects found for Texas are most likely related to the relaxation of credit constraints after the passage of the amendment.

I argue that the spending effects found are due to the lifting of the housing collateral constraint. While the passage of the legislative change in Texas most likely did not directly change homeowners' wealth, if Texas experienced a more rapid expansion in house prices than the control group over the sample period, then the measured effects could possibly reflect direct wealth effects. Given that house prices increased dramatically in the US over my sample period, this could have created differential wealth effects if house prices in Texas evolved differently than their counterparts in the control states. While I control for house prices in some specifications, it is potentially informative to investigate any differences in the patterns of house price changes between Texas and the control states over my sample period. Figure 3.4 plots annual real house prices for Texas and the general control group (all non-Texas states in the sample). The series were normalized on their respective values in year 1994. The figure suggests that, on average, house values over the observation period in Texas and the control group have evolved similarly. In summary, there does not seem to be evidence that wealth effects are driving the results. My estimates of the spending effects in Texas can thus be seen as further evidence that the spillover of house prices on non-housing expenditures is most likely due to collateral effects rather than wealth effects.

3.4.2 County level analysis

Here, I consider the effects of the Texas legislative change on retail spending using the county-level data. As I've noted earlier, I have *annual* data for Texas and *all* remaining 49 states, for years 1997 and 2002. Here, because of the five-year horizon, I miss the important response of retail sales in 1999, so I cannot analyze the dynamic pattern of the estimated effects as I have done in the state-level analysis. However, one advantage of this county-level data, relative to the state-level data, is that the cross-sectional variation *within* states is potentially informative about differences in the treatment effect due to observed heterogeneity; to wit, I relax here the assumption that the treatment effect is constant within states. First, the main results from using all 49 states as a control group are presented. Next, I re-estimate the models using the matched states as the control, to examine whether the results are sensitive to the choice of the control group. Finally, as in the state-level analysis, a series of robustness checks are conducted to confirm that the results found are likely to be related to the relaxation of credit constraints in Texas.

Table 3.7 presents the difference-in-differences estimates of the *constant* treatment effect for the county-level, imposing the restriction that $\beta_4 = 0$. Since the legislative change in Texas went into effect in January 1998, I consider the time period 1997 to be pre-treatment and 2002 to be post-treatment. Again, all variables are in logs, so that the treatment coefficient is interpreted as a percentage change; t-statistics are in parentheses. For each of the models, the levels and differences, I consider two specifications. In the first specification, I include in x a vector of dummies to account for state-level fixed effects, and the natural log of real per capita income. In the second specification, I include these variables plus state-level housing prices and mortgage rates. Differences in the housing variables across counties may affect household spending independently from the housing collateral channel. For instance, with higher house prices and lower mortgage rates, a Texas household could have easily refinanced its original mortgage to finance spending, even in the absence of access to home equity loans.

The results shown in Table 3.7 indicate that, on average, there appears to have been no positive spending effect for Texas households of the legislative change: overall spending grew by, at most, 0.8% more than for the control group (column (iv), differenced model with the full set of control variables). The treatment coefficients are also statistically insignificant. Thus, the average results for total spending appear to provide no evidence that Texas households had been credit constrained, since there was no differential spending growth from before to after the legislative change. As I noted earlier, the five-year horizon aspect of the county-level data does not enable me to capture the dynamics of retail sales between 1997 and 2002; to wit, I miss the large response of retail sales observed in the state-level data around 1999 and 2000.

The absence of an average spending effect may, however, conceal heterogeneous responses that balance out. The level of aggregation provided by this data makes it possible to investigate whether there are systematic differences in the spending responses across counties in the data. Such differences could be important in uncovering the importance of credit constraints in the data. For instance, according to Census data from the American Community Survey, the percentage of households with home equity loans in Texas across counties in 2002 ranged from 0.93% to 11.8%, with a mean of 6.7% and standard deviation of 3.2%. These figures motivate models that allow for heterogeneous treatment effects, as the extent to which credit constraints bind might vary across counties. To this end, I relax the restriction on δ_4 and consider two implications of the credit-constraint hypothesis. First, if the legislative change had any effect on spending in Texas, I would expect that there would be larger responses for lower-income households: lower-income counties suffer potentially more than high-income counties from asymmetric information problems, and so they are more likely to face credit constraints that bind. Second, if any spending effects found are related to gaining access to housing equity, there would be larger responses for households with the a larger value of home equity: households in counties where home values are higher will benefit more, all other things the same, from relaxed credit constraints. Tables 3.8 and 3.9 contain the difference-in-differences estimates of the heterogenous treatment effects for the county-level data.

Table 3.8 allows heterogeneity across counties in the treatment effects according to income. I set z_{it} to be a binary variable equal to 1 if a county's real per capita income is greater than the median real per capita income in Texas in 1997, and zero otherwise. This binary variable allows me to distinguish low-income from high-income counties prior to the amendment (i.e., in 1997). In this case, $\delta_3 + \delta_4$ is the difference-in-differences estimate. I find that low-income counties increased spending, relative to the control group, by 3% according the my preferred specification (column (iv) of Table 3.8, differenced model with the full set of control variables). The increase in spending is statistically significant. On the other hand, the change in spending is essentially zero for the high-income counties.

Table 3.9 allows heterogeneity across counties in the treatment effects according to house prices. I set z_{it} to be a binary variable equal to 1 for counties in which real median housing prices in 1990 exceeded the Texas state median house price in that year, and zero otherwise.[27] This binary variable allows me to distinguish low-house-price counties from high-house-price counties prior to the amendment. I find that high-house-price counties increased spending, relative to the control group, by 2.4%, according the my preferred specification (column (iv) of Table 3.9, differenced model with the full set of control variables). The increase in spending is statistically significant. On the other hand, the change in spending is essentially zero for

[27](the latest year prior to the amendment for which county-level house price data are available)

the low-house-price counties. This result is expected since higher home values amplify the effects of relaxing collateral constraints because higher home equity gains can be transferred into higher borrowing and thus higher spending.

In summary, while I find no solid evidence of an average spending effect of the legislative change in Texas, the results in Tables 3.8 and 3.9 suggest a significant positive spending effect of the Texas legislative change for low-income counties and high-house-price counties. The estimates of the heterogenous treatment effects confirm that the spending effects found are likely to be related to the presence of credit constraints.

As I have done in the state-level data, I consider the sensitivity of my results to the choice of the control group in the county-level data. Tables 3.10 through 3.12 replicate the county-level analysis using the nine matched states as a control group. Table 3.10 shows that the estimated treatment effects range from 1.1% and 2.9%; for the preferred specification, the estimated treatment effect is 2% (column (iv) of Table 3.10, differenced model with the full set of control variables). However, while the estimates are economically non-trivial, they are statistically insignificant and provide no solid support for a positive spending effect of the legislative change. Tables 3.11 and 3.12 replicate the analysis in Tables 3.8 and 3.9, using the matched states as a control group. The Tables allow for a heterogeneity in the treatment effects according to income and house prices, respectively. Again, I find convincing evidence that *low* income counties in Texas increased spending, relative to the control group, by 4.2% according the my preferred specification. Similarly, high house price counties in Texas increased spending, relative to the control group, by 3.6% according the the preferred specification. In summary, the treatment coefficients are generally robust across control groups. This suggests that my estimates of the spending effect of the legislative change do not rest on the choice of the control group, which gives credibility to my findings and confirms that the effects I found are likely to be related to the presence of credit constraints.

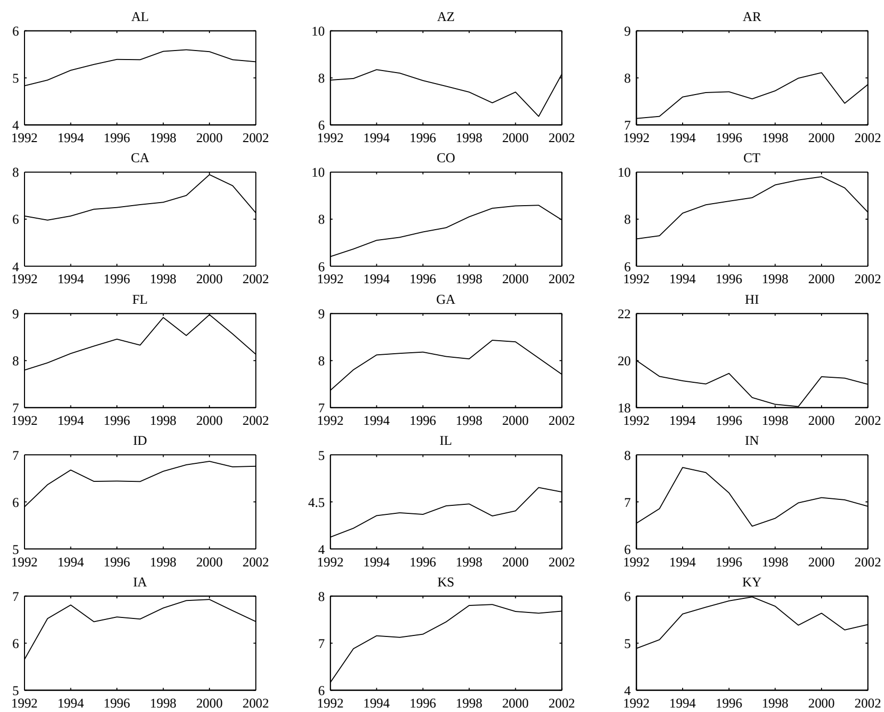
As I noted in the state-level analysis, there could possibly be a wealth effect following from the treated group (i.e., Texas) having experienced a more rapid growth in housing wealth than the control group due to increasing house prices. Table 3.13 presents results for a test of the equality of mean house price growth for counties across Texas and the control group. The results show that the average house value across the observation period for Texas has evolved *slower* than the average house value in the control group. This suggests that the spending effect I found for Texas is not a housing-wealth effect, and so my estimates can be seen as further evidence that the spillover of house prices to non-housing expenditures is most likely due to collateral effects.

3.5 Summary and conclusion

In this study, I investigate the importance of collateralized credit constraints by exploiting a Texas legislative change introduced in 1997 that gave households access to housing equity as collateral for consumption loans. The legislative change is used to identify whether collateralized credit constraints had any influence on retail spending in Texas. I conduct my analysis using both county-level and state-level data. The effect of the legislative change is estimated using a differences-in-differences approach, comparing the change in spending from before to after the Texas amendment for two groups: households in Texas that were likely to be constrained prior to the reform, and households in others states that were not constrained.

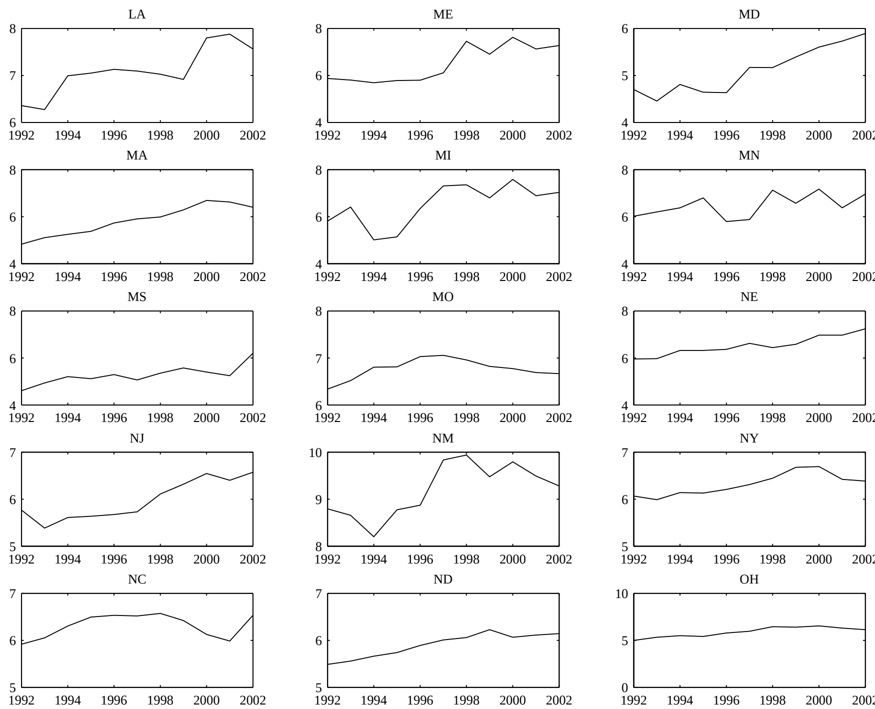
At the state-level, I find that spending increases by up to 15% from before to after the amendment, relative to the change in spending by the control groups. While there was no solid evidence in the county-level data of a sizeable and significant average treatment effect, I find that the magnitude of the spending response is correlated with the amount of equity potentially released by the amendment and that lower-income counties were facing more binding credit constraints: spending in Texas increases by up to 4.2% for low-income counties, and 3.6% for high-house-price counties.

I argue that my estimates of the spending effects in Texas provide further evidence that the link between house prices and non-housing expenditures is most likely due to collateral effects rather than wealth effects. My findings suggest that credit constraints are important, at least for aggregate spending in Texas.



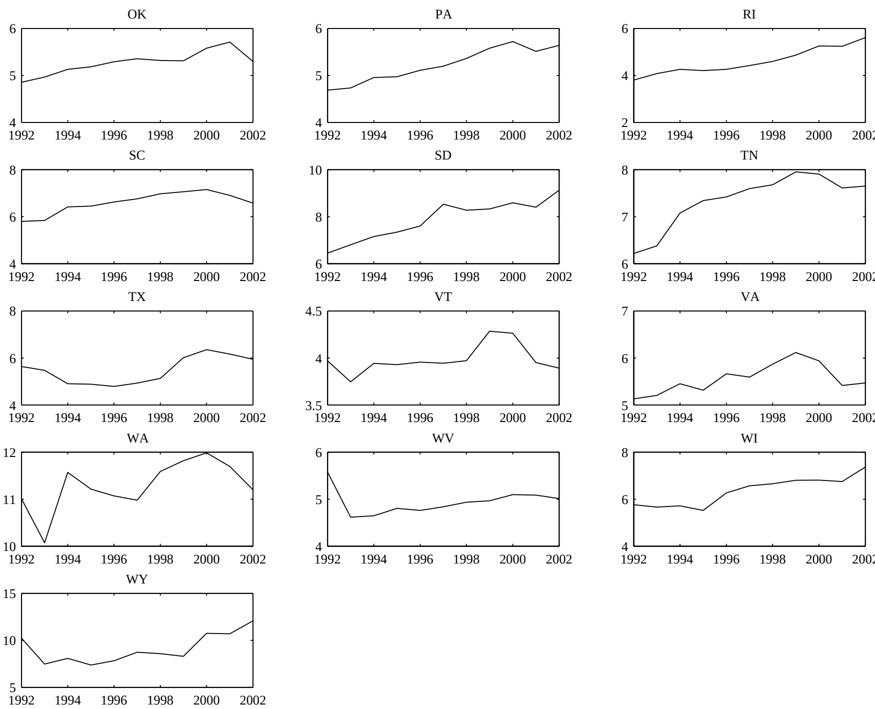
(a)

Figure 3.1: Imputed state-level Real retail sales data: Real per capita retail sales (thousands of US dollars) for all 42 states in the state-level sample.



(b)

Figure 3.1: (cont'd)



(c)

Figure 3.1: (cont'd)

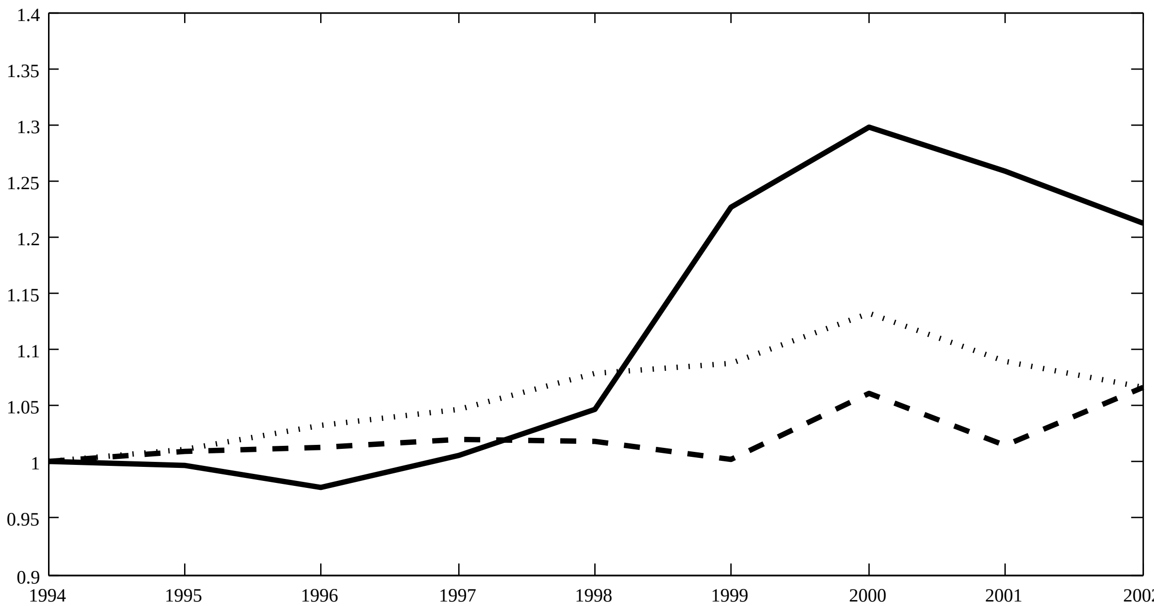


Figure 3.2: State-level Real per capita retail sales for Texas and the controls: The general control group consists of the 42 other states in the state-level sample (dashes). The matched control group consists of the nine states that are similar to Texas along the three dimensions discussed in the text: Wyoming, Louisiana, North Dakota, New Mexico, Oklahoma, Arkansas, Arizona, Alabama and Nebraska. Series are normalized on their respective 1994 values.

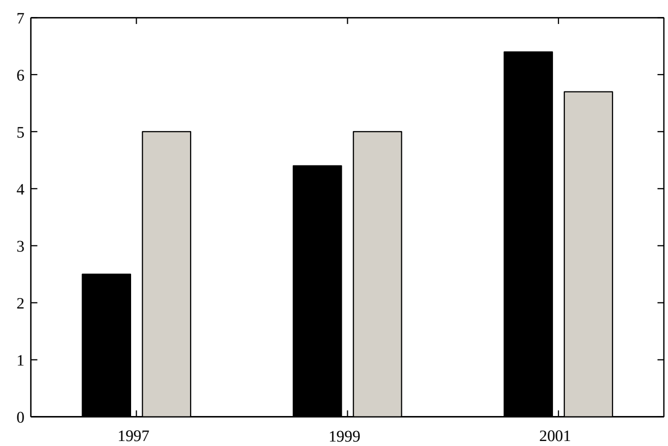


Figure 3.3: Percent of homeowners with home equity loans: Data are shown for Texas (black) and the US (grey). Data shown for years 1997, 1999, and 2001 are from the American Housing Survey (AHS) and Texas Comptroller of Public Accounts.

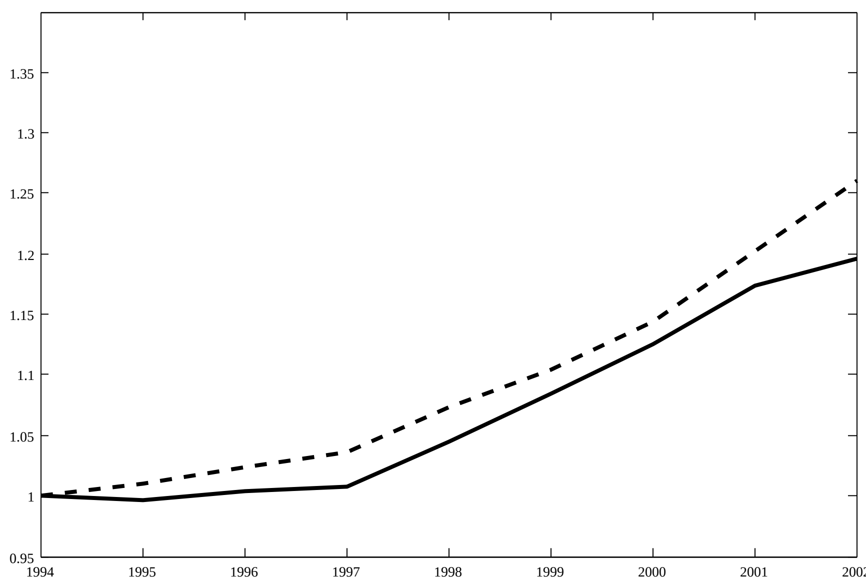


Figure 3.4: State-level Real House Prices: Data is shown for the general control group consisting of the 42 other non-Texas states in the state-level sample (dashes), and for Texas (solid). Series are normalized on their respective 1994 values.

Table 3.1: Descriptive statistics: retail sales

	Means		
	Pre	Post	Change
Panel A: State-level sample			
1. Texas	5101.5	5920.6	819.1
2. Matched states	6669.9	6949.3	279.4
3. All non-Texas states	6363.5	6930.2	566.7
Panel B: County-level sample			
1. Texas	3925.9	4123.8	197.9
2. Matched states	4057.7	4241.4	183.7
3. All non-Texas states	4360.3	4556.3	196.0

Notes: Means of real per capita retail spending are shown for Texas and the two control groups, before and after the Texas legislative change. The county-level sample consists of 3006 counties, of which 244 are in Texas and 475 are in the matched states.

Table 3.2: State-level treatment effects, all other states as control

Levels	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1998	0.063	0.048	0.046
		(1.97)	(1.86)	(1.82)
NT		473	473	474
Differences	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1998	0.013	0.012	0.016
		(1.51)	(1.49)	(1.65)
NT		430	430	430
state-effects		no	yes	yes
time trend		no	yes	yes
income		no	yes	yes
housing prices		no	no	yes
mortgage rates		no	no	yes
oil prices		no	no	yes

Notes: State-level treatment effect estimates from the models in levels and differences (i.e., equations 3.1 and 3.7 in the text), using all 42 non-Texas states as a control group. L_t is a dummy variable taking on the value 1 if time t belongs to the treatment period. NT is the number of panel observations. The t-ratios in parentheses are heteroscedasticity and autocorrelation consistent.

Table 3.3: STATE-LEVEL TREATMENT EFFECTS, ALL OTHER STATES AS CONTROL

Levels	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1999	0.104	0.090	0.089
		(2.02)	(1.99)	(1.97)
NT		473	473	474
Differences	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1999	0.152	0.151	0.149
		(2.04)	(2.04)	(2.04)
NT		430	430	430
state-effects		no	yes	yes
time trend		no	yes	yes
income		no	yes	yes
housing prices		no	no	yes
mortgage rates		no	no	yes
oil prices		no	no	yes

Notes: State-level treatment effect estimates from the models in levels and differences (i.e., equations 1 and 7 in the text), using all 42 non-Texas states as a control group. L_t is a dummy variable taking on the value 1 if time t belongs to the treatment period. NT is the number of panel observations. The t-ratios in parentheses are heteroscedasticity and autocorrelation consistent.

Table 3.4: State-level treatment effects, matched states as control

Levels	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1998	0.083	0.061	0.063
		(1.86)	(1.38)	(1.36)
NT		110	110	110
Differences	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1998	0.042	0.042	0.045
		(1.96)	(1.96)	(2.00)
NT		100	100	100
state-effects		no	yes	yes
time trend		no	yes	yes
income		no	yes	yes
housing prices		no	no	yes
mortgage rates		no	no	yes
oil prices		no	no	yes

Notes: State-level treatment effect estimates from the models in levels and differences (i.e., equations 1 and 7 in the text), using the matched states as a control group. L_t is a dummy variable taking on the value 1 if time t belongs to the treatment period. NT is the number of panel observations. The t-ratios in parentheses are heteroscedasticity and autocorrelation consistent.

Table 3.5: State-level treatment effects, matched states as control

Levels	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1999	0.116	0.101	0.100
		(1.95)	(1.71)	(1.69)
NT		110	110	110
Differences	L_t=1	(1)	(2)	(3)
$\widehat{\delta}_3$	$\geq$ 1999	0.167	0.164	0.165
		(2.06)	(2.06)	(2.05)
NT		100	100	100
state-effects		no	yes	yes
time trend		no	yes	yes
income		no	yes	yes
housing prices		no	no	yes
mortgage rates		no	no	yes
oil prices		no	no	yes

Notes: State-level treatment effect estimates from the models in levels and differences (i.e., equations 1 and 7 in the text), using the matched states as a control group. L_t is a dummy variable taking on the value 1 if time t belongs to the treatment period. NT is the number of panel observations. The t-ratios in parentheses are heteroscedasticity and autocorrelation consistent.

Table 3.6: State-level treatment effects, placebo tests

	(1)	(2)	(3)
Wyoming	-0.04	-0.048	-0.047
	(-2.01)	(-1.96)	(-1.94)
Louisiana	-0.023	-0.018	-0.023
	(-1.88)	(-1.74)	(-1.84)
North Dakota	0.022	0.028	-0.029
	(1.86)	(1.92)	(-1.91)
New Mexico	-0.055	-0.05	-0.05
	(-2.05)	(-2.03)	(-2.01)
Oklahoma	-0.008	-0.008	0.002
	(-1.23)	(-1.28)	(0.38)
Arkansas	0.029	0.028	0.024
	(1.90)	(1.89)	(1.84)
Arizona	-0.071	-0.071	-0.070
	(-2.06)	(-2.07)	(-2.07)
Alabama	0.000	0.001	0.000
	(-0.06)	(0.12)	(0.04)
Nebraska	0.016	0.014	-0.014
	(1.70)	(1.61)	(-0.86)

Estimated treatment effects are from the differenced model (i.e., equation 7) assuming 1999 as the treatment year. Each of the nine matched states is alternatively considered as treated while the remaining eight states (excluding Texas) are part of the control group. The three model specifications are the same as in Tables 2 to 5.

Table 3.7: County-level treatment effects, all other states as control

	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	0.001	0.004	0.005	0.008
	(0.15)	(0.54)	(0.91)	(1.28)
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.263	0.263	0.016	0.027
NT	6012	6012	3006	3006

County-level treatment effect estimates from the models in levels and differences (i.e., equations 1 and 7 in the text), using all 49 non-Texas states as a control group. Models assume a constant treatment effect (i.e., $\delta_4 = 0$). NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.8: County-level treatment effects, all other states as control

	$z_{it} = DY_i$			
	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	0.054	0.057	0.027	0.030
	(1.76)	(1.78)	(1.96)	(1.96)
$\widehat{\delta}_4$	-0.082	-0.081	-0.033	-0.033
	(-1.86)	(-1.85)	(-2.08)	(-2.08)
treatment low income	0.054	0.057	0.027	0.030
treatment high income	-0.028	-0.024	-0.006	-0.003
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.263	0.263	0.016	0.027
NT	6012	6012	3006	3006

See notes to Table 7. Models assume heterogeneous treatment effects (i.e., $\delta_4 0$ is estimated and $z_{it} = DY_i$). DY_i is an income binary variable, equal to 1 for high income counties, 0 for low income counties. NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.9: County-level treatment effects, all other states as control

	$z_{it} = HP_i$			
	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	-0.077	-0.074	-0.010	-0.007
	(-1.96)	(-1.95)	(-1.42)	(-1.12)
$\widehat{\delta}_4$	0.157	0.157	0.031	0.031
	(2.00)	(2.00)	(2.08)	(2.09)
treatment low house price	-0.077	-0.074	-0.010	-0.007
treatment high house price	0.080	0.083	0.021	0.024
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.263	0.264	0.016	0.027
NT	6012	6012	3006	3006

See notes to Table 7. Models assume heterogeneous treatment effects (i.e., $\delta_4 0$ is estimated and $z_{it} = HP_i$). HP_i is a house price binary variable, equal to 1 for high house price counties, 0 for low house price counties. NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.10: COUNTY-LEVEL TREATMENT EFFECTS, MATCHED STATES AS CONTROL

	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	0.013	0.029	0.011	0.020
	(0.81)	(0.28)	(0.88)	(0.37)
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.174	0.174	0.010	0.018
NT	1438	1438	719	719

County-level treatment effect estimates from the models in levels and differences (i.e., equations 1 and 7 in the text), using the nine matched states as a control group. Models assume a constant treatment effect (i.e., $\delta_4 = 0$). NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.11: County-level treatment effects, matched states as control

	$z_{it} = DY_i$			
	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	0.049	0.066	0.032	0.042
	(1.36)	(0.63)	(1.69)	(0.72)
$\widehat{\delta}_4$	-0.055	-0.056	-0.033	-0.033
	(-1.49)	(-1.52)	(-2.07)	(-2.07)
treatment low income	0.049	0.066	0.032	0.042
treatment high income	-0.006	0.010	-0.001	0.009
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.174	0.174	0.011	0.018
NT	1438	1438	719	719

See notes to Table 10. Models assume heterogeneous treatment effects (i.e., $\delta_4 0$ is estimated and $z_{it} = DY_i$). DY_i is an income binary variable, equal to 1 for high income counties, 0 for low income counties. NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.12: County-level treatment effects, matched states as control

	$z_{it} = HP_i$			
	Levels		Differences	
	(i)	(ii)	(iii)	(iv)
$\widehat{\delta}_3$	-0.077	-0.062	-0.005	0.005
	(-1.73)	(-0.66)	(-0.46)	(0.09)
$\widehat{\delta}_4$	0.182	0.182	0.032	0.031
	(1.91)	(1.90)	(2.10)	(2.09)
treatment low house price	-0.077	-0.062	-0.005	0.005
treatment high house price	0.105	0.120	0.027	0.036
state-effects	yes	yes	yes	yes
income	yes	yes	yes	yes
housing prices	no	yes	no	yes
mortgage rates	no	yes	no	yes
R^2	0.180	0.179	0.011	0.018
NT	1438	1438	719	719

See notes to Table 10. Models assume heterogeneous treatment effects (i.e., $\delta_4 0$ is estimated and $z_{it} = HP_i$). HP_i is a house price binary variable, equal to 1 for high house price counties, 0 for low house price counties. NT is the number of panel observations. R^2 is the adjusted R-squared. The t-statistics in parentheses are heteroscedasticity consistent and account for intra-state (cluster) correlation.

Table 3.13: Test for mean house price growth

	N	Mean
A. Counties in Texas	244	0.016
		(2.0)
B. Counties in all non-Texas states	2762	0.238
		(59.5)
C. Difference	–	-0.222
	–	(-24.7)

Notes: Test of equality in mean house price growth for the county-level data across Texas and the control group. t-statistics are in parentheses.

3.6 Appendix. The 1997 Texas Constitutional Amendment

This appendix gives an abridged version of Article 16, Section 5 of the Texas Constitution, with the salient amendments from Proposition 8 given in italics. Sub-section (6) lists 15 additional stipulations [(C) through (Q)] that are aimed at consumer protection.

Article 16. General Provisions

Sec. 50. Homestead; Protection from forced sale; Mortgages, trust deeds and liens.

(a) The homestead of a family, or of a single adult person, shall be, and is hereby protected from forced sale, for the payment of all debts except for:

1. the purchase money thereof, or a part of such purchase money;

2. the taxes due thereon;

3. an owelty of partition imposed against the entirety of the property by a court order or by a written agreement of the parties to the partition, including a debt of one spouse in favor of the other spouse resulting from a division or an award of a family homestead in a divorce proceeding;

4. the refinance of a lien against a homestead, including a federal tax lien resulting from the tax debt of both spouses, if the homestead is a family homestead, or from the tax debt of the owner;

5. work and material used in constructing new improvements thereon, if contracted for in writing, or work and material used to repair or renovate existing improvements thereon ...

6. *an extension of credit that:*

    (a) *is secured by a voluntary lien on the homestead created under a written agreement with the consent of each owner and each owner's spouse;*

(b) *is of a principal amount that when added to the aggregate total of the outstanding principal balances of all other indebtedness secured by valid encumbrances of record against the homestead does not exceed 80 percent of the fair market value of the homestead on the date the extension of credit is made; ...*

7. *a reverse mortgage;...*

Chapter 4

Credit Constraints and Intertemporal Consumption: Theory and Evidence from a Credit Reform in Texas

4.1 Introduction

The passage of a 1997 credit reform in Texas, described in details in chapter 3, has removed severe restrictions on the ability of homeowners to use home equity gains for consumption loans. In chapter 3, I use a differences-in-differences approach comparing the change in spending from before to after 1997 for Texas and a control group, and I find sizable positive aggregate spending effects of the reform. Such effects can be interpreted as evidence for the relaxation of borrowing constraints, but with the following caveat. While such spending effects highlight the potential importance of the passage of the reform for consumption dynamics in Texas, they only provide reduced form evidence or partial equilibrium effects, and bear no clear relationship to a well known structural model. In order to predict the effect of a policy experiment, one should model the deep parameters that are related to preferences and resource constraints governing individual behavior.

Hence, a more adequate approach, and one I follow here, is to investigate the extent to which a general equilibrium model with financial frictions can explain the aggregate spending effects of the Texas reform. A general equilibrium model with heterogenous agents (constrained and unconstrained) and a framework specifying the link through which home equity gains are transferred into higher borrowing and higher spending, permits a better understanding and interpretation of the effects of the Texas credit reform. I embed these considerations into a simple model of aggregate consumption in an economy with collateralized constraints tied to housing values, as in Iacoviello (2004). As pointed out by Dynan, Elmendorf and Sichel (2006), financial innovation enhances the ability to borrow and thus reduces the fraction of credit constrained households in the economy. The 1997 credit reform in Texas is a form of financial innovation that increased credit availability.[1] Therefore, its passage will reduce the extent to which credit constraints bind and increase borrowing by constrained households. To test this hypothesis, I derive a properly specified microfounded aggregate consumption Euler equation for Texas, test it and identify its structural parameters across two subperiods, pre 1997 and post 1997. In particular, estimating the share of credit constrained households in total consumption and the loan-to-value ratio across both subperiods permits inference about the overall importance of the housing collateral effects induced by the passage of the credit reform.

The main results are as follows: I find compelling evidence for an effect of the credit reform. In particular, I find that the fraction of total consumption accruing to constrained households has decreased after the passage of the reform, and that this smaller fraction is more than offset by an increase in the loan-to-value ratio. The estimates suggest that the credit reform has induced a strong feedback from collateral values to consumption dynamics through the effect that it has generated on borrowing.

There is a large body of literature linking liquidity constraints to consumption and using intertemporal consumption Euler equations to estimate the structural parameters of a model (See, Campbell and Mankiw 1989, Jappelli and Pagano 1989, Iacoviello 2004). My paper differs from existing studies in important ways. First, as in Iacoviello (2004), I explicitly link liquidity constraints to observable variables. Second, I evaluate the importance of a novel policy experiment – the Texas credit reform – quantitatively based on a dynamic general equilibrium model. Third, most studies on liquidity constraints have kept the assumption that the structural parameters remain constant throughout the sample period. This is potentially misleading if shocks hitting liquidity constraints are big enough to induce shifts in these

[1] As argued in Bernanke (2007), access to homeowners net worth affects their borrowing and spending because it affects costs of credit. As in Iacoviello (2004,2005), the availability of credit is affected in my setup, rather than the cost of credit, but the same intuition carries over.

parameters over time. The Texas credit reform provides an exogenous source of variation in the data, allowing me to relax this assumption.

4.2 The Theoretical model

I consider a discrete time, infinite horizon economy populated by two groups of households: patient households that lend in equilibrium and impatient households that borrow in equilibrium. There is a continuum of measure 1 of agents in each of the two groups, and the economic size of each group is measured by its consumption share. Households receive an exogenous perishable endowment and have preferences defined over consumption, and housing.[2] They can trade houses, consumption goods and a riskless bond. Aggregate housing is in constant supply. However, housing prices will fluctuate due to shifts in housing demand across the two groups of households and to the allocation of housing between them. The model follows closely the one used in Iacoviello (2004).

4.2.1 Patient households

Within the patient households group (denoted by p), a representative agent maximizes

$$E_0 \left[\sum_{t=0}^{\infty} \beta^t \left(log\ C_t^p + \kappa\ log\ H_t^p \right) \right]$$

where E_0 denotes the expectation operator, C_t^p and H_t^p denote consumption and housing services, respectively. β is the discount factor, and $\kappa > 0$ is the relative weight on housing services in the utility function. The flow of funds, in real terms, is

$$C_t^p + Q_t \left(H_t^p - H_{t-1}^p \right) + R_{t-1} B_{t-1}^p = B_t^p + Y_t^p \tag{4.1}$$

[2] There is no aggregate saving in the model of this paper, so total consumption always equals total income, which is exogenous.

The patient household chooses consumption C_t^p, and housing H_t^p (priced at Q_t) to maximize her utility subject to (1). She receives a random endowment Y_t^p, lends to impatient households in real terms $-B_t^p$ and receiving back $R_{t-1}B_{t-1}^p$, taking R_{t-1}, the real interest rate paid on loans made between $t-1$ and t as given. Housing is treated as a durable good that never depreciates. The Lagrangian is

$$\Lambda^p = E_t \, log\Big(B_t^p + Y_t^p - Q_t \left(H_t^p - H_{t-1}^p\right) - R_{t-1}B_{t-1}^p\Big) + \beta \; E_t \, log\Big(B_{t+1}^p + Y_{t+1}^p - Q_{t+1}\left(H_{t+1}^p - H_t^p\right) - R_t B_t^p\Big) + \kappa \; log H_t^p + \beta \; \kappa \; E_t log H_{t+1}^p + ...$$

The first-order conditions with respect to respectively B^p and H^p gives the patient household's consumption (2) and housing demand (3)

$$\frac{1}{C_t^p} = \beta E_t \left(\frac{R_t}{C_{t+1}^p}\right) \tag{4.2}$$

$$\frac{1}{C_t^p} Q_t = \kappa \frac{1}{H_t^p} + E_t \left(\frac{\beta Q_{t+1}}{C_{t+1}^p}\right) \tag{4.3}$$

4.2.2 Impatient households

There is a fraction ξ of households who are impatient in the economy. An impatient household receives a random endowment Y_t^i, and borrows from the patient household. Her maximum borrowing is given by the expected present value of her housing wealth times the loan-to-value (LTV) ratio χ. The real obligations $R_t B_t^i$ are thus limited by

$$R_t B_t^i \leq \chi E_t \left(Q_{t+1} H_t^i\right) \tag{4.4}$$

This form of borrowing limit is standard in the macro literature (See Kiyotaki and Moore, 1997), and could arise for instance due to liquidation costs: in case of default, legal and other costs amount to a fraction $1-\chi$ of the value of the house. Within the impatient households group (denoted by i), a representative agent maximizes

$$E_0 \left[\sum_{t=0}^{\infty} \beta'^t \left(ln\ C_t^i + \kappa\ ln\ H_t^i \right) \right]$$

where $\beta' < \beta$, so that impatient households discount the future more heavily than the patient ones. The flow of funds, in real terms, is

$$C_t^i + Q_t \left(H_t^i - H_{t-1}^i \right) + R_{t-1} B_{t-1}^i = B_t^i + Y_t^i \tag{4.5}$$

Without loss of generality, following Iacoviello (2004), I assume that impatient households do not give any weight to the future, so that $\beta' = 0$. Otherwise, they share their preferences with the patient households. The Lagrangean is

$$\Lambda^i = E_t\ log\Big(B_t^i + Y_t^i - Q_t \left(H_t^i - H_{t-1}^i \right) - R_{t-1} B_{t-1}^i \Big) + \kappa\ log H_t^i - \Phi_t\ E_t \left(R_t B_t^i - \chi\ Q_{t+1} H_t^i \right)$$

The first-order conditions with respect to respectively B^i and H^i gives the impatient household's consumption (6), and housing demand (7)

$$\frac{1}{C_t^i} = R_t \Phi_t \tag{4.6}$$

$$\frac{1}{C_t^i} Q_t = \kappa \frac{1}{H_t^i} + E_t \left(\chi \Phi_t Q_{t+1} \right) \tag{4.7}$$

where Φ is the Lagrange multiplier on the borrowing constraint. Without uncertainty, the assumption $\beta' < \beta$ guarantees that impatient households are constrained in and around the steady state. This follows from the fact that the steady-state consumption Euler equation for the patient household implies that $R = \frac{1}{\beta}$, the household time preference rate. Combining the latter result with the steady-state consumption Euler equation of the impatient household yields $\Phi = \frac{\beta}{C^i} > 0$. Therefore, the borrowing constraint will hold with equality. However, when there is uncertainty, there might be periods in which the borrowing constraint is not necessarily binding. Specifically, in good states of the world, the precautionary saving motive might outweigh impatience as impatient households will try to restore some assets, borrowing less than the limit to insure against bad income shocks. In this paper, I take as given that uncertainty is small enough relative to the degree of impatience so as to rule out this possibility. As long as the impatient households do not give *any* weight to the future, they end up being constrained in equilibrium.

Note that for the impatient household, the consumption Euler equation holds with the addition of Φ, the Lagrange multiplier on the borrowing constraint. This implies that the current marginal utility of the impatient household's consumption is affected by the shadow value of the borrowing constraint. There is a distortion in housing demand since housing can be used as collateral. It is therefore straightforward to consider the implications of fluctuations in the expected value of housing wealth for impatient households. Along the equilibrium path, such fluctuations affect the borrowing and spending capacity of such households: the effect is larger the larger the loan-to-value ratio χ, since χ measures, ceteris paribus, the liquidity of housing wealth. Note that the 1997 Texas credit reform is akin to a shock to χ. Evidently, such shock leads to a protracted increase in debt and consumption.

Next, I consider the implications of the optimality conditions in this simple model for the purpose of deriving a relationship between house prices and aggregate consumption.

4.2.3 The Euler equation for aggregate consumption

Here, I linearize the model around the deterministic steady state. Let lower-case letters denote percentage deviations from the steady state. Linearizing the first order condition in (2) delivers, after neglecting second-order terms, the standard Euler consumption equation stating that consumption of the patient household is negatively affected by unexpected surprises to the real interest rate:

$$c_t^p = E_t c_{t+1}^p - r_t \tag{4.8}$$

Combining the first order conditions in (6) and (7) gives

$$\kappa \frac{C_t^i}{H_t^i} = Q_t - E_t \left(\frac{1}{R_t} \chi Q_{t+1} \right) \tag{4.9}$$

Linearizing (9) delivers the consumption Euler equation for the impatient household

$$c_t^i = (1+\omega)q_t - \omega E_t q_{t+1} + \omega r_t + h_t^i \tag{4.10}$$

where $1+\omega = \frac{1}{1-\chi\beta}$ is the inverse of the downpayment needed to purchase one unit of housing.[3] Having derived the consumption Euler equations for the patient and impatient households, the final step is to obtain the aggregate Euler equation. Let aggregate consumption c_t be as follows

$$c_t = \alpha c_t^i + (1-\alpha)c_t^p \tag{4.11}$$

where α denotes the consumption share of impatient households. Note that, as emphasized in Iacoviello (2004), the consumption shares of each group of households do not necessarily correspond to the mass of each group in the economy. Therefore, α does not identify the fraction of impatient households in the economy, but the fraction of consumption in total output of this group. This distinction is crucial because heterogeneity implies that income distribution matters in this model. Substituting (4.8) and (4.10) in (4.11) yields

$$c_t = \alpha\big((1+\omega)q_t - \omega E_t q_{t+1} + \omega r_t + h_t^i\big) + (1-\alpha)\big(E_t c_{t+1}^p - r_t\big) + \upsilon_t \tag{4.12}$$

One problem with the above expression is that the conditional expectation of the patient household's consumption, c_{t+1}^p, is not observed. However, rational expectations imply

$$E_t c_{t+1}^p = c_{t+1}^p + \epsilon_{t+1}^{c^p}$$

$$E_t q_{t+1} = q_{t+1} + \epsilon_{t+1}^q$$

where $\epsilon_{t+1}^{c^p}$ and ϵ_{t+1}^q are forecast error terms. Making the above rational expectations assumptions explicit gives

$$\Delta c_t^p = r_t + \epsilon_t^p \tag{4.13}$$

[3] In the framework presented, I will not be able to identify β and χ separately from the data. From now on, I aim at identifying $\chi\beta$ together.

$$c_t^i = (1+\omega)q_t - \omega\Delta q_{t+1} + \omega r_t + h_t^i + \epsilon_t^i \tag{4.14}$$

We can express equation (4.12), the aggregate consumption Euler equation, in first difference form as follows

$$\Delta c_t = \alpha\Delta c_t^i + (1-\alpha)\Delta c_t^p \tag{4.15}$$

Substituting (4.13) and (4.14) into (4.15) yields, after appropriately rearranging terms, to an approximate Euler equation for the aggregate economy, expressing aggregate consumption growth as a function of observable variables

$$\Delta c_t = \psi_1\left(\Delta q_t + \Delta h_t^i\right) + \psi_2\left(\Delta r_t - \Delta\Delta q_{t+1}\right) + \psi_3 r_t + \zeta_t \tag{4.16}$$

where $\Theta = \{\psi_1, \psi_2, \psi_3\}$ are functions of the underlying structural parameters. I can freely estimate $p = 2$ parameters: α, the share of consumption accruing to impatient\constrained households, and $1+\omega$, the inverse of the downpayment needed to purchase a house. These structural parameters can be recovered using the following relationships: $\alpha = \psi_1$, and $\omega = \frac{\psi_2}{\psi_1}$. The error term ζ_t is a linear combination of the structural shock υ_t from equation (4.12) and the rational expectations forecast error terms. Note that equation (4.16) is essentially the same as equation (4.19) in Iacoviello (2004).[4] It is not a consumption function but an equilibrium condition that holds regardless of the structure of the economy since it is derived only from a simple intertemporal optimization problem. As pointed out by Hansen and Singleton (1982), complete specification of the model is not necessary here; the stochastic process for income need not be specified nor the process for consumption determined. Knowledge of the Euler equation is sufficient for estimating the model.

All we need at this point is the assumption of rational expectations, which imply that one can use mathematical expectations in lieu of the agents expectations; Therefore, the

[4] Iacoviello (2004) derives the aggregate Euler equation in a slightly different way. He replaces the conditional expectation of the patient households consumption by the long term interest rate. He also assumes a more general functional form for housing demand.

aggregate consumption Euler equation can be used to form the orthogonality condition

$$E_t\left\{\zeta_t(\Theta)\right\} = 0, \quad t = 1, ..., T \tag{4.17}$$

Where

$$\zeta_t\left(\Theta\right) = \Delta c_t - \psi_1\left(\Delta q_t + \Delta h_t^i\right) - \psi_2\left(\Delta r_t - \Delta\Delta q_{t+1}\right) - \psi_3 r_t$$

One orthogonality condition is, of course, not enough to estimate $p = 2$ parameters. We need at least as many orthogonality conditions as free parameters. But again, these are also delivered by the theoretical model. Under rational expectations, the error in the forecast of $\Delta\Delta q_{t+1}$ should be uncorrelated with information dated t and earlier. It follows that

$$E_t\left\{\zeta_t(\Theta) z_t\right\} = 0, \quad t = 1, ..., T \tag{4.18}$$

where z_t denotes the $n \times 1$ vector of variables in the agents' information sets at time t satisfying $E_t[\upsilon_t z_t] = 0$, and T is the number of observations. The above n moment conditions are the basis for estimating the model via the Generalized Method of Moments (GMM) proposed in Hansen (1982). The estimator is given by

$$\Theta = \underset{\boldsymbol{\Theta}}{\operatorname{argmin}}\left(\frac{1}{T}\sum_{t=1}^{T}\zeta_t(\Theta)z_t\right)' W_T\left(\frac{1}{T}\sum_{t=1}^{T}\zeta_t(\Theta)z_t\right) \tag{4.19}$$

where W_T is a heteroscedasticity and autocorrelation consistent positive semi-definite weighting matrix. If there are more moment conditions than parameters (i.e., $n > p$), the model is over-identified. A test of the over-identifying restrictions is based on the j-test statistic of Hansen (1982)

$$j = \left(\frac{1}{\sqrt{T}}\sum_{t=1}^{T}\zeta_t(\widehat{\Theta})z_t\right)' \widehat{S}_T^{-1}\left(\frac{1}{\sqrt{T}}\sum_{t=1}^{T}\zeta_t(\widehat{\Theta})z_t\right) \to \chi^2(k) \tag{4.20}$$

where $\widehat{S}_T^{-1}$ is the heteroscedasticity and autocorrelation consistent estimate of the long-run covariance matrix of the sample moments, and the scalar k denotes the number of over-identifying restrictions.

4.3 Empirical methods

4.3.1 Data and sample design

I use quarterly Texas data over the time period 1986:I-2010:I. By restricting my sample to start in 1986, I avoid periods of high oil price volatility and the major housing bubble of the early 1980s in Texas. This episode also coincides with the passage of the Tax Reform Act of 1986 (TRA 86) which altered the relative price of mortgage borrowing by eliminating tax-deductability of interest on all credit except mortgage debt for households. After 1986 the housing finance markets in the entire US have largely moved to a system where mortgage institutions are less regulated.

A shortcoming here is the unavailability of measures for consumption expenditures by households at the state-level. Retail sales are a common proxy as they are an important subset of consumer expenditures. Hence, as a measure of consumption, I use the log change in real retail sales per capita.[5] The real short-term interest rate is the difference between the quarterly 3-month treasury bill rate and the quarter-on-quarter change in the log of the GDP deflator. The real housing price (logged and first differenced) is the OFHEO repeat sales house price index. As a proxy for housing demand, I follow Iacoviello (2004) and use total residential permits per capita (logged and first differenced).[6] Implicitly, this assumes that most of the variation in housing demand is due to variations in the housing demand of constrained households. As noted in Iacoviello (2004), this assumption is plausible if most of the investment in housing at the margin is done by first-home buyers who are typically constrained. All real values denote nominal values deflated by the GDP deflator. I obtain the data from the database of the Federal Reserve Bank of Saint Louis (FRED). To get per

[5]Unfortunately, even retail sales are not *directly* observable at the state level. However, I can impute retail sales for Texas using data on quarterly states sales tax revenues and sales tax rates, as in Garrett, Hernàndez-Murillo and Owyang (2005). The resulting series is essentially the same as the one used in the analysis of chapters 2 and 3 of the dissertation.

[6]Iacoviello (2004) used total residential housing investment. This data is not available at the state-level, so I use total residential housing permits as a proxy instead.

capita values, I divide by state population estimates from the Current Population Survey (CPS). The set of instruments z_t is described below.

4.3.2 Empirical model

The orthogonality condition I choose is the one of equation (4.18). The specification takes the form:

$$E_t \left\{ \left(\Delta c_t - \alpha \left(\Delta q_t + \Delta h_t \right) - \alpha\omega \left(\Delta r_t - \Delta\Delta q_{t+1} \right) - (1-\alpha) r_t \right) z_t \right\} = 0 \tag{4.21}$$

I estimate the above equation using two alternative sets of instruments. In order to take into account the first order moving average term in the errors, I use variables dated $t-2$ and earlier.[7] In column (1), the instruments are three lags of each of the growth rate in house prices Δq, the growth rate in housing investment Δh, and the growth rate in disposable income Δy. I also use one lag of the ratio of consumption over disposable income *coy*. In column (2), I use these plus one lag of the growth in oil prices Δop. In each case, the set of instruments include current values of the interest rate variables Δr and r. The latter assumes that the interest rate variables are exogenous in the aggregate consumption Euler equation for Texas. Fluctuations in interest rates are mainly influenced by monetary policy, and so it is reasonable to assume that they are independent of the Texas macro variables.

First, I estimate the equation over the full sample period. Next, I present subsample estimates that allow me to infer about the overall importance of the Texas credit reform.

4.4 Empirical results

4.4.1 Full sample estimates

Estimates of the structural parameters for the full sample and their standard errors are reported in Table 4.1. All regressions include a constant term (not reported) which captures

[7] See Campbell and Mankiw (1989) for a discussion on how this helps to mitigate time aggregation worries.

higher order terms related to precautionary savings motives of the consumers and approximation errors due to linearization. I recover direct estimates of α, the share of consumption accruing to constrained agents, and $(1+\omega)$, the approximate inverse of the downpayment needed to purchase a house. The implied estimate of α ranges from 0.27 to 0.32 and is significantly different from zero. Interestingly, these numbers are within the range of the various studies that have estimated from consumption Euler equations the fraction of rule-of-thumb/constrained agents in the US (See, Campbell and Mankiw 1989, Iacoviello 2004, Iacoviello 2005, Iacoviello and Neri 2010). At first, this may seem surprising in light of the fact that, over most of the sample and when the Texas credit reform passed, no other US state imposed severe restrictions on the access to home equity for discretionary household spending. Given that in most economies housing equity represents the major form of collateral for borrowing, one should expect, a priori, households in Texas to be significantly more constrained than households in the average US state. However, the existence of legal restrictions on lending or borrowing need not necessarily imply that credit constraints bind and distort optimal consumption paths. For instance, Texas citizens, prior to the 1997 credit reform, had the opportunity to take cash out when refinancing their current home mortgages, and thus were able to tap indirectly into their home equity gains for general spending purposes.

The model also provides an estimate of how constrained households can increase their spending in the short-run by borrowing against the value of their housing collateral. The estimate of $\omega = \frac{\chi\beta}{1-\chi\beta}$ is between 2.28 and 3.25, but is not precisely estimated. These estimates can be used to back out χ, the implied loan-to-value ratio over the full sample period. Assuming $\beta = 0.99$, as standard in the real business cycle literature, the implied estimate of χ is comprised between 0.70 and 0.77.

So far, I have kept the assumption that the structural parameters were constant throughout the entire sample. The latter does not account for the potential structural shifts induced by the 1997 credit market reform. Market innovations following credit reforms affect housing markets by enhancing the ability of households to borrow, and thus have the potential to reduce the fraction of credit constrained households in the economy (See, Dynan, Elmendorf, and Sichel 2006). Campbell and Hercowitz (2005) argue that these market innovations also drastically reduce the equity requirements associated with collateralized borrowing. Hence, my hypothesis is that the credit reform has increased credit availability and as a result spurred the spending of constrained households in Texas. By nature of the Texas policy experiment, there may have been changes in the time series properties of the data from before to after 1997, and such changes are potentially informative for the issue at hand. I exploit these changes in order to assess the impotence of the effects of the credit reform, namely the potential spillover effects on the consumption demand of households.

Next, I perform a sample split analysis, relaxing the assumptions implied by the constant parameter model. My aim is to estimate the deep parameters of the model in each subsample and see how they are affected by the structural shifts brought by the 1997 credit reform. If the credit reform has truly relaxed credit constraints of households in Texas, we expect, *ceteris paribus*, a lower share of credit constrained households in total consumption and a higher loan-to-value ratio in the later period.

4.4.2 Subsamples estimates

I now redo the exercise so that I can recover direct estimates of the structural parameters of the model over the two subsamples. The first subsample starts in 1986:I and ends in 1997:IV. The second starts in 1998:I, in line with the passage of the Texas legislative change that became effective 1998:I, and ends in 2010:I. This way, we have two samples of approximately equal length.

Subsample estimates of the structural parameters and their standard errors are reported in Table 4.2. In the period which precedes the passage of the 1997 credit reform, the implied estimate of α, the fraction of consumption accruing to constrained agents, ranges from 0.24 to

0.30. In the later period, this number decreases, ranging from 0.13 to 0.14. On the other hand, the estimate of $\omega = \frac{\chi\beta}{1-\chi\beta}$ is between 1.69 and 2.52 in the first period; while economically non-trivial the estimate is statistically indistinguishable from zero given its large standard errors. Assuming $\beta = 0.99$, the estimates imply loan-to-value ratios ranging from 0.64 to 0.72. In the later period, however, the estimate of ω is between 13.82 and 14.71, and is very precisely estimated. This implies that the loan-to-value ratio after the passage of the credit reform is around 0.95. The differences in loan-to-value ratio estimates across subperiods are in line with reasonable priors, given that before the passage of the credit reform in Texas, equity withdrawal was severely restricted. Hence, the smaller share of credit-constrained agents in the later period is coupled with a significantly larger loan-to-value ratio. This provides support for the claim that the 1997 credit reform has relaxed borrowing constraints. The estimates potentially imply a strong feedback from collateral values to spending dynamics, through the effect that the credit reform has generated on borrowing.

4.4.3 Robustness analysis

The model works well and fits the data, in the sense that I do not reject the overidentifying restrictions.[8] Table 4.2 also shows ρ, the spearman rank correlation between actual consumption growth and the one-step-ahead forecast, as well as the root mean squared error (RMSE) of the estimating equation. This allows me to further inspect the goodness-of-fit of the model across both periods. Results suggest that the model has significantly more predictive power for consumption dynamics in the later period. The correlation between actual consumption growth and its one-step-ahead forecast resulting from the aggregate Euler equation estimation is around 0.50 in the later period. The root mean squared error (RMSE) of the one-step ahead predictions is also significantly lower in the later period. Overall, this could reflect that the housing collateral effects induced by the 1997 credit reform have helped in reasonably

[8] The point estimates, however, tend to be slightly sensitive to the choice of the exact starting sample period.

explaining and predicting consumption dynamics in Texas, by increasing the sensitivity of spending growth to housing price growth.

I now perform a robustness exercise. I consider a variable that, under certain circumstances, can have predictive power for consumption growth, namely the change in the Index of Consumer Sentiment (ICS) from the University of Michigan's consumer sentiment survey (Δics).[9] Several studies have argued that consumer sentiment has predictive power for consumption growth beyond what could be expected from the usual models of consumption dynamics. This variable might capture changes in economic uncertainty that act as shifters of the marginal utility of consumption.[10] Carroll, Fuhrer, and Wilcox (1994) find that the University of Michigan's Index of Consumer Sentiment is an important variable for explaining consumption growth in the US. Acemoglu and Scott (1994) report similar finndings for consumer sentiment in the United Kingdom. I add this variable as an additional regressor to the baseline specification as follows

$$\Delta c_t = \alpha \left(\Delta q_t + \Delta h_t^i\right) + \alpha\omega \left(\Delta r_t - \Delta\Delta q_{t+1}\right) + (1-\alpha) r_t + \sigma \Delta ics + \zeta_t \tag{4.22}$$

Table 4.3 reports the results. Since the estimates do not change much across specifications, I only report the results using the instruments of column (2) of Table 4.2. The coefficient on the change in the Index of Consumer Sentiment, σ, is negative in the first period and positive in the later period. However, it is consistently statistically insignificant. Most importantly, the inference from the other parameters is in line with the baseline results, although the estimate are somewhat less precisely estimated.

4.4.4 Quantifying the spillovers from the credit reform

In principle, the 1997 credit reform can affect housing demand and housing prices. To gain insight into this, we can study equations (4.3) and (4.7), which determine the equilibrium

[9] A consumer sentiment index is not available at the state-level. However, the ICS report provides sentiment indices by geographic regions. According to the Census Bureau's definition, Texas is part of the southern region. I then use data for the Index of Consumer Sentiment (ICS) for the south.

[10] Iacoviello (2004) uses the change in unemployment expectations from the University of Michigan's consumer sentiment survey as a proxy instead.

behavior of housing demand and consumption for patient and impatient households, respectively. Obviously, the housing demand of patient households is not affected by the credit reform. For impatient households, the credit reform, which is akin to a persistent shock to the loan-to-value ration χ, lowers the user cost and thus leads to an increase in housing prices. But If houses are perceived as being fungible wealth, this increases in the value of housing will also increase lifetime consumption of patient households, owing to an increase in their intertemporal budget constraints.[11] Hence, if pure housing wealth effects are plausible, the observed increase in spending in Texas reflects not only the increase in spending by constrained households, but also the increase in the spending of unconstrained households.

The question is: how much of the observed increase in spending growth in Texas, from before to after 1997, is due to the relaxation of borrowing constraints that is induced by the passage of the credit reform? To answer this question, I first predict spending growth over 1998-2010, using the parameter values obtained from estimating the model over the first subsample. The latter is like a *counterfactual*, since it is a prediction of spending growth in the absence of the passage of the credit reform. Then I subtract this from a prediction of spending growth over the same time period, using the parameter values obtained from estimating the model over the second subsample. The resulting series measures the contribution of the credit reform to predicted consumption growth. Table 4.4 presents the contribution of the collateral effects induced by the passage of the credit reform to consumption growth. The spending growth due to the direct collateral effects of the reform (i.e., the predicted spending growth net of the *counterfactual* predicted spending growth) is around 8.4% (annualized, average over 1999-2000). Comparing this number to the corresponding actual spending growth of 11.7% over the same period suggests that around 73% of the observed increase in spending growth in Texas was due to the passage of the credit reform. Hence, there is a non-negligible

[11]Such pure wealth effects are however weakened to the extent that households plan to continuously occupy houses that increase in value: while their total wealth rises with house prices, their net wealth does not since the implicit cost of living in them rises as well. This point has been stressed by Buiter (2008), Sinai and Souleles (2005), and Iacoviello (2011), among others.

feedback from home values to consumption growth even in presence of a shrinking group of constrained agents in the later period.

4.5 Summary and conclusion

In this paper, I derive an aggregate Euler consumption equation in which the consumption of collateral constrained households is dependent on their home values. I then estimate the structural parameters over two subsamples, in an effort to assess the importance of the 1997 credit reform for consumption dynamics in Texas. I find compelling evidence for an effect of the credit reform. The effect is the combined result of two findings: I estimate a larger share of constrained households in the Euler equation in the earlier period (between 24 and 30 percent of total consumption), compared to the later period (between 13 and 14 percent of total consumption); second, I estimate a strong feedback from collateral values to consumption dynamics, through the effect that the credit reform has generated on borrowing.

A drawback of the simple approach presented in this paper is that the aggregate consumption Euler equation is based on optimality conditions, and thus it is a necessary but not a sufficient condition for an optimal dynamic path. It is not a consumption function, and in this sense I cannot say much about how the *level* of spending in Texas responds to the 1997 credit reform. However, my focus in this paper is on *whether* and *how* the credit reform affected credit constraints and the availability of credit in Texas; and my estimates – which are essentially based on how the *change* in spending relates to the *change* in housing prices, seem to give a reasonable answer to that question.

Table 4.1: FULL ESTIMATES OF THE CONSUMPTION EULER EQUATION

Estimates	(1)	(2)
α	0.32	0.27
	(3.04)	(2.55)
ω	3.25	2.28
	(0.97)	(0.68)
χ	0.77	0.70
Instruments		
	$\Delta q_{t-2},...,\Delta q_{t-4}$	$\Delta q_{t-2},...,\Delta q_{t-4}$
	$\Delta h_{t-2},...,\Delta h_{t-4}$	$\Delta h_{t-2},...,\Delta h_{t-4}$
	$\Delta y_{t-2},...,\Delta y_{t-4}$	$\Delta y_{t-2},...,\Delta y_{t-4}$
	Δr_t	Δr_t
	r_t	r_t
	coy_{t-2}	coy_{t-2}
		Δop_{t-2}
j-statistic	0.56	0.49
$RMSE$	0.06	0.05
ρ	0.19	0.19

The table reports GMM estimates of the structural parameters of equation (21). Estimates are based on quarterly data over the full sample period. z_t is the vector of instruments. χ is the loan-to-value ratio that is implied by the estimate of ω. Heteroscedasticity and autocorrelation consistent t-statistics are reported in parentheses. Autocovariances are weighted using the optimal Parzen kernel, computed using the Method of Bandwidth Selection proposed in Newey and West (1987).The last row reports the p value associated with Hansen's (1982) j test of the model's overidentifying restrictions. $RMSE$ is the one-step-ahead root mean squared prediction error. ρ denotes the Spearman rank correlation coefficient between the one-step-ahead forecast of consumption growth and actual consumption growth. The GMM results reported are obtained using the iterated GMM estimator.

Table 4.2: Subpsample estimates of the consumption Euler equation

	1986:I - 1997:IV		1998:I - 2010:I	
Estimates	(1)	(2)	(1)	(2)
α	0.30	0.24	0.13	0.14
	(1.92)	(1.80)	(2.00)	(2.15)
ω	2.52	1.69	14.71	13.82
	(0.57)	(0.33)	(2.34)	(2.41)
χ	0.72	0.64	0.95	0.94
Instruments				
	$\Delta q_{t-2}, ..., \Delta q_{t-4}$	$\Delta q_{t-2}, ..., \Delta q_{t-4}$	$\Delta q_{t-2}, ..., \Delta q_{t-4}$	$\Delta q_{t-2}, ..., \Delta q_{t-4}$
	$\Delta h_{t-2}, ..., \Delta h_{t-4}$	$\Delta h_{t-2}, ..., \Delta h_{t-4}$	$\Delta h_{t-2}, ..., \Delta h_{t-4}$	$\Delta h_{t-2}, ..., \Delta h_{t-4}$
	$\Delta y_{t-2}, ..., \Delta y_{t-4}$	$\Delta y_{t-2}, ..., \Delta y_{t-4}$	$\Delta y_{t-2}, ..., \Delta y_{t-4}$	$\Delta y_{t-2}, ..., \Delta y_{t-4}$
	Δr_t	Δr_t	Δr_t	Δr_t
	r_t	r_t	r_t	r_t
	coy_{t-2}	coy_{t-2}	coy_{t-2}	coy_{t-2}
		Δop_{t-2}		Δop_{t-2}
j-statistic	0.77	0.76	0.46	0.56
$RMSE$	0.07	0.06	0.04	0.04
ρ	0.05	0.05	0.50	0.50

The table reports GMM estimates of the structural parameters of equation (21) across two subsamples. Estimates are based on quarterly data over two subperiods. See notes to Table 1.

Table 4.3: Robustness analysis

	1986:I - 1997:IV	1998:I - 2010:I
Estimates		
α	0.33	0.10
	(2.22)	(1.63)
ω	2.87	18.28
	(0.71)	(1.74)
σ	-0.22	0.06
	(-1.29)	(0.40)
χ	0.75	0.96
j-statistic	0.57	0.55
$RMSE$	0.07	0.04
ρ	0.13	0.49

The table reports GMM estimates of the structural parameters of equation (21). Estimates are based on quarterly data over two subperiods. The specification adds the change in the Index of Consumer Sentiment (ICS) to the set of regressors. See notes to Table 1.

Table 4.4: SPENDING EFFECTS OF THE CREDIT REFORM

	$(X = \Delta C^c)$	$(X = \Delta C^a)$	$(\Delta C^c/\Delta C^a)$
X_{1999}	0.088	0.224	0.39
$[(X_{2000} + X_{1999}]/2$	0.084	0.117	0.73

The spending effects of the credit reform. ΔC^a is the actual year-on-year consumption growth. ΔC^c is the predicted year-on-year consumption growth net of the *counterfactual* spending growth, computed as explained in the text.

Chapter 5

Conclusion

This dissertation examines the effects of house prices on household behavior from an empirical point of view. The relationship between house values and household expenditures and saving is important given the fluctuations in house prices that occur over time. The papers in this dissertation contribute to understanding how house prices affect households behavior as well as the implications of changing house prices on aggregate non-housing consumption expenditures. I present evidence conclusions on the spill-over of housing markets on the real economy using careful analysis.

In particular, the dissertation shows that housing wealth impacts household spending through its role as borrowing collateral. I find that there does not seem to be solid support for the claims that households feel richer (poorer) as house prices rise (fall) and they adjust their consumption accordingly; to wit, the so-called direct or net *housing wealth effects* do not seem to hold in a careful analysis of the data. Instead, changing house values impact the spending of households who face binding constraints. The so-called *housing collateral effect* simply follows from the fact that when the homeowner experiences an increase in current or expected housing wealth, the value of the collateral she can offer is higher, and thus she can borrow more to finance extra spending.

The results in Chapter 2 suggest that there is a great deal of cross-state heterogeneity in the effects of house prices on spending, and this heterogeneity is at least partly explained by different levels of mortgage market development across states. This provides support for recent arguments that the main channel through which housing wealth affect non-housing consumption is through affecting collateralized constraints. Chapter 3 examines a legislative change Texas, lifting severe restrictions on the ability of homeowners to use home equity as collateral for consumption loans. Results suggest that such credit market reform had sizeable and significant effects on overall spending in Texas, providing support for claims that credit constraints are important and can matter for the aggregate economy. Chapter 4 develops a simple two-agent dynamic general equilibrium model in which home values affect the debt capacity and consumption possibilities of a subset of households, with the aim to estimate the effects of the 1997 Texas credit reform. It then derives a properly specified microfounded aggregate consumption Euler equation for Texas, tests it and identifies its structural parameters across two subperiods, pre 1997 and post 1997. The findings suggest that the fraction of total consumption accruing to constrained households has decreased after the passage of the reform, and that this smaller fraction is more than offset by an increase in the loan-to-value ratio. The estimates suggest that the credit reform has induced a strong feedback from collateral values to consumption dynamics through the effect that it has generated on borrowing.

One important line of research is to consider the relationship between housing markets dynamics and business investment. Financing productive business ventures is one of the most important functions of financial markets. Unlike large established firms, small firms and entrepreneurs do not have access to external capital markets. In this sense small firms and entrepreneurs may potentially face binding constraints. Personal wealth (e.g., housing, pensions) can serve as an important source of collateral for these credit-constrained, small business owners. The evidence on the link between personal wealth and entrepreneurial activity is mixed; some studies find that the relationship between personal wealth and entrepreneurial activity is important while others find weaker evidence for such a link. Much work remains, in order to fully understand how personal wealth impact the binding constraints of entrepreneurs. The Texas natural experiment examined in the context of household spending in Chapter 3, provides a unique opportunity to estimate how lifting borrowing constraints affect entrepreneurship or small business activity in Texas relative to other states in the US. The economic response to this unique event in Texas can help us better understand the role of credit markets in the overall economy.

Bibliography

Abadie, A. and J. Gardeazabal (2003, March). The economic costs of conflict: A case study of the basque country. *American Economic Review 93*(1), 113–132.

Acemoglu, D. and A. Scott (1994). Consumer confidence and rational expectations: Are agents' beliefs consistent with the theory? *The Economic Journal 104*, 1–19.

Agarwal, S., C. Liu, and N. S. Souleles (2007, December). The reaction of consumer spending and debt to tax rebates-evidence from consumer credit data. *Journal of Political Economy 115*(6), 986–1019.

Aoki, K., J. Proudman, , and G. Vlieghe (2004). House prices, consumption, and monetary policy: a financial accelerator approach. *Journal of Financial Intermediation 13(4)*, 414–435.

Bai, J. and S. Ng (2002). Determining the number of factors in approximate factor models. *Econometrica 70*, 191–221.

Bennefield, R. L. (2003, May). Home values: 2000. *Census 2000 Brief, US Census Bureau*. www.census.gov/prod/2003pubs/c2kbr-20.pdf.

Bernanke, B., J. Boivin, and P. Eliasz (2005). Measuring monetary policy: A Factor Vector Autoregressive Approach (FAVAR). *Quarterly Journal of Economics 120*, 387–422.

Bernanke, B. S. (2007). The financial accelerator and the credit channel. *Remarks at the Conference on the Credit Channel of Monetary Policy in the Twenty-first Century, Federal Reserve Bank of Atlanta, GA, June 15*.

Bertrand, M., E. Duflo, and S. Mullainathan (2004, February). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Buiter, W. (2008). Housing wealth isnt wealth. *NBER Working paper 14204*.

Calomiris, C., S. Longhofer, and W. Miles (2009). The (mythical?) housing wealth effect. *NBER Working paper 15075*.

Campbell, J. and Z. Hercowitz (2005). The role of collateralized household debt in macroeconomic stabilization. *NBER Working Paper 11330*.

Campbell, J. and G. Mankiw (1989a). Consumption, income and interest rates: Reinterpreting the time series evidence. *NBER macroeconomics annual, ed. by O Blanchard and J Fisher*, 185–216.

Campbell, J. Y. and N. G. Mankiw (1989b). Consumption, income and interest rates: Reinterpreting the time series evidence. In *NBER Macroeconomics Annual 1989, Volume 4*, NBER Chapters, pp. 185–246. National Bureau of Economic Research, Inc.

Cardarelli, R., T. Monacelli, A. Rebucci, and L. Sala (2009). Housing finance, housing shocks, and the business cycle: Evidence from OECD countries. *Unpublished manuscript*.

Carroll, C., J. Fuhrer, and D. Wilcox (1994). Does consumer sentiment forecast household spending? if so, why? *American Economic Review 84(5)*, 1397–1408.

Carroll, C., M. Otsuka, and J. Slacalek (2006). How large is the housing wealth effect? a new approach. *NBER Working paper 12746*.

Carter, C. and R. Kohn (1994). On gibbs sampling for state space models. *Biometrika 81*, 541–553.

Case, K., J. Quigley, and R. Shiller (2005). Comparing wealth effects: The stock market versus the housing market. *Advances in Macroeconomics 5*, 1–34.

Del Negro, M. and C. Otrok (2007). 99 luftballons: Monetary policy and the house price boom across U.S. states. *Journal of Monetary Economics 54:8*, 2269–2290.

Duca, J., D. Gould, and L. Taylor (1998, March/April). What does the asian crisis mean for the U.S. economy? *Southwest Economy* (2).

Dynan, K., D. Elmendorf, and D. Sichel (2006). Can financial innovation help to explain the reduced volatility of economic activity? *Journal of Monetary Economics 53(1)*, 123–150.

Faust, J. (1998). The robustness of identified var conclusions about money. *Carnergie-Rochester Conference Series on Public Policy 48*, 207–244.

Forni, M. and L. Gambetti (2010). The dynamic effects of moentary policy: A structural factor model approach. *Journal of Monetary Economics 57*, 203–16.

Forni, M., D. Giannone, M. Lippi, and L. Reichlin (2009). Opening the black box: Structural factor models with large cross-sections. *Econometric Theory 25*, 1319–1347.

Francis, N., M. T. Owyang, and T. Sekhposyan (2009). The local effects of monetary policy. *Working paper 2009-048A*.

Fry, R. and A. Pagan (2007). Some issues in using sign restrictions for identifying structural vars. *Working Paper No. 14, NCER*.

Garrett, T., R. Hernandez-Murillo, and M. Owyang (2005). Does consumer sentiment predict regional consumption? *Federal Reserve Bank of Saint Louis Review 87(2)*, 123–135.

Geweke, J. and G. Zhou (1996). Measuring the pricing error of the arbitrage pricing theory. *Review of Financial Studies. 9*, 557–587.

Greenspan, A. and J. Kennedy (2005). Estimates of home mortgage originations, repayments, and debt on one-to-four-family residences. *Finance and Economics Discussion Series 2005-41, Board of Governors of the Federal Reserve System.*.

Gross, D. B. and N. S. Souleles (2002, February). Do liquidity constraints and interest rates matter for consumer behavior? Evidence from credit card data. *The Quarterly Journal of Economics 117*(1), 149–185.

Hansen, L. (1982). Large sample properties of generalized method of moment estimators. *Econometrica 50*, 1029–1054.

Hurst, E. and F. Stafford (2004, December). Home is where the equity is: Mortgage refinancing and household consumption. *Journal of Money, Credit and Banking 36*(6), 985–1014.

Iacoviello, M. (2004). Consumption, house prices, and collateral constraints: A structural econometric analysis. *Journal of Housing Economics 13*, 304–320.

Iacoviello, M. (2005). House prices, borrowing constraints and monetary policy in the business cycle. *The American Economic Review 95*, 739–764.

Iacoviello, M. and S. Neri (2010, April). Housing market spillovers: Evidence from an estimated DSGE model. *American Economic Journal: Macro 2*, 125–64.

Imbens, G. W. and J. M. Wooldridge (Summer 2007). Lecture notes.

Jappelli, T. and M. Pagano (1989). Aggregate consumption and capital market imperfections: an international comparison. *American Economic Review 79(5)*, 10881105.

Jappelli, T. and M. Pagano (1994, February). Saving, growth, and liquidity constraints. *The Quarterly Journal of Economics 109*(1), 83–109.

Jarocinski, M. and F. Smets (2008). House prices and the stance of monetary policy. *Federal Reserve Bank of Saint Louis Review 90(4)*, 339–366.

Johansen, S. (1991). Estimation and hypothesis testing of cointegration vectors in gaussian vector autoregressive models. *Econometrica 59(6)*, 1551–1580.

Kennedy, N. and P. Andersen (1994). Household savings and real house prices: An international perspective. *Bank For International Settelments Working paper N 21*.

Kim, C.-J. and C. R. Nelson (2000). *State-space models with regime switching.* MIT Press.

Kiyotaki, N. and J. Moore (1997). Credit cycles. *Journal of Political Economy 105(2)*, 211–248.

Lastrapes, W. (2005). Estimating and identifying vector autoregressions under diagonality and block exogeneity restrictions. *Economics Letters 87*, 75–81.

Lastrapes, W. and D. McMillin (2004). Cross-country variation in the liquidity effect: The role of financial markets. *Economic Journal. 114(498)*, 890–915.

Leeper, E., C. Sims, and T. Zha (1996). What does monetary policy do? *Brookings Papeers on Economic Activity 2*, 1–63.

Leth-Petersen, S. (2010, June). Intertemporal consumption and credit constraints: Does total expenditure respond to an exogenous shock to credit? *American Economic Review 100*(3), 1080–1103.

Lettau and Ludvigson (2004). Understanding trend and cycle in asset values:reevaluating the wealth effect on consumption. *The American Economic Review 94*, 276–299.

Liu, Z., P. Wang, and T. Zha (2009). Do credit constraints amplify macroeconomic fluctuations? *Working paper*.

Ludvingson, S. and S. Ng (2009). Macro factors in bond risk premia. *Review of Financial Studies. 22:12*, 5027–5067.

Mishkin, F. S. (2007). Housing and the monetary transmission mechanism. *FEDS working paper 2007-40, Federal Reserve Board*.

Moench, E. and S. Ng (2010). A factor analysis of housing market dynamics in the u.s. and the regions. *Forthcoming: Econometrics Journal*.

Morgan., J. (2006). Skimming to froth from the punchbowl. *Economic Research (New York)*.

Mulligan, C. (2009). Housing boom and bust: Structures have leveraged claim on housing output. *Working paper*.

Newey, W. K. and K. D. West (1987). A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix. *Econometrica 55(3)*, 703–708.

Paustian, M. (2007). Assessing sign restrictions. *The B.E. Journal of Macroeconomics 7(1)*.

Rapash, D. and J. Strauss (2006). The long-run relationship between consumption and housing wealth in the eight district states. *Federal Reserve Bank of Saint Louis Regional Economic Development 2*, 104–147.

Rubio-Ramirez, J., D. Waggoner, and T. Zha (2007). Structural vector autoregressions: Theory for identification and algorithms for inference. *Federal Reserve Bank of Atlanta Working Paper No. 2008-18*.

Siegel, S. (1956). *Nonparametric Statistics for the Behavioral Sciences.* New York: McGraw-Hill.

Sims, C. A. (1980). Macroeconomics and reality. *Econometrica 48(1)*, 1–48.

Sinai, T. and N. Souleles (2005). Owner-occupied housing as a hedge against risk. *Quarterly Journal of Economics 120*, 763789.

Skelton, E. C. (1997, Third quarter). Will texas voters see equity in home equity lending? *Federal Reserve Bank of Dallas Financial Industry Issues*, 1–6.

Souleles, N. S. (1999, September). The response of household consumption to income tax refunds. *American Economic Review 89*(4), 947–958.

Stephens, M. (2008, 04). The consumption response to predictable changes in discretionary income: Evidence from the repayment of vehicle loans. *The Review of Economics and Statistics 90*(2), 241–252.

Stock, J. and M. Watson (2005). Macroeconomic forecasting using diffusion indexes. *Journal of Business and Economic Statistics 20*, 147–162.

Theil, H. (1971). *Principles of Econometrics*. Wiley.

Uhlig, H. (2005). What are the effects of monetary policy on output? Results from an agnostic identification procedure. *Journal of Monetary Economics 52*, 381–419.

Vargas-Silva, C. (2008a). The effect of monetary policy on housing: A factor-augmented vector autoregression (favar) approach. *Applied Economics Letters 15*, 749 – 752.

Vargas-Silva, C. (2008b). Monetary policy and the us housing market: A VAR analysis imposing sign restrictions. *Journal of Macroeconomics 30*, 977–990.

Wooldridge, J. M. (2002). *Econometric Analysis of Cross Section and Panel Data*. MIT.

Yamashita, T. (2007, November). House price appreciation, liquidity constraints, and second mortgages. *Journal of Urban Economics 62*(3), 424–440.

Yoshida, J. (2008). Technology shocks and asset price dynamics: The role of housing in general equilibrium. *Working Paper No. 1071523, SSRN*.

Zeldes, S. P. (1989, April). Consumption and liquidity constraints: An empirical investigation. *Journal of Political Economy 97*(2), 305–46.