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Essays on Housing Supply and House Price Volatility

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Essays on Housing Supply and House Price Volatility

Abstract
A typical U.S. family devotes about a quarter of its annual income and half or more of its net worth to housing. Both the level and volatility of house prices thus have important implications for household behavior and welfare, as well as for the aggregate U.S. economy. Recent research has emphasized the importance of housing supply in determining house prices in different U.S. markets. This dissertation comprises three chapters, each of which focuses on constraints on housing supply, house price volatility, or the link between them.

In Chapter One, I use theory and empirical evidence to understand the impact of supply regulation on price dynamics. My estimates confirm that construction lags and marginal costs play critical and complementary roles in driving up costs on the margin, distorting the elasticity of housing supply and amplifying volatility.

In Chapter Two, I use detailed data on zoning and records of housing transactions in the Boston metropolitan area to estimate the channels by which regulation affects the type and quantity of residential construction. I find that restrictive zoning, particularly large minimum permitted lot sizes, drastically increases the costs of new construction, which leads to fewer, larger houses being built.

In Chapter Three, written jointly with Todd Sinai, we test the hypothesis that owning a home hedges a household against correlated changes in the future cost of housing. We find that the cross-sectional variation in house values subsequent to a move is substantially lower for home owners who moved between more highly covarying cities.

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ESSAYS ON HOUSING SUPPLY AND HOUSE PRICE VOLATILITY

Andrew Paciorek

A DISSERTATION

in

Real Estate

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

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2011

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For Kate

Without whom I would not have found the motivation, the will,
or the sense of humor needed to finish this dissertation.
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I began my graduate studies in 2006 as the only student in the Wharton Real Estate doctoral program. Any initial misgivings about my unique status were quickly overcome as I met and came to know the faculty and administrators of the Real Estate Department. It is a wonderful group of people who, both collectively and individually, never failed to offer their help or advice whenever I needed it. I would like to take this opportunity to thank a few of them, as well as some others who helped me along the way.

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counsel at many important junctures.

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Though I never really intended it, many of my choices in life have paralleled those of my brother Christopher. Had I set out to pick a role model, I could not have done better. Finally, but most importantly, I thank my parents for their quiet but consistent love and support throughout my life.
ABSTRACT

ESSAYS ON HOUSING SUPPLY AND HOUSE PRICE VOLATILITY

Andrew Paciorek

Supervisor: Todd Sinai, Associate Professor of Real Estate and Business and Public Policy

A typical U.S. family devotes about a quarter of its annual income and half or more of its net worth to housing. Both the level and volatility of house prices thus have important implications for household behavior and welfare, as well as for the aggregate U.S. economy. Recent research has emphasized the importance of housing supply in determining house prices in different U.S. markets. This dissertation comprises three chapters, each of which focuses on constraints on housing supply, house price volatility, or the link between them.

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In Chapter Two, I use detailed data on zoning and records of housing transactions in the Boston metropolitan area to estimate the channels by which regulation affects the type and quantity of residential construction. I find that restrictive zoning, particularly large minimum permitted lot sizes, drastically increases the costs of new construction, which leads to fewer, larger houses being built.

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Contents

Acknowledgements iii
Abstract v
Contents vii
List of Tables x
List of Figures xii

1 Supply Constraints and Housing Market Dynamics 1

1.1 Introduction 2
1.2 Comparison With Previous Work 6
1.3 The Basics of Housing Supply and Demand 8
1.4 A Dynamic Structural Model of Housing Markets 9

1.4.1 Demand 11
1.4.2 Supply 12
1.4.3 Empirical Implementation 16

1.5 Data 19
1.5.1 Demand Shifters 22
1.6 Estimation Strategy 24
1.7 Reduced-Form/Myopic Model Estimates ......................................... 29
  1.7.1 IV ....................................................................................... 32
1.8 Structural Model Estimates .............................................................. 34
  1.8.1 “First Stage” ......................................................................... 34
  1.8.2 Parameter Estimates .............................................................. 35
  1.8.3 Fixed Costs ............................................................................ 39
  1.8.4 Robustness Checks .................................................................. 41
1.9 Model Simulations ........................................................................... 43
  1.9.1 Solution Method ...................................................................... 44
  1.9.2 Demand Estimation ................................................................. 45
  1.9.3 Simulations ............................................................................. 48
1.10 Conclusion ..................................................................................... 52

2 Zoned Out: Estimating the Impact of Local Housing Supply Restrictions 77
  2.1 Introduction ................................................................................. 78
  2.2 A Dynamic Model of Local Housing Supply ..................................... 81
    2.2.1 House Size Decision ............................................................. 84
    2.2.2 Decision to Build ................................................................ 86
  2.3 Data ............................................................................................ 91
  2.4 Estimation and Results ................................................................. 95
    2.4.1 Hedonic Pricing Equation ...................................................... 95
    2.4.2 Lot Size .............................................................................. 99
    2.4.3 Variable Costs .................................................................. 100
    2.4.4 Construction ..................................................................... 103
  2.5 Simulations of Alternative Zoning Regimes ..................................... 113
  2.6 Conclusion .................................................................................. 114
### 3 Does Home Owning Smooth the Variability of Future Housing Consumption?

<table>
<thead>
<tr>
<th>Section</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>3.1 Introduction</td>
<td>141</td>
</tr>
<tr>
<td>3.2 A Simple Model of Housing Consumption with Migration</td>
<td>146</td>
</tr>
<tr>
<td>3.2.1 Intuition</td>
<td>146</td>
</tr>
<tr>
<td>3.2.2 Model setup</td>
<td>148</td>
</tr>
<tr>
<td>3.2.3 Variance in the Marshallian Demand for Housing</td>
<td>151</td>
</tr>
<tr>
<td>3.2.4 Endogenizing Initial Housing Consumption</td>
<td>153</td>
</tr>
<tr>
<td>3.3 Data</td>
<td>154</td>
</tr>
<tr>
<td>3.4 Estimation Strategy</td>
<td>156</td>
</tr>
<tr>
<td>3.4.1 Selection Bias</td>
<td>161</td>
</tr>
<tr>
<td>3.5 Results</td>
<td>163</td>
</tr>
<tr>
<td>3.5.1 Conditional Variance Estimates</td>
<td>163</td>
</tr>
<tr>
<td>3.5.2 Within City Pair Identification</td>
<td>167</td>
</tr>
<tr>
<td>3.5.3 Ex Post Probability of Owning</td>
<td>169</td>
</tr>
<tr>
<td>3.6 Magnitudes</td>
<td>170</td>
</tr>
<tr>
<td>3.7 Conclusion</td>
<td>172</td>
</tr>
<tr>
<td>3.8 Mathematical Appendix</td>
<td>174</td>
</tr>
<tr>
<td><strong>Bibliography</strong></td>
<td>187</td>
</tr>
</tbody>
</table>
# List of Tables

1.1 Data ................................................................. 65

1.2 WRLURI Values for Top 10 MSAs by Population .................. 66

1.3 Myopic Model Elasticity Estimates, OLS .............................. 67

1.4 Myopic Model Elasticity Estimates, IV ................................. 68

1.5 “First-Stage” Regressions ................................................. 69

1.6 Structural Model Estimates .............................................. 70

1.7 Estimated Cost Parameters and Elasticities for Top 10 MSAs by Population .................. 71

1.8 Fixed Costs and Regulation ............................................. 72

1.9 Robustness Checks ..................................................... 73

1.10 Demand Side (Rent) .................................................. 74

1.11 Demand Side (User Cost) ............................................ 75

1.12 Simulation Results .................................................. 76

2.1 Data ................................................................. 127

2.2 MassGIS Primary Use Codes ........................................... 128

2.3 Pioneer Insitute Housing Regulation Data .............................. 129

2.4 Average Hedonic Results by Year ................................... 130

2.5 Lot Size - Zoning Relationship ....................................... 131

2.6 Variable Costs ..................................................... 133

2.7 Myopic Model Estimates ............................................ 135
2.8 Dynamic Model Estimates .................................................. 137
2.9 Simulation Results .............................................................. 139

3.1 A Highly Stylized Example .................................................. 180
3.2 Summary Statistics, By Sample .............................................. 181
3.3 Baseline Percentage Effect of Covariance on Housing Expenditure Vari-
    ance .................................................................................. 182
3.4 Interacted Percentage Effect of Covariance on Housing Expenditure Vari-
    ance .................................................................................. 183
3.5 Baseline Effect of Covariance on Ex Post Probability of Owning ............. 184
3.6 Interacted Effect of Covariance on Ex Post Probability of Owning ............. 185
3.7 Magnitudes .......................................................................... 186
List of Figures

1.1 House Price Comparison ...................................................... 54
1.2 Housing Investment Comparison ........................................... 55
1.3 Volatility vs. Regulation ....................................................... 56
1.4 Regulation Index Map ........................................................ 57
1.5 Approval Delay Index Map .................................................... 58
1.6 Volatility Map ................................................................. 59
1.7 Housing Demand .............................................................. 60
1.8 Housing Supply ............................................................... 61
1.9 Timeline ....................................................................... 62
1.10 Regulation Survey Questions ............................................... 63
1.11 Impulse Response .......................................................... 64

2.1 Pioneer Institute Data Map ................................................... 116
2.2 MassGIS Zoning Codes ....................................................... 117
2.3 Price Gradient by Year ........................................................ 118
2.4 Predicted Price by Year ....................................................... 119
2.5 Predicted Price by Tract ....................................................... 120
2.6 Price Gradient by Tract ....................................................... 121
2.7 House Area by Tract .......................................................... 122
2.8 New Construction by Tract .................................................... 123
<table>
<thead>
<tr>
<th>Section</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.9   Flexible Net Price Coefficient Estimate</td>
<td>124</td>
</tr>
<tr>
<td>2.10 Net Price Histogram</td>
<td>125</td>
</tr>
<tr>
<td>2.11 Net Price by Tract</td>
<td>126</td>
</tr>
<tr>
<td>3.1   Covariance Histogram</td>
<td>178</td>
</tr>
<tr>
<td>3.2   Flexible Covariance Coefficient Estimate</td>
<td>179</td>
</tr>
</tbody>
</table>
Chapter 1

Supply Constraints and Housing Market Dynamics
1.1 Introduction

Recent experience in the United States has made painfully clear the importance of housing market volatility. Housing spending comprises about 25 percent of the median household’s total income, and housing wealth makes up 55 percent of the median household’s net worth¹. Large swings in the price of housing thus have important microeconomic effects: Increases benefit homeowners through expansion of paper wealth and relaxed borrowing constraints, while declines tighten those constraints and may leave households “underwater” on their mortgages and unable or unwilling to move. The macro implications of housing dynamics, meanwhile, are more important today than ever, following a serious recession and the largest residential real estate boom and bust in at least half a century.

As in most fields of economics, understanding the housing market means understanding both demand and supply. While the literature on housing demand is voluminous, progress in understanding the supply side has been much slower (DiPasquale 1999). But recent contributions to the literature on housing supply have emphasized the importance of construction costs, particularly the costs of complying with zoning and other regulatory constraints, and the degree to which investment in the housing stock responds to house prices.

In this paper I expand on the existing literature by focusing on the role of regulation and other supply constraints in amplifying house price volatility, as well as raising price levels. Intuitively, when supply is unable to keep pace with demand shocks quickly and cheaply, more of the shocks carry through into prices. In contrast with previous work, I trace out the channels by which regulation affects the housing market and employ a dynamic structural model to explicitly estimate the impact of regulation on costs. I find that it is lags and marginal costs — costs that rise with each additional house built on the margin in a given year — that explain much of the observable differences in elasticities and volatility across

¹Calculated using 2007 data from the Panel Study of Income Dynamics.
Such differences can be stark, as may be seen in figures 1.1 and 1.2. The median price of a home in the San Francisco area was about three times as high as in Atlanta between 1990 and 2000. Yet the housing stock of San Francisco grew by an average of just half a percentage point per year, while that of Atlanta grew by 3.5 percentage points per year. Moreover, price volatility in San Francisco was far greater than in Atlanta: Even apart from the trend, the standard deviation of house prices was about twice as high relative to the mean in San Francisco. Homeowners who purchased in San Francisco thus not only paid more on average, they faced far greater uncertainty about the capital gain (or loss) they could expect to realize when they moved to a new house or new city.

Several papers argue that observed differences in construction and house price levels across metropolitan areas are due to differences in regulation and community opposition to new construction, rather than shortages of land or higher building costs (Glaeser, Gyourko and Saks 2005a, Quigley and Raphael 2005). Areas with strong demand and tightly constrained supply experience rising prices and incomes but little construction, becoming “superstar cities” like San Francisco and Boston (Gyourko, Mayer and Sinai 2006). Other cities, such as Atlanta and Phoenix, are also in high demand but impose comparatively few regulations on supply, resulting in substantial expansion of the housing stock and (until recently) muted price growth.

The strong statistical association between regulation and housing volatility is easy to see in the data. Figure 1.3 shows a scatter plot and smoothing spline of within-city house price volatility against the Wharton Residential Land Use Regulatory Index (WRLURI), with each dot representing a single metropolitan statistical area (MSA). A simple regression of price volatility on the regulation measure indicates that a one standard deviation increase in regulation across cities is associated with about a 30 percent increase in volatility. We

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2See below for details on the data and the construction of these measures.
can get a sense of the geographical nature of this relationship by comparing figures [1.4] and 1.6. In these maps, the darker blue areas, indicating more regulation and higher volatility, line up quite closely across the figures.

My goal is to use a model of housing supply to explain the causal mechanism underlying this relationship. While the empirical literature has convincingly demonstrated that housing supply conditions can vary widely across regions, housing supply models have remained mostly ad hoc. Econometric models relating supply to prices and other fundamentals have imposed no theoretical structure on these relationships, leading to confusion even over relatively simple questions such as whether investment should relate to price levels or changes (Mayer and Somerville 2000). Through the careful application of theory to data on a panel of cities, I make a series of contributions to the literature on housing supply.

Building on preexisting models of investment in durable goods, I develop a dynamic theory of housing supply that is grounded in the optimization problem of owners of undeveloped land. These owners must decide when to build new houses, taking into account currently available information and their rational expectations about future prices. Fluctuations in prices are driven by demand shocks, such as changes in wages or immigration patterns. The impact of these shocks on both prices and investment differs depending on the supply environment, such as the amount of land available, the fixed and marginal costs of building, and the amount of time needed to build.

My model is explicitly designed so that the parameters can be estimated, and my primary contribution is empirical. I estimate the structural parameters of the model at the level of metropolitan areas, using data on house prices and construction, and show how they vary with observed levels of housing regulation, particularly regulatory permitting and construction lags. In doing so, I deal with a series of empirical challenges. First, by starting with a microeconomic optimization problem, I am able to properly specify an estimating equation that relates prices, new housing investment, and expectations about the future. Because
development lags vary across the cities in my sample, I have to carefully model the role of expectations and their impact on my estimates. Finally, I use demand-side variables that are plausibly uncorrelated with supply shocks and forecast errors to identify the supply side parameters.

I find that regulatory costs of all kinds can add tens of thousands of dollars to the cost of building an additional house on the margin in more regulated cities relative to less regulated ones. Importantly, while regulations that raise the average cost of new housing or reduce the amount of available land can lead to higher house prices, it is marginal costs — which rise with each additional house built in a given year — and construction lags that are leading causes of volatility[^1] Regulatory-induced lags have particularly large effects, both by adding costs on the margin and by forcing landowners and developers to forecast further into the future when planning new development, thus lowering the correlation between actual prices and new supply.

Other authors have not looked closely at how different frictions on the supply side carry through into observed dynamics. Using the estimated parameters, particular the impact of supply regulation on costs, I solve and simulate the model to explore the importance of various constraints. I find that more regulation can significantly increase both the level and variance of house prices. The model can explain sizable differences in volatility across metropolitan areas. For example, I predict that price volatility should be about 55 percent higher in San Francisco than in Atlanta, compared with 100 percent higher in the data.

In the next section I compare my contributions with previous work in the literature. I then discuss the basics of supply and demand in the housing market before laying out my dynamic model of housing supply. In Section 1.5 I lay out the data used for estimation, including the exogenous demand shifters used to identify the supply side. Sections 1.6

[^1]: Both lags and increasing marginal costs of this kind could result from a variety of types of regulation, from annual limits on building permits to minimum lot size requirements to the discretionary actions of homeowners’ associations and local government.
through 1.8 detail the precise estimation techniques, use reduced-form regressions and IV specifications to illustrate the patterns in the data, and then present the structural estimates. In Section 1.9 I solve and simulate the model to show how the estimated supply parameters carry through into volatility. The final section discusses caveats and concludes.

1.2 Comparison With Previous Work

Academic work on housing supply has expanded by leaps and bounds since DiPasquale’s (1999) review of the literature to that date. A series of papers over the last decade have made clear that constraints on housing supply vary markedly across regions and metropolitan areas of the United States (Glaeser et al. 2005a, Gyourko et al. 2006, Quigley and Raphael 2005). Differences in supply regulation are crucial for explaining why some cities, such as New York and San Francisco, have experienced dramatic price growth but relatively little new construction. Others, such as Atlanta, have expanded their housing stocks substantially while real prices have risen little.

The impact of these constraints, and regulation in particular, on the dynamics of housing markets has been explored by few empirical papers. Using a metropolitan-level panel and regressions of housing permits on price changes interacted with regulation variables, Mayer and Somerville (2000) find that land use regulation lowers the steady state of new construction and the price elasticity of supply. They also find much more substantial impacts from regulations that lengthen the construction process than from those that simply add fees or other costs.

While these are important contributions, these papers are reduced-form in nature, positing simple relationships between price and investment. As DiPasquale (1999) noted, we still know little about the microeconomics of housing supply, in part because appropriate data are lacking. I start with a supply model that relies on a microeconomic optimization
problem, specifically that of the landowner/developer, as in Murphy (2010).

Unlike Murphy (2010), who estimates cost parameters using microdata from a single metropolitan area, I focus on observable constraints on the supply side using variation at the city level. Although the foundation is different, my model leads to a similar investment equation to that of Topel and Rosen (1988). In contrast with their work, I explicitly incorporate land as an input, which has important implications for dynamic optimization; in particular, investment adjustment costs are not necessary to get forward-looking behavior. My paper is also closely related to Saiz’s (2010) empirical study of urban growth with topographical constraints on the availability of land, as well as Saks (2008), who examines the impact on local labor markets of regulatory constraints on housing supply.

Other recent models of housing dynamics include Glaeser and Gyourko (2007) and Nieuwerburgh and Weill (2009), both of which are spatial equilibrium models in the tradition of Rosen (1979b) and Roback (1982). Glaeser and Gyourko (2007) establish a series of stylized facts about the housing market, including that house prices are positively correlated in the short run and mean-reverting in the longer run, and that both prices and construction are highly volatile, particularly in inelastically supplied coastal markets. Their model succeeds in explaining some of the observed patterns, including mean-reversion, but fails at reproducing others, such as the positive serial correlation of price changes at one- and three-year intervals.

Using similar data to mine, Huang and Tang (2010) examine the relationship between supply constraints, including both regulation and land availability, and the sizes of cities’ housing booms and busts from 2000 to 2009. They argue that more constrained cities experienced larger price run-ups from 2000 to 2006 and larger price declines in the subsequent

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4The existence of vast new datasets on transactions of new and existing homes offers interesting new opportunities for housing supply research. Murphy (2010) and Epple, Gordon and Sieg (2010) are among the few to date to employ such data. In Paciorek (2011b), I adapt the model in this paper to use microdata to examine supply at the local level, focusing in particular on the impact of regulatory and geographic constraints.
period, which generally accords with my findings in this paper. Interestingly, however, some of the largest price swings in the most recent cycle occurred in relatively unconstrained cities like Las Vegas, which is hard to fit with a supply-side explanation alone (Glaeser, Gyourko and Saiz 2008).

My focus on the initial development decision is related to the real estate applications of the real options literature, including Grenadier (1996), who looks at overbuilding, and Bar-Ilan and Strange (1996), who analyze the impact of investment lags on the relationship between investment and uncertainty. A series of papers on land conversion in cities with stochastic shocks, including Titman (1985) and Capozza and Helsley (1990), examine the effect of uncertainty on the timing of development and the option value of land. In a similar vein, Wheaton (1999) provides theoretical support for the important role of supply constraints, including development lags, in housing cycles. While this literature has provided useful theory, there are few empirical applications. In one exception, Cunningham (2007) introduces urban growth controls into a real options model and estimates the impact of an urban growth boundary around Seattle on price volatility. Finally, while different in focus and scope, this paper also has links to the real business cycle literature, starting with Kydland and Prescott (1982), who consider the implications of “time to build” for macroeconomic fluctuations.

1.3 The Basics of Housing Supply and Demand

Before introducing any notation, it is worth establishing the basics of an equilibrium model of the housing market via a simple graphical representation. Figures 1.7 and 1.8 show the demand and supply sides, respectively. In Figure 1.7, a downward-sloping demand curve relates the implicit rental cost of owning a house in a given period to the quantity of housing demanded at that rent. In the short run, the housing capital stock is fixed and is
thus represented by a vertical line in the figure.

Moving to the supply side in Figure 1.8, we see an upward sloping supply curve relating the expectation of the next period’s price to the investment that will come online in that period, under the maintained assumption that it takes one period to build houses. On the margin, the cost of building an additional house \( C(I_t) \) must be equal to the expected price. As the expected future price rises, investment rises in step. The model is closed by positing a user cost relation — including interest rates, depreciation, and the full path of expected future rents — between housing rents and prices, as well as a transition equation for the capital stock.

An unexpected and permanent increase in demand in period \( t \) is represented by an upward shift in the demand curve. In the short run, supply is fixed, so rents rise. This pushes the expected future price above the cost of construction. Investment continues until the price falls back and the system returns to its steady state. When marginal costs are higher or delays longer, the supply curve is more steeply sloped, so the investment response is lessened and the return to steady state takes longer. This is the intuition underpinning my results.

Although there is a time lag in the model, the supply side is myopic in the sense that the expectation of the next period’s price translates directly into a level of investment, with no comparison by landowners of expected prices in different periods. Generalizing this world to a fully dynamic one with forward-looking agents requires explicitly modeling the choice of when to develop, which I take up in the next section.

1.4 A Dynamic Structural Model of Housing Markets

In formulating a model of housing supply, it is valuable to consider some of the special features of housing that differentiate the housing market from that of other goods or services.
First, a house is not merely durable but extremely so. Although millions of new houses are built in a typical year, the current bust notwithstanding, the median age of housing units in the United States is about 35 years.\(^5\) Thanks to this durability, housing both provides a flow of services and serves as a long-term investment, making forward-looking behavior imperative for home buyers.

Second, the major input into housing is the land on which it is built, which is in fixed supply within a radius around a given location. This is not to say that there is a shortage of land in the world. But empty land, frequently on the outskirts of major cities, is poorly substitutable for land in desirable locations. Landowners thus have some market power, unlike purveyors of reproducible widgets, and can time their decision to sell or develop the land. This timing decision forms the core of my dynamic model of housing supply, and it differentiates my model from most previous approaches in the literature.

Since I employ data on house prices and investment at the metropolitan level, my model focuses on cities, indexed by \(j\), which I define as infinitely divisible areas of measure \(A_j\). The capital stock of housing in \(j\) at time \(t\) is denoted \(K_{j,t}\), and new investment is \(I_{j,t}\), with each period’s capital stock equal to the depreciated last period capital stock plus investment:

\[
K_{j,t} = K_{j,t-1} (1 - \delta_{j}) + I_{j,t}
\]

Each unit of housing takes up one unit of land, so the stock of undeveloped land is \(A_j - K_{j,t}\). Houses do not differ in quality and are perfectly substitutable.\(^6\) The population of the city, \(n_{j,t}\), is exogenous and evolves deterministically. Endogenizing \(n_{j,t}\) would require modeling households’ choice among multiple cities, perhaps using a Rosen/Roback-style spatial

\(^5\)2007 American Housing Survey
\(^6\)The permits data that I use contain no information on housing quality, which is why the model ignores the margin of housing quality in the investment decision. In ongoing work, I use detailed microdata to incorporate quality into the model and estimate related parameters and how they vary with observed regulation at the neighborhood level (Paciorek 2011b).
equilibrium approach, as in Glaeser and Gyourko (2007). Changes in population are an
important source of local housing demand, but for simplicity I incorporate all unexpected
shocks in the demand shock $\epsilon^D_{j,t}$.

### 1.4.1 Demand

Since I focus primarily on modeling and estimating the responses of cities with different
supply constraints to demand shocks, I keep the demand side of the model relatively simple,
the graphical version discussed above and displayed in Figure 1.7. The inverse
demand for housing in city $j$ at time $t$ is given by

$$\log (R_{j,t}) = \phi + \phi_K \log (K_{j,t}) + \phi_n \log (n_{j,t}) + \epsilon^D_{j,t}$$ (1.1)

where $R_{j,t}$ is the rent paid implicitly by homeowners or explicitly by renters in each period.
With $\phi_K$ negative and $\phi_n$ positive, the amount of housing desired by the exogenously given
population $n_{j,t}$ is inversely proportional to the rent; that is, the demand curve is downward-
sloping. The demand shock, $\epsilon^D_{j,t}$, drives the dynamics of the model.

The price of a house is equal to the present value of current and future rent. Taking
into account property taxes ($\omega_{j,t}$), the mortgage interest rate ($r_t$) and their deductibility
from income taxes ($\tau_{j,t}$), as well as a risk premium $\gamma$ and depreciation $\delta_j$, we are left with
a standard formula for the user cost of housing (Poterba 1984, Himmelberg, Mayer and
Sinai 2005).

$$R_{j,t} = P_{j,t} (r_t + \omega_{j,t} - \tau_{j,t} (r_t + \omega_{j,t}) + \delta_j + \gamma_{j,t} - E_t [g_{j,t+1}])$$ (1.2)

Here $E_t [g_{j,t+1}]$ denotes the expectation of growth rate in house prices over the next year.

---

7 Using the mortgage interest rate here implies that houses are entirely financed by debt, with no down
payment, but the results are not sensitive to the choice of interest rate.
taken with respect to all relevant information at time $t$; in other words, the model relies on rational expectations. The primary difficulty in calculating the user cost is that expectations (and the risk premium) are unobserved by the econometrician; one advantage of modeling housing supply is that it allows me to endogenize expectations in a principled way.

### 1.4.2 Supply

Owners of undeveloped land, whom I index by $i$, choose how much land to develop each period.\footnote{We can extend the model to cover multi-unit dwellings by reinterpreting $A_j$ as the total number of houses that would exist if all land were developed to some maximum feasible density, which is what I do in the empirics, as described in section 1.5 below.} I avoid explicitly modeling the market for land or the production function for structures by assuming that the construction industry is perfectly competitive, so that development risk is borne by the landowner/developer, who also receives any economic profits. In practice, housing developers buy or option land and undertake much of the risk involved in the process, but I elide the distinction between developers and original landowners because my data do not allow me to distinguish between them empirically.

The development and construction of a house in $j$ started at $t - L_j$ takes $L_j$ periods and is irreversible once begun. A building permit must be acquired one year before the house is finished; this is approximately the amount of time that a single-family building project takes to go from permit to start to completion, according to data from the Census Bureau. See Figure 1.9 for a timeline of the process. Upon completion, the landowner/developer sells it and receives the price of housing at that time ($P_{j,t}$) less the fixed labor, materials, and regulatory costs associated with building the structure ($C_{j,t}$). I assume all fixed per unit costs of construction are paid on completion but are known with certainty at the time the decision is made.

In addition to the fixed costs, a coherent model requires costs in a given area to increase on the margin as more investment is undertaken in any period; otherwise all parcel owners
would want to develop at the same time. Along with construction lags, which I discuss below, this is one of the two primary channels by which regulation can affect dynamics, in addition to driving up fixed costs. I incorporate increasing marginal costs by attributing to each landowner $i$ a random shock to the cost of building $\chi_{i,j,t-L_j}$. Since this is the only parcel-level heterogeneity in the model, I can sort the landowners according to this shock without loss of generality. Within a given city and time period, these cost shocks follow a mean-zero cumulative distribution $F_{j}^{-1} \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right)$ plus an overall mean cost shifter $\epsilon_{j,t}^{S}$. The scale parameter of this distribution $\sigma_{j}^{X}$ varies across cities, allowing different regulatory regimes to have disparate impacts on the cost of building on the margin. The mean cost $\epsilon_{j,t}^{S}$ affects all landowners in $j$ equally and serves as a city-level supply shock.

The cost of construction may also vary with the amount of undeveloped land that remains available in the city. Costs are likely to increase as the city’s best land is developed, and the gradient may vary across cities either due to regulatory or topographical constraints (Saiz 2010).\textsuperscript{9} Let $\eta_j (K_{j,t-1}, A_j)$ denote a cost function that depends on the level of the capital stock relative to the total land area of the city.\textsuperscript{10}

Since construction always takes at least one period, landowners must form expectations about the path of house prices in order to decide whether to develop a given parcel now or wait. If a landowner chooses not to build on a given parcel at $t$, she will face precisely the same choice one period in the future, after receiving any income from the current use of the land ($\bar{U}_{j,t}$), such as farming or the operation of a parking lot.

The state space $(S_{j,t})$ comprises all information known at $t$, including the evolution of

\textsuperscript{9}This sort of variation in costs can also capture a gradient in demand, as in a traditional monocentric city model.

\textsuperscript{10}Note that I do not incorporate these costs as persistent heterogeneity at the parcel level, which would be a more literal interpretation of the role of topography. Cities typically develop first on flat land in desirable locations before growing into less amenable locations, such as hills or wetlands. Unfortunately, a model of housing activity at the metropolitan level is intractable with both individual-level shocks and persistent individual heterogeneity, and the data I use in this paper would not allow me to estimate such a model properly in any case. Nevertheless, my specification should do a good job of capturing costs that increase as the city is “built out”.

13
the demand shock up to \( t \) and the capital stock \( L_j - 1 \) periods in the future. The future capital stock up to that point is known with certainty because the investment decisions have already been made in periods prior to \( t \). In the simplest case, in which the demand and supply shocks follow first-order Markov processes, the state space contains the capital stock and current shock realizations:

\[
S_{j,t-L_j} = \{ K_{t-1}, \epsilon_{j,t-L_j}^D, \epsilon_{j,t-L_j}^S \}
\]

Using the above notation, a landowner’s expected time \( t \) value from building on parcel \( i \) is

\[
V_{j,t}^B \left( S_{j,t-L_j} \right) = \chi_{i,j,t-L_j} = \beta^{L_j} \left( \log P_{j,t} | S_{j,t-L_j} \right) - \log C_{j,t} - \eta_j (K_{j,t-1}, A_j) - \epsilon_{j,t-L_j}^S - \chi_{i,j,t-L_j}
\]

I specify the price and cost terms in logs because it is an empirical regularity that log investment increases linearly with log price, whether or not expectations about the future are taken into account\(^{11}\). Given this, it is unsurprising that most previous research on housing supply has specified a log-log relationship between investment and price, and following that tradition allows for straightforward comparison. Since I have no \textit{a priori} theoretical understanding about the cost terms, specifically the functional form of \( \eta (\cdot) \) or the distribution of \( \chi \), it is perfectly reasonable to have them relate linearly to log price rather than the price level\(^{12}\).

Alternatively, the flow value from not building plus the expected value of the option to

\(^{11}\)I have estimated flexibly nonlinear versions of the model using generalized additive modeling techniques (Hastie and Tibshirani 1990, Wood 2006) and do not find substantial departures from the specification described here.

\(^{12}\)The concavity of the logarithm also introduces risk aversion, which is reasonable given that the parcel owners are small entities.
build (or not) tomorrow is

\[ V_{j,t}^N (S_{j,t-L_j}) = \beta^{L_j} U_j + \beta E \left[ \max \left\{ V_{j,t+1}^B - \chi_{i,j,t-L_j}, V_{j,t+1}^N \right\} | S_{j,t-L_j} \right] \]

There is an equivalence between heterogeneity in fixed costs and in the value of the outside option, since a higher outside option functions exactly like an increase in the fixed cost of construction. I attribute all of this heterogeneity to costs, with \( \eta_j (\cdot) \) capturing the increasing return from the outside option as land becomes scarce and \( \epsilon_{j,t-L_j}^S \) incorporating any unobservable shocks to the outside option value.

Since \( \chi_{i,j,t-L_j} \) follows a continuous probability distribution with full support over the real line and the total land area is divided among infinitely many small parcels, some parcels will be developed in every city and period. That is, investment \( I_{j,t} \) must be strictly positive, so that

\[ K_{j,t} > K_{j,t-1} (1 - \delta_j) \]

This is a reasonable requirement for MSAs taken as a whole, since even cities in secular decline, like Detroit, have new construction in every period. Thanks to the lag, each parcel owner must decide in period \( t - L_j \) whether to develop her parcel for delivery at \( t \). Given the various continuity assumptions, there must be a parcel owner (\( i^* \)) who is precisely indifferent between building and not building. For this owner,

\[ V_{j,t}^B - \chi_{i^*,j,t-L_j} = V_{j,t}^N \]

\[^{13}\text{For established neighborhoods, which may see no construction for years at a time, a different formulation is required (Paciorek 2011b). In particular, when investment is zero, there is in general no parcel for which the value of building and not building are equal.}\]
or

\[
\beta L_j \left( E \left[ \log P_{j,t} | S_{j,t-L_j} \right] - \log C_{j,t} \right) - \eta_j \left( K_{j,t-1}, A_j \right) - \chi_{i,j,t-L_j} - \epsilon_{j,t-L_j} = \beta L_j \bar{U}_{j,t} + \beta E \left[ \max \left\{ V_{j,t+1}^B - \chi_{i,j,t+1}, V_{j,t+1}^N \right\} | S_{j,t-L_j} \right]
\]

(1.3)

where \( F_j^{-1} \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right) = \chi_{i,j,t-L_j} \) because the owner is on the margin. This equates the value of building on the marginal parcel today to the discounted expected value of having the same choice tomorrow, plus the current income payment.

### 1.4.3 Empirical Implementation

To estimate Equation (1.3) using a standard panel of MSA-level house prices and investment — described in detail below — I make a series of additional simplifying assumptions, some of which can be relaxed later. First, the discount factor \( \beta \) is known to the econometrician \( a \) priori. Second, the supply shocks \( \epsilon_{j,t-L_j} \) are serially uncorrelated, an assumption that can be tested. Finally, \( \eta_j \left( \cdot \right) \) and \( F_j^{-1} \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right) \) have known functional forms. Specifically, I assume that \( \eta_j \left( K_{j,t}, A_j \right) = \sigma \eta_j K_{j,t} A_j \), which is essentially the density of housing over a fixed area, and that \( \chi_{i,j,t-L_j} \overset{iid}{\sim} \text{logistic} \left( 0, \sigma \chi_j \right) \), which means

\[
F_j^{-1} \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right) = \sigma \chi_j \log \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right).
\]

Given these assumptions, I can rewrite Equation (1.3) as follows:

\[
\beta L_j \left( E \left[ \log P_{j,t} | S_{j,t-L_j} \right] - \log C_{j,t} \right) - \sigma \eta_j K_{j,t} A_j - \sigma \chi_j \log \left( \frac{I_{j,t}}{A_j-K_{j,t-1}} \right) - \epsilon_{j,t-L_j} = \beta L_j \bar{U}_{j,t} + \beta E \left[ \sigma \chi_j \log \left( \exp \left( V_{i,j,t+1}^B / \sigma_j \right) + \exp \left( V_{i,j,t+1}^N \sigma_j \right) \right) | S_{j,t-L_j} \right]
\]

(1.4)

where the last term applies the fact that \( \chi_{i,j,t-L_j+1} \) follows an iid logistic distribution, so that the expectation of the maximum is equal to the logit inclusive value.
To deal with the unobservable value function $V_{i,j,t+1}^N$ on the right-hand side of Equation 1.4, I employ the representation theorem of Hotz and Miller (1993), who show that value functions can often be rewritten as functions of conditional choice probabilities (CCPs), defined as the probabilities that a given alternative is chosen given the state of the world. Applying the logistic CDF, we can write the CCP of building next period as

$$Pr[B|S_{j,t-L_j+1}] = \frac{\exp \left( \frac{V_{i,j,t+1}^B}{\sigma_j^N} \right)}{\exp \left( \frac{V_{i,j,t+1}^B}{\sigma_j^N} \right) + \exp \left( \frac{V_{i,j,t+1}^N}{\sigma_j^N} \right)}$$

Thanks to the assumption that each city has a continuum of identical small landowners, this probability of building is precisely equal to $\frac{I_{j,t+1}}{A_j - K_{j,t}}$, the ratio of parcels actually developed to the amount of available land. Substituting this into the previous expression, rearranging and taking the logarithm, we have

$$\log \left( \exp \left( \frac{V_{i,j,t+1}^B}{\sigma_j^N} \right) + \exp \left( \frac{V_{i,j,t+1}^N}{\sigma_j^N} \right) \right) = \frac{V_{i,j,t+1}^B}{\sigma_j^N} - \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right)$$

I can then plug this expression back into Equation 1.4 and expand the $V^B$ term to get

$$\beta L_j \left( E \left[ \log P_{j,t} - \beta \log P_{j,t+1}\right|S_{j,t-L_j} \right] - (\log C_{j,t} - \beta \log C_{j,t+1}) \right) - \sigma_j^N \left( K_{j,t-1} - \beta K_{j,t} \right) \right) - \sigma_j^N \left( \log \left( \frac{A_j - K_{j,t-1}}{1 - \frac{I_{j,t}}{A_j - K_{j,t}}} \right) \right) - \beta E \left[ \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) \right|S_{j,t-L_j} \right] \right)$$

The Hotz and Miller (1993) two-step approach to estimating dynamic models is a popular alternative to full-solution methods (e.g., Keane and Wolpin (1997)) when the model is too complex to repeatedly solve numerically. See Murphy (2010) and Bishop (2008) for recent examples. A CCP approach is particularly attractive here because I have already assumed away the unobserved heterogeneity that can make the traditional two-step estimator less palatable when applied to individual-level data. Arcidiacono and Miller (2008) discuss an extension of the Hotz and Miller (1993) approach that can incorporate various forms of unobserved heterogeneity.

14 The Hotz and Miller (1993) two-step approach to estimating dynamic models is a popular alternative to full-solution methods (e.g., Keane and Wolpin (1997)) when the model is too complex to repeatedly solve numerically. See Murphy (2010) and Bishop (2008) for recent examples. A CCP approach is particularly attractive here because I have already assumed away the unobserved heterogeneity that can make the traditional two-step estimator less palatable when applied to individual-level data. Arcidiacono and Miller (2008) discuss an extension of the Hotz and Miller (1993) approach that can incorporate various forms of unobserved heterogeneity.
where the future supply shock disappears because I assume that the shocks are serially un-
correlated. This is almost exactly the same relation that results from writing the problem of
a single utility-maximizing agent for each MSA and deriving an Euler equation. Intuitively,
this must be the case because there are no cross-parcel spillovers, so maximizing the total
utility of all parcel owners gives the same result as maximizing utility individually, apart
from some minor technical considerations.

There are two remaining complications that prevent estimation of Equation 1.4. The
first is the presence of unobservable expectations, specifically
\[ E \left[ \log P_{j,t} - \beta \log P_{j,t+1} \mid S_{j,t-L_j} \right]. \]
Although I observe the realized prices, I cannot re-
late realizations and expectations without making further assumptions. Following much
of the literature on estimating dynamic models such as this one, I assume that agents
form expectations rationally, so that the equation \( \nu_{j,t-L_j} = (\log P_{j,t} - \beta \log P_{j,t+1}) - E_{t-L_j} [\log P_{j,t} - \beta \log P_{j,t+1}] \) defines a mean-zero forecast error\(^{15}\). That is, the subjective
expectations of landowners are equal to the conditional expectations.

Applying this definition of \( \nu_{j,t-L_j} \) to Equation 1.5, we get

\[
\beta L_j ((\log P_{j,t} - \beta \log P_{j,t+1}) - (\log C_{j,t} - \beta \log C_{j,t+1}))
\]
\[
- \sigma^\eta \left( \frac{K_{j,t-1}}{A_j} - \beta \frac{K_{j,t}}{A_j} \right) - \sigma^\chi \left( \log \left( \frac{I_{j,t}}{A_j - K_{j,t-1}} \right) - \beta \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) \right)
\]
\[
+ m_j + m_t
\]
\[
= \epsilon_{j,t-L_j} + \nu_{j,t-L_j}
\]

\[ (1.6) \]

\(^{15}\) I ignore the error in the forecast of next-period investment \( (\log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) - E \left[ \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) \mid S_{j,t-L_j} \right] ) \), since it does not cause any endogeneity complications. This is because my estimation strategy does not rely on any assumptions about the orthogonality of the composite investment term and the error, including its own forecast error. The intuitive explanation is that the composite investment term in these equations is the “dependent variable”, and mean-zero errors in the dependent variable in a regression — classical measurement error, e.g. — do not lead to endogeneity.
Since the outside value of land is not observed, I have folded $\beta^{L_1} \tilde{U}_{j,t}$ into $e^{S}_{j,t-L_j}$. I also include fixed effects $m_j$ and $m_t$ to capture unobservable differences across MSAs and years in the outside option value and the supply shock. Equation 1.6 comprises only observable values and explicitly unobservable error terms, which means it can serve as a basis for estimation, subject to the second remaining complication, that of endogeneity.

There are at least three possible sources of endogeneity in Equation 1.6: First, the unobserved supply shock $e^{S}_{j,t-L_j}$ will in general be correlated with realized prices in city $j$ at time $t$, since prices are determined in equilibrium. Second, the forecast error $\nu_{j,t-L_j}$ is correlated with the realized value $\log P_{j,t} - \beta \log P_{j,t+1}$ by construction, since the forecast error is defined to be mean independent of the expectations. Third, the housing stock in period $t$ includes investment that comes online in $t$, leading mechanically to endogeneity of the housing density term.

Dealing with endogeneity requires a set of exogenous demand shifters that are correlated with the relevant observables but uncorrelated with both the supply shock $e^{S}_{j,t-L_j}$ and the forecast error $\nu_{j,t-L_j}$. I discuss my identification strategy after first detailing my data.

1.5 Data

Relative to macroeconomic data, where there is typically one realization of a given time series, one advantage of researching housing dynamics is that there is substantial heterogeneity in housing markets across cities and regions within the United States. Since supply-side factors like regulation and topography differ widely across metropolitan areas, housing market heterogeneity allows us to examine the impact of these factors on market dynamics. Essentially, each city is a separate laboratory experiment with different supply- and demand-side conditions.

16 In the next section I also specify how I allow the parameter values with $j$ subscripts to vary across MSAs using observable data.
Table 1.1 summarizes the data used in this paper. I calculate the house price series using FHFA (née OFHEO) repeat-sales indices deflated by the CPI and pegged to the mean house price in each city from the 2000 Census. This provides a dollar-valued measure of prices that controls as best as possible for changes in the types of houses that transact in any given period.\(^{17}\)

I specify new housing investment in each MSA and year using the number of housing permits issued in that area in the previous year. This lag accounts for the fact that single-family homes take a bit under a year to complete even after the builder acquires a permit. I use permits data rather than starts or completions because the Census Bureau has a detailed inventory of permits that is finely geographically disaggregated. Although it is possible to abandon permits before starting, and even to abandon units under construction before completion, Census Bureau estimates indicate that only around two percent of permitted structures are not built, which is not surprising given the substantial costs involved in preparing for and acquiring a permit. I also use the permits data to calculate the total stock of housing in each MSA and year by interpolating from decennial census figures.

I focus on the role of three variables that capture supply constraints. The first is the Wharton Residential Land Use Regulatory Index (WRLURI), which is a measure of local regulatory constraints compiled from a 2005 survey of municipal officials (Gyourko, Saiz and Summers 2008). Figure 1.10 presents example questions from the survey, such as “What is the current length of time required to complete the review of residential projects in your community?”\(^ {18} \) WRLURI is derived from sub-indices that cover a variety

\(^{17}\)The Case-Shiller price indices distributed by Standard & Poor’s, which are the most popular alternative to the FHFA series, do not offer sufficient breadth or length for my purposes. There are 20 MSA-level Case-Shiller indices, which at best go back to only 1987, compared with hundreds of MSAs for the FHFA, many of which start in the early 1980s or before. Although there are some differences between two sets of indices during the most recent boom period, the correlations between the two indices over time are still above 0.9 in all metro areas with data from each and above 0.95 in most. See http://www.fhfa.gov/webfiles/1163/OFHEO_FHFA12008.pdf for a comparison of the two methodologies.

\(^{18}\)See http://real.wharton.upenn.edu/~gyourko/Wharton_residential_land_use_reg.htm for full details.
of different regulatory constraints, from financial exactions to zoning restrictions to delays in the approval process. In the context of the model, WRLURI can be interpreted as affecting lags, construction cost, and the amount of available land. That said, Gyourko et al. (2008) note that the overall index is most highly correlated with the sub-index related to average delays, which should capture some or all of the regulatory-induced construction lags. In the empirical work below, I specifically examine the role of the Approval Delay Index (ADI), which tries to measure the total delay that regulation imposes on the acquisition of a permit. To complement the ADI, I use a version of WRLURI that strips out the ADI as a measure of other sorts of regulation that directly affect costs and land use.

Figures 1.4 and 1.5 map WRLURI and the ADI for every MSA in my sample, with each color representing one quintile. Table 1.2 shows the WRLURI and ADI values for the top 10 MSAs by average population over the period from 1984 to 2008, as well as San Francisco, with both regulation variables standardized to have mean zero and standard deviation one. The coloration of the map and most of the values displayed in the table match the standard intuition for which markets are heavily regulated: Coastal cities (San Francisco, New York) generally display very high levels of regulation by both measures, while interior cities (Atlanta, Chicago) are typically much less regulated.

The second supply-side variable is a measure of the amount of land in each metropolitan area that is not available for development because it is steeply sloped, with a gradient greater than 15 percent. I calculate the amount of developable land in an MSA by subtracting this measure from the total land area in square miles of each MSA’s component counties. I further scale this measure by the number of units per square mile in Manhattan,

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19 In practice, the development cycle may be even longer, since getting to the permitting stage may require substantial expenditure and years of negotiation with the relevant authorities (Rybczynski 2007).

20 This is similar to the measure used in Saiz (2010), but for comparability with my other data I calculate the steeply sloped land area of the component counties of each MSA, rather than using a fixed radius around the central business district. I thank Albert Saiz for generously providing me with the raw GIS maps underlying his estimates.
a particularly densely settled area. This ratio of the housing stock to this measure of developable “slots”, which I refer to as the density of housing, can be thought of as the degree to which an MSA is currently developed relative to Manhattan. If costs rise as metropolitan areas “fill up”, perhaps because the available land is more expensive to build on or because the outside option for the land is more valuable, the density should capture this effect.

The last measure is an estimate from the RS Means Company of the real cost of constructing a 2000 square foot average-quality house, including labor and materials but excluding land and regulatory costs (Gyourko and Saiz 2006). The RS Means measure should translate into an increase in fixed construction costs in the model \( C_{j,t} \). The RS Means data are available in a panel by MSA and year, but WRLURI is observed only once for each MSA — in 2005, when the survey was conducted — while the Saiz measure is essentially time-invariant.

### 1.5.1 Demand Shifters

As in any model of market equilibrium, the quasi-differenced price term in Equation [1.6] is likely to be correlated with the supply shocks precisely because prices are set in equilibrium. Consistently estimating the supply equation requires one or more variables that are correlated with house prices and uncorrelated with supply shocks. Given that I allow the supply parameters to differ across MSAs, these exogenous variables must also provide variation across both the time \( t \) and MSA \( j \) dimensions.

To get variation in annual housing demand at the MSA level, I rely on two plausibly exogenous variables. The first (\( \text{industry}_{j,t} \)) follows Bartik (1991) in using imputed shifts in local labor demand. The second (\( \text{migration}_{j,t} \)) makes novel use of county-level

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21This is an arbitrary benchmark, but it is convenient and easily conceptualized. In practice there is no hard cap on the number of units that can be built in a given MSA; even Manhattan could be built to a much higher density than it currently is without running into a technological capacity constraint (Glaeser, Gyourko and Saks 2005b).

22Bartik-style instruments have been used in a variety of settings to yield exogenous variation in local
migration data from the Internal Revenue Service to form an exogenous demand shifter similar to that of Saiz (2007), who uses “shift-shares” in international immigration patterns as exogenous local demand shocks in U.S. cities.

To form my Bartik-style labor demand variable I use MSA by industry employment data from the Census Bureau’s County Business Patterns (CBP). Bartik’s (1991) innovation was to interact national-level shifts in industry-specific employment with the average shares (across time) of employment or compensation that those industries have in particular cities. For example, when auto industry employment and/or compensation decreases nationwide due to a systemic negative demand shock, the city of Detroit and its surrounding MSA should be particularly negatively affected. To ensure that local conditions in particular MSAs with sizable shares of total national employment in a given industry do not feed back into $industry_{j,t}$, I omit city $j$ from the “national” shift in employment when calculating the variable for city $j$.

To provide a useful check on the employment shift-share variable, which is quite popular in the literature, I also employ county-level migration data from the IRS. The idea behind migration$_{j,t}$ is similar in spirit to that of industry$_{j,t}$: While inflows and outflows of migrants from MSA $j$ are likely endogenous with respect to local supply shocks, we can impute overall inflows for MSA $j$ using the other outflows from MSAs that typically send many migrants to $j$. For example, outflows from New York to Philadelphia, Washington, Los Angeles, and other cities change in response to New York-specific shocks. The sum of these outflows can be used to impute in-migration to Boston, because Boston typically receives a large share of its in-migrants from New York.

Both variables are exogenous to local supply shocks under reasonable but non-verifiable

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It is also possible to construct an industry shift variable using alternative data, such as paid compensation from the Bureau of Economic Analysis’s Regional Economic Information System (REIS). The results are quite similar.
conditions. The \textit{industry}_{j,t} requires that a city’s housing supply shocks are not systematically correlated with national industry shocks that differentially affect that city. Similarly, \textit{migration}_{j,t} will be exogenous provided that supply shocks in a given city are not correlated with out-migration from other cities that usually send lots of migrants to the first city.\footnote{As a robustness check, I also estimate the model using a version of the migration variable that includes only city pairs that are more than a set distance apart, such as 100 miles. See Section \ref{robustness}.}

1.6 Estimation Strategy

As noted above, least squares estimation of Equation \ref{equation:1.6} would yield inconsistent estimates for at least three reasons: The market price of housing is determined in equilibrium and is therefore endogenous, the forecast error \( \nu_{j,t} \) will be correlated with time \( t \) realizations, and the lagged housing density in \( t+1 \) is mechanically correlated with shocks to new investment in \( t \).

The first and third endogeneity concerns can be addressed in straightforward fashion: I use the employment and migration variables detailed in the previous section to instrument for the house price term, and I use the first lag of the quasi-differenced density to instrument for the contemporaneous value.\footnote{Using the lag in this manner requires that the supply shocks be uncorrelated across time, conditional on the MSA and year fixed effects. I examine this assumption in the section on robustness checks.} The set of underlying instruments for \( j \) at \( t \), which I denote \( Z_{j,t-1} \), is thus

\[
Z_{j,t-1} = \left\{ \text{industry}_{j,t-1}, \text{migration}_{j,t-1}, \frac{K_{j,t-2}}{A_j} - \beta \frac{K_{j,t-1}}{A_j} \right\}
\]

The employment and migration share variables are strongly correlated, both individually and jointly, with house prices conditional on the fixed effects; that is, there is a valid first stage. I explore the strength of the exogenous demand shifters for identifying the structural
model parameters in detail in the appendix.

The relationship between the forecast error and endogeneity is more complicated to address. The standard approach in rational expectations models is to use variables dated at or before the time the expectations are formed; under the rational expectations assumption anything in the information set of the agents must be orthogonal to the future forecast errors. It is neither easy nor desirable to do that in this case, because the true forecast lag \( L_j \) differs across cities and may not be perfectly observable, since the Approval Delay Index component of WRLURI likely only captures differentials in lags caused by regulation, rather than the overall size of the time needed to plan before building.\(^{26}\) Moreover, the effect of the forecast error resulting from regulation is not a nuisance in this case but something I am particularly interested in estimating.

Instead, I adopt a hybrid approach, using \( Z_{j,t-1} \) for prices and investment at period \( t \). This roughly corresponds with the time at which permits are issued, one year before construction on a house is completed, which is the bare minimum amount of time needed for the entire process.\(^{27}\) Importantly, however, under the rational expectations assumption these instruments will still be correlated with the forecast error between \( t - L_j \) and \( t - 1 \). To simplify the notation, let \( \hat{P}_{j,t} = \log P_{j,t} - \beta \log P_{j,t+1} \). Consider the forecast error \( \nu_{j,t-L_j} \), which is defined as above by

\[
\nu_{j,t-L_j} = \hat{P}_{j,t} - E_{t-L_j} \left[ \hat{P}_{j,t} \right] = \left( \hat{P}_{j,t} - E_{t-1} \left[ \hat{P}_{j,t} \right] \right) - \left( E_{t-L_j} \left[ \hat{P}_{j,t} \right] - E_{t-1} \left[ \hat{P}_{j,t} \right] \right)
\]

The first term in parentheses in the second line is the forecast error at \( t - 1 \) and the second term is the forecast error between \( t - L_j \) and \( t - 1 \). Under rational expectations, the first term is mean independent of information available at \( t - 1 \), since that information is

\(^{26}\)For example, construction projects in all cities may take an additional year to plan before the city-specific approval delay reported in the ADI.

\(^{27}\)See Figure 1.9
incorporated into the conditional expectation, while the second term is not. Along with the mean independence of the instruments from the supply shocks, this implies that

\[
E\left[\epsilon_{j,t-L_j} + \nu_{j,t-L_j} | Z_{j,t-1}\right] = E\left[\left(E_{t-L_j} \left[\hat{P}_{j,t}\right] - E_{t-1} \left[\hat{P}_{j,t}\right]\right) | Z_{j,t-1}\right]
\]

Rather than making the somewhat implausible assumption that the ADI exactly measures the total lag, I make the less stringent assumption that

\[
E\left[E_{t-L_j} \left[\hat{P}_{j,t}\right] - E_{t-1} \left[\hat{P}_{j,t}\right] | m_j, m_t, Z_{j,t-1}, D_j\right] = \alpha_0^j + \alpha_1^j E\left[\hat{P}_{j,t} | m_j, m_t, Z_{j,t-1}, D_j\right]
\]

where \(D_j\) denotes the delay index in MSA \(j\). In essence, this assumption means that the ADI, interacted with realized prices, serves as a proxy variable for the residual forecast error in Equation 1.7 in the sense of Wooldridge (2002, p. 68). One complication is that the ADI may not be redundant in the main estimating equation; that is, delays may drive up costs on the margin as well as increasing the forecast error. Consequently, the ADI term in the specifications below will capture both the measurement error and true costs, and I will not be able to separate the two effects without relying on nonlinearities in the moment condition.

I allow the parameters with \(j\) subscripts in Equation 1.3, \(\sigma^\chi_j\) and \(\sigma^\eta_j\), to vary by MSA by interacting the primary observables with WRLURI and its sub-indices. Importantly, I take regulation as exogenously given, rather than allowing it to respond to conditions in the housing market or even vary over time. This seems reasonable given that I estimate the model over a relatively short time span, and levels of regulation likely move slowly over time. This simplification is also necessary, both for data reasons — my measure of regulation is observed only once for each city — and to keep the model tractable. I do, 

\[^{28}\text{Other authors endogenize zoning in empirical urban models (Saiz 2010, Epple et al. 2010), while a}\]
however, use preliminary data from a new round of the Wharton survey as a robustness check; the results are similar to my preferred estimates.

Since I am trying to identify both the main effects and interactions with the WRLURI indices, I must specify what functions of the exogenous $Z_{j,t-1}$ and WRLURI I use as the actual instrument set $\hat{Z}_{j,t-1}$. Following a common practice in the econometric literature, I run regressions to get

\[
\hat{L} \left[ \log P_{j,t} - \beta \log P_{j,t+1} | m_j, m_t, Z_{j,t-1} \right]
\]

and

\[
\hat{L} \left[ \frac{K_{j,t-1}}{A_j} - \beta \frac{K_{j,t}}{A_j} | m_j, m_t, Z_{j,t-1} \right],
\]

the linear projections of the quasi-differenced log price and housing density onto the fixed effects and the exogenous industry employment, migration, and lagged density variables. I then multiply these projections by the relevant components of WRLURI for the specification in question and use the projections and the interactions as the instruments in a second-step IV procedure.\(^{29}\)

The advantage of this approach is that it is likely to be more efficient than using an arbitrary set of functions of $Z_{j,t-1}$ and WRLURI as instruments, since it directly imposes the interaction in the instrument set. The disadvantage is that, with exactly as many instruments as endogenous variables, I cannot directly test the overidentifying restrictions that implicitly underly the estimates.

Finally, to estimate Equation \(1.6\) I must either specify or estimate the discount factor $\beta$. Identifying the discount rate has proven to be extremely challenging for other researchers, so I follow much of the literature and simply assume that $\beta = .95$, a commonly accepted value.\(^{30}\) Even after assuming a value for $\beta$, I must still choose how to deal with the compound discount factor $\beta^{L_j}$, since I cannot simultaneously identify it with $\sigma_j^\chi$, $\sigma_j^n$, and the variance of the error term. As I have already argued, assuming values for the construction lag $L_j$, such as the ADI, is not particularly attractive given that the true magnitude of the voluminous literature considers its determinants in a theoretical setting. See Calabrese, Epple and Romano (2007) and Fischel (2001) for just two examples of the latter type.

Note the distinction between this and the typically inconsistent “forbidden regression” (Wooldridge 2002, pp. 236-237).

The results are not sensitive to alternatives in the range of .90 to .99.
lag may be larger than what is reported, even if the ADI appropriately captures differences in the lag. Moreover, one of the points of this paper is to study the effects of increasing the lag. While I must do so indirectly, I certainly do not want to assume away an empirical question of interest. Instead, I let $L_j = g(D_j)$, where $g(\cdot)$ is an increasing function relating the ADI to the actual lag. I then divide the entire equation through by $\beta g(D_j)$ and estimate the normalized equation.

Applying this normalization and Equation 1.7 to Equation 1.6, specifying the interactions using WRLURI excluding the ADI ($Wx_j$) and the ADI ($D_j$), and taking the expectation with respect to $\hat{Z}_{j,t}$ and the fixed effects yields

$$E \left[ (\log P_{j,t} - \beta \log P_{j,t+1}) - (\log C_{j,t} - \beta \log C_{j,t+1}) \right. $$

$$- \frac{\sigma^\eta + \sigma^\eta Wx Wx_j + \sigma^\eta D D_j}{\beta g(D_j) - \alpha^1 D_j} \left( \frac{K_{j,t-1} - \beta A_j}{A_j} \right) $$

$$- \frac{\sigma^\chi + \sigma^\chi Wx Wx_j + \sigma^\chi D D_j}{\beta g(D_j) - \alpha^1 D_j} \left( \log \left( \frac{1}{1 - \frac{I_{j,t}}{A_j - K_{j,t} - 1}} \right) - \beta \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) \right) $$

$$+ m_j + m_t | \hat{Z}_{j,t}, m_j, m_t \right] $$

$$= 0$$

This moment condition could form the basis of an exactly identified nonlinear GMM estimator with fixed effects. To simplify estimation a bit, take the partial derivative of the coefficient on the investment term with respect to $Wx_j$

$$\frac{\partial}{\partial Wx_j} \left( \frac{\sigma^\chi + \sigma^\chi Wx Wx_j + \sigma^\chi D D_j}{\beta g(D_j) - \alpha^1 D_j} \right) = \frac{\sigma^\chi Wx}{\beta g(D_j) - \alpha^1 D_j} > 0$$
and with respect to $D_j$

$$\frac{\partial}{\partial D_j} \left( \frac{\sigma \chi + \sigma \chi W x_j + \sigma D_j}{\beta^g(D_j) - \alpha^1 D_j} \right) = \frac{\sigma \chi D_j}{\beta^g(D_j) - \alpha^1 D_j} - \left( \frac{\sigma \chi + \sigma \chi W x_j + \sigma D_j}{(\beta^g(D_j) - \alpha^1 D_j)^2} \right) \left( \beta^g(D_j) \log (\beta) g'(D_j) - \alpha^1 \right) > 0$$

These partial derivatives indicate that the coefficient on the investment term is (weakly) increasing in $W x_j$ and in $D_j$, as is the coefficient on the capital stock term. I assume away any interaction and linearize the compound parameters in these variables, yielding:

$$E \left[ (\log P_{j,t} - \beta \log P_{j,t+1}) - (\log C_{j,t} - \beta \log C_{j,t+1}) \right. - \left( \bar{\sigma}^n + \sigma^n W x_j + \sigma^n D_j \right) \left( \frac{K_{j,t-1}}{A_j} - \beta \frac{K_{j,t}}{A_j} \right) - \left( \bar{\sigma}^\chi + \sigma^\chi W x_j + \sigma^\chi D_j \right) \left( \log \left( \frac{A_j - K_{j,t-1}}{I_{j,t}} \right) - \beta \log \left( \frac{I_{j,t+1}}{A_j - K_{j,t}} \right) \right) \left( \beta^g(D_j) \log (\beta) g'(D_j) - \alpha^1 \right) > 0$$

I use this moment condition as the basis for a linear-in-parameters IV estimator.

### 1.7 Reduced-Form/Myopic Model Estimates

Before presenting estimates from the full model, I provide some basic regression and IV results that generally follow Equation 1.9 but ignore forward-looking behavior on the part of landowners. These results illustrate the patterns in the data in a transparent way and

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Footnote: The denominator in both expressions, $\beta^g(D_j) - \alpha^1 D_j$, must be positive because the expectation of the forecast error, conditional on the instruments, is bounded by the conditional expectation of the quasi-differenced price term.
mimic typical approaches from the existing literature that can be compared with my struc-
tural estimates. They can also be interpreted as a reduced form of the structural model,
although they are misspecified in that they ignore forward-looking behavior.

To specify these regressions, I start with moment condition 1.9 and make several sim-
plications. First, and most importantly, I assume that agents are myopic and do not take
into account future prices or investment, which means setting all the one-period-ahead
terms \( \beta P_{j,t+1} \), e.g.) to zero. Second, I use the fact that \( \log \left( \frac{p}{1-p} \right) \approx \log (p) \) for small \( p \)
to simplify the investment term, since the probability of investment for any slot is never
more than 0.002 in my data. Third, I isolate the investment term on the left-hand side and
linearize the resulting price coefficient so as to parallel typical regression models relating
investment and price. These changes yield the equation

\[
\log \left( \frac{I_{j,t}}{A_j - K_{j,t-1}} \right) = (\bar{\lambda}_P + \lambda^{PW}W_{x_j} + \lambda^{PD} D_j) (\log P_{j,t} - \log C_{j,t}) \\
+ (\bar{\lambda}_K + \lambda^{KW}W_{x_j} + \lambda^{KD} D_j) \frac{K_{j,t-1}}{A_j} + m_j + m_t + \zeta_{j,t} \tag{1.10}
\]

in which I have appended \( \zeta_{j,t} \) as an error term that is mean zero across \( j \) and \( t \) by assump-
tion.

Ignoring any endogeneity concerns about \( \zeta_{j,t} \) for the moment, I estimate several ver-
sions of Equation 1.10 using ordinary least squares; the results are presented in Table 1.3.
In the first column I regress the log investment probability on log price and the housing
density, with no interactions, while including MSA and year fixed effects to pick up per-
sistent differences in MSA supply-side conditions or nationwide year-specific shocks. The
fixed effects allow me to focus on the effects of transitory city-specific shocks.

On average across years and MSAs, I find that a 1 percent increase in price is associated
with a 1.34 percent increase in investment, with a standard error of just 0.05 percent. Mean-
while, a 1 percentage point higher density is associated with 1.59 percent less investment.
Since the stock is less than five percent of developable area in almost all MSAs and shifts quite gradually within a given MSA, most of the within-MSA volatility is attributable to changes in price.

In principle I would like to take advantage of the RS Means construction cost data in both these reduced-form estimates and the structural model. Column (2) in Table 1.3 shows the results of including log construction costs as a covariate; unsurprisingly, higher construction costs reduce investment, all else equal. The standard errors are large enough that I cannot reject the null hypothesis that the coefficients on construction costs and price are the same, as the model suggests is appropriate. The downside of the RS Means data is that they are only available for a subset of MSAs — a little over half the sample of MSA years that are otherwise available. Since including construction costs in the regression leaves the coefficients nearly unchanged from an unreported version of column (1) that uses the same sample as column (2), I omit them from subsequent regressions.

The final two columns in the table break apart the supply elasticity and density coefficient and examine how they vary across cities. In column (3) I interact both log price and the density with the top-line WRLURI (“Regulation”), which I standardize so that it has a mean of zero and standard deviation of one across MSAs. The first line of column (3) indicates that a city with an average level of regulation has an elasticity of about 1.7, while each one standard deviation increase in regulation reduces the elasticity by about 0.4. The interaction of regulation with the density is also highly statistically significant and very large relative to the mean effect of -0.67, indicating that more regulation may cause cities to “fill up” more quickly. Even without a structural interpretation or clear identification, the sheer size of the effect of regulation on these estimates is noteworthy.

Finally, column (4) repeats the regression from column (3) but separately interacts log price with the Approval Delay Index (“Delays”) and a version of WRLURI that excludes the ADI (“Regulation excl. Delays”). While the two sub-indices are highly correlated, the
first should correspond to an increase in the amount of time it takes to prepare for and acquire a building permit, while the second should capture all other cost-shifting regulations imposed by local governments, such as density restrictions, open space requirements, and so forth. Delays are likely to affect the supply elasticity by increasing the forecast error and thus lowering the correlation between \( \log P_{j,t} \) and \( E_{t-L_j} [\log P_{j,t}] \), while the “everything else” measure works by raising costs faster in regulated areas as investment increases.

Both measures have a substantial effect on the estimated supply elasticity. A city with average delays and average other regulation has an estimated elasticity of about 1.7, while one additional standard deviation of delay reduces the elasticity by about 0.4 and a one standard deviation increase in other regulation reduces the elasticity by 0.14. These estimates suggest that delays may be a particularly important component of regulation, at least with respect to supply elasticity. Interestingly, the magnitude of the density coefficient is decreasing in delays but much more sharply increasing in other forms of regulation. Cities seem to be “filling up” more quickly when there is more regulation, but not when there are more delays alone. This is a comforting result, suggesting that the “everything else” measure of regulation may in fact be picking up density limitations or other related factors.

1.7.1 IV

While enlightening, these parameter estimates are likely to be inconsistent — even ignoring dynamic considerations — for two reasons: First, as noted above, unobserved supply shocks will be correlated with realized prices, which are determined in equilibrium. Second, the error in the forecast of \( \log P_{j,t} \) is correlated with that term by construction, since it must be mean independent of the expectation under the rational expectations assumption.

32 The changes in forecast error are similar in spirit to a measurement error problem, but in this case I am interested in estimating the changes in the coefficient that result from differential forecast error magnitudes across cities, rather than in simply overcoming a threat to identification. See the discussion in Section 1.6 above.
To deal with these issues, I employ migration$_{j,t-1}$ and industry$_{j,t-1}$, as discussed in Section 1.6 above. I project log $P_{j,t}$ onto these two variables and interact the projection with WRLURI and its sub-indices as needed to identify the interaction terms. Table 1.4 uses this IV method to reestimate the specifications from Table 1.3. The elasticity estimates in the first line are similar to those from the previous table, while the effects of rising density are nearly identical. As usual with IV relative to least squares, the standard errors on the elasticities are quite a bit larger.\footnote{The two variables are extremely robust predictors of house prices conditional on the fixed effects; I discuss the “strength” of these exogenous variables for identifying the full dynamic model in the next section.}

The pattern of the interaction terms in columns (3) and (4) are also similar to Table 1.3. The effect of delays on the elasticity is more negative in column (4), while the effect of other regulation is estimated to be nearly zero. Non-delay regulation does have a very large effect on the stock/area coefficient, however, while the ADI interaction is positive, again suggesting that delays affect the elasticity of supply with respect to price, while other forms of regulation lead to higher prices as cities use up available land.

With or without IV, the coefficients estimated in this section are economically sizable. An elasticity of about 1 means that a typical change in log investment of 0.25 must drive up prices on the margin by 25 percent for the system to be in equilibrium.\footnote{The 0.25 figure is the mean across MSAs and years of the absolute change in log investment.} At the mean house price in my sample of about $140,000, that price increase is $35,000. For a city like San Francisco, which has an approval delay six months above the mean — about 2.2 standard deviations — the predicted supply elasticity using column (4) is about 0.5. This corresponds to an increase in marginal costs of more than 50 percent, which is $200,000 at San Francisco’s high price level, for each 0.25 increase in log investment.

These results are informative but only suggestive of the true cost parameters and elasticities because they do not take dynamics into account. \textit{A priori}, we should expect the true elasticities — the response of supply to a one-time increase in price — to be substan-
tially higher than these myopic estimates, since price changes are positively autocorrelated (Glaeser and Gyourko 2007). That is, a price increase in a given city today is likely to be followed by another increase tomorrow, so what appears to be a small response of investment to a large increase in price may simply be a rational response to price dynamics. This is precisely what I find empirically, as I discuss in the next section.

1.8 Structural Model Estimates

1.8.1 “First Stage”

Before moving on to the structural estimates, it is valuable to consider whether the exogenous demand shifters provide sufficient variation to identify the structural parameters in my model; in other words, whether my instruments are sufficiently “strong”. This is particularly important given the “black box” nature of $industry_{j,t}$ and $migration_{j,t}$. The well-known benchmark that the first stage should have an F statistic of greater than 10 does not technically apply in this case because I have multiple endogenous regressors in most of my specifications. Instead, Stock and Yogo (2003) suggest examining the smallest eigenvalue of the matrix equivalent of an F statistic, taking into account all of the regressors and instruments simultaneously.

Using this metric, all of the specifications below easily reject the null that the projections of the endogenous variables onto the exogenous demand shifters are weak instruments in the second-step IV specification, according to any of the benchmarks provided in Stock and Yogo (2003). Since this test for weak instruments is not particularly transparent, I also present a complementary set of “first-stage” regressions that better illustrate why the instruments are not weak. Specifically, I show regressions of some of the endogenous terms in Equation 1.9 on $\hat{Z}_{j,t-1}$, which is the set of linear projections of the endogenous covari-
ates on the exogenous variables $Z_{j,t-1}$ and the fixed effects. I then report F-statistics and p-values for the relevant projection instrument for each endogenous variable. The “first-stage” regressions shown in Table 1.5 correspond to a version of Equation 1.9 that includes interactions with the top-line WRLURI ($W_j$), the estimates of which are shown in column (2) of Table 1.6 below.

The results in Table 1.5 make clear that the demand shifters have a strong relationship with the endogenous covariates. The relevant projection instruments

$\hat{L}[(\log P_{j,t} - \beta \log P_{j,t+1}) | m_j, m_t, Z_{j,t-1}]$ for $(\log P_{j,t} - \beta \log P_{j,t+1})$, and so forth) always have F-statistics far above 10, and the F-statistics for the density variables are enormous because the capital stock and housing density move so slowly over time. Importantly, columns (2) and (4) indicate that the interaction terms are also very strongly correlated with the relevant interacted projection instruments.

### 1.8.2 Parameter Estimates

Having established the basic correlations in the data, including the strength of the instruments, I now turn to estimating the structural parameters of the dynamic supply model. Table 1.6 shows estimates for several variants of Equation 1.9. Column (1) is a baseline specification that does not allow for any MSA-level heterogeneity, so the reported parameter values are averages across high- and low-regulation cities.

The value for the parameter $\bar{\sigma}_\chi$ on the first line of the table can be interpreted as the percentage increase in the cost of constructing a single house, on the margin, that results from a 1 percent increase in investment in a given period. Increasing investment by 1 percent leads to 0.53 percent higher costs, which we can invert to get a supply elasticity.

\footnote{Note that although I normalize the coefficient on the price term to be one in the structural estimates, making it in essence the “left-hand side variable”, it is really the investment term that is the choice or dependent variable. Under the assumptions required for GMM or IV estimation, there is no true left-hand side, so the choice of normalization is up to the econometrician.}
of about 2. Holding other factors fixed, increasing log investment by 0.25, which is the mean across MSAs and years of the absolute change in log investment, increases costs on the margin by about 13 percent. For an average home in my sample, which is worth about $140,000, that corresponds to an increase in price of more than $18,000.

The impact of using up land — that is, increasing the capital stock relative to the developable land area in a city — is less important for annual dynamics, but still relevant over the long run. The estimated value of $\bar{\sigma}^\eta$ in column (1) indicates that a 1 percentage point increase in the housing density increases costs by more than 450 log points. While this parameter is superficially larger than the marginal cost parameter, it can only be interpreted once we note that the overall housing stock changes quite slowly. Even booming cities like Las Vegas and Phoenix in the mid-2000s add only a couple hundredths of a percentage point to their densities in a given year. The mean absolute change in the density is about 0.006, and a shift of that magnitude changes costs less than 3 percent, or $4,000 for the average home.

Over the long run, however, density or land scarcity can be an important factor in a city’s growth, as argued by Saiz (2010). For example, Las Vegas more than quadrupled its housing stock between 1980 and 2008, bringing its density (relative to Manhattan) up from 0.1 to nearly 0.5. Ignoring the level of regulation, this would suggest an increase in costs and prices on the margin of more than 150 log points, which is several hundred thousand dollars. Even in the price boom of the mid-2000s, however, prices in Las Vegas did not rise anywhere near that much.

To help explain why, we can examine the impact of construction costs and regulation. Column (2) shows that the estimated parameter values are similar when the R.S. Means construction cost is included in quasi-differenced form, as in Equation 1.9.\[36\] The small changes in the coefficients are due almost entirely to the reduction in sample size when construction costs are included.
cient on the cost term is small and statistically indistinguishable from zero, despite the fact that our null, based on the theory, should be a coefficient of 1. The likely explanation for this is that the construction cost measure is highly smoothed relative to reality, so that the quasi-differencing and fixed effects remove nearly all of the relevant variation.

Regulation plays a more interesting and important role. In column (3) I interact the marginal cost and density parameters with the top-line WRLURI measure of regulation. As in the reduced-form/myopic estimates, we can see a strong impact of regulation, raising marginal costs — including via delays and the forecast error — and by extension reducing the price elasticity of supply. Since the regulation measure is standardized to have mean zero, the first line of the column indicates that an MSA with an average level of regulation has a marginal cost increase of 0.39 percent for each 1 percent increase in investment.

For a city with regulation one standard deviation above the mean, this figure rises to 0.53 percent. Multiplying this by 0.25, which is a typical change in log investment, and the average price of housing in my sample yields an increase of $19,000 per house on the margin. For a very regulated and expensive city like Boston, with a standardized WRLURI value of about 2 and average house price of $250,000, the same increase in investment would increase costs by $43,000, nearly a fifth the price of the house.\footnote{Of course, part of the reason that house prices in Boston and similar cities are so high in the first place is because of regulation. Part of the effect on the price level would show up in the fixed costs, that is, per-house construction costs that do not change with the level of investment. Since I focus on dynamics and volatility, I include MSA fixed effects here and then decompose the effects of regulation on those fixed effect terms below.}

In column (4) I break apart regulation into its two subcomponents, delays and the “everything else” measure that comprises all the WRLURI sub-indices except the ADI. For a city with average delays and average “other” regulation, a 1 percent increase in investment raises the marginal per-house cost of construction by 0.34 percent. Approval delays of one standard deviation more than the mean city increase this effect by 0.09, with a standard error of 0.03.\footnote{As discussed extensively above, the delays can affect this cost parameter through three complementary...} A single standard deviation in other regulation (\(\sigma W\)) has a similarly sized

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\footnote{As discussed extensively above, the delays can affect this cost parameter through three complementary...}
impact on marginal costs.

The density parameters for an average regulation city in columns (3) and (4) are smaller than the un-interacted parameter in column (1), though the standard errors are larger. I find essentially no effect of regulation in column (3), but the standard error is large enough that I cannot draw any meaningful conclusions. In column (4) I find no significant relationship between the ADI and density, but I do find a significant positive effect of regulation excluding delays. A one standard deviation increase in this measure increases the cost of density by 0.75, or about a sixth of the average cost per unit of density. As I noted in the reduced form work above, this suggests that WRLURI excluding the ADI is picking up density restrictions or other factors that raise costs faster as more land is developed.

In Table 1.7, I show how marginal costs vary across cities with different measured levels of regulation. I use the estimated parameter values for the marginal cost of investment in column (4) of Table 1.6 and the regulation levels reported in Table 1.2 to impute the marginal cost of investment in each city. The first column shows the approximate percentage impact on marginal costs of a 25 log point increase in investment, which is a good yardstick because it is the average absolute change in investment across all MSAs and years in my sample. The second column multiplies this percentage by the average real house price in each city over the period from 1984 to 2008.

Cities with substantial levels of regulation, either via approval delays (the ADI) or other components of WRLURI, have much higher marginal costs of investment. For example, the impact of the 25 log point change in investment is nearly twice as large in New York and San Francisco (both a bit under 15 percent) as in Atlanta (9 percent), and more than twice as large as in Houston (less than 7 percent). When translated into dollar terms, these differences are even larger, since average prices are much higher in regulated coastal cities.
than in relatively unregulated cities in the interior. Prices in Atlanta would shift by just $15,000 in response to a 25 point baseline change in investment, while in New York they would shift three times as much and in San Francisco more than four times as much.

The natural response of landowners and builders to these higher marginal costs is to mitigate their increases in investment in response to demand-driven price increases. As a result, price elasticities are lower on the coasts than in the interior. For example, I estimate that the price elasticity of supply in both New York and San Francisco is about 1.7, while the elasticity in Atlanta is 2.8 and in Houston 3.8. These elasticity estimates are larger than have been previously estimated in the literature, likely because my structural estimates explicitly account for dynamics; the myopic estimates in section 1.7.1 are much closer to those of Saiz (2010), for example.

1.8.3 Fixed Costs

Although marginal costs and lags drive elasticities, MSA-level fixed costs — in essence, the MSA fixed effects in my estimates in Table 1.6 — are also important in determining volatility. Even if the elasticity of new supply with respect to price shocks is very high, the impact on the total stock and thus the feedback mechanism will be small if the level of new supply is low with respect to the stock. For example, the elasticity of new supply in San Francisco is less relevant for price volatility because the average level of investment is so small relative to the number of extant homes. Consequently, it is important to examine both fixed and marginal costs.

In Table 1.8 I take the MSA fixed effect estimates from the model in column (4) of Table 1.6 which includes both delays and other regulation as separate interactions, and regress them on the WRLURI measures. This provides estimates of the effect of regula-

39These costs are “fixed” with respect to the level of new investment; they are imposed equally on all new houses regardless of how many are built in a given year.
tion on fixed costs and the nonresidential value of land in each MSA, on average across time. The advantage of doing this fixed cost decomposition separately from the marginal cost estimation above, rather than omitting the fixed effects there and having regulation enter directly, is that the marginal cost estimates will be consistent with the fixed effects included even if there are correlated omitted variables or other sources of endogeneity that are constant within MSAs.

It is clear from Table 1.8 that regulation does have demonstrable effects on these costs. For example, in column (1) we see that a one standard deviation increase in regulation increases costs by about 2.7 percent, which translates to $3,800 for the average house price in my sample of $140,000. When I break apart delays and other regulation in column (2), we can see that most of this effect comes from the non-delay components of WRLURI: A one standard deviation increase in this measure increases fixed costs by about 2.6 percent. The explanatory power of regulation in these regressions is low — the \( R^2 \) is just 0.04 — which may be because we have already accounted for substantial effects of regulation on costs via the marginal cost and density terms discussed above.

Costs of these magnitudes are important, and they are if anything likely to be underestimated if more housing regulation is endogenously imposed in response to rapid growth and investment at the MSA level. If high-investment (low-cost) cities impose more regulation, this would dampen the positive estimated causal impact of regulation on costs. Conversely, if factors other than regulation are positively correlated with both low investment and high regulation, then these costs will instead be overestimated. In Section 1.9 I take these estimates and those in the previous section and simulate the model to show how differences in fixed and marginal costs caused by regulation affect volatility.
1.8.4 Robustness Checks

In estimating the structural model I make a series of sometimes restrictive assumptions. In this section I check the robustness of my results to alternative specifications, which are shown in Table 1.9. Each column is a reestimate of column (3) from Table 1.6 — the model with WRLURI interacted with the marginal cost and density parameters — using a different specification.

Arguably the most important assumption underlying the estimates is the exogeneity of regulation, in particular WRLURI and its subcomponents. I rely in particular on the notion that regulation is constant over time, or at least that it does not shift in response to house price volatility or supply elasticities over the relatively short horizon of my data\[^{[10]}\] Although I cannot directly test this assumption without better data and substantially complicating the model, I am able to provide suggestive evidence using preliminary data from a new version of the Wharton survey\[^{[41]}\]

Using the raw data from the new survey, I compute versions of WRLURI and the ADI following as closely as possible the original methodology\[^{[42]}\] I find that the 2010 WRLURI and ADI are highly correlated with the 2005 versions, with correlation coefficients at the MSA level of about 0.7 and 0.6 respectively. This is despite the fact that there are likely to be some errors in the preliminary new data that have not yet been corrected and despite different samples of responding jurisdictions within the MSAs.

To further examine the importance of possible changes in regulation over time, we can turn to column (1) of Table 1.9 which replaces my standard (2005) regulation measure with the one derived from the 2010 survey. The results are broadly similar to those in column (3) of Table 1.6. For a city with average regulation, a one percent increase in investment

---

\[^{[10]}\]Regulation that responds to or is otherwise endogenous to long-run price levels would only bias my estimates of the fixed costs, not the main estimates, thanks to the MSA fixed effects.

\[^{[41]}\]I am grateful to Joe Gyourko and Anita Summers for making the raw 2010 survey data available to me prior to their publication.

\[^{[42]}\]Changes in the survey questions between rounds necessitate some judgment in this process.

41
raises costs on the margin by 0.23 percent, somewhat less than the 0.39. More importantly, the interaction effect with WRLURI is 0.20, even larger than previously estimated. The density parameters, meanwhile, are generally similar but too noisily estimated to draw any robust conclusions.

Since it is this $\sigma^{\chi W}$ term that is vital in explaining differences in elasticities across housing markets, it is greatly comforting that the estimate is similar using the new round of the survey. While this is not definitive proof that regulation has not endogenously changed over the full 25-year sample period, it goes some way towards easing any concerns.

In column (2) I estimate the model including the R.S. Means construction cost data in the price term, essentially normalizing the coefficient on costs to be one, as theory suggests. This increases the main effect of investment on costs and reduces the interaction with regulation, leaving it less than a standard error away from zero. Given the results in column (2) of Table I.6, this is not a particularly surprising finding. There I found that, after including the fixed effects and quasi-differencing, there was no measurable effect of observed construction costs on the cost of investment. Lumping construction costs in with the price term, then, serves to wash out the interaction with regulation.

The final three columns examine the possibility that particular forms of within-sample correlation in supply shocks render one or more of the instruments endogenous. For example, if the supply shocks $\epsilon_{j,t}$ are follow a first-order autoregression over time, then I cannot use the first lag of the quasi-differenced density term as an exogenous shifter of the contemporaneous value.\footnote{Other than through this channel, serial correlation does not in general affect the consistency of IV or GMM estimates, although it does require an adjustment to the standard errors beyond making them robust to heteroskedasticity (Hayashi 2000, pp. 406-412).} In column (3) I instead use the second lag, $\frac{K_{j,t-2}}{A_j} - \beta \frac{K_{j,t-2}}{A_j}$, as part of $Z_{j,t-1}$. The results are nearly identical to those in column (3) of Table I.6.

Alternatively, supply shocks could be spatially rather than temporally correlated. This could render my migration-based demand shifter endogenous, since it relies on the as-
assumption that supply shocks in a given city are uncorrelated with outflows from other cities, which are presumably affected by their own supply shocks. In column (4) I drop migration\(_{j,t}\) entirely and rely solely on the industry\(_{j,t}\) to get exogenous variation in house prices. In column (5), I use an alternative version of the migration variable in which, for the calculation of the value for MSA \(j\), I exclude MSAs that are less than 100 miles away from \(j\). This should alleviate concerns that the variable could be correlated with local supply shocks that are spatially autocorrelated at relatively close distances.

Compared with column (3) in Table 1.6, the estimates do differ somewhat, particularly in column (4). The main marginal cost estimate in (4) is a little under half the 0.39 previously estimated, while in (5) it is slightly larger. The housing density parameters also differ somewhat, which is unsurprising given that they are never very precisely estimated. Importantly, however, the interactions between regulation and marginal costs, the \(\sigma^{\chi W}\) parameter, are if anything larger than the preferred estimate. All told, I continue to find strong effects of regulation on marginal costs and thus supply elasticities throughout the robustness checks in Table 1.9.

### 1.9 Model Simulations

Although the estimated parameters are interesting in themselves, one of the most valuable parts of estimating a structural model is the ability to perform simulations of various scenarios, including counterfactuals. In this section I show the results of a series of simulations, first varying parameters one at a time to demonstrate the impact and then showing how the estimated parameters for two cities — San Francisco and Atlanta — imply substantially different amounts of volatility, even given the exact same demand shocks.
1.9.1 Solution Method

The use of conditional choice probabilities enables me to consistently estimate the dynamic model of housing supply without fully solving it. But I must solve the model to simulate the behavior of housing markets under different policy regimes. The model can be numerically solved for a given set of parameters in one of two ways: perturbation around a deterministic steady state, or fixed point iteration on the marginal condition, Equation 1.3. Perturbation allows for rapid solution and can be implemented using standard software, but it may introduce substantial approximation error. Conversely, fixed point iteration is very slow but can approximate the solution of the model arbitrarily well (Aruoba, Fernandez-Villaverde and Rubio-Ramirez 2006). I solve the model numerically using fixed point iteration over a finite grid covering $S_{j,t}$ and then interpolate between grid points using splines. I calculate expectations by integrating over the relevant distributions using quadrature methods.

The solution algorithm works as follows: Given a guess of investment $I(S_{j,t})$ at each grid point and a spline interpolation between them, solve for the $I(S_{j,t})$ at each grid point that satisfies the rational expectations assumption and the marginal condition. That is, find the level of investment that implies a next-period capital stock consistent with prices that justify that amount of investment, and repeat until convergence. Given a fine enough grid and a flexible enough interpolation, we can approximate the true $I$ function arbitrarily well. In practice, I can solve the model in a reasonable amount of time with a state space of no more than two or three dimensions. In the simulations presented here, I include in the state space only the capital stock, a demand shock and a shock to the user cost of housing. This allows me to highlight the feedback effects of supply that serve to dampen the impact of demand shocks on prices.
1.9.2 Demand Estimation

Before I can actually perform any simulations using the supply side parameters I estimated above, I must first estimate the demand-side relationship between the capital stock and rents, as well as estimating a parsimonious time-series relationship for the demand shocks. Estimating the theoretical demand curve, which relates spot housing rents to the capital stock as in Equation 1.1, is not a trivial endeavor for at least two reasons. First, as noted by Topel and Rosen (1988), demand shocks likely drive much of the high-frequency variation in investment. Finding variation in the housing stock that is orthogonal to these demand shocks is thus quite difficult. Second, all of the available data on rents are for apartments and cover only a relatively small subset of metropolitan areas.

I have tried various approaches to estimating the underlying demand curve using data on apartment rents from REIS, a firm that tracks the commercial real estate sector, as well as tenants’ and owners’ equivalent rent data from the Consumer Price Index, which are available for an even smaller subset of MSAs. In all cases I find substantially higher price volatility than rent volatility, a similar phenomenon to that discovered by Shiller (1981) for stock prices. I also find lower inverse elasticities of demand with respect to rent than with respect to prices. That is, prices are more responsive to changes in the capital stock than rents. This is hard to square with the usual user cost equation, as in Equation 1.2 since a transitorily low capital stock (and corresponding high rents and prices) should induce more construction and lower rents in the future. Prices should thus increase less in response to a low capital stock than spot rents.

This phenomenon could simply be a result of using inconsistent rent and house price data. Regardless, to match the observed data as well as possible I relate prices to the size of the capital stock in two steps. First I estimate an inverse demand curve following Equation 1.1 by using lags of the capital stock to instrument for the current capital stock, under the assumption that the time dependence of the demand shocks is limited. I include the log
wage, along with MSA and year fixed effects and MSA-specific time trends, to soak up as much variation as possible in the demand shocks.

\[
\log (R_{j,t}) = \phi_K \log (K_{j,t}) + \phi_n \log (n_{j,t}) \\
+ \phi_w (wage_{j,t}) + \phi_j + \phi_t + \phi_{jt} + \epsilon_{j,t}^D
\]

I start with an least squares regression of this equation and then instrument for the capital stock at \( t \) with its first, second and third lags in turn. Using the first lag as an instrument is valid if the demand shocks are serially uncorrelated, while the second lag is a valid instrument if the dependence of the shocks lasts no more than one period, and so forth. Since the correlation of the time \( t \) stock with its own lag is smaller as the length of the lag increases the estimates become increasingly noisy, although the first stage remains quite strong, with an F statistic well over 10 in all cases. I constrain \( \phi_K = -\phi_n \) for the purposes of recovering parameters for simulation, since the population-to-stock ratio is nearly constant over time within each MSA, as we should expect if household sizes have been roughly constant within each MSA over the last 30 years.

Table 1.10 shows the estimates of Equation 1.1. I find inverse elasticities of demand in the range of -1.7 to -2.8, which implies rent elasticities of demand of -0.35 to -0.60. These figures are in line with other estimates in the literature, many of which take quite different approaches to estimation.\footnote{See, e.g., Hanushek and Quigley (1980).} Using the one-lag IV inverse elasticity estimate of -2.14, I back out a value for the intercept of 1.1 that corresponds roughly to the average MSA in my sample.

The second step is to use the user cost relation in Equation 1.2 to translate movements in rents into prices. For the remaining variables in Equation 1.2 — interest rate \( r_t \), income tax rate \( \tau_{j,t} \), property tax rate \( \omega_{j,t} \) — I use the ex post real interest rate on a conventional 30-
year fixed rate mortgage; state and federal tax rates from the NBER’s TAXSIM database; and property tax data from Emrath (2002) and the Significant Features of Fiscal Federalism series. For \( \gamma_{j,t} \), the risk premium of owner-occupied housing, I use the Flavin and Yamashita (2002) estimate of 2.0 percent. To calculate MSA-specific depreciation rates, I subtract the population growth rate in each MSA from the ratio of investment to the capital stock and take the average. Under the assumption that the average population-to-stock ratio is constant over time in each MSA, any additional average growth in the capital stock must be going to replace units lost to depreciation.

Rather than explicitly allow these observable terms to vary in simulation, I instead use the average values for each MSA and hold them constant over time. For each MSA and year, I then calculate the log rent implied by (time-varying) house prices and the constant user cost terms. By regressing this log implicit rent (\( \log(\tilde{R}_{j,t}) \)) on actual log rent, log population and the log housing stock, I am able to estimate the direct effect of changes in the capital stock on log prices, apart from the indirect effect implied by changes in rent. This direct effect could be due to endogenous changes in the risk premium or some effect on expected capital gains outside the relatively simple framework I impose here. Regardless, the residuals from this regression serve as a “user cost shock” that incorporates changes in interest rates, risk premia, and taxes in a single variable.

\[
\log(\tilde{R}_{j,t}) = \pi_R \log(R_{j,t}) + \pi_K \log(K_{j,t}) + \pi_n \log(n_{j,t}) + \pi_j + \pi_t + \pi_{jt} + \psi_{j,t}
\]

My estimates of the preceding equation are shown in Table 1.11. As in the inverse demand

45 Using the mortgage interest rate here implies that houses are entirely financed by debt, with no down-payment, but the results are not sensitive to the choice of interest rate.

46 This approach requires that the demand shocks follow a first-order Markov process, which is what I assume for simulation purposes. The estimates of the effect of the capital stock on price conditional on rent are similar if I include additional lags of rent.
estimates, I use various lags of the capital stock to instrument for the current capital stock. I also include fixed effects and time trends, as well as constraining \(\pi_K = -\pi_n\) to ensure stationarity. Depending on the specification, I find a range of large estimates of this direct effect of the capital stock on house prices. The OLS estimate of the price-stock elasticity in the first column is downward biased because the capital stock at time \(t\) includes new investment, which depends on prices. For simulation purposes I use -2.46, which is the estimate using the first lag of the capital stock as an instrument. This is the most conservative choice, apart from the clearly biased OLS estimate, since the more negative estimates in subsequent columns imply a larger feedback effect of investment into prices.

Finally, using my estimates of the last two equations, I calculate the demand and user cost residuals and estimate MSA by MSA first-order vector autoregressions to capture the interdependence of these shocks over time. I find no strong effects of the lagged user cost shock on the current demand shock, but I do find large positive effects for the remaining three coefficients. I use the average VAR coefficients of about 0.8 (demand shock on lagged demand shock), 0.8 (user cost shock on lagged user cost shock), and 0.5 (user cost shock on lagged demand shock) to parameterize the demand-side processes.

1.9.3 Simulations

One way of showing how cities with different supply constraints respond to demand shocks is to plot their impulse response functions. In Figure 1.11 I plot the impulse responses of San Francisco and Atlanta to a one-time demand shock of about 3 percent, which is the average standard deviation of the demand innovations in the MSA vector autoregressions estimated above. Since the demand shock follows an AR(1) process with a coefficient of 0.8, the shock decays relatively slowly over time. In addition, a follow-on direct effect on house prices results from the impact of the demand shock on the future user cost shock.

Although the shocks in each city are identical, the resulting rent and price processes are
very different. \footnote{To ensure comparability in the figures, I have normalized the price, investment and capital stock paths in each city by dividing by the mean.} By construction, rents in both cities jump by the same amount, but they drop back much more quickly in Atlanta. Meanwhile, house prices in Atlanta jump by less than in San Francisco and are back to baseline 10 years after the shock. In San Francisco prices take 20 years to return to baseline.

The explanation for this difference is evident in the bottom two panels: In equilibrium, the initial investment response in percentage terms appears much the same in the two cities, but because average investment is so much higher in Atlanta, the same percent increase means a much larger increase in the size of the capital stock. Consequently, by year 10 the stock in Atlanta has increased by about 0.4 percentage points, compared with 0.1 percentage points in San Francisco. This explains why rents return to baseline faster and why prices never jump as much in the first place, since the supply response is built into expectations.

An alternative way to examine dynamics is to repeatedly simulate the model with randomly drawn house price shocks and examine the moments of the resulting price and investment paths. The first four lines of Table 1.12 present basic results for a series of simulations with different supply parameters but identical demand-side conditions. As a baseline I use a one-year lag and the marginal cost of investment \( \sigma^\chi \) from Equation 1.6 implied for a city with regulation at the MSA median by the results in column (4) from Table 1.6. The second line (“Low Regulation”) uses the fixed and marginal costs implied for cities at the 25th percentile of delays and the all-else regulation measure, while the third line (“High Regulation”) uses the costs implied for cities at the 75th percentile of those measures. The fourth line uses a two-year lag but no direct changes in marginal costs. In each case I simulate the model using the same 100 randomly chosen 25-year paths for the demand shock and then average the relevant moment across the simulations. \footnote{To focus on short-run effects, I ignore any effects from changing the housing density, that is, the degree}
Comparing the first and second lines of the table, we can see that lowering regulation reduces the mean price and the standard deviation of log prices relative to the baseline. Volatility by this measure is about half a percentage point lower. Since the demand shocks are identical in each case, these differences must result from differences in investment. The standard deviation of log investment is appreciably higher when regulation is lower because the elasticity is higher: Landowners and builders are able to respond to demand shocks by increasing investment and the capital stock when demand is high, thus attenuating the impact of the shocks on price.

The converse is true when regulation is higher. Comparing the third line with the second, we can see that going from the 75th percentile of regulation to the 25th percentile reduces the volatility of prices by 20 percent, even as it increases the volatility of investment by about half. Moving from a one-year lag to a two-year lag, in the fourth line, has similarly sized effects on price and investment volatility. I find these sizable differences despite conservatively choosing parameters such as the elasticity of demand and the persistence of demand shocks.

The last two lines of the table show how real-world differences in regulation can have dramatic impacts on housing markets. I simulate the model using the supply parameters estimated for San Francisco and Atlanta, relying in particular on their differing values of the ADI and the other components of WRLURI. The demand-side parameters, including the rent and user cost intercepts and elasticities, are precisely the same in both simulations, with the sole exception of the population growth rates, which I set equal to their actual values in each city. This is a simple but reasonable approximation to the true migration response to differences in house prices, which I do not incorporate in the model or simulations.

The highly regulated city (San Francisco) has much higher and more volatile prices and much lower investment than the less regulated city (Atlanta). In relative terms, investment of land scarcity.
is similarly volatile in San Francisco, precisely because prices are also more volatile, but because average investment is so low relative to the capital stock, even large changes in investment have a minimal feedback effect on price. This reemphasizes the point that both the elasticity — in percentage terms — and the average level of new investment matter for volatility.

Since I completely shut down migration and use identical demand shocks, rather than allowing different demand shock variances across cities, these results are not fully realistic, but the implications are striking and well in accord with the patterns that we observe empirically. First, the table shows that the model does a good job of reproducing the actual price and investment levels in both cities. Despite the fact that the only city-specific heterogeneity is in the supply side parameters and the population growth rate, the price levels are almost exactly the same in the simulations as in the data, and there are similarly large differences in new construction.

In terms of volatility, the standard deviation of log house prices in Atlanta from 1984 to 2008, after regressing out the time trend, was about 0.08. The comparable figure for San Francisco was 0.16, twice as large. Looking back at Table 1.12, we see that even this limited simulation can explain a percentage difference in volatility of about 55 percent. The fact that the model does a poor job of replicating the level of price volatility on average across cities is quite interesting. It may relate to the puzzle, noted above, that house price volatility is much greater than rent volatility. Nevertheless, it is the relative values that matter most here, and the simulations confirm that the model predicts wide disparities in volatility in markets with different supply-side parameters.
1.10 Conclusion

After the experience of recent years, the importance of volatility in house prices and housing investment is abundantly clear. Understanding the factors that govern differences in volatility requires knowledge of both the demand and supply sides of the housing market. Although we have learned a great deal about the importance of the supply side in recent years, there is much still to be done.

This paper makes several contributions to our knowledge of housing supply and the role it plays in determining house price volatility. Building on previous work, I develop a dynamic structural model of housing supply that is grounded in a basic microeconomic optimization problem. I then use the model to carefully identify key structural supply-side parameters and show how they vary across metropolitan areas with observed levels of regulation. I find that regulation of all kinds causes delays and adds tens of thousands of dollars to the cost of house on the margin in a more regulated city relative to a less regulated one.

In contrast with the existing literature, I am able to use the theory and data to explore the mechanisms by which regulation affects volatility. Delays and higher marginal costs reduce supply elasticities and, as a consequence, amplify the volatility of house prices. My simulations suggest that, even in a model with no inter-metropolitan migration and identical demand shocks, observed regulation can explain a large fraction of the difference in volatility of house prices between a highly regulated city like San Francisco and a relatively unregulated one like Atlanta. Although housing regulation has deep and complex roots, this conclusion has important policy implications, both for local governments and for private groups like homeowners’ associations that often oppose new construction.

One caveat to my conclusions is that I do not study the benefits of housing supply regulation to any of the involved parties. Homeowners have a strong incentive to protect
their property values, both by limiting the exposure of their homes to potentially noxious adjacent uses — the traditional justification for zoning — and by preventing nearby new construction that could, in effect, compete with their own homes and drive down prices when they look to sell in the future. The effect of regulation on price *levels* can thus be seen as a transfer to current homeowners from prospective future homebuyers, who face higher prices, and some current land owners, who may be prevented from fully developing their land and selling at the market price. Although we have started to get a handle on the costs of regulation, future research should focus on quantifying the benefits, without which it is difficult to evaluate the welfare impacts.

That said, what is striking about volatility is that it negatively affects current owners as well as prospective future ones. This volatility particularly hurts homeowners looking to cash out — often, the old — and younger, less wealthy buyers seeking their first homes. Other owners may be at least partially hedged, to the extent that the price of their current home covaries with the price of their desired future one (Sinai and Souleles 2005, Paciorek and Sinai 2010), but even hedged owners face problems if they end up “underwater” on their mortgages (Ferreira, Gyourko and Tracy 2010). One important implication of this paper is thus that future work on the distributional effects of housing supply regulation should concentrate not only on its effects on price levels but also on volatility.
Figure 1.1: House Price Comparison

Mean House Price

- San Francisco
- Atlanta
Figure 1.2: Housing Investment Comparison

Housing Investment

- San Francisco
- Atlanta
Solid curve is a penalized regression spline relating standard deviation of detrended log house prices in each MSA to measured regulation. WRLURI standardized to have mean zero and standard deviation one. Dashed curves show +/- two standard errors.
Figure 1.4: Regulation Index Map

Wharton Residential Land Use Regulation Index

WRLURI standardized to have mean zero and standard deviation one.
Figure 1.5: Approval Delay Index Map

Approval Delay Index

ADI standardized to have mean zero and standard deviation one.
Figure 1.6: Volatility Map

House Price Volatility, 1985-2008

Standard deviation of detrended log house prices in each MSA.
Figure 1.7: Housing Demand

Graphical representation of housing demand curve.

Demand

Implicit Rent

Housing Stock

\( D(R_t^*) \)

\( R_t^* \)

\( K_t^* \)
Graphical representation of myopic housing supply curve.
Figure 1.9: Timeline

Investment Timeline

- Decision made; Expectations formed
- Permit
- Completion
Figure 1.10: Regulation Survey Questions

Wharton Survey on Residential Land Use (Example Questions)

2. Which of the following are required to approve zoning changes, and by what vote?

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>Yes, by simple majority</th>
<th>Yes, by more than simple majority</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>Local Planning commission</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Local Zoning Board</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Local Council, Managers, Commissioners</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>County Board of Commissioners</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>County Zoning Board</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Environmental Review Board</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

5. Does your community place annual limits on the total allowable:

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>No</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. of building permits – single family?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of building permits – multi-family?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of residential units authorized for construction – single family?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of residential units authorized for construction – multi-family?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of multi-family dwellings?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of units in multi-family dwellings?</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

10. What is the current length of time required to complete the review of residential projects in your community?

For single-family units: _______ months For multi-family units: _______ months

12. What is the typical amount of time between application for rezoning and issuance of a building permit for development of:

<table>
<thead>
<tr>
<th></th>
<th>Less than 3 mos.</th>
<th>3 to 6 mos.</th>
<th>7 to 12 mos.</th>
<th>13 to 24 mos.</th>
<th>If above 24, How long?</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than 50 single family units</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>50 or more single family units</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Multi-family units</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Impulse response functions for San Francisco and Atlanta, relative to mean values, in response to a one standard deviation demand-driven house price shock.
<table>
<thead>
<tr>
<th>Variable</th>
<th>Number of MSAs</th>
<th>Time Period</th>
</tr>
</thead>
<tbody>
<tr>
<td>Real dollar-valued house price index (FHFA, Census, CPI)</td>
<td>380</td>
<td>1975-2008 (in part)</td>
</tr>
<tr>
<td>Total housing permits (Census)</td>
<td>381</td>
<td>1981-2008</td>
</tr>
<tr>
<td>Wharton Residential Land Use Regulation Index (WRLURI)</td>
<td>252</td>
<td>Observed once</td>
</tr>
<tr>
<td>Share of land within 50 km unavailable for development</td>
<td>298</td>
<td>Constant</td>
</tr>
<tr>
<td>Real physical construction cost, avg. quality 2000 s.f.</td>
<td>174</td>
<td>1982-2008</td>
</tr>
<tr>
<td>house (RS Means)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Demand side:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average real wage (BEA, CPI)</td>
<td>381</td>
<td>1975-2008 (in part)</td>
</tr>
<tr>
<td>Population (Census)</td>
<td>380</td>
<td>1980-2008</td>
</tr>
<tr>
<td>Instruments:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bartik employment (County Business Patterns)</td>
<td>381</td>
<td>1977-2007</td>
</tr>
<tr>
<td>Migration (IRS)</td>
<td>381</td>
<td>1983-2007</td>
</tr>
<tr>
<td>MSA</td>
<td>WRLURI (W&lt;sub&gt;j&lt;/sub&gt;)</td>
<td>ADI (D&lt;sub&gt;j&lt;/sub&gt;)</td>
</tr>
<tr>
<td>--------------</td>
<td>------------------------</td>
<td>---------------------</td>
</tr>
<tr>
<td>New York</td>
<td>0.94</td>
<td>2.54</td>
</tr>
<tr>
<td>Los Angeles</td>
<td>0.78</td>
<td>1.24</td>
</tr>
<tr>
<td>Chicago</td>
<td>0.22</td>
<td>0.74</td>
</tr>
<tr>
<td>Houston</td>
<td>-0.27</td>
<td>-0.92</td>
</tr>
<tr>
<td>Atlanta</td>
<td>0.19</td>
<td>-0.04</td>
</tr>
<tr>
<td>Philadelphia</td>
<td>1.41</td>
<td>1.47</td>
</tr>
<tr>
<td>Washington</td>
<td>0.60</td>
<td>1.23</td>
</tr>
<tr>
<td>Dallas</td>
<td>-0.27</td>
<td>-0.36</td>
</tr>
<tr>
<td>Riverside</td>
<td>0.86</td>
<td>0.75</td>
</tr>
<tr>
<td>Phoenix</td>
<td>1.02</td>
<td>0.87</td>
</tr>
<tr>
<td>San Francisco</td>
<td>1.21</td>
<td>2.20</td>
</tr>
</tbody>
</table>

WRLURI (W<sub>j</sub>), ADI (D<sub>j</sub>) and WRLURI excl. ADI (W<sub>xj</sub>) standardized to have mean zero and standard deviation one. Mean house price calculated using real prices in 2000 dollars.
<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Price</td>
<td>1.34</td>
<td>1.46</td>
<td>1.67</td>
<td>1.71</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td></td>
<td></td>
<td>-0.44</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.04)</td>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
<td></td>
<td>-0.38</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.04)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
<td>-0.14</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.04)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Density</td>
<td>-1.59</td>
<td>-1.40</td>
<td>-0.67</td>
<td>-1.10</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.20)</td>
<td>(0.18)</td>
<td>(0.15)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td></td>
<td></td>
<td>-0.54</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.10)</td>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
<td></td>
<td>0.40</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.08)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
<td>-1.06</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.11)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log Construction Costs</td>
<td>-1.36</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.20)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>9318</td>
<td>4469</td>
<td>6394</td>
<td>6394</td>
</tr>
<tr>
<td>MSA/Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instrumented</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>

Dependent variable is the log ratio of permits to available construction “slots”, as defined in the text. Density is lagged by one period and multiplied by 100. Heteroskedasticity-robust standard errors in parentheses. Regulation, Delays, and Regulation excl. Delays standardized to have mean zero and standard deviation one.
<table>
<thead>
<tr>
<th>Table 1.4: Myopic Model Elasticity Estimates, IV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>Log Price</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Regulation</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Density</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Regulation</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Log Construction Costs</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>N</td>
</tr>
<tr>
<td>MSA/Year FE</td>
</tr>
<tr>
<td>Instrumented</td>
</tr>
</tbody>
</table>

Dependent variable is the log ratio of permits to available construction “slots”. IV using linear projections of log price onto *industry* and *migration* instruments, as described in the text. Density is lagged by one period and multiplied by 100. Heteroskedasticity-robust standard errors in parentheses. Regulation, Delays, and Regulation excl. Delays standardized to have mean zero and standard deviation one.
Table 1.5: “First-Stage” Regressions

<table>
<thead>
<tr>
<th>Dependent term:</th>
<th></th>
<th>x W_j</th>
<th></th>
<th>x W_j</th>
</tr>
</thead>
<tbody>
<tr>
<td>( L \left[ \left( \log P_{j,t} - \beta \log P_{j,t+1} \right)</td>
<td>m_j, m_t, Z_{j,t-1} \right] )</td>
<td>0.678*</td>
<td>0.005</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
<td>(0.088)</td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>( \hat{L} \left[ \left( K_{j,t-1} \right) \left( \beta A_j \right) \right]</td>
<td>m_j, m_t, Z_{j,t-1} \right] )</td>
<td>0.452</td>
<td>1.210*</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.025)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>( \hat{L} \left[ \left( K_{j,t-1} \right) \left( \beta A_j \right) \right]</td>
<td>m_j, m_t, Z_{j,t-1} \right] )</td>
<td>-1.000</td>
<td>-0.250</td>
<td>0.954*</td>
</tr>
<tr>
<td></td>
<td>(0.216)</td>
<td>(0.250)</td>
<td>(0.011)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>( \hat{L} \left[ \left( K_{j,t-1} \right) \left( \beta A_j \right) \right]</td>
<td>m_j, m_t, Z_{j,t-1} \right] )</td>
<td>1.320</td>
<td>1.150</td>
<td>0.048</td>
</tr>
<tr>
<td></td>
<td>(0.117)</td>
<td>(0.135)</td>
<td>(0.006)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>F-stat for coefficient with *</td>
<td>78.7</td>
<td>2400</td>
<td>7750</td>
<td>19600</td>
</tr>
<tr>
<td>Pr(&gt;F)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>N</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
<td>5740</td>
</tr>
<tr>
<td>MSA/Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

These regressions correspond to column (3) of Table 1.6 as adapted from Equation 1.9. Each column is the regression of an endogenous term on MSA and year fixed effects and the full set of projection instruments and interactions (\( \hat{Z}_{j,t-1} \)). The starred coefficient is the relevant instrument for the dependent variable in that column. Heteroskedasticity-robust standard errors in parentheses. \( W_j \) is the top-line WRLURI index value for MSA \( j \); it is standardized to have mean zero and standard deviation one.
### Table 1.6: Structural Model Estimates

<table>
<thead>
<tr>
<th>Description</th>
<th>Parameter</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marginal cost of investment</td>
<td>$\bar{\sigma}^X$</td>
<td>0.53</td>
<td>0.54</td>
<td>0.39</td>
<td>0.34</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.09)</td>
<td>(0.12)</td>
<td>(0.12)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td>$\sigma^W$</td>
<td>0.14</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.04)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
<td>$\sigma^D$</td>
<td>0.09</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.03)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
<td>$\sigma^{WX}$</td>
<td>0.09</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.03)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Density</td>
<td>$\bar{\sigma}^\eta$</td>
<td>4.52</td>
<td>5.30</td>
<td>4.51</td>
<td>4.34</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.93)</td>
<td>(1.40)</td>
<td>(2.01)</td>
<td>(2.22)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td>$\sigma^{\eta W}$</td>
<td>0.03</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.86)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Delays</td>
<td>$\sigma^{\eta D}$</td>
<td>-0.93</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.09)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>... x Regulation excl. Delays</td>
<td>$\sigma^{\eta WX}$</td>
<td>0.75</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.42)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Construction costs</td>
<td></td>
<td>0.00</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.14)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| N                                         | 8441      | 4044 | 5738 | 5738 |
| MSA/Year FE                               | Yes       | Yes  | Yes  | Yes  |
| Instrumented                              | Yes       | Yes  | Yes  | Yes  |

IV estimates of variants of Equation 9 using linear projections onto industry and migration instruments, as described in text. Heteroskedasticity-robust standard errors in parentheses. Regulation, Delays and Regulation excl. Delays standardized to have mean zero and standard deviation one.
Table 1.7: Estimated Cost Parameters and Elasticities for Top 10 MSAs by Population

<table>
<thead>
<tr>
<th>MSA</th>
<th>Marginal Cost of Investment</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Percent</td>
<td>Dollars</td>
<td>Elasticity</td>
</tr>
<tr>
<td>New York</td>
<td></td>
<td>14.34</td>
<td>$45,000</td>
<td>1.74</td>
</tr>
<tr>
<td>Los Angeles</td>
<td></td>
<td>12.26</td>
<td>$41,000</td>
<td>2.04</td>
</tr>
<tr>
<td>Chicago</td>
<td></td>
<td>9.91</td>
<td>$19,000</td>
<td>2.52</td>
</tr>
<tr>
<td>Houston</td>
<td></td>
<td>6.66</td>
<td>$8,000</td>
<td>3.75</td>
</tr>
<tr>
<td>Atlanta</td>
<td></td>
<td>8.97</td>
<td>$15,000</td>
<td>2.79</td>
</tr>
<tr>
<td>Philadelphia</td>
<td></td>
<td>14.24</td>
<td>$24,000</td>
<td>1.76</td>
</tr>
<tr>
<td>Washington</td>
<td></td>
<td>11.76</td>
<td>$29,000</td>
<td>2.13</td>
</tr>
<tr>
<td>Dallas</td>
<td></td>
<td>7.27</td>
<td>$11,000</td>
<td>3.44</td>
</tr>
<tr>
<td>Riverside</td>
<td></td>
<td>11.87</td>
<td>$22,000</td>
<td>2.11</td>
</tr>
<tr>
<td>Phoenix</td>
<td></td>
<td>12.44</td>
<td>$20,000</td>
<td>2.01</td>
</tr>
<tr>
<td>San Francisco</td>
<td></td>
<td>14.66</td>
<td>$69,000</td>
<td>1.71</td>
</tr>
</tbody>
</table>

Increase in cost of a house on the margin after an increase in log investment of 25 log points, which is the average absolute change in log investment across all MSAs and years. Calculated using parameters shown in column (4) of Table 1.6. See text for details.
Table 1.8: Fixed Costs and Regulation

<table>
<thead>
<tr>
<th></th>
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</thead>
<tbody>
<tr>
<td>Regulation</td>
<td>0.027</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Delays</td>
<td>0.003</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Regulation excl. Delays</td>
<td>0.026</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>(Intercept)</td>
<td>0.656</td>
<td>0.657</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
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<tr>
<td>N</td>
<td>167</td>
<td>167</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.04</td>
<td>0.04</td>
</tr>
</tbody>
</table>

Regression of estimate of log fixed costs from model in column (4) of Table 1.6 on WR-LURI measures of regulation, as described in text. Standard errors in parentheses. Regulation, Delays, and Regulation excl. Delays standardized to have mean zero and standard deviation one.
### Table 1.9: Robustness Checks

<table>
<thead>
<tr>
<th>Description</th>
<th>Parameter</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marginal cost of investment</td>
<td>$\bar{\sigma}^X$</td>
<td>0.23</td>
<td>0.50</td>
<td>0.41</td>
<td>0.19</td>
<td>0.41</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.10)</td>
<td>(0.17)</td>
<td>(0.14)</td>
<td>(0.10)</td>
<td>(0.12)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td>$\sigma^{\chi W}$</td>
<td>0.20</td>
<td>0.04</td>
<td>0.13</td>
<td>0.19</td>
<td>0.14</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.04)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.04)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Density</td>
<td>$\bar{\sigma}^\eta$</td>
<td>1.74</td>
<td>6.68</td>
<td>5.66</td>
<td>0.89</td>
<td>4.76</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(2.24)</td>
<td>(3.25)</td>
<td>(3.20)</td>
<td>(1.62)</td>
<td>(1.97)</td>
</tr>
<tr>
<td>... x Regulation</td>
<td>$\sigma^{\eta W}$</td>
<td>1.39</td>
<td>-2.33</td>
<td>-0.33</td>
<td>1.36</td>
<td>-0.06</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.58)</td>
<td>(2.12)</td>
<td>(1.26)</td>
<td>(0.78)</td>
<td>(0.83)</td>
</tr>
</tbody>
</table>

| N               | 4584  | 3427  | 5738  | 6022  | 5738  |
| MSA/Year FE     | Yes   | Yes   | Yes   | Yes   | Yes   |
| Instrumented    | Yes   | Yes   | Yes   | Yes   | Yes   |

IV estimates of variants of Equation 1.9 using linear projections onto *industry* and *migration* instruments, as described in text. Heteroskedasticity-robust standard errors in parentheses. Regulation standardized to have mean zero and standard deviation one.

Each column in this table parallels column (3) in Table 1.6. Column (1) uses preliminary data from the 2010 Wharton Residential Land Use Survey in place of the 2005 survey data in all other estimates. Column (2) includes the R.S. Means construction cost data in the price term. Column (3) uses the second lag of the quasi-differenced density term as an instrument instead of the first lag. Column (4) estimates the model using only *industry$_{jt-1}$* not *migration$_{jt-1}$*. Column (5) uses an alternative version of *migration$_{jt-1}$* that excludes city pairs within 100 miles of each other. See text for further details.
Table 1.10: Demand Side (Rent)

<table>
<thead>
<tr>
<th>Description</th>
<th>Parameter</th>
<th>OLS</th>
<th>IV (Lag 1)</th>
<th>IV (Lag 2)</th>
<th>IV (Lag 3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Pop.; -Log Stock</td>
<td>$\phi_n - \phi_K$</td>
<td>1.71</td>
<td>2.14</td>
<td>2.57</td>
<td>2.81</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.11)</td>
<td>(0.13)</td>
<td>(0.18)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>Log Wage</td>
<td>$\phi_w$</td>
<td>1.25</td>
<td>1.26</td>
<td>1.28</td>
<td>1.29</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>N</td>
<td></td>
<td>1150</td>
<td>1150</td>
<td>1150</td>
<td>1150</td>
</tr>
<tr>
<td>MSA/Year FE</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>MSA time trends</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instrumented</td>
<td></td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Dependent variable is log rent. Heteroskedasticity-robust standard errors in parentheses. The second column uses first lag of log stock as an instrument for log stock, the third column uses the second lag, and so forth. Log population and log capital stock coefficients are constrained to be equal and opposite in sign, as discussed in text.
Table 1.11: Demand Side (User Cost)

<table>
<thead>
<tr>
<th>Description</th>
<th>Parameter</th>
<th>OLS</th>
<th>IV (Lag 1)</th>
<th>IV (Lag 2)</th>
<th>IV (Lag 3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Rent</td>
<td>$\pi_R$</td>
<td>1.01</td>
<td>0.87</td>
<td>0.64</td>
<td>0.19</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.07)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Log Pop.; -Log Stock</td>
<td>$\pi_n; -\pi_K$</td>
<td>0.60</td>
<td>2.46</td>
<td>5.50</td>
<td>11.41</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.23)</td>
<td>(0.28)</td>
<td>(0.50)</td>
<td>(1.35)</td>
</tr>
<tr>
<td>N</td>
<td></td>
<td>1150</td>
<td>1150</td>
<td>1150</td>
<td>1150</td>
</tr>
<tr>
<td>MSA/Year FE</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>MSA time trends</td>
<td></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Instrumented</td>
<td></td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Dependent variable is log implicit rent, as defined in text. Heteroskedasticity-robust standard errors in parentheses. The second column uses first lag of log stock as an instrument for log stock, the third column uses the second lag, and so forth. Log population and log capital stock coefficients are constrained to be equal and opposite in sign, as discussed in text.
Table 1.12: Simulation Results

<table>
<thead>
<tr>
<th>Scenario</th>
<th>Price Mean</th>
<th>Std. Dev. of Log</th>
<th>Investment Mean</th>
<th>Std. Dev. of Log</th>
<th>Elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>$171,000</td>
<td>0.040</td>
<td>14,100</td>
<td>0.089</td>
<td>1.67</td>
</tr>
<tr>
<td>Low Regulation</td>
<td>$154,000</td>
<td>0.035</td>
<td>14,400</td>
<td>0.120</td>
<td>2.53</td>
</tr>
<tr>
<td>High Regulation</td>
<td>$184,000</td>
<td>0.042</td>
<td>13,800</td>
<td>0.075</td>
<td>1.34</td>
</tr>
<tr>
<td>Two-Year Lag</td>
<td>$171,000</td>
<td>0.043</td>
<td>14,100</td>
<td>0.081</td>
<td>1.05</td>
</tr>
<tr>
<td>San Francisco (actual)</td>
<td>$466,000</td>
<td>0.044</td>
<td>6,300</td>
<td>0.065</td>
<td>1.14</td>
</tr>
<tr>
<td></td>
<td>$473,000</td>
<td>0.159</td>
<td>3,900</td>
<td>0.373</td>
<td></td>
</tr>
<tr>
<td>Atlanta (actual)</td>
<td>$169,000</td>
<td>0.029</td>
<td>30,000</td>
<td>0.066</td>
<td>1.63</td>
</tr>
<tr>
<td></td>
<td>$162,000</td>
<td>0.077</td>
<td>51,800</td>
<td>0.286</td>
<td></td>
</tr>
</tbody>
</table>

Average results from 25-year simulations using the same set of 100 randomly drawn demand shock paths in each row. Rows for Low Regulation and High Regulation use estimated supply costs for cities at the 25th and 75th percentiles of regulation, respectively. Rows for San Francisco and Atlanta are simulations using the same demand shock paths but the estimated supply- and demand-side parameters of those cities.
Chapter 2

Zoned Out: Estimating the Impact of Local Housing Supply Restrictions
2.1 Introduction

Between 1978 and 2008, the average price of a house in the Boston metropolitan area rose from about $125,000 to more than $330,000 in real terms. During this period, with the exception of a brief and small surge in the mid-1980s during the “Massachusetts Miracle”, construction of new homes never exceeded 7,000 units per year. By comparison, prices in Atlanta averaged about $155,000 in 1980 — $30,000 higher than Boston — yet increased to just $180,000 by 2008. Meanwhile, the population in Atlanta boomed, with the annual construction of new housing averaging nearly 50,000 units.[1]

These data indicate that there has been high and rising demand to live in both Boston and Atlanta, but this demand has manifested itself very differently. Basic economics offers a simple explanation for why increasing demand pushes up prices for one good but leads to expanding quantities of another: The price elasticity of supply is low for the first good and high for the second. In the context of housing, this implies that builders and developers can easily respond to increases in price in Atlanta but cannot in Boston.

A growing literature on housing supply has sought to explain the patterns of growth across American cities using precisely this idea. A number of papers have emphasized the importance of construction costs, particularly the costs of complying with zoning and other regulatory constraints, in determining how housing supply responds to changes in demand (Glaeser et al. 2005a, Quigley and Raphael 2005). This paper advances that agenda by studying housing supply in greater Boston, which is a prime example of what Gyourko et al. (2006) term a “superstar city”, a high-demand city with constrained housing supply.

There are at least two reasons to focus a single metropolitan area. First, there is substantially more detailed data on zoning than is available across metro areas, which offers the opportunity to focus on the channels by which regulation affects supply. Second, the

---

[1] Prices calculated using FHFA repeat sales indices pegged to the average price of housing in the 2000 Census.
fact that amenities, such as school quality or open space, are not evenly distributed within a metropolitan area means that there are important welfare questions relating not only to the total quantity of building in the area but also the patterns of development. Put another way, it is important to understand both why there is a low level of new construction in the Boston area, and also why that new construction takes place where it does.

To examine these issues I combine rich new microdata on housing construction with detailed information on the zoning regimes in the towns and cities of eastern Massachusetts. I use this data to estimate a dynamic structural model of housing supply, which is based on the optimization problem of owners of undeveloped land. These landowners must decide when to build new houses, and how large to build them, taking into account the zoning in force and their rational expectations about future prices. Estimating the model allows me to recover the parameters relating supply restrictions to variable costs — that is, the cost per square foot — and the quantity of new construction.

I find that zoning provisions can more than double the effective cost per square foot of new construction on the margin in areas zoned for multifamily housing. Conversely, when land is zoned for low-density single-family housing, costs per square foot remain low but fixed costs are high, which means little new construction takes place. The most important provisions of the zoning code are minimum lot sizes, although other factors, including municipal restrictions on multifamily housing, bylaws to protect wetlands and annual limits on building permit issuance, also play a role.

In addition, the relationship between house prices and new construction is tenuous and varies with the strength of regulation. In areas zoned for large minimum lots, I cannot reject the null hypothesis that the elasticity of supply is zero. Taken together, my results indicate that both the quantity of new construction and the sizes of new houses in eastern Massachusetts are determined in large part by the zoning regime.

The large theoretical literature on zoning tends to focus on questions of political econ-
omy (Fischel 1985, Fischel 2001, Calabrese et al. 2007). Most of the empirical work on supply regulation is relatively recent and focuses on cross-metropolitan comparisons, like the one above between Atlanta and Boston. My paper relates most closely to a trio of papers on housing supply, each of which uses different data and takes a different econometric approach.

I employ essentially the same data on zoning as Glaeser and Ward (2009), who document the patterns of zoning in eastern Massachusetts and examine the long-run relationship between supply regulation and new construction. They note that minimum lot sizes are strongly correlated with the density of existing housing in 1940, suggesting that zoning primarily seeks to maintain the status quo. They also argue that current density levels are too low to maximize land values, which means zoning is in fact a limiting factor in development.

In contrast with Glaeser and Ward (2009), I use detailed microdata on transaction and new construction to estimate a structural model of housing supply, which allows me to examine the implicit and explicit costs of zoning. The model is similar to both Murphy (2010) and Paciorek (2011a). Murphy (2010) combines detailed transactions data, from the same source as my data in this paper, with a unique dataset of undeveloped parcels in the San Francisco Bay Area. He uses this data to estimate a dynamic structural supply model, which is the basis for the model in this paper, and recovers fixed and variable costs at a fine level of geographic disaggregation. Unlike this paper, Murphy (2010) does not incorporate supply constraints or explore their relationship with these costs.

The third related paper, Paciorek (2011a), uses metropolitan-level data on zoning and other forms of regulation to estimate a similar structural model. In that work, I focus primarily on housing dynamics, arguing that constraints lead not only to higher prices but also to increased volatility, since supply is not able to quickly or cheaply respond to demand. While the model employed here is similar to both Murphy (2010) and Paciorek (2011a),
it takes a different approach to estimation, casting the main estimating equation for the 
quantity of new supply in the form of a Poisson regression.

The argument that regulation is at least partially responsible for governing new housing 
supply is intuitive, but given that zoning seems to enshrine in law the prevailing type of 
housing, it may already be in line with what the market demands. Ultimately, this is an 
empirical question, which may have different answers in different parts of the country. 
For example, using a structural model of the production function for housing, Epple et 
al. (2010) find no evidence that zoning distorts the provision of housing in a particular 
suburb of Pittsburgh. In contrast, the results in this paper strongly imply that zoning, rather 
than market prices of housing transactions, play the most significant role in determining 
the quantity and quality of new housing supply in the Boston area. Indeed, simulations 
using my empirical estimates indicate that changing the minimum lot size in all localities 
to one favoring multifamily construction would quadruple new housing construction and 
substantially reduce the size of new homes.

In the next section, I lay out my dynamic model and show how it can be estimated in 
stages to recover the variable cost parameters and the effect of zoning on the quantity of 
new construction. I then discuss the data used for estimation, followed by the estimation 
techniques and results. In section 2.5 I present a series of simulations of construction 
in greater Boston under alternative zoning regimes. Section 2.6 concludes by discussing 
caveats and avenues for future study.

2.2 A Dynamic Model of Local Housing Supply

My model of housing supply describes the decision problem of owners of undeveloped land 
in a large metropolitan region who are deciding whether to build a house on their land and, 
conditional on building, how large a house to build. They take into account both (expected)
house prices at the time the house is completed and sold, including the marginal price of additional square footage, as well as the cost environment, which is critically impacted by regulation and zoning. The model is similar to that of Murphy (2010), although it differs in several key details, particularly the inclusion of observed cost shifters and the specification of the final estimating equation for new construction. Although I specify the model as an individual optimization problem, I estimate it using census tract-level data, as discussed in sections 2.3 and 2.4 below. The aggregation techniques are related to those in Paciorek (2011a).

The model can be estimated in two separable stages. In the first stage, I recover parameters of the hedonic price function, which allow me to predict, for each tract \( (j) \) and year \( (t) \), the price of a house of a given size, as well as the marginal price per square foot of the house. The second stage uses the price estimates and data on new construction and cost shifters to recover the parameters of the structural equations governing the size and quantity of new construction.

The theory underlying hedonic price regressions offers essentially no guidance on the correct functional form (Rosen 1974). To keep things as simple as possible, I specify a linear relationship between the price \( (P_{i,t}) \), square footage \( (x_{i,t}) \), and lot size \( (l_{i,t}) \):

\[
P_{i,t} = P_{j,t}^0 + P_{j,t}^x x_{i,t} + P_{j,t}^l l_{i,t} + \eta_{i,t} \tag{2.1}
\]

This equation specifies the price that house \( i \) will sell for given its tract \( (j) \) and the year \( (t) \); I provide detail on estimation in Section 2.4. These price parameters are taken as given by landowners, and I assume throughout the paper that prices are exogenous with respect to the quantity of new construction in any given tract or year. This is reasonable given that

\(^2\)Introducing nonlinear pricing substantially complicates the estimating of the variable cost parameters. I have examined the fit of the model using spline techniques, and there do not appear to be any substantial departures from linearity within the domain of the data.
the amount of new construction in tracts around Boston is almost always tiny relative to the existing stock, and that buyers can easily substitute across nearby tracts since they are all within the same labor market.

Although my data include information on other characteristics for some houses, I focus on the choice of house size, measured in square feet. To keep the model tractable, I do not endogenize the choice of lot size, despite the fact that developers may have some control over it, particularly in suburban or rural areas where larger lots are subdivided for construction. I find that there is no economically or statistically significant relationship between lot size and the hedonic return to an additional square foot of lot, while there is an extremely strong relationship between the zoning regime and the size of the lot. In essence, it appears the lot size is chosen for the builder by the zoning code, which is determined in large part by the lot size of existing homes in the area (Glaeser and Ward 2009).

The total amount of land available for housing in tract \( j \) is \( A_j \). This amount excludes areas that are 1) covered by water, 2) too steep to build on (Saiz 2010), or 3) protected from development. Given a current capital stock consisting of \( K_{j,t-1} \) houses and an average lot size of existing homes of \( \tilde{l}_{j,t-1} \), the amount of land still available for development starting at \( t - 1 \) is \( A_j - K_{j,t-1}\tilde{l}_{j,t-1} \).[3] The remaining land is subdivided into \( N_{j,t} \) parcels, of sizes \( \{l_{i,t}\} \), so that

\[
N_{j,t} \sum_i l_{i,t} \frac{1}{N_{j,t}} = A_j - K_{j,t-1}\tilde{l}_{j,t-1}
\]

This equation states that the number of unbuilt parcels times the average lot size per available parcel equals the total amount of unbuilt land. This definition of lot size means that, for example, an apartment in a 100-unit building on ten acres of land would have a lot size of 0.1 acres. I discuss below how I operationalize this definition given my data.

The owner of a parcel of land \((i)\) in tract \( j \) at time \( t - 1 \) must decide whether to build a

---

[3] I incorporate below the fact that “available land” may already be used for some productive purpose, such as agriculture or commerce. Though the theory does not allow for the possibility of redeveloping parcels already used for housing, I do incorporate this possibility into the empirics, as I discuss below.
new house on her parcel for sale at \( t \) and, conditional on building, how large a house \((x_{i,t})\) to build. I ignore spillovers across parcels, which could result from ownership of larger tracts of land that are then subdivided into parcels. For example, it may be cheaper for one landowner to build two houses on two adjacent parcels than for two separate landowners to do the same. The effects of the organization of land ownership and builders is an interesting topic but is outside the scope of this paper. Since I aggregate my data to the tract-level for estimation, spillovers of this sort are of concern only to the extent that land ownership patterns differ across tracts. This is likely to be due in large part to the current level of development: A farm can be subdivided into multiple parcels, while existing homes on smaller lots that are individually owned usually cannot. In the empirics I explicitly account for the preexisting level of development in each tract.

It takes one year to build a house from the time the decision is made, so the landowner must form expectations about future prices\(^4\). In contrast with Paciorek (2011a), I assume the final decision is made at the time construction begins, with additional lags in the permitting process simply raising costs. Unlike that paper, I do not have any direct information on permitting lags in my primary data set. The landowner makes her decision by maximizing her expected profit from selling the house, net of construction and opportunity costs, including the costs of regulation.

### 2.2.1 House Size Decision

Conditional on deciding to build, the owner of parcel \( i \) solves the following problem

\[
\max_{x_{i,t}} \left\{ E_{t-1} \left[ \frac{P_{x,j,t}^{x}}{x_{i,t}} \right] x_{i,t} - V C_{i,t} (x_{i,t}) \right\}
\]

\(^4\)This roughly accords with Census data on the length of time from permit to final construction of single-family houses; the lag for multifamily units is somewhat longer.
The landowner must form expectations about future marginal prices because it takes one year to build a house from the time the decision is made. Let the cost of construction vary with the size of the house \( x_{i,t} \), according to the quadratic function

\[
VC_{i,t} = \max_{x_{i,t}} VC_{i,t}(x_{i,t}) = \max_{x_{i,t}} \frac{1}{\alpha^x} \exp \left( \alpha^Z (Z_{j,t}) + \alpha^x x_{i,t} + \xi_{i,t-1} \right)
\]  

(2.3)

This functional form allows costs to vary, by tract and year, according to the zoning and regulatory regime, represented for now only by the matrix \( Z_{j,t} \). The variable cost function incorporates \( \xi_{i,t-1} \), an independent and identically distributed shock to marginal costs that is realized at time \( t - 1 \) when the decision to build is made. This shock accounts for differences in the choice of house size that cannot be accounted for by either tract-level observables or the lot size.

Taking first-order conditions with respect to \( x_{i,t} \) yields the necessary and sufficient condition for the optimal house size (\( \hat{x}_{i,t} \)):

\[
E_{t-1} \left[ \log P_{j,t}^x \right] = \alpha^0 (Z_{j,t}) + \alpha^x \hat{x}_{i,t} + \xi_{i,t-1}
\]

In this equation, the only term other than the choice of house size that varies by parcel is the marginal cost shock \( \xi_{i,t-1} \). Since the key term is \( \alpha^0 (Z_{j,t}) \), which incorporates zoning that varies at the tract or municipal level, it makes sense to estimate the equation after aggregating to the tract-level. Taking the conditional expectation of both sides with respect to the tract and year yields

\[
E_{t-1} \left[ \log P_{j,t}^x \right] = \alpha^Z (Z_{j,t}) + \alpha^x \hat{x}_{j,t} + \xi_{j,t-1}
\]  

(2.4)

\*I do not include the lot size here precisely because the lot size of new units is a function of the zoning regime. In the empirics I parcel out the direct and indirect effects (via the lot size) of zoning.

85
where \( x_j, t = E_{j,t} [x_{i,t}] \) and so forth. Estimating this equation using only the subset of parcel owners who actually build requires that \( \xi_{i,t-1} \), the parcel-specific shock to the marginal cost, be uncorrelated with the parcel-specific shock to fixed costs described below. Otherwise, a selection problem results because prospective houses with high fixed costs and (presumably) high marginal costs are not built.

Given the parameters of this equation, as well as \( E_{t-1} [P_{j,t}] \), I can solve for the optimal \( \hat{x}_{i,t} \):

\[
\hat{x}_{i,t} = \frac{E_{t-1} [P_{j,t}] - (\alpha^0 (Z_{j,t}) + \xi_{i,t-1})}{\alpha^x}
\]

I can then plug this expression back into Equation 2.3 to get the variable cost for parcel \( i \), which I then divide into the average variable cost for a given tract \( (VC_{j,t}) \) plus a nonlinear function of the shock.

\[
VC_{i,t} = VC_{j,t} + g(\xi_{i,t-1}) = \frac{1}{2\alpha^x} \left( \left( E_{t-1} [P_{j,t}] \right)^2 - \left( \left( \alpha^0 (Z_{j,t}) \right)^2 \right) \right) + g(\xi_{i,t-1})
\]

### 2.2.2 Decision to Build

Taking into account her predicted variable cost, the owner of parcel \( i \) decides whether to build by comparing the expected values of building and of waiting one period. The value of building is given by

\[
V_{i,t}^B - \chi_{i,t-1} = \beta E_{t-1} [P_{i,t}] - FC_{j,t} - VC_{j,t} - \epsilon_{j,t-1}^S - \chi_{i,t-1}
\]

(2.5)

where \( FC_{j,t} \) are the fixed costs, independent of house size, that must be paid to build in tract \( j \) in year \( t \). These fixed costs, like the variable costs, are allowed to differ according to the regulatory environment. The remaining terms — \( \chi_{i,t-1} \) and \( \epsilon_{j,t-1}^S \) — represent independent and identically distributed parcel-specific and tract-level cost shocks, respectively, as in Paciorek (2011a). This equation also contains a nonlinear function of \( \xi_{i,t-1} \), the parcel-
level shock to marginal costs, but I subsume this term in $\chi_{i,t-1}$ for notational convenience. I assume that both fixed and marginal costs are paid up front, while the gain from selling is not realized until completion, one year after the decision to build is made, and is thus discounted by a factor $\beta$ and considered in expectation.\footnote{Census data indicate that a sizable minority of houses, about 30 percent in 2010, are built directly or indirectly by the owner of the property without any intention of selling immediately. In my framework, this can be interpreted as the landowner selling the house to herself. Instead of receiving an immediate cash payment, she instead receives the equivalent net present value of the future rental stream.}

If she decides not to build, the parcel owner receives the current income stream from her property ($U_{j,t}$) — e.g., from operation of a farm or parking lot — as well as the discounted expected value of having the same choice next year. In the context of this model, heterogeneity in $U_{j,t}$ is indistinguishable from heterogeneity in the fixed cost $FC_{j,t}$ and the cost shock $\chi_{i,t-1}$. I attribute all of the heterogeneity to costs, with the proviso that the effects of any cost shifters must be interpreted accordingly.

$$V_{i,t}^N = \beta U_{j,t} + \beta E_{t-1} \left[ \max \{V_{i,t+1}^B, V_{i,t+1}^N\} \right]$$

These two equations, defining $V_{i,t}^B$ and $V_{i,t}^N$, are the heart of the model. Murphy (2010) uses a dynamic discrete choice framework to estimate a similar model with data from San Francisco. He relies in part on data on empty parcels of land that allow him to observe the annual choice to build or not on a parcel by parcel basis. His use of the parcel dataset requires him to exclude large developments and new subdivisions. This is a potentially important omission; regardless, similar information on parcels is not available for the Boston-area data that I use in this paper.

Paciorek (2011a) employs an alternative approach using data at the level of metropolitan statistical areas (MSAs). In that paper, I make the assumptions that there are many small landowners in each city and that $\chi_{i,t-1}$ follows a continuous probability distribution with full support on the real line. I use these assumptions to derive an estimating equation...
from the indifference condition for the single parcel owner in each MSA who must be on
the margin between building and not building. These assumptions are palatable for MSAs
taken as a whole because even U.S. metropolitan areas in secular decline have experienced
some new construction every year.

Census tracts, the level of analysis in this paper, are substantially smaller, typically
consisting of between 1,000 and 10,000 people. Approximately 25 percent of the census
tract-years in my data experienced no new construction, while more than 60 percent had
five or fewer new houses built. The assumptions in Paciorek (2011a) are thus very poor
approximations to reality at this level of aggregation.

I rely instead on the framework of Poisson regression, which is designed for settings
where the response variable takes on small discrete values. If $\chi_{i,t} \sim \text{logistic}(0, \sigma^X_j)$,
then

$$
Pr(B_{i,t}) = \frac{\exp \left( \frac{V^B_{i,t}}{\sigma^X_j} \right)}{\exp \left( \frac{V^B_{i,t}}{\sigma^X_j} \right) + \exp \left( \frac{V^N_{i,t}}{\sigma^X_j} \right)}
$$

Equation 2.6 can be rewritten as

$$
\frac{Pr(B_{i,t})}{1 - Pr(B_{i,t})} = \exp \left( \frac{1}{\sigma^X_j} \left( V^B_{i,t} - V^N_{i,t} \right) \right)
$$

For probabilities near zero — a reasonable approximation in this case because few, if any,
properties in a tract are developed in any year — the odds ratio is approximately equal to
the probability, so that

$$
\frac{Pr(B_{i,t})}{1 - Pr(B_{i,t})} \approx Pr(B_{i,t})
$$

Given this, I can write a model for the expectation of $I_{j,t}$, the number of new houses
built in tract $j$ for completion at time $t$, with respect to $\chi_{i,t-1}$ as the sum of the parcel
probabilities of development\footnote{The expectations from here out are conditional on the data, but I simplify the notation for clarity.}

\[ E[I_{j,t}] = \sum_i Pr(B_{i,t}) \]
\[ = N_{j,t} \sum_i \frac{Pr(B_{i,t})}{N_{j,t}} \]
\[ = N_{j,t} \sum_i \exp \left( \frac{1}{\sigma_j^2} \left( V_{i,t}^B - V_{i,t}^N \right) \right) \]
\[ \approx N_{j,t} \exp \left( \frac{1}{\sigma_j^2} \sum_i \frac{V_{i,t}^B - V_{i,t}^N}{N_{j,t}} \right) \]

The second line in this expression makes explicit the fact that we can write the number of new units constructed as the number of undeveloped parcels of land \((N_{j,t})\) times the average probability that a given parcel in \(j\) is developed. Since I intend to estimate the model with tract-level data, I rewrite the average of the individual probabilities (third line) as the approximate probability of the average (fourth line). The latter is the first-order Taylor approximation to the former and is reasonable provided the variance of expected profits does not differ too much across tracts. Regardless, it is necessary because I do not observe any data for parcels that are not built, which means I need to effectively impute the observables in the value functions for all such parcels.

Taking tract-year averages, let \(V_{j,t}^B = \sum_i \frac{V_{i,t}^B}{N_{j,t}}\) and so forth. Replacing \(N_{j,t}\) with its definition from Equation \(2.2\) and rearranging yields

\[ E[I_{j,t}] = \exp \left( \log \left( \frac{A_j - K_{j,t-1} \tilde{I}_{j,t-1}}{I_{j,t}} \right) + \frac{1}{\sigma_j^2} \left( V_{j,t}^B - V_{j,t}^N \right) \right) \]

This precisely fits the Poisson model of the conditional mean, with the log of the quantity of unbuilt parcels serving as the “exposure”, with a coefficient normalized to one (Winkelmann 2008, Cameron and Trivedi 1998).
Plugging in the definitions of $V_{i,t}^B$ and $V_{i,t}^N$ from above yields

$$E[I_j,t] = \exp \left( \log \left( \frac{A_j - K_{j,t-1} \tilde{l}_{j,t-1}}{l_{j,t}} \right) \right)$$

$$+ \frac{1}{\sigma_j^x} \left( \beta E_{t-1} [P_{j,t}] - FC_{j,t} - VC_{j,t} - \epsilon_{j,t-1}^S \right. \left. - \beta U_{j,t} - \beta E_{t-1} \left[ E \left[ \max \{V_{j,t+1}^B, V_{j,t+1}^N\} \right] \right] \right)$$

Given the iid logistic distribution of $\chi_{i,t-1}$, I can replace the unconditional expectation of the maximum of the value of building and the value of not building, which is taken with respect to next period’s individual shock ($\chi_{i,t}$), with the logit inclusive value.

$$E[I_j,t] = \exp \left( \log \left( \frac{A_j - K_{j,t-1} \tilde{l}_{j,t-1}}{l_{j,t}} \right) \right)$$

$$+ \frac{1}{\sigma_j^x} \left( \beta E_{t-1} [P_{j,t}] - FC_{j,t} - VC_{j,t} - \epsilon_{j,t-1}^S \right. \left. - \beta U_{j,t} - \beta E_{t-1} \left[ \sigma_j^x \log \left( \frac{\exp \left( \frac{V_{j,t+1}^B}{\sigma_j^x} \right) + \exp \left( \frac{V_{j,t+1}^N}{\sigma_j^x} \right) \right)}{\exp \left( \frac{V_{j,t+1}^B}{\sigma_j^x} \right)} \right] \right)$$

The primary remaining complication for using this expression as an estimating equation is that $V_{j,t+1}^N$, the value of not building, is not observable by the econometrician. Following both Murphy (2010) and Paciorek (2011a), I employ the conditional choice probability approach of Hotz and Miller (1993), who noted that there is a relationship between value functions and the probability of taking an action. In my case, I can use the logistic distribution of $\chi_{i,t}$, as in Equation 2.6, to replace $V_{j,t+1}^N$ with $Pr(B_{i,t+1})$, as follows:

$$E[I_j,t] = \exp \left( \log \left( \frac{A_j - K_{j,t-1} \tilde{l}_{j,t-1}}{l_{j,t}} \right) \right)$$

$$+ \frac{1}{\sigma_j^x} \left( \beta E_{t-1} [P_{j,t}] - FC_{j,t} - VC_{j,t} - \epsilon_{j,t-1}^S \right. \left. - \beta U_{j,t} - \beta E_{t-1} \left[ V_{j,t+1}^B - \sigma_j^x \log \left( Pr(B_{i,t+1}) \right) \right] \right)$$
Substituting in for $V_{j,t+1}^R$ using its definition according to Equation 2.5, we get

$$E[I_{j,t}] = \exp \left( \log \left( \frac{A_j - K_{j,t-1} \tilde{l}_{j,t-1}}{l_{j,t}} \right) + \frac{1}{\sigma_j^2} (\beta E_{t-1} [P_{j,t} - \beta P_{j,t+1}] ight.$$

$$- (FC_{j,t} - \beta E_{t-1} [FC_{j,t+1}]) - (VC_{j,t} - \beta E_{t-1} [VC_{j,t+1}]) - \epsilon_{j,t-1}^S ) \right) + \beta E_{t-1} \left[ \log \left( Pr \left( B_{i,t+1} \right) \right) \right] \right) + 1 \sigma_j^2 \right)$$

(2.7)

Since the outside value of land is not observed in my data, I have folded $\beta U_{j,t}$ into $\epsilon_{j,t-1}^S$. This equation is the basis for my estimating equation. Before fleshing it out more fully, in particular by specifying how fixed and variable costs relate to observable supply constraints like zoning, I detail the data I use for estimation.

### 2.3 Data

Table 1.1 provides an overview of the data used in this paper. The primary data source is a set of merged transaction and assessment records for every home sale in Massachusetts from 1993 through 2008. These data come from DataQuick, a large real estate data firm, and include detailed information on a host of house and transaction characteristics, including the sales price, house size, lot size and precise location. Most importantly for my purposes, the data include the year the house was built, as recorded in the assessment data. This allows me to pinpoint the time and location of all new units built.

A comparison with published Census data on building permits suggests that the year the house is listed as being built corresponds most closely to the year permits are issued, after which it takes almost a year to start and finish the house. Therefore I add one to the year of construction in the data to correspond to the year a house is finished and sold, which is what my theory incorporates. The comparison with permits data also indicates that the

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8I am grateful to Fernando Ferreira for sharing these data with me.
DataQuick data capture about half of all new units built in Massachusetts, with the fraction captured decreasing somewhat towards the end of the sample because a house must be sold at least once to show up in the data at all. I drop the final year of the DataQuick construction sample, 2008, and include year fixed effects in all of my estimates to capture the changing coverage over time.

Even so, it is likely that the DataQuick data are not fully representative of new construction in Massachusetts; that is, the new units included are not a random sample. In particular, large apartment buildings with many jointly owned units will not show up in my data, which means that the sample consists entirely of single-family homes and condominiums. This is an important limitation of my analysis, and the results should be considered as primarily applying to the construction of owner-occupied units.

I calculate the existing stock of housing in each tract-year by taking the stock as reported in the 2000 Census and extrapolating forward and backward using the number of new units reported in the DataQuick data. I assume that there is no depreciation, so that \[ K_{j,t} = K_{j,t-1} + I_{j,t} \]. My estimate of depreciation for the Boston metropolitan area from Paciorek (2011a) was less than 0.003.

To explore the effects of zoning, I focus on Massachusetts, particularly the Boston area, for two reasons. First, it is generally seen as having a housing market that is highly regulated, with new development made both very difficult and very expensive by local governments and community opposition (Glaeser and Ward 2009, Gyourko et al. 2008). Consequently, over the last few decades, rising demand has led to substantial increases in house prices with relatively little expansion of the housing stock. This contrasts sharply with the massive population growth but generally subdued house prices in major metropolitan areas in the South, such as Atlanta or Dallas.

Second, as in much of New England, zoning and housing supply regulation in Massachusetts are controlled primarily at the town or city level, with some oversight by the
state government. Because county governments are vestigial or even nonexistent, I do not have to worry about overlapping zoning regimes or multiple authorities over new construction.\textsuperscript{9} Perhaps as a result of this relatively simple governmental structure, Massachusetts has two unique publicly available and data sets on zoning and other regulatory constraints on housing supply.

The first is a geographic information system (GIS) data layer produced by MassGIS, a state agency, that details the primary use zoning at every point in Massachusetts. In addition to providing detailed information on zoning codes, MassGIS also assigns a primary use variable that enables direct comparison of zoning across different municipal regimes, at the cost of losing substantial detail. Using GIS software, I calculate the fraction of land in each tract that is assigned a given primary use code, such as “R1”, which is single-family residential with a minimum lot size of at least 80,000 square feet, or about 1.8 acres. The full list of codes and their descriptions are provided in Table 2.2. I employ other MassGIS data layers to calculate the total land area of each Census tract as well as the fraction of that area that is covered by water or protected from development due to public ownership or other legal constraint. I also exclude areas that are steeply sloped and therefore unsuitable for development, following Saiz (2010).\textsuperscript{10}

Figure 2.2 shows each zoning code’s share of new housing built from 1994 to 2009, as well as its share of Massachusetts land area that is not steeply sloped, covered by water or protected from development. While these values do not condition on any characteristics, such as prices, it is informative to compare the two sets of bars. Particularly restrictive single-family zoning, such as that represented by codes RA or R1, has a much lower share of new construction that of land area, while the reverse is true for codes R3 through R5. Multifamily housing is only permitted in very small parts of the state, although it accounts

\textsuperscript{9}One exception to this rule is the Cape Cod Commission, a regional planning authority with substantial control over development in Barnstable County. Cape Cod lies outside my area of analysis, however.

\textsuperscript{10}I thank Albert Saiz for providing the detailed GIS data underlying his estimates.
for a larger share of housing than of land. Interestingly, some 10 percent of new housing is built on land that is, by this measure, not zoned for residential development. This may be because of miscoding, because a variance was issued, or because the land was rezoned after it was coded by MassGIS. Regardless, I include the OTH category in my estimates to account for this phenomenon.

The second data source on housing regulation is a database produced by the Pioneer Institute, a local public policy research group, in cooperation with Harvard’s Rappaport Institute for greater Boston.\footnote{The Pioneer data are somewhat similar to the Wharton Residential Land Use Regulation Index (Gyourko et al. 2008), which I employ in Paciorek (2011a). Unfortunately for the purposes of comparison with that paper, the coverage of the Wharton index within Massachusetts is not sufficient for direct use here. The variables I construct from the Pioneer database are collectively quite highly correlated with the Wharton index, however.} The Pioneer database includes a large number of variables that detail the regulatory environment as of 2004 in 187 towns and cities in eastern Massachusetts (Dain 2005, Glaeser and Ward 2009).\footnote{A map of the coverage is provided in Figure 2.1. The Pioneer data cover more than half of the municipalities in Massachusetts.} The information was compiled from interviews with local government officials and reviews of legal documents.

The Pioneer data consist of more than 150 different variables, which is far too many to analyze individually. I keep five indicator variables that vary over time within the sample as municipalities were observed to change their zoning codes. These variables include whether the municipality has provisions for cluster zoning (“Cluster”) or inclusionary zoning (“Include”), limitations on the number of building permits issued per year in the town or for a given project (“Growphase”), bylaws limiting the development of wetlands (“Wetbylaw”), or sewer regulations more stringent than those of the state (“Septrule”).\footnote{For most of these variables, a higher value means less permissive zoning. The possible exceptions are cluster zoning, which allows for more construction if new units are “clustered” on a portion of the lot, and inclusionary zoning, which provides incentives for building affordable housing.}

I take all other variables for which there is nearly full coverage of the municipalities in the sample and use factor analysis to narrow the dimensionality. I keep the first four...
factors, which correspond roughly to strict frontage requirements (“Front”), limitations on multifamily housing (“Mult”), pavement width of new subdivision roads (“Pave”), and the stringency of minimum land area requirements (“Land”). In all four cases a higher value corresponds to a more restrictive zoning regime by that dimension. All of the Pioneer variables, including my constructed factors, are listed in Table 2.3.

With the exception of the few Pioneer variables that explicitly vary over time, I assume throughout this paper that the zoning code and other regulatory variables are constant and exogenous over my time frame, from 1993 to 2008. While not ideal, this is likely to be a reasonable approximation of reality, particularly with respect to the zoning code, which seems to be written to perpetuate the characteristics of the preexisting housing stock (Glaeser and Ward 2009). Endogenizing the zoning code is a potentially interesting extension, but it is likely to be more relevant in long-run studies of urban growth, rather than over the relatively short horizon in this paper.

2.4 Estimation and Results

2.4.1 Hedonic Pricing Equation

The first stage in estimation is to recover the parameters of the hedonic pricing function, which relates the sales price of a house to its observable attributes, location and the year in which it is sold. Because of the large size of the transactions data set, I am able to estimate the hedonic parameters at a fine level of disaggregation, namely census tract by year.

I follow Equation 2.1 in specifying the 20,000 separate regressions, with one important change. Ideally the data would specify the pro-rated lot size of multifamily units by dividing the total size of the lot under the structure by the number of units. In practice more than 15 percent of transacted units are assigned a lot size of zero. To avoid excluding these
transactions, in addition to including the lot size \((l_{i,t})\), I also include an indicator variable for whether the lot size is equal to zero:

\[
P_{i,t} = P_{j,t}^{0} + P_{j,t}^{x}x_{i,t} + P_{j,t}^{l}l_{i,t} + P_{j,t}^{zl}l_{i,t}1[l_{i,t} = 0] + \eta_{i,t}
\]  

I run individual regressions for each census tract \(j\) in Massachusetts in each year \(t\) using all of the available transactions in that tract and year, supplemented with transactions in nearby tracts (and the same year) to get up to 1,000. Augmenting the tract-year samples in this way both increases the stability of the estimates and mimics the “comps” approach used by real estate agents to set listing prices.\(^{14}\)

Summarizing the results of so many regressions is most easily done graphically. First, I show the average results by year, weighted by the number of units built in each tract over the entire sample. Figure 2.3 and the fourth column of Table 2.4 show that the estimated average marginal price per square foot has increased drastically in real terms over the last 20 years, from $50 to $130 per square foot. Meanwhile, the average predicted price of a new house — roughly speaking, the constant term in the regressions — rose from under $300,000 in 1993 to $500,000 at the peak of the market in 2005, before dropping back by about 10 percent over the next three years.\(^{15}\) Interestingly, as the third column of Table 2.4 and Figure 2.4 show, the average price dropped substantially more from 2005 to 2008 than the marginal price per square foot. This suggests that larger houses — which typically sell to wealthier buyers and are located on larger lots in the suburbs — lost less of their value during the first part of the recent housing downturn. This is an interesting pattern worthy

\(^{14}\)I have experimented with using smaller numbers of transactions, which reduces the dependence of the tract parameter estimates on neighboring tracts but increases the noise in the hedonic estimates. I find similar results throughout.

\(^{15}\)The average new house is substantially larger than the average existing house, which is why these averages are higher than would be reported for the Boston housing market taken as a whole, as can be seen by comparing the first two columns of Table 2.4. Because the relationship between lot size and house price is noisy (last two columns), I average over the two marginal lot size prices in making these predictions.
of further study, particularly once the data are extended to include the years since 2008.

We can also look at the hedonic results in the cross-section by mapping them. Figure 2.5 shows the predicted price of a new house in each tract, averaged over all years in the sample. The areas toward the red side of the color spectrum signify higher prices and are mostly located in the western suburbs of Boston. The more urban areas, including smaller cities such as Worcester and Lowell, clearly have low predicted prices, but this could be either because of different hedonic coefficients or because the houses are small.

Figure 2.6 shows the average hedonic coefficient on the house size for each tract; it is clearly highly correlated with overall prices, which is not surprising. There are differences however: While hard to see in these maps, prices in some high-density places — particularly high-demand suburb very close to Boston — have relatively low overall prices but high size gradients. Differences such as these help to identify differences in fixed and variable costs.

Although these regressions identify the parameters of the hedonic pricing equation for each tract-year, there is an additional step needed before I can use them in estimation. The model in Section 2.2 assumes that the decision to build is made one year before the house is completed and sold. Consequently, it incorporates expected prices rather than their realized values. There are two possible strategies to deal with the fact that I do not observe expected prices but only their realizations. The first, which is employed for estimation of a similar model at the metropolitan level in Paciorek (2011a), is to use lagged variables — in that case, exogenous demand shifters — as instruments for the realized prices. This is valid under the assumption of rational expectations, since all information available to agents when the expectations are formed should be orthogonal to the unobserved forecast error. This works only for estimation; in order to simulate the model, Paciorek (2011a) has to make further assumptions and completely specify the transition equations for the dynamics of the model.
The second route, taken by Murphy (2010) and other similar papers in the dynamic discrete choice literature, is to specify and estimate transition equations for the variables in the state space before estimating the model. These transition equations are assumed to be used by agents in forming expectations about prices and future conditional choice probabilities. Although I find similar results using both strategies when possible, I adopt the latter because the former method is not easily applied in my econometric setting. One problem is that the expectation of the logarithm of the future probability of building appears in the dynamic model in Equation 2.7 but I cannot use the actual log probabilities in estimation because investment is frequently equal to zero.

I specify a first-order Markov process for each parameter of the pricing equation: \( P_{0,j,t} \), \( P_{x,j,t} \), \( P_{l,j,t} \) and \( P_{zl,j,t} \). Since the prices are not stationary, at least over the period covered by my data, I include a linear deterministic time trend. To maximize my use of available information, I allow current prices to depend on lag prices nonlinearly, rather than using a simple autoregressive specification. The advantage of doing this is that it parsimoniously captures the fact that the next-period’s price may respond differently to the current price depending on how far above or below trend it is. Real estate markets have pronounced cycles, so prices slightly above trend may lead to yet higher prices before they eventually revert when they rise too high (Glaeser and Gyourko 2007, Case and Shiller 1989).

Using \( P_{0,j,t} \) as an example, I specify the following transition equation

\[
P_{0,j,t} = f \left( P_{0,j,t-1} \right) + \tau t + m_j + \zeta_{j,t}
\]

where \( m_j \) is a tract fixed effect and \( f (\cdot) \) is a nonlinear function that I estimate using generalized additive modeling techniques (Hastie and Tibshirani 1990, Wood 2006). Given the tract, year and current prices, I can thus predict what the next period’s price would be and use this prediction as the expected price.
2.4.2 Lot Size

The model described above treats the size of lots used for new construction as exogenous, in part to maintain tractability. It is valuable to examine the relationship between the lot sizes of newly constructed houses with zoning and other regulation as well as the marginal price per lot acre, which I estimate above. To do so, I run the following tract-year regression:

\[
    l'_{j,t} = \gamma P_{l,t} + \gamma P_{zl,t} + Z_{j,t} \gamma Z + m_c + m_t + \varphi_{j,t} \tag{2.9}
\]

which includes both the price per acre \( P_{l,t} \) and the price shift if the lot size is recorded as zero \( P_{zl,t} \), as well as county and year fixed effects. The zoning variables are incorporated in the matrix \( Z_{j,t} \), which premultiplies the vector of coefficients \( \gamma Z \). The dependent variable, \( l'_{j,t} \), is the average lot size in tract \( j \) at time \( t \), conditional on the lot size being positive.

Additionally, to check whether lots of reported size zero are simply a random sample of all lots, I estimate a fractional logit model, with the fraction of units in each tract with a zero lot size (\( F_{l,t} = \sum \{1 [l_{i,t} = 0] \} \)) as the dependent variable (Wooldridge 2002). The fractional logit model is almost precisely the same as a standard logit, except that the dependent variable is allowed to take on values along the interval from zero to one, rather than only the endpoints. With \( \Lambda \) denoting the logistic CDF, the model is

\[
    F_{l,t} = \Lambda \left[ \phi P_{l,t} + \phi P_{zl,t} + Z_{j,t} \phi Z + m_c + m_t + \varphi_{j,t} \right] \tag{2.10}
\]

The results from estimating equations 2.9 and 2.10 are shown in Table 2.5. The first column shows in particular that neither marginal price has an economically or statistically significant relationship with the chosen lot size.\(^{16}\) There is, however, a very strong and monotonic relationship with the primary use variables. For example, compared with the

\[^{16}\text{The standard errors throughout the paper are clustered at the tract level, since the primary use code information varies by tract and is constant over time.}\]
omitted category, which is R2, changing the entire tract to a larger lot size of R1 would increase the size of a new unit by 0.88 acres, with a standard error of 0.13 acres, where the average lot size of new houses across all tract-years is about 0.9 acres. Conversely, changing from R2 to high-density multifamily zoning, code MH, would reduce the size of each unit by 1.29 acres. This is by no means a surprising result, since these codes are defined primarily by the minimum lot sizes they require, but the quantification of the effect of each code will be useful for my simulations below.

The effects of the Pioneer codes, which are shown on the second page of the table, are generally small and mostly insignificant. The exceptions include cluster zoning, which is associated with smaller lot sizes. Large frontage requirements and more restrictions on multifamily housing, meanwhile, are appropriately found to increase lot sizes.

The second column of Table 2.5, which shows the results of the fractional logit regression, also shows no meaningful relationship between marginal prices and the prevalence of lot sizes recorded as zero. Interestingly, however, there are strong effects of zoning, with the multifamily codes (ML, MM, MH) in particular leading to many more zeros. This confirms the intuition that homes recorded in the DataQuick data as having a lot size of zero are likely located in multifamily structures, where the “lot size” is not a well-defined concept. These results indicate that most municipal assessment records do not use my definition of lot size, the pro-rated share of each structure’s lot. This has implications for how I specify the lot size variables in my estimates below.

2.4.3 Variable Costs

Before estimating the variable cost parameters, it is instructive to compare Figure 2.6, the map of average tract prices per square foot, with Figure 2.7, which shows the mean square footage of new houses in each tract. We should expect landowners to try to take advantage of Figure 2.7. See again Table 2.2 for the code definitions and Figure 2.2 for the prevalence of these codes.
of higher prices per square foot on the margin by building larger houses. This is more or less what appears to happen. For example, new houses built in the western suburbs, where prices per square foot are highest, tend to be larger than elsewhere. The goal of the structural estimation is thus to see whether, holding the marginal price constant, there are differences in the size of houses built that are correlated with zoning.

I modify Equation 2.4 in several ways to take account of the data. In one specification, I exclude lot size — which I have just shown is largely a function of the zoning regime — and include only the zoning variables. In the other, I include both lot size and the set of zoning variables to explore how much of zoning’s effect comes directly through the lot size. In this version, I incorporate whether the lot size is zero in addition to the actual reported lot size, as in the hedonic price and lot size regressions.

In both specifications, I include county \((m_c)\) and year \((m_t)\) fixed effects to account for systematic variation in variable costs across time or space. In addition, I need to parametrize how the Pioneer Institute and MassGIS primary use code variables affect costs and therefore the size of new houses that are constructed. I do this simply by including them as linear terms in the model, with one coefficient for each variable. The variable cost estimating equation, including the lot size terms, is thus

\[
E_{t-1} \left[ \log P_{jt} \right] = \alpha_x \hat{x}_{jt} + \alpha_l l_{jt} + \alpha_z Z_{jt} + Z_{jt} \alpha^Z + m_c + m_t + \xi_{jt-1} \tag{2.11}
\]

where \(Z_{jt}\) is a matrix comprising all of the zoning variables as well as a constant term and \(\alpha^Z\) is a vector of coefficients for these variables.\(^{18}\) The parameters represent the effect of changing the covariates on the marginal cost of construction per square foot of house size.

Intuitively, these parameters are identified by comparing the sales price per square foot of

\(^{18}\)Note that only the primary use code terms actually vary by tract \((j)\) and year \((t)\). As shown in Table 2.3, the constructed factors vary only by municipality, while the remainder of the Pioneer variables vary by municipality and year.
house size with the sizes of houses built; if zoning and regulation had no effect, the cost per square foot would be identical across regimes and $\alpha^Z$ would be a vector of zeros.

I estimate Equation 2.11 via GMM, with all the variables other than $\hat{x}_{j,t}$ — the dependent variable — as instruments. The results are shown in Table 2.6. When the lot size terms are included, in column (1), the costs per square foot are generally similar across the single-family residential codes, although they are somewhat lower in R3 and R2 (the omitted category) than in residential codes with larger or smaller minimum lots. These coefficients are only marginally above their standard errors, which range from $15 to $20.

The differences between single- and multifamily regimes is much more stark, with codes ML, MM and MU driving up costs by more than $100 per square foot relative to R2, with standard errors between $20 and $30. This estimates are quite large compared with the average sales price per square foot — which is also the average marginal cost, by assumption — of about $80. The costs imposed by other forms of regulation are estimated to be fairly minimal, although limitations on building permits (Growphase) seem to drive marginal costs up a bit, while inclusive zoning (Include) reduces them.

The most important effect in column (1), however, comes through the lot size. Each one acre increase in the size of the lot decreases the marginal cost per square foot by $44, with a standard error of $5, while having a lot size recorded as zero causes a level shift of more than $114, with a standard error of $10. These estimates indicate that it is much more costly to built large units when lots are small or the area is zoned for multifamily units, since landowners and builders do not build larger houses even holding constant the marginal price per square foot.

In column (2) I exclude the lot size terms to look at the overall effect of zoning and regulation. The coefficient on house size remains the same as in the first column, at 0.16,

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19This gives the same parameter values as isolating $\hat{x}_{j,t}$ and estimating via least squares, but then the standard errors need to be recalculated.
which means that the cost per square foot rises by 16 cents for each square foot increase in size. This convexity is necessary for the marginal price, which is constant, to equal the marginal cost at exactly one point in the house size domain.

Unsurprisingly, the zoning cost estimates are now larger, and a nearly monotonic relationship appears in the primary use codes, with costs rising as the minimum lot zoning becomes more stringent, from R3 to R4 to R5 and into multifamily. Changing a tract from entirely R2, the baseline, to medium-density multifamily (MM) would increase the marginal cost per square foot by nearly $250, more than three times the average marginal price per square foot of $80. Since tracts are large enough that few consist entirely of a single use code, the effective differences across tracts are smaller than these coefficients suggest, but they are still very large. Taken together, these results indicate that the sizes of new houses in eastern Massachusetts are determined in large part by the zoning regime, particularly the primary use codes, rather than differences in marginal prices.

2.4.4 Construction

In considering the role of regulation in new construction, it is instructive to compare Figure 2.5, the map of average tract new house prices, with Figure 2.8, which shows average new construction per tract. Holding regulation and prices constant, both theory and common sense tells us to expect more construction in each tract when the tracts are larger in area, so I divide construction by area in acres in the figure. The most interesting result is that there is essentially no discernible pattern in construction, particularly when comparing it to the obvious differences across tracts in prices. This suggests that something may be preventing landowners and builders from taking advantage of high prices.

Given the variable cost parameters, I can now turn to estimating Equation 2.7 and ex-

\textsuperscript{20}As discussed above, it is unlikely that prices are high or low because of the level of new construction, precisely because construction in all tracts is so low relative to the existing stock.
amining the effect of regulation on fixed costs and the quantity of new construction. There are two major issues remaining to resolve. First, although I have estimates of $A_j$, the total developable land area in each tract, and $K_{j,t-1} l_{j,t-1}$, the amount of land already in use, subtracting the latter from the former leads to a negative result in about 10 percent of the tract-year observations. This is because the two numbers come from different data sources, and my estimate of the average lot size of existing houses may be mismeasured in some cases, as I have already noted for the cases when the lot size is recorded as zero.

This creates a problem for estimating the equation as currently specified, since the logarithm is not defined over negative values. An additional related concern is that, although the theory does not allow for it to maintain simplicity, land that is currently used for housing can in practice be redeveloped for new construction, which means that currently developed land should not be subtracted from total land one-for-one. The easiest way to account for both issues is to include the logarithm of the total amount of land $\log A_j$ and the logarithm of currently developed land $\log \left( K_{j,t-1} l_{j,t-1} \right)$ as separate terms in the model, each with their own free coefficients rather than as offsets. Since the sum of log numbers is not equal to the log of the sum, this is not exactly what the model specifies, but it is a good approximation given the realities of the data.

I also include $\log l_{j,t}$, the logarithm of the average size of developable lots, as a separate term with its own coefficient. Since the mean lotsize of new units is not observed for tract-year observations that experience no new construction, I impute for these observations both the actual lot size and the probability of the lot size being zero using available data, including the lot size of existing homes in that tract, prices, and the zoning regime. I then calculate the lot size that would result if all of the lot sizes recorded as zeros were actually of size 0.01 acres. I take the logarithm of that number and include it in the estimation, along with the fraction of properties in that tract that were of size zero ($F_{j,t}^{l}$), to best capture the effect of lot size on the probability of new construction.
As in the variable cost equation, I incorporate zoning into the fixed cost terms linearly, with \( Z_{j,t} \) denoting a matrix of zoning variables and \( \lambda Z \) a vector of coefficients. After making all of these changes and grouping the price and variable cost terms, which I denote with hats since they have already been estimated, I get the following estimating equation

\[
E[I_{j,t}] = \exp \left( \lambda A \log (A_j) + \lambda K \log \left( K_{j,t-1}I_{j,t-1} \right) + \lambda l \log (l_{j,t}) + \lambdazl F_{j,t} \right.
\]

\[
+ \frac{1}{\sigma_j^2} \left( \beta E_{t-1} \left[ \left( \hat{P}_{j,t} - \beta \hat{P}_{j,t+1} \right) \right. \right.
\]

\[
- \left. \left. \left( \hat{VC}_{j,t} - \beta \hat{VC}_{j,t+1} \right) \right] \right)
\]

\[
\left. - (Z_{j,t} - \beta Z_{j,t+1}) \frac{\lambda Z}{\sigma_j^2} + \beta E_{t-1} \left[ \log (Pr(B_{i,t+1}))) \right] + m_c + m_t \right)
\]

(2.12)

Since the supply shock \( \epsilon_{j,t-1} \) is not observed, I incorporate county and year fixed effects to absorb as much of the tract-level variation in costs and construction as possible. It can be shown that any remaining unobserved variation, even if orthogonal to the covariates, leads to overdispersion in the Poisson model (Winkelmann 2008). There are various ways to model overdispersion, such as assuming a negative binomial distribution instead of the Poisson. The easiest and most robust approach, however, is to continue to use the Poisson model, which consistently estimates the parameters as long as the specification of the conditional mean of \( I_{j,t} \) is correct. It is then important to calculate standard errors that are robust to overdispersion, which I do throughout this paper.

The remaining issue with Equation (2.12) is that I do not observe the expected future log probability of building \( E_{t-1} \left[ \log (Pr(B_{i,t+1}))) \right] \). Since actual investment is frequently equal to zero, I cannot substitute in the realized values and instrument to overcome endogeneity from the forecast error. I instead follow the approach taken by Murphy (2010) and other similar papers in the conditional choice probability literature by estimating a flexible relationship between the probability of building and the variables in the state space, then

\[\text{The mean and variance of the Poisson distribution are equal. If the variance of the dependent variable in a Poisson model is appreciably greater than the mean, which is common in empirical applications, the data are described as exhibiting overdispersion.}\]
using the state transition equations to forecast the next period’s state space and, in turn, the probability of building.

I discussed the transition equations for prices in Section 2.4.1 above. I estimate similar transition equations for the average lot size and the fraction of lots that have size zero, again allowing for a deterministic trend and a flexible effect of the first lag. The remaining variables in the state space, such as the capital stock and the zoning covariates, either do not vary over time or are assumed to transition deterministically.

The conditional choice probability model for $Pr(B_{i,t})$ relates the number of new units built in each tract-year to the state space, including prices and zoning. Since all of the state-space variables follow first-order Markov processes, I do not include any lagged covariates, but I do incorporate tract fixed effects and a deterministic time trend. I also allow for a flexible effect of prices minus variable costs, using the same generalized additive modeling techniques as above.

It is important to note the distinction between this approach and actually estimating Equation 2.12. The parameters recovered from estimating this flexible model of construction do not have a direct structural interpretation, since they incorporate both the direct effect of changing contemporaneous covariates as well as the indirect effect via expectations about the future. The goal is simply to optimally predict the number of units that will be constructed conditional on the state space.

In Equation 2.12 I allow the log developable area, log already developed area, and log lot size to each enter with their own coefficients. If I allow for the same flexibility in the expected future log probability, and separate the expectation of the logarithm of future investment into its components, these terms wind up quasi-differenced in the resulting
estimating equation:

\[
E[I_{j,t}] = \exp \left( \log (E_{t-1}[I_{j,t+1}]) + \lambda^A (1 - \beta) \log (A_j) \right.
\]
\[
+ \lambda^K \left( \log \left( K_{j,t-1} \tilde{I}_{j,t-1} \right) - \beta E_{t-1} \log \left( K_{j,t-1} \tilde{I}_{j,t} \right) \right)
\]
\[
+ \lambda^l \left( \log (l_{j,t}) - \beta E_{t-1} \log (l_{j,t+1}) \right) + \lambda^{zl} \left( F_{j,t}^l - \beta E_{t-1} F_{j,t+1}^l \right)
\]
\[
+ \frac{1}{\sigma^Z_j} \left( \beta E_{t-1} \left[ (\hat{P}_{j,t} - \beta \hat{P}_{j,t+1}) - (\hat{V} C_{j,t} - \beta \hat{V} C_{j,t+1}) \right] \right)
\]
\[
- (Z_{j,t} - \beta Z_{j,t+1}) \frac{\lambda^Z}{\sigma^Z_j} + m_c + m_t \right)
\]

\[\text{(2.13)}\]

Before considering the results of the dynamic model, it is useful to estimate a simpler myopic version that excludes expected future covariates, according to

\[
E[I_{j,t}] = \exp \left( \lambda^A \log (A_j) + \lambda^K \log \left( K_{j,t-1} \tilde{I}_{j,t-1} \right) + \lambda^l \log (l_{j,t}) + \lambda^{zl} F_{j,t}^l
\]
\[
+ \frac{1}{\sigma^Z_j} \left( \beta E_{t-1} \left[ (\hat{P}_{j,t} - \beta \hat{P}_{j,t+1}) - (\hat{V} C_{j,t} - \beta \hat{V} C_{j,t+1}) \right] \right)
\]
\[
- (Z_{j,t} - \beta Z_{j,t+1}) \frac{\lambda^Z}{\sigma^Z_j} + m_c + m_t \right)
\]

\[\text{(2.14)}\]

This provides for more transparent examination of the patterns in the data. In addition, under the maintained assumption that all of the stochastic state variables transition according to first-order Markov processes, the “myopic” version is really a reduced form that allows each term to capture both direct effects on current construction and indirect effects via expectations of the future. This is similar to the conditional choice probability model for new investment described above, although the specification differs.

The results of this reduced form estimation are shown in Table 2.7. The first column uses county fixed effects, while the second replaces them with municipal fixed effects, at the cost of losing identification for the Pioneer zoning variables that are constant within a municipality over time. The final column mimics the second but allows for the coefficient on price to differ with regulation, which corresponds to heterogeneity in the scale parameter.

\[\text{22 Following much of the literature on estimating dynamic models, I set the discount factor } \beta = 0.95 \text{ rather than trying to estimate it.}\]
of the logistic cost shock $\sigma^\gamma_j$.

In all three columns I find coefficients on log (buildable) area of just over 1, although in all cases I can reject the null hypothesis that it equals one. In a Poisson regression, the coefficient can be interpreted as the effect of a one-unit change in the covariate on the log of new construction, so a coefficient of 1.16 (first line of column (1)) implies that a one percent increase in buildable area leads to a 1.16 percent increase in new construction, all else equal.

Interestingly, the coefficient on the log of area already built and devoted to housing is negative (from -0.08 to -0.16) but only statistically distinguishable from zero in the second column, since the standard error is about 0.08. This suggests that — at least under current zoning — redeveloping existing lots is not much more costly than building new on previously nonresidential property. The average lot size, meanwhile, is an important determinant of new construction in a tract, with a large and significant negative coefficient. In addition, when a larger fraction of new units are observed to have a zero lot size, there is more construction overall. Since the lot size and the prevalence of zeros depend critically on zoning, as shown previously, this is already strong evidence of the effect of zoning on new construction.

To examine the effect of zoning, I have grouped the primary use codes into a smaller number of categories, because the estimates are noisier than in the variable cost results. For example, agricultural/residential (RA) and very low-density residential (R1) are grouped into the new category RL, as shown in Table 2.2. In columns (1) and (2), I find a reasonably strong negative effect of category RL on new construction, but minimal effects from high-density residential (RH) or multifamily (MF) relative to the omitted category, which is still R2. These specifications also exhibit a very strong negative effect of codes MU and OTH — decreases in construction on the order of 100 percent if the tract is entirely composed of land under these codes — which is not surprising given that MU and OTH comprise areas
designated for mixed-use and non-residential construction, respectively. I discuss column (3), where prices are interacted with zoning, below.

The effect of other forms of regulation is muddled in column (1): Zoning that is unfriendly to multifamily construction seems to reduce the number of new units built, but the remaining terms are never statistically distinguishable from zero. In columns (2) and (3), however, I get larger estimates once I include the municipal fixed effects, which allow me to identify the effect of the time-varying regulation variables using only variation within town or city over time.

I find that bylaws protecting wetlands (Wetbylaw), inclusive zoning (Include), and stringent sewer or septic tank requirements (Septrule) all reduce new construction by about 10 to 15 percent, although the latter two effects are larger and statistically distinguishable from zero only in column (2), before the price interactions are thrown in. The remaining coefficients are negative but small.

Finally, but perhaps most interestingly, we turn to the coefficient on “Net Price”, which is just the expected price minus expected variable costs conditional on building, or $E_{t-1} \left( \hat{P}_{j,t} - \hat{V}_{C,j,t} \right)$ from Equation 2.14. In both columns (1) and (2) I find small negative coefficients, of -0.03 and -0.02, respectively. In neither case can I reject the null hypothesis that the true coefficient is zero or a small positive number, since the standard errors are 0.03 and 0.05, respectively. Nevertheless, this is difficult to square with the model, since the coefficient should be the reciprocal of a logistic scale parameter that must be positive. Basic intuition about housing, or indeed almost any good, also indicates that the supply curve ought to be positive.

I take two approaches to further examining this somewhat perplexing result. First, I re-estimate Equation 2.14 but allow for a flexible effect of net price on construction by using a generalized additive model. The result of this procedure is shown in Figure 2.9 along with black dashed lines representing the 95 percent confidence interval that results from a
tract-level block bootstrap procedure to account for clustering.\footnote{Although the confidence interval shows that the effect is somewhat noisily estimated, there is a fairly clear pattern: Construction is increasing in net price as long as net price is low, but the effect levels off or even turns negative at higher values of net price.}

The positive effect appears to be strongest for net prices less than approximately $400,000, with an additional positive effect up to $600,000. These two values are marked with vertical red dashed lines, and their location in the distribution of net price is shown in Figure 2.10. Approximately one quarter of the observations have estimated net prices below $400,000 and about 65 percent are under $600,000.

We can get a better idea of how to interpret this result by mapping census tracts and coloring them according to whether they have net prices under $400,000; between $400,000 and $600,000; or greater than $600,000. Figure 2.11 shows that the lowest net prices are located in or near relatively dense urban areas, including the immediate Boston suburbs, Worcester, Lowell, Lawrence and Brockton. The next highest category comprises mostly municipalities near these cities, along with a band of towns and cities in Plymouth and Bristol counties, at the bottom of the map. Taken together, the map establishes a strong pattern of association between urban areas and lower net prices, which in turn have a positive effect on construction when increased on the margin within the bottom half of the distribution.

One supply-side explanation for this phenomenon is that zoning regime may alter the effect of net price on construction, as well as having the direct effect on the level of construction noted in columns (1) and (2) of Table 2.7. I examine this possibility in column (3), by interacting the net price term with the zoning covariates. Although the standard errors on the interaction terms are large relative to the coefficients, there is a discernible pattern. For tracts entirely under R2 zoning, construction falls by about 5 percent for each

\footnote{I exclude observations in the top and bottom 1 percent of net price, to focus attention on the relevant part of the curve. This has essentially no effect on the shape of the curve over the domain shown, precisely because it is estimated flexibly.}
$100,000 increase in net price, although the standard error is 5 percent so no meaningful conclusions can be drawn. Under RH zoning, the effect increases by 12 percentage points to about 7 percent per $100,000 of net price, while under multifamily zoning, construction rises by a full 40 percent per $100,000.

Including the price interactions also changes the main effects of zoning somewhat. For example, for net price at the mean of the sample, a tract with entirely MF zoning would have 0.8 log points more construction than under R2. In other words, both the average level of construction and its sensitivity to price and variable cost changes are much higher when zoning allows for multifamily units.

The other interesting coefficient is on the Growphase factor, which primarily reflects annual limits on the issuance of building permits within a given town or to a certain project. The existence of such limits serves to reduce the responsiveness of construction to price by about 6 percent, with a standard error of 3 percent. Qualitatively, this result fits with the intuition that annual limits on building permits should reduce the price elasticity of new supply but leave the average level of construction unchanged.

The results of the full dynamic model are provided in Table 2.8. Comparison with the myopic model results in Table 2.7 indicate that many of the coefficients are now substantially larger. The intuition for this is that a one-unit increase in one of the quasi-differenced terms corresponds to a one-unit increase in the underlying variable today, holding constant the expected future values of that variable. To the extent this is a realistic possibility, it is reasonable to think that builders would take advantage of, for example, a one-year relaxation of zoning by building as much as possible right away.

If the zoning change is permanent, then both the current and expected future value change by the same amount, which means the coefficient of interest must be multiplied by $1 - \beta$, which is 0.05. Alternatively, if the current and expected future values of the variable are not highly correlated, conditional on the other covariates, then the estimated coefficient
in the dynamic model may be smaller than in the myopic version. This is true for the lot size coefficients, for example.

Qualitatively, the results of the dynamic model are generally similar to those of the myopic version, so I discuss them only briefly. Once the municipal fixed effects are included, in columns (2) and (3), a strong negative relationship is apparent between new construction and the stringency of the primary use code. A permanent change of an entire tract from R2, the omitted category, to MF, would lead to an increase of about 0.8 log points of new construction. If the change were temporary, it would be predicted to increase construction by more than 16 log points, an enormous change. This is probably not a realistic calculation, particularly since the model is estimated on data that do not include changes in primary use over time, but it emphasizes the importance of the zoning code for construction. The pattern of the coefficients on the price interaction terms in column (3) are also qualitatively similar to those in Table 2.7.

Although one object of estimating the model of construction was to recover the structural parameters of the fixed cost function, in particular the effect of various types of zoning on fixed costs, the results as estimated do not really allow for this. To isolate fixed costs I need to be able to divide the estimated zoning coefficients through by the reciprocal of the price coefficient, which is equal to the scale parameter of the logistic cost shock that determines which landowners decide to build in a given tract and year.

In practice, however, since I find a small or even negative price coefficient when I average across all observations, recovering meaningful fixed cost parameters is not possible. Taking the model literally, a price coefficient close to zero means that the variance of the cost shocks is too large relative to the effect of prices to recover with my data. I interpret this as meaning that within much of the Boston area, particularly tracts zoned for low-density residential construction, random shocks rather than prices are governing the provision of new supply.
2.5 Simulations of Alternative Zoning Regimes

In the previous section I presented very strong evidence that the zoning regime plays a very important role — even more important than, say, the sales price of new houses — in determining the quality and quantity of new residential construction that takes place in census tracts in the Boston area. To provide better intuition about the scale of these effects, I will now use those estimates to simulate the level of new construction that would occur under alternative regimes. Although there are a number of potentially interesting simulations that would make use of the dynamic estimates, I am primarily interested here in permanent changes in the zoning regime.

Accordingly, I use the results in the second column of Table 2.6, the variable cost estimates when lot size is excluded from estimation, and the myopic estimates from column (2) of Table 2.7, which incorporates municipal fixed effects but does not interact the price coefficient with zoning. Before simulating the construction quantity model, I use the estimated lot size equations, 2.9 and 2.10, to predict how the sizes of lots would change. Importantly, I do not allow prices to adjust in these scenarios: I am simply trying to show what happens to the supply side when zoning is tightened or loosened relative to present values, not to perform a full partial or general equilibrium analysis.

The first line of Table 2.9 shows that about 8,000 new houses are built in an average year in the Boston metropolitan area, and the average square footage of those houses is about 2,300. In my baseline simulation, I leave all exogenous variables the same but use the fitted lot sizes given current zoning. This procedure predicts slightly less construction, about 7,600 houses, of modestly larger size.

---

24Since Table 2.6 includes the finer-grained primary use codes, I reestimate the same model with the broader categories from Table 2.7 and use those results.

25Of course, as noted above, my price estimates are generally small and insignificant anyway. This is probably a consequence of the strict zoning regime, however, so there is no reason to think it would remain true after the large changes I simulate.

26Using the expectation of the lot size conditional on zoning rather than the actual lot size values does not
Compared with the baseline, changing all primary use zoning to low-density residential (RL) leads to six percent less construction and pushes up the average size by about 13 percent. Changing all zoning to RH increases construction by about 20 percent to 9,076 but actually increases the average size of new housing a bit. This mixed result is the consequence of changing both lower-density residential areas (RL and R2) and multifamily areas (MF) to RH.

The really sizable effects come from completely upzoning the region to allow multifamily housing throughout. The number of new units per year triples relative to the baseline, and the average size of new units falls by a third. If I also remove all other restrictions by setting Cluster, Include, Growphase, Wetbylaw, and Seprule equal to zero, the number of new units is even larger, about 26,000.

Clearly this is a huge change relative to the current regime, and these results should not be taken too literally, since I am not allowing prices to adjust or for general equilibrium effects, for example in the labor market. That said, taken together with the parameter estimates, these simple simulations do illustrate the most important point of this paper: Zoning, rather than the market price of housing, determines how many new houses get build in greater Boston, where those houses are built, and how large they are.

2.6 Conclusion

This paper uses detailed data on new construction and on regulation in the Boston area to show how zoning shapes the quantity, location, and quality of new housing construction. I specify and estimate a dynamic structural model of housing supply and recover key parameters that relate zoning to the marginal cost per square foot of new construction and the fixed costs of supply. Simulations using these estimates indicate that far fewer houses are give the same results because the construction equation is nonlinear.
built, and that those built are substantially larger, than would be expected under zoning with less restrictive minimum lot sizes, among other provisions. Indeed, where zoning provides for low-density single-family residential housing, I find essentially no relationship between price and new construction, which is a remarkable result: The supply curve is effectively vertical in these areas.

This is an important set of empirical results, in that it quantifies the effect of supply regulation at the local level in a theoretically rigorous way. There are at least two important caveats, however, both of which suggest avenues for future research. First, this paper only models and estimates the supply side of the housing market, which means I cannot perform more complicated equilibrium analysis, as in Paciorek (2011a). By incorporating demand, many additional interesting policy questions can be considered, such as whether supply restrictions in one jurisdiction have significant spillovers in neighboring areas.

Second, this paper, like much of the empirical literature on housing supply restrictions, makes no attempt to determine the benefits of zoning to existing homeowners. If building dense multifamily housing in already developed areas has large negative externalities, a certain level of zoning or homeowner opposition to new development via the political process may be optimal. In addition, Calabrese et al. (2007) use a model of Tiebout competition among local jurisdictions to argue that zoning improves aggregate welfare by allowing better matching of public goods to preferences, although there are also significant distributional implications. Alternatively, if zoning is primarily a tool used by existing homeowners to maintain their property values by restricting competing supply, then zoning at best is a transfer and at worst results in deadweight loss. In practice all of these explanations for zoning are likely to be true to some extent, which means that separating and quantifying them is an important question for further empirical study.
See Table 2.2 for code definitions. “OTH” comprises all codes not otherwise included in that table, including areas zoned for commercial or industrial use.
Figure 2.3: Price Gradient by Year

Estimated Price Gradient

Dollars per square foot

Year

1995 2000 2005

60 80 100 120

118
Figure 2.4: Predicted Price by Year

Predicted Price of Average New House

Note: Excludes variation from lot size coefficients
Predicted Price of New House

Predictions of new house prices in each tract, averaged across years in sample.
Figure 2.6: Price Gradient by Tract

Price per Square Foot

Price per square foot of housing in each tract, averaged across years in sample.
Average house area in square feet in each tract, averaged across years in sample.
New Construction per Acre of Land

New units divided by land area in acres in each tract, averaged across years in sample.
Figure 2.9: Flexible Net Price Coefficient Estimate

Effect of price net of variable costs on new construction, estimated using a generalized additive model. Top and bottom 1% of observations by net price are excluded from estimation. Black dashed lines represent the tract-level block-bootstrapped 95% confidence interval.
Histogram of Net Price

Histogram of price net of variable costs.
The "net price" is the expected price of a house once sold minus variable costs.
Table 2.1: Data

<table>
<thead>
<tr>
<th>Variable</th>
<th>Number of Tracts</th>
<th>Time Period</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dataquick Construction</td>
<td>1361</td>
<td>1994-2008</td>
</tr>
<tr>
<td>Dataquick Sales</td>
<td>1361</td>
<td>1993-2008</td>
</tr>
<tr>
<td>MassGIS Primary Use Codes</td>
<td>1361</td>
<td>Observed once (Various)</td>
</tr>
<tr>
<td>Pioneer Institute (Factors)</td>
<td>835</td>
<td>Observed once (2004)</td>
</tr>
<tr>
<td>Pioneer Institute (Other)</td>
<td>835</td>
<td>Annual through 2004</td>
</tr>
</tbody>
</table>

The “factors” are constructed by the author from non-time varying Pioneer Institute variables; see Table 2.3. The “other” Pioneer variables are time-varying and are included individually in the analysis. The MassGIS zoning data reflect the zoning regime in different years depending on the municipality; nearly all are from the late 1990s through the mid-2000s.
<table>
<thead>
<tr>
<th>Codes</th>
<th>Raw</th>
<th>Coarse</th>
<th>Description</th>
<th>Minimum Lot Size or Density</th>
</tr>
</thead>
<tbody>
<tr>
<td>RA</td>
<td>RL</td>
<td></td>
<td>Residential/Agricultural Mix</td>
<td></td>
</tr>
<tr>
<td>R1</td>
<td>RL</td>
<td></td>
<td>Single-Family Residential</td>
<td>80,000+ sq. ft.</td>
</tr>
<tr>
<td>R2</td>
<td>R2</td>
<td></td>
<td>Single-Family Residential</td>
<td>40,000 – 79,999 sq. ft.</td>
</tr>
<tr>
<td>R3</td>
<td>RH</td>
<td></td>
<td>Single-Family Residential</td>
<td>20,000 – 39,999 sq. ft.</td>
</tr>
<tr>
<td>R4</td>
<td>RH</td>
<td></td>
<td>Single-Family Residential</td>
<td>15,000 – 19,999 sq. ft.</td>
</tr>
<tr>
<td>R5</td>
<td>RH</td>
<td></td>
<td>Single-Family Residential</td>
<td>5,000 – 14,999 sq. ft.</td>
</tr>
<tr>
<td>ML</td>
<td>MF</td>
<td></td>
<td>Multifamily, Low Density</td>
<td>3-8 dwelling units/acre</td>
</tr>
<tr>
<td>MM</td>
<td>MF</td>
<td></td>
<td>Multifamily, Medium Density</td>
<td>9-20 dwelling units/acre</td>
</tr>
<tr>
<td>MH</td>
<td>MF</td>
<td></td>
<td>Multifamily, High Density</td>
<td>21+ dwelling units/acre</td>
</tr>
<tr>
<td>MU</td>
<td>MU</td>
<td></td>
<td>Mixed Use</td>
<td></td>
</tr>
<tr>
<td>OTH</td>
<td>OTH</td>
<td></td>
<td>Other</td>
<td></td>
</tr>
</tbody>
</table>

I calculate the “OTH” category by summing across all codes not otherwise included in this table, including areas zoned for commercial or industrial use. For some estimates I combine several similar categories together, leading to the smaller set of codes in the “Coarse” column.
Table 2.3: Pioneer Insitute Housing Regulation Data

<table>
<thead>
<tr>
<th>Variable</th>
<th>Description</th>
<th>Dimension of Variation</th>
<th>Values</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Factors:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Front</td>
<td>Frontage</td>
<td>Municipality</td>
<td>Standardized</td>
</tr>
<tr>
<td>Mult</td>
<td>Multifamily zoning</td>
<td>Municipality</td>
<td>Standardized</td>
</tr>
<tr>
<td>Pave</td>
<td>Subdivision road widths</td>
<td>Municipality</td>
<td>Standardized</td>
</tr>
<tr>
<td>Land</td>
<td>Land area</td>
<td>Municipality</td>
<td>Standardized</td>
</tr>
<tr>
<td><strong>Other:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cluster</td>
<td>Cluster zoning provisions</td>
<td>Municipality x Year</td>
<td>Indicator (0/1)</td>
</tr>
<tr>
<td>Include</td>
<td>Inclusionary zoning provisions</td>
<td>Municipality x Year</td>
<td>Indicator (0/1)</td>
</tr>
<tr>
<td>Growphase</td>
<td>Limitations on permits</td>
<td>Municipality x Year</td>
<td>Indicator (0/1)</td>
</tr>
<tr>
<td>Wetbylaw</td>
<td>Wetlands bylaws</td>
<td>Municipality x Year</td>
<td>Indicator (0/1)</td>
</tr>
<tr>
<td>Septrule</td>
<td>Stringent septic regulations</td>
<td>Municipality x Year</td>
<td>Indicator (0/1)</td>
</tr>
</tbody>
</table>
Table 2.4: Average Hedonic Results by Year

<table>
<thead>
<tr>
<th>Year</th>
<th>Sales Price</th>
<th>Predicted Sales Price</th>
<th>Price / Square Foot (House)</th>
<th>Price / Acre (Lot)</th>
<th>Price / Zero Lot Size</th>
</tr>
</thead>
<tbody>
<tr>
<td>1993</td>
<td>166,000</td>
<td>286,000</td>
<td>47</td>
<td>38,000</td>
<td>-15712</td>
</tr>
<tr>
<td>1994</td>
<td>167,000</td>
<td>284,000</td>
<td>46</td>
<td>79,000</td>
<td>-15067</td>
</tr>
<tr>
<td>1995</td>
<td>166,000</td>
<td>284,000</td>
<td>49</td>
<td>58,000</td>
<td>-10269</td>
</tr>
<tr>
<td>1996</td>
<td>169,000</td>
<td>289,000</td>
<td>53</td>
<td>93,000</td>
<td>-10873</td>
</tr>
<tr>
<td>1997</td>
<td>175,000</td>
<td>297,000</td>
<td>57</td>
<td>65,000</td>
<td>-10285</td>
</tr>
<tr>
<td>1998</td>
<td>187,000</td>
<td>311,000</td>
<td>62</td>
<td>102,000</td>
<td>-13880</td>
</tr>
<tr>
<td>1999</td>
<td>202,000</td>
<td>333,000</td>
<td>70</td>
<td>35,000</td>
<td>-14726</td>
</tr>
<tr>
<td>2000</td>
<td>228,000</td>
<td>359,000</td>
<td>83</td>
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“Mean Sales Price” is the average sales price for all new and existing houses sold in that year.
“Predicted Sales Price” (Figure 2.4) is the predicted sales price for an average new home, smoothed over annual variation in the lot size coefficients. “Price / Acre (Lot)” is the hedonic coefficient on lot size, inclusive of any zeros. “Price / Zero Lot Size” is the hedonic coefficient on an indicator variable for whether the lot size is recorded as zero.
Table 2.5: Lot Size - Zoning Relationship

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<td>(0.24)</td>
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<td>(0.68)</td>
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Table 2.5 (Cont.): Lot Size - Zoning

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<td>Growphase</td>
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<td>(0.13)</td>
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<td>Wetbylaw</td>
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<td>(0.11)</td>
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<tr>
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<td>Yes</td>
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<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
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First column is a regression of the tract-year lot size in acres, conditional on being positive, on covariates. Second column is a fractional logit regression with dependent variable equal to the fraction of properties in each tract-year that have a lot size of zero. Standard errors clustered at the tract level in parentheses.
Table 2.6: Variable Costs

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Table 2.6 (Cont.): Variable Costs

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<td>Year FE</td>
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Effect of covariates on marginal cost per square foot of house size. House size expressed in square feet; average lot size expressed in acres. “Zero Lot Size” is the fraction of properties in each tract-year that have a lot size of zero. Standard errors clustered at the tract level in parentheses.
Table 2.7: Myopic Model Estimates

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<td>(0.07)</td>
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Table 2.7 (Cont.): Myopic Model

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N 12290  12290  12290
County FE Yes No No
Muni FE No Yes Yes
Year FE Yes Yes Yes

Tract-year Poisson regressions of new construction on covariates. “Zero Lot Size” is the fraction of properties in each tract-year that have a lot size of zero. Standard errors clustered at the tract level in parentheses.
### Table 2.8: Dynamic Model Estimates

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<td>Fraction RH</td>
<td>4.32</td>
<td>7.31</td>
<td>7.23</td>
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<tr>
<td></td>
<td>(1.11)</td>
<td>(1.87)</td>
<td>(1.85)</td>
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<tr>
<td>Fraction MF</td>
<td>2.28</td>
<td>16.38</td>
<td>16.53</td>
</tr>
<tr>
<td></td>
<td>(2.85)</td>
<td>(4.24)</td>
<td>(4.18)</td>
</tr>
<tr>
<td>Fraction MU</td>
<td>-12.23</td>
<td>-35.80</td>
<td>-32.02</td>
</tr>
<tr>
<td></td>
<td>(5.72)</td>
<td>(17.45)</td>
<td>(17.12)</td>
</tr>
<tr>
<td>Fraction OTH</td>
<td>-13.87</td>
<td>-15.73</td>
<td>-15.96</td>
</tr>
<tr>
<td></td>
<td>(2.49)</td>
<td>(2.62)</td>
<td>(2.62)</td>
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<tr>
<td>Cluster</td>
<td>-0.11</td>
<td>-0.06</td>
<td>-0.09</td>
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<tr>
<td></td>
<td>(0.07)</td>
<td>(0.08)</td>
<td>(0.08)</td>
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<tr>
<td>Include</td>
<td>-0.11</td>
<td>-0.10</td>
<td>-0.09</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Growphase</td>
<td>-0.03</td>
<td>-0.05</td>
<td>-0.12</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
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<tr>
<td>Wetbylaw</td>
<td>0.08</td>
<td>0.08</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Septrule</td>
<td>0.04</td>
<td>-0.00</td>
<td>-0.02</td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Front</td>
<td>0.48</td>
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<tr>
<td></td>
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<tr>
<td>Mult</td>
<td>0.63</td>
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<tr>
<td></td>
<td>(0.23)</td>
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(Continued on next page)
Table 2.8 (Cont.): Dynamic Model

<table>
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<tr>
<td>Pave</td>
<td>-0.12</td>
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<td></td>
<td>(0.23)</td>
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<td></td>
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<tr>
<td>Land</td>
<td>0.51</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.21)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price ($100,000)</td>
<td>-0.03</td>
<td>-0.05</td>
<td>-0.30</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Net Price x RL</td>
<td>0.03</td>
<td></td>
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<tr>
<td></td>
<td>(0.12)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price x RH</td>
<td>0.22</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price x MF</td>
<td>1.00</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.37)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price x MU</td>
<td>1.28</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.83)</td>
<td></td>
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<tr>
<td>Net Price x OTH</td>
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<td></td>
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<tr>
<td></td>
<td>(0.22)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price x Cluster</td>
<td>0.15</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Net Price x Include</td>
<td>0.00</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.09)</td>
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<tr>
<td>Net Price x Growphase</td>
<td>-0.11</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.09)</td>
<td></td>
<td></td>
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<tr>
<td>Net Price x Wetbylaw</td>
<td>0.03</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td></td>
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</tr>
<tr>
<td>Net Price x Septrule</td>
<td>0.03</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| N             | 9818    | 9818    | 9818    |

| County FE     | Yes     | No      | No      |
| Muni FE       | No      | Yes     | Yes     |
| Year FE       | Yes     | Yes     | Yes     |

Tract-year Poisson regressions of new construction on co-variates, including expected future construction as an offset. All terms quasi-differenced, as described in text. “Zero Lot Size” is the fraction of properties in each tract-year that have a lot size of zero. Standard errors clustered at the tract level in parentheses.
### Table 2.9: Simulation Results

<table>
<thead>
<tr>
<th></th>
<th>New units per year</th>
<th>Average size</th>
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</thead>
<tbody>
<tr>
<td>Actual</td>
<td>7,944</td>
<td>2,324</td>
</tr>
<tr>
<td>Baseline simulation</td>
<td>7,646</td>
<td>2,462</td>
</tr>
<tr>
<td>All RL</td>
<td>7,199</td>
<td>2,831</td>
</tr>
<tr>
<td>All RH</td>
<td>9,076</td>
<td>2,536</td>
</tr>
<tr>
<td>All MF</td>
<td>22,442</td>
<td>1,646</td>
</tr>
<tr>
<td>All MF; no reg.</td>
<td>26,202</td>
<td>1,667</td>
</tr>
</tbody>
</table>

Simulations of number of new houses constructed per year and average house size in square feet under various zoning regimes, keeping estimated year and municipal fixed effects as estimated. “All MF; no reg.” indicates that all primary use code zoning has been set to multifamily, and all time-varying regulations are set to zero.
Chapter 3

Does Home Owning Smooth the Variability of Future Housing Consumption?
3.1 Introduction

With the median U.S. family devoting about one-third of its annual income and 45 percent
or more of its net worth to housing, fluctuations in house prices and annual housing costs
have the potential to generate significant consumption volatility. Most analysts have fo-
cused on the effects on households of the sizable year-to-year fluctuations in house prices
within a housing market. However, the effects of housing cost volatility may be mitigated
merely by owning one’s house (Sinai and Souleles 2005). Instead, a potentially significant
source of housing cost uncertainty is faced by households who anticipate moving to dif-
ferent housing markets. While the average standard deviation in real annual house price
growth within a housing market is 5.6 percent, the differential growth in housing costs
across markets has a standard deviation of 7.4 percent\(^1\). Thus a chance of moving to a new
housing market creates uncertainty about the future price of housing, and the act of relocat-
ing could induce volatility in housing and non-housing consumption due to unanticipated
differences in housing costs.

In this paper, we show that simply owning a house in the present can partially insure
a household against uncertain housing costs due to potential moves in the future. The
reason is that households tend to move between housing markets with correlated house
prices, so their current houses often are worth more precisely when their next house is
more expensive. This positive correlation between wealth and house prices mitigates the
decline in housing consumption due to higher prices alone, or dampens the increase in
housing consumption due to lower prices.

We illustrate this idea in a simple two-period representative agent model with two loca-
tions and stochastic house prices. The model predicts a negative relationship between the

\(^1\)The figure of 7.4 percent is the standard deviation in the difference between the annual house price
growth in one’s own housing market versus other markets. Calculated using the Federal Housing Finance
Agency’s conventional mortgage repeat sales index, deflated by the CPI, for 168 metropolitan areas over the
1982-2007 time period.
variance of housing consumption for households who recently moved and the covariance in the house prices of the origin and destination cities. It also predicts that the hedging benefit of high covariance would be strongest for households who own more housing.

Using household-level microdata from the U.S. Census, we find that home owners who recently moved between highly covarying cities have less conditional variance in their house values than do home owners who moved between low-covariance cities. This result remains even after controlling for origin and destination Metropolitan Statistical Area (MSA) fixed effects, so that our estimate of the hedging effect of covariance is identified solely from the pairing of MSAs. We also control for household-level determinants of housing demand.

Overall, we find that a one standard deviation increase in covariance reduces the variance of subsequent housing consumption for the average household by about 10 percent in our preferred specification. The reduction in variance is especially pronounced — as much as 18 percent — for home owners who we predict were likely to own larger houses before moving. The total effect of covariance can be sizable for those households who move between highly covarying cities. An average household experiences nearly a 30 percent reduction in its variance of housing spending if it moves between the 95th percentile covarying city pair relative to a move at the median. For a high-income family, the same comparison yields a reduction in variance of 40 percent.

This paper makes contributions in several areas. First, previous research has estimated how the hedging potential of home ownership affects households’ *ex ante* choices of tenure mode or the quantity of housing to consume (Sinai and Souleles 2005, Han 2008, Sinai and Souleles 2009), or has considered the hedging properties of homeownership in theory (Ortalo-Magne and Rady 2002). Other researchers have noted that, due to its nature as a consumption commitment and illiquid asset, owning a home shifts households’ financial investment choices (Cocco 2004) and also affects the volatility of consumption by changing
how consumption responds to income shocks (Chetty and Szeidl 2007). In contrast, this paper shows that owning a home does reduce the ex post variance of housing consumption, so that households are correct in believing that owning a house can hedge future housing costs.

Second, this paper contributes to the consumption smoothing literature by providing an important example of what Cochrane (1991) calls an “informal institution” that provides consumption insurance. Consumption smoothing has been examined in a number of contexts, such as unemployment insurance (Gruber 1997, Browning and Crossley 2001, Chetty and Szeidl 2007) and welfare (Gruber 2000), typically in the sense that there are institutions that facilitate a consistent level of consumption when there are unexpected changes in income. In our context, owning a home enables a household to better maintain a level of housing consumption in the face of unexpected changes in prices. Hurst and Stafford (2004) and Hryshko, Luengo-Prado and Sorensen (2010) consider the effect of the liquidity provided by housing equity on the smoothing of non-housing consumption. While we do not provide direct empirical evidence on non-housing consumption, if owning a home hedges future housing consumption it should also reduce the variance of non-housing consumption, since the entire consumption bundle is affected by changes in house prices.

Our empirical approach is in the spirit of Cochrane (1991), who regresses a household’s change in consumption on a proxy for an idiosyncratic shock to income, such as illness, and Gruber (1997), who compares how consumption responds to unemployment when unemployment insurance is more or less generous. There are two important distinctions between our context and these papers: First, our shocks are to relative prices (of housing) rather than income. Second, we implement a more complex empirical strategy that infers the consumption response to a shock from the cross-sectional conditional variance in housing consumption subsequent to a move. We take this approach not only because we do not observe in our data housing consumption prior to moving, but even if we did, the durable
nature of housing implies that the amount of housing consumption prior to moving would be a poor proxy for the latent desired housing consumption after moving.

Our empirical strategy relies on the notion that housing consumption for households who move between housing markets with a higher covariance should more closely match their latent housing demand since unexpected price shocks in the destination market are matched by price changes in the origin. In brief, we estimate the latent demand for housing and test whether the variance of the deviation of realized housing spending from predicted spending varies with the covariance of house prices between the origin and destination markets. The two-stage conditional variance procedure includes controls at both stages of the estimation for household demographics, the expected hedging benefit of homeownership, and origin and destination MSA fixed effects.\(^2\) The demographic controls account for predictable differences in the level of housing demand, variation in the demographic composition of movers across MSA pairs, and any heteroskedasticity related to observable household characteristics. The MSA fixed effects account for differences among MSAs that could affect the level of housing wealth or spending, such as the level of house prices, or the variance, such as households departing an origin MSA having systemically higher variance in wealth. Because of the origin and destination MSA fixed effects, our estimates of the covariance effect are identified from the pairing of MSAs.

We find two other pieces of evidence consistent with the hedging interpretation of the relationship between covariance and the subsequent variance in housing spending. First, the theoretical prediction that the housing hedge should have the largest effect for households who owned more housing before moving enables us to use household-level variation to relax the identifying assumption of the same (conditional) distribution of housing demand for all movers from an origin city or to a destination city, and instead allow that

\(^2\)Our two-stage estimation process is similar to that proposed by Engle (1982) to test for Autoregressive Conditional Heteroskedasticity (ARCH) disturbances in time series applications. It is also similar to Breusch and Pagan’s (1978) test for heteroskedasticity.
distribution to vary by MSA pairs. We show that, among households who move between a origin-destination city pair, the effect of covariance on the conditional variance in subsequent housing spending is largest for households who had relatively high incomes relative to prices in the origin or had high predicted housing consumption in the origin MSAs. In addition, households with higher covariances are more likely to own a house in their destination MSAs, consistent with the hedge ensuring that they have enough wealth to buy a home.

Finally, we use a generalized additive model (GAM) to allow for a nonlinear effect of covariance in house prices on the variance (and mean) of housing consumption after a move. The GAM also allows us to nonlinearly control for covariates such as household income and age. The estimated relationship between variance and covariance is noticeably nonlinear, with a larger effect on the margin for households with high covariance. For example, the relationship for households below the mean of covariance is about one third as large as for those above. This indicates that high-covariance households have a better hedge on the margin, as well as on average.

Our discussion proceeds as follows: In the next section, we outline a simple model of housing consumption with migration. We use this model to derive the direct effect of the covariance of house price changes on the variance of subsequent housing expenditures, and discuss how the response of the initial housing choice to that covariance can induce a second-order indirect effect. In Section 3.3, we discuss our data and detail our use of cross-sectional and time-series variation to proxy for variance across states of the world. Then, in Section 2.4, we describe our conditional variance empirical strategy. We present our results in Section 3.5 and interpret the magnitudes in Section 3.6. Section 3.7 concludes.
3.2 A Simple Model of Housing Consumption with Migration

3.2.1 Intuition

In this section, we focus on what happens when, because it previously owned a home, a household’s wealth is not independent of the house prices it faces after a move. Since a large fraction of household wealth is allocated to housing, if house prices in the market a home-owning household is moving from covary positively with house prices in the market the household is moving to, the household will be wealthier (due to selling its prior house) precisely when the next house is more expensive and poorer when the next house is relatively cheap. Standard housing demand models recognize that households with more wealth should buy more housing, all else equal, and those that face higher prices should buy less. These wealth and price effects on housing demand will offset each other for those households for whom house prices in the former and next housing markets covary more strongly, and especially for those who have allocated more of their wealth to housing, thus providing a natural hedge against house price volatility.

This hedging intuition suggests that the potential variance in housing consumption in the destination market should be lower for households who moved between more highly covarying housing markets than for those who moved between more independent housing markets. For high covariance households, the effect on housing demand of the varying housing prices they face — due to moving between different markets or from moving at different points in time — would be undone more by the effect on housing demand of their housing wealth.

An example of this intuition is demonstrated in Table 3.1, where we consider the effect on housing demand of the polar cases of perfectly positive (negative) covariance in house
price growth between the origin city, A, and destination city, B. For the sake of the example, we assume that a household’s wealth is entirely made up of their house in city A, and we use as parameters two sets of estimates of the elasticity of housing demand with respect to wealth ($\epsilon_w$) and house prices ($\epsilon_p$).

In the first row of Table 3.1, the household faces 20 percent growth in house prices in city B, which would reduce its demand for housing there. However, in the case of perfect covariance, house prices also grow commensurately in city A, making the household 20 percent wealthier and raising its demand for housing in city B. Under Cobb-Douglas preferences with wealth and own-price demand elasticities of 1 and -1 respectively, the price effect and wealth effect exactly net out and there is no change in housing consumption after the move (in the second-to-last column). Under other plausible estimates of the elasticities, such as from Rosen (1979a) (in the last column), the wealth and price elasticities are not equal and so housing consumption in city B responds to the price change even in the perfect covariance case. In this particular example, the price elasticity dominates the wealth elasticity and housing demand falls by 5 percent. Conversely, in the second row, a 20 percent decline in house prices in city B also implies a 20 percent decline in wealth in the perfect covariance case. This yields either no net effect on housing demand in city B, or a 5 percent increase, depending on the elasticities used.

By contrast, the bottom two rows of Table 1 assume house price growth is perfectly negatively covarying in cities A and B. Thus, a 20 percent rise in house prices in city B is accompanied by a 20 percent decline in house prices in city A, and vice versa. In this case, the price effect and wealth effect work in the same direction. Under Cobb-Douglas parameters, housing demand in city B falls by 40 percent when prices rise in B or grows by 40 percent when prices fall in B. Under the parameters estimated in Rosen (1979a), the

---

3Glaeser and Gyourko (2007) survey estimates of price elasticities of housing demand and find -1.0 to be in the middle of the distribution.
effect on housing demand is $-35$ percent or $+35$ percent, respectively.

The intuition of the paper can be seen by comparing the positive covariance and negative covariance cases. The potential variation in housing demand in city B is much greater for the negative covariance households regardless of which elasticities we use. This result follows from the wealth and price elasticities having opposite signs, so when the covariance is positive, the household’s exposure to volatility in prices in city B is hedged by wealth changes due to co-movements in price in city A. When the covariance is negative, the household suffers from a negative hedge, so that their wealth is lowest precisely when prices in B are highest.

3.2.2 Model setup

To generalize this intuition and provide guidance for the econometric specification, we outline a simple dynamic consumption model. Since our focus is on the relationship between house prices in current and future housing markets, our model encompasses multiple locations with stochastic house prices but abstracts away from other complications. In particular, we ignore transactions costs from moving and keep the decision of when and where to move exogenous and known ex ante; we discuss the implications of endogenous moving in the section on empirics below.

We consider the decisions of an infinitely-lived agent. At the beginning of time period $t$, he moves to city $m_t$ from city $m_{t-1}$. He enters the period with wealth $w_t$ and receives (exogenous and known ex ante) labor income $l_t$, the sum of which he must divide between non-housing consumption ($c_t$), housing investment ($h_t$) and investment in financial assets.

$^4$For example, Davidoff (2006) allows for the covariance of labor income and housing costs; Shore and Sinai (2010) consider the effect of the fixed cost of moving; Sinai and Souleles (2005) endogenize house prices in the origin city; Piazzesi, Schneider and Tuzel (2007) allow for correlations between the returns of housing and other assets; Ortalo-Magne and Prat (2009) endogenize house prices and portfolio returns in a multi-city model; and Davidoff (2010) allows for changes in the marginal utility of housing to be correlated with the marginal utility of long-term care.
(\(s_t\)). There is no rental sector in the model, so the agent must purchase a home in \(m_t\) that costs \(h_t = P^m_t q_t\) for a house of size \(q_t\). For simplicity, we let one unit of housing produce one unit of housing services and define the utility function accordingly. We denote per-period utility over numeraire consumption and housing services by \(u(c_t, q_t)\).

At the end of period \(t\) he receives stochastic returns of \(1 + r_{t+1}\) per dollar invested in financial assets and \(1 + \pi^{m_t}_{t+1} = \frac{P^{m_t}_{t+1}}{P^{m_t}_t}\) per dollar invested in housing\(^5\). These sum to his next-period wealth \(w_{t+1}\), so that

\[
w_{t+1} = q_t P^m_t \left(1 + \pi^{m_t}_{t+1}\right) + s_t \left(1 + r_{t+1}\right)
\]

The dividend portion of the housing return, the rental value, is consumed in-kind by living in the house. House price growth may be correlated — positively or negatively — across markets, but we assume that house price growth rates in all markets are uncorrelated with financial returns\(^6\). We also assume that it is not possible to go long or short either housing market except through the purchase of a home, so investment in housing cannot be divorced from the consumption of housing services.

House prices and financial returns are stochastic, so the agent must form (rational) expectations about the future and maximize over consumption, housing, and financial investment accordingly. He discounts next period’s utility by a factor of \(\beta\). Since the agent has an infinite horizon and stable preferences, we can easily write his problem using the

\(^5\)We do not restrict \(s_t\) to be positive, which allows the agent to borrow to finance housing consumption. For example, if \(s_t\) is negative and \(r_{t+1}\) is known with certainty, the “financial asset” acts like a fixed-rate mortgage. Since households must borrow or lend at the same rate of return and taxes are ignored in the model, there is no reason to borrow to finance the purchase of housing while simultaneously investing in a financial asset. Extending the model to allow for this common behavior does not materially affect the analysis.

\(^6\)Flavin and Yamashita (2002) show that house price growth in the Panel Study of Income Dynamics (PSID) had a correlation with the S&P 500 of nearly zero over the period 1968-1992. We calculate that house price growth rates in more than 90 percent of metropolitan statistical areas have correlations with stock returns of between -0.2 and 0.2.
Bellman equation. He solves

$$V(w_t; P_t, S_t) = \max_c q_t, s_t \{ u(c_t, q_t) + \beta E [V(w_{t+1}; P_{t+1}, S_{t+1}) | P_t, S_t] \} \quad (3.2)$$

subject to the budget constraint

$$c_t + q_t P_t^{m_t} + s_t = w_t + l_t$$

The state space $S_t$ can contain any information available at time $t$ that is useful for predicting future returns. We write $P_t$, the full vector of house prices in all cities at time $t$, separately to emphasize the importance of house prices in our model.

We make a set of standard assumptions about preferences in order to analyze the model. First, we assume that the per-period utility function $u(c_t, q_t)$ is twice continuously differentiable, strictly increasing and strictly concave in both consumption and housing. Consequently, the value function is twice continuously differentiable, strictly increasing and strictly concave, so that the agent is risk-averse with respect to post-move wealth. Second, we assume that $u'_c(c_t, q_t)$, the derivative of the utility function with respect to consumption, is twice differentiable and convex. This implies that the agent is a precautionary saver, meaning that he saves more for post-move consumption in response to an increase in the variance of post-move wealth.\footnote{See Kimball (1990) and citations therein for a full discussion of the mathematics of the precautionary saving motive.}

Under these assumptions, it is straightforward to derive Euler equations that define the agent’s choices of $q_t$, $s_t$ and $c_t$. They are

$$-u'_c(c_t, q_t) P_t^{m_t} + u'_q(c_t, q_t) + \beta E \left[ u'_c(c_{t+1}, q_{t+1}) P_t^{m_t} (1 + \pi_{t+1}) \right] = 0 \quad (3.3)$$
\[-u_c(c_t, q_t) + \beta E[u_c(c_{t+1}, q_{t+1})(1 + r_{t+1})] = 0 \quad (3.4)\]

where letter subscripts denote the derivative of the function with respect to that argument.

### 3.2.3 Variance in the Marshallian Demand for Housing

In this subsection we derive theoretical predictions for the relationship between the co-variance of pre- and post-move house prices and the variance in post-move housing consumption, holding pre-move housing investment, \(q_{t-1}\), and all future decisions fixed.\(^8\) This relationship depends crucially on how \(q_t\) responds to changes in house prices in the origin \((P_{m_{t-1}})\) and destination \((P_{m_t})\). For housing to serve as a hedge, housing demand must be decreasing in destination house price and increasing in wealth (and thus origin house price).

It is possible to use equations 3.3 and 3.4 to derive exact conditions under which these statements are true in our model. We defer the math to Appendix 3.8 because the intuition is clear: Housing demand falls with higher prices, holding future prices constant, as long as housing is not a Giffen good. Demand rises in wealth as long as housing and consumption are complementary — or at least sufficiently non-substitutable — and the return on the financial asset exceeds the monetary return on housing in expectation, so that people are not tempted to over-consume housing just to save. This property of the returns follows from a no-arbitrage condition and the observation that part of the return on housing, the rental value, is consumed in-kind.

We proceed by examining the Marshallian demand function for housing in period \(t\),

\[
q(w_t, P_{m_t}) = \arg \max_{q_t} \{u(c_t, q_t) + \beta E[V(w_{t+1}; P_{t+1}, S_{t+1}) | P_t, S_t]\} \quad (3.5)
\]

\(^8\)We consider the implications of allowing \(q_{t-1}\) to vary in response to changes in variance or covariance in Section 3.2.4.
which is again subject to the budget constraint. Taking a first-order Taylor approximation to \( q(w_t, P_{mt}^m) \) at any point \((\bar{w}, \bar{P})\) yields

\[
\text{Var}[q(w_t, P_{mt}^m)] \\
\approx \text{Var}[q(\bar{w}, \bar{P}) + q_w(\bar{w}, \bar{P})(w_t - \bar{w}) + q_p(\bar{w}, \bar{P})(P_{mt}^m - \bar{P})] \\
= (q_w(\bar{w}, \bar{P}))^2 \text{Var}[w_t] + (q_p(\bar{w}, \bar{P}))^2 \text{Var}[P_{mt}^m] \\
+ 2 (q_w(\bar{w}, \bar{P})) (q_p(\bar{w}, \bar{P})) q_t^{-1} \text{Cov}[P_{mt}^{m_{t-1}}, P_{mt}^m]
\]

(3.6)

The second equality simply applies the definition of \( w_t \) in Equation 3.1 and our assumption that financial returns are uncorrelated with housing returns.

The last two lines of equation 3.6 shows that post-move housing demand has higher variance when variance in wealth is greater (the first term) or when the variance in destination house prices is greater (the second term). Since wealth comprises investments in financial assets and the origin house, greater variance in returns for either asset yields higher variance in the destination housing demand. This occurs because housing demand increases in wealth \((q_w(\cdot) > 0)\) and decreases in price \((q_w(\cdot) < 0)\), so volatility in either wealth or purchase price carries over into variance in housing demand.

Importantly, a higher cross-market price covariance reduces the variance in housing demand, all else equal. Any decrease in housing demand due to higher house prices at the destination are at least partially offset by the greater wealth from the higher sale price on the origin house, and vice versa. Equation 3.6 has immediate empirical implications. Conditional on origin and destination variance in house prices, higher covariance should yield a lower variance of housing demand. In addition, the \( q_t \) term multiplying the covariance term shows that the reduction in variance should be more pronounced for households who owned more housing in the prior period.

It is worth noting that greater covariance should also reduce the variance of non-housing
consumption, thanks to the income effect: Households will have less wealth to spend on non-housing consumption as house prices rise, and vice versa. By the same intuition as that underlying Equation 3.6, this income effect is offset when covariance is higher because origin house prices rise in tandem with destination house prices. This implication contrasts with Chetty and Szeidl (2007), who find that home ownership increases the sensitivity of consumption to income shocks. Our results apply to the variance in consumption around a move; Chetty and Szeidl’s (2007) result holds when the commitment nature of home ownership precludes moving. While the smoothing of non-housing consumption is another important channel by which the covariance hedge can improve welfare, our data do not include information on non-housing consumption, so we will not be able to test it empirically.

### 3.2.4 Endogenizing Initial Housing Consumption

The analytical results above hold when wealth entering period $t$ does not change in response to differences in covariance. However, housing consumption might respond to the expected benefit of home owning as a hedge. Supporting evidence has been found in prior research: Sinai and Souleles (2005) and Sinai and Souleles (2009) find that households are more likely to purchase a house if it is expected to provide a larger hedge, while Han (2008) and Han (2010) find theoretical and suggestive empirical evidence that households purchase more housing in that circumstance.

Our theoretical framework also generates this same marginal effect of covariance on the intensive margin of housing consumption in the period before the move ($t - 1$). One mechanism is that pre-move housing is more valuable when the expected hedge is stronger because it is better at reducing post-move consumption volatility. It is straightforward to show the basis for this intuition using our model of housing investment; the math is provided in Appendix 3.8.
Another possible mechanism for an endogenous response of pre-move housing choices to covariance is due to the precautionary saving motive (Kimball 1990). Households whose houses provide a better hedge and thus are insured against future house price risk may choose to save less, spending more on both housing and non-housing consumption before moving. This suggests that the household will decrease financial saving \( s_{t-1} \) in favor of \( c_{t-1} \) and possibly \( q_{t-1} \).

Regardless of the mechanism, when housing consumption and saving before the move respond to the hedging benefit of homeownership, there is a second channel — other than the direct hedging effect in Equation 3.6 — by which house price covariance can affect the variance of housing consumption in period \( t \). Namely, households who enter period \( t \) with more wealth will experience more variance in consumption, all else equal, since they have more dollars in risky investments. Although this mechanism is not the focus of this paper, we will need to account for it in our empirical analysis. Consequently, we explicitly control for the effect of the expected covariance on \( w_t \). We further explain this approach in Section 3.5.

### 3.3 Data

For our empirical work, we use the 5 percent sample from the 2000 Census Individual Public-Use Microsample (IPUMS) that contains household-level responses to the 2000 U.S. Census long-form questionnaire.\(^9\) We chose this data source because it reports a household’s MSA of residence in 2000 and in 1995, as well as household characteristics and housing spending in 2000.\(^10\) From the two observations on the MSA of residence, we can infer moving between MSAs and subsequently match those moves to covariance.

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\(^9\) Ruggles, Sobek, Alexander, Fitch, Goeken, Hall, King and Ronnander (2004)

\(^10\) An MSA is designed to correspond to a labor market area, and typically contains one or more focal cities and their surrounding suburbs.
in house prices across MSA pairs. Another benefit of the IPUMS data is that it contains enough observations that we can control nonparametrically for unobservable differences among origin or destination MSAs.

For house price data, we take the Federal Housing Finance Agency (FHFA) MSA-level house price indices, deflate by the Consumer Price Index, and peg them to the average house prices reported for each MSA in the 2000 Census. We use this real dollar-valued house price measure to calculate a cross-MSA covariance matrix\footnote{The results are similar, if less well grounded in our theoretical results, if we use covariances of real house price growth.} These indices use repeat sales of houses with conventional mortgages to estimate constant-quality house price indexes for nearly 400 MSAs. Although the FHFA indices begin as early as the mid-1970s for some MSAs, we use the period starting in 1982 since more MSAs are available in the data starting at that time. We end the series in 1999 in order to use data prior to the observations in the 2000 IPUMS\footnote{The results are robust to changes in the horizon of the covariance calculation, such as using the whole period of data availability, from 1982 to 2007.}

In the IPUMS, we use all single-family, one- or zero-couple households that own a home, of which there are approximately 3.3 million in the sample\footnote{This group excludes the few home-owning households with a household head — the first person listed on the Census form — under the age of 25.} Of this group, we keep those who have moved domestically in the last five years and have both their current and previous metropolitan statistical areas of residence identified\footnote{Since migration is reported for individuals and we do not want to explicitly account for household formation, we assume that the origin MSA of the household head is the origin MSA of the household. Unfortunately, the IPUMS does not identify all residents of many metropolitan areas. The IPUMS places the highest priority on identifying Public Use Microdata Areas (PUMAs). When PUMAs cross MSA boundaries, the MSA identifier may be suppressed to maintain maximum confidentiality. Still, most residents of most metropolitan areas are identified as such. See \url{http://usa.ipums.org/usa/volii/incompmetareas.shtml} for complete details.}. This leaves a sample of about 150,000 households, across 284 origin MSAs, 297 destination MSAs, and about 26,000 origin-destination pairs. Since the 17 years of FHFA data are not available for all MSAs identified in the IPUMS, this further limits our sample to about 100,000 households,
156 origin MSAs and 167 destination MSAs. Finally, to mitigate the effects of data reporting errors for house values or transitorily low incomes, we drop the top and bottom 1 percent of the observations based on their self-reported house price to income ratios and exclude any household with a MSA median house price-income ratio of above 10.

The summary statistics provided in Table 3.2 show how the observable characteristics change as the sample size diminishes. Average house value, household income, fraction married, and share college-educated rise, and average age declines. Most of the changes come from restricting the sample to movers and residents of MSAs, who tend to be better-educated and higher-income than rural residents. In the final sample, average house value is about $230,000, household income is nearly $100,000, and more than three-quarters of household heads are married. Figure 3.1 shows the distribution of covariances, standardized to have mean zero and standard deviation one, imputed to our IPUMS households. Most covariances are quite low, reflecting households who move to or from MSAs with low house price variances. However, there is a significant tail of higher covariances, due to households moving between highly correlated high-variance MSAs.\(^{15}\)

### 3.4 Estimation Strategy

Equation 3.6 reflects the notion that when a household faces particular realizations of house price growth in their origin and destination housing markets, the effect on housing consumption in the destination is dampened when house prices in the two markets move together. The hedging effect implies that if households were to draw repeatedly from the distributions of house prices in their origin and destination markets, higher covariance (more hedged) households would experience lower variance of subsequent housing consumption.

\(^{15}\)The highest covariances in our data are among cities in the Northeast and among cities in California. Our results are robust to excluding regional groups of households, such as all those who moved from or to an MSA in California.
It is this relationship between covariance of house prices and the variance of subsequent housing spending, conditional on the distributions of house prices, that we seek to estimate.

One empirical approach would be simply to test whether the change in housing consumption between the purchase of one house in an origin city and another in the destination differs depending on whether the house price growth over that same time period was similar in the origin and destination housing markets or not. That approach would parallel Gruber’s (1997) study of the consumption-smoothing effects of unemployment insurance, which estimated whether the change in consumption around an unemployment shock varied with the generosity of unemployment insurance. However, unlike in other consumption-smoothing research, we do not directly observe the shock — a household’s realization of house price growth. Using the variance in housing consumption will enable us to estimate the difference in response for high- and low-covariance households while controlling nonparametrically for the unobserved distribution of house price shocks.

Our approach is to estimate the conditional variance across households in housing spending in a destination city subsequent to a move and relate that to the covariance in house prices between the origin and destination.\(^\text{16}\) The key identifying assumption is that all households departing the same origin city draw from the same distribution of initial house price shocks. Equation 3.6 refers to a variance in housing consumption taken over a set of possible realizations of house prices for one representative household. Since we observe only one housing choice per household, we estimate the variance of housing consumption across a number of households, each having drawn one realization from a common distribution. We take several steps to make this identifying assumption palatable.

First, we calculate the mean of house values conditional on demographic characteristics, in essence estimating the deviation of realized housing spending from predicted spend-

\(^{16}\text{Although common in time-series and financial econometrics, conditional variance estimation for its own sake is fairly rare in cross-sectional applications. See Shore (2010) for one example. Carroll and Ruppert (1988) provides a useful summary of the literature on estimating conditional variance functions to that date.}
ing. We also condition on fixed effects for the origin and destination MSAs, accounting for differences among them that could affect housing wealth or spending, such as the level of house prices. Second, when we relate the variance of house values to the covariance in house prices, we again condition on household characteristics and origin and destination MSA fixed effects. This controls for the possibility that the demographic composition of movers across MSA pairs could vary in a way that is systematically related to the covariance. It also controls for all households departing an origin MSA having systemically higher variance in wealth, or a destination MSA imposing more variance in housing values on movers, independent of the MSA of origin.

Finally, we can use variation at the household level to relax the assumption of the same (conditional) wealth distribution for all movers from an origin city and instead allow that distribution to vary by MSA pairs. Equation 3.6 predicts that, among households who move between a origin-destination city pair, the effect of covariance on the conditional variance in subsequent housing spending should be largest for households who owned a large quantity of housing before moving. We proxy for the quantity of housing owned with whether a household has a low income relative to house prices in the origin MSA. We also estimate a regression model to fit the quantity of housing owned conditional on a large set of covariates and fixed effects. To test this prediction, we interact these two measures with covariance and estimate the conditional variance with origin and destination fixed effects, as well as origin x destination fixed effects, at the cost of losing our estimate of the main effect of covariance.

Our estimation strategy also addresses another empirical challenge that follows from the fact that households infrequently adjust their housing consumption. Prior consumption-smoothing research has used households’ pre-shock consumption as a measure of their desired consumption. Since households rarely adjust their housing consumption, we cannot easily compute the change in housing consumption by taking the difference across two
years. For most households that difference is zero and, even for households who move, both their prior housing consumption and their new consumption can be quite different from the latent desired amount of housing consumption assumed in the theory (Edin and Englund 1991). In any case, in our data we also do not observe housing consumption prior to moving. Instead, our estimation procedure embeds a prediction of a household’s latent demand for housing.

Using the same notation as in Section 3.2, we assume that our data are generated by the following heteroskedastic model, where $q_{i,t}$ is the value of the house purchased by household $i$ when it moves from $m_{i,t-1}$ to $m_{i,t}$ at time $t$. In addition, $X_i$ is a vector of covariates other than covariance:

$$P^m_{2000}q_{i,t} = \alpha Cov[P_{t}^{m_{i,t-1}}, P_{t}^{m_{i,t}}] + X_i \beta + \eta_i \sqrt{e^{\delta Cov[P_{t}^{m_{i,t-1}}, P_{t}^{m_{i,t}}]+X_i \gamma}}$$

Equation (3.7) emphasizes that self-reported house values, which are what is reported in the Census data, are a measure of the price per unit quantity of housing in 2000 ($P^m_{2000}$)

\[ E[\eta_i | X_i] = 0 \]

\[ Var[\eta_i | X_i] = 1 \]

The specified functional form for the variance is convenient because it both guarantees positive predicted variances and allows us to interpret changes in variance in (approximate) percentage terms. We include the additional covariates to control for cross-sectional differences in household attributes that are correlated with covariance and might affect the quantity of housing purchased.

Equation (3.7) emphasizes that self-reported house values, which are what is reported in the Census data, are a measure of the price per unit quantity of housing in 2000 ($P^m_{2000}$)

\[ 17 \] Housing tenure choice in the origin also is not available in the IPUMS. Among homeowners in the Panel Study of Income Dynamics (PSID) who moved across state lines in the prior year, about 60 percent were previously homeowners. Since households who were renters in the origin would not benefit from a hedge, this data omission should make it more difficult for us to find an effect of covariance on the mean and variance of housing consumption.

\[ 18 \] We examine the functional form further below by estimating a nonlinear model using splines.
multiplied by the quantity purchased when the household moved in year $t$, whereas the theoretical results in Section 3.2 relate only to the quantity of housing consumed. However, since all households in the Census report their house values at approximately the same time, we can assume that the price per unit quantity is the same for all agents in a given destination MSA. Then, when we take the log of the squared residuals in the second stage, the fixed effect for the destination MSA absorb the MSA-level price component. The remaining differences in the log variance of house values must reflect differences in the variance of the quantity owned.

We estimate the conditional variance of house values in two stages. In the first stage, we regress house values ($P_{2000}^{m_i,t}q_{i,t}$) on origin-destination covariance ($\text{Cov} \left[ P_{t}^{m_i,t-1}, P_{t}^{m_i,t} \right]$), full sets of origin and destination fixed effects, and the vector of covariates $X_i$ from equation 3.7.

$$P_{2000}^{m_i,t}q_{i,t} = \alpha \text{Cov} \left[ P_{t}^{m_i,t-1}, P_{t}^{m_i,t} \right] + X_i \beta + \lambda_{m_i,t-1} + \lambda_{m_i,t} + \eta_i \quad (3.8)$$

This regression yields an estimate of the conditional mean of house values as well as conditionally mean-zero residuals ($\hat{\eta}_i$). We then run the second-stage regression

$$(\hat{\eta}_i)^2 = \delta \text{Cov} \left[ P_{t}^{m_i,t-1}, P_{t}^{m_i,t} \right] + X_i \gamma + \rho_{m_i,t-1} + \rho_{m_i,t} + \nu_i \quad (3.9)$$

where $\nu_i$ is an error term defined by $E \left[ \nu_i | \text{Cov} \left[ P_{t}^{m_i,t-1}, P_{t}^{m_i,t} \right], X_i, \rho_{m_i,t-1}, \rho_{m_i,t} \right] = 0$. Equation 3.9 follows from the facts that (1) we can always write a variable as the sum of its conditional expectation plus a conditionally mean-zero error term and (2) we can “plug in” consistent estimate of the first-stage errors (i.e., the residuals) on the left side and still get consistent estimates of $\delta$ and $\gamma$ from equation 3.7.\(^{19}\) We report bootstrapped standard errors for the conditional variance (second stage) estimates.

\(^{19}\)Note that using squared residuals biases the estimate of the conditional variance function, although it remains consistent. This can be corrected by studentization of the residuals (Carroll and Ruppert 1988, p. 78). The correction has virtually no effect on our estimates, so we leave it out to maintain simplicity.
3.4.1 Selection Bias

Our model assumes that all households own their homes and have no control over their migration decisions. A potential critique of our empirical approach is thus that households might change where they choose to move, or whether they buy or rent, based on how well the price of the houses they sold tracked prices in the MSA they moved to. However, it turns out that any potential bias, if anything, will make it harder for us to discern an effect.

We can reasonably reject any concern about differential migration — choosing a destination because of covariance — because it requires that a substantial fraction of households change their migration decisions in response to price swings in the destination or origin. In practice, migration flows are nearly constant from year to year. Using U.S. Internal Revenue Service data on migration from the 1980s to the present, we regressed the logarithm of the number of households moving across an MSA pair in a given year on MSA-pair dummies. The $R^2$ from this regression is about 0.95, leaving only a small set of households that could be affected by annual swings in house prices. If we add to the regression the percentage by which the destination house price is above or below the origin house price, we get an elasticity of about 0.1, which is extremely precisely estimated thanks to the large sample size. Even if we multiply this elasticity by 0.2, which is about the 99th percentile of absolute annual swings in the house price gap, we still get a change in the number of migrants across a given city pair of only 2 percent. An average house price swing would shift migration by less than 0.5 percent.

A second potential issue could arise if households who experienced low covariances are less likely to buy a house after moving and thus do not show up in our sample of home owners. This pattern is precisely what we find in section 3.5.3 below. That said, any selection bias from the endogeneity of the tenure decision most likely leads us to underestimate the

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20This would require the price to deviate from the present value of renting. One possibility is that the user cost relationship is not constant. Alternatively, the household may have a short horizon, as in Sinai and Souleles (2005) or Campbell and Cocco (2007).
effect of covariance on the variance in housing spending.

Consider again the empirical model in Equation 3.7 that relates house value to cross-market house price covariance. Let $H_{i,t}$ be an indicator variable for whether household $i$ purchases a home after moving to location $m_{i,t}$. We observe this household’s desired housing purchase only if $H_{i,t} = 1$. Suppose that $Pr[H_{i,t} = 1]$ depends on the same set of covariates, including fixed effects, as $q_{i,t}$, so that

$$H_{i,t} = 1 \left[ \alpha' \text{Cov}(P_{m_{i,t}}^{m_{i,t}-1}, P_{m_{i,t}}^{m_{i,t}}) + X_i \beta' + \phi_i > 0 \right]$$

$$\text{corr} (\eta_i, \phi_i) \neq 0$$

This is similar to a standard selection model, where the conditional probability of owning and conditional housing consumption can be modeled as correlated random variables. Assuming that the correlation is positive — so households who have an unobservable taste for home owning also desire relatively more expensive houses — the conditional distribution of house prices is probabilistically truncated from the left. That is, households from the low end of the house price distribution are more likely to opt in and become homeowners when the covariance rises. This biases estimates of both the conditional mean and conditional variance.

If $\eta_i$ is conditionally normally distributed, then the lesser probabilistic truncation as covariance rises leads to an increase in the conditional variance of house values. This result is straightforward to show for the case in which $\eta_i$ and $\phi_i$ are conditionally bivariate normal, although it does not hold for all distributions in general. Since the distribution of home values in our sample, conditional on covariates, does appear to be approximately normal, any selection should bias our estimates toward

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21 To actually estimate a selection-corrected version of the model that is not identified solely from functional form assumptions would require an instrument that affects the probability of owning but not the demand for housing.
3.5 Results

3.5.1 Conditional Variance Estimates

We present our results in two parts. The first section relates the conditional variance of house values to the covariance between origin and destination MSA pairs, while the second examines the possibility that the covariance hedge might also make it easier for households to buy a house, rather than rent, in the destination.

In Table 3.3, we report our main result: The conditional variance of house values among households who move between more highly covarying MSAs is lower, even controlling for a wide set of covariates in both the housing demand stage and the variance stage. The estimates reported in this table are from the second stage of the conditional variance estimation laid out in Section 3.4, which relates the conditional variance in house values across households who move between an MSA pair to the covariance in house prices of the two MSAs.

In the first column of Table 3.3 we estimate the log variance of house prices conditional on origin-destination covariance and full sets of origin and destination dummy variables, but no household-level covariates. We standardize the covariates so the estimated coefficients can be interpreted as the marginal effects of a one-standard deviation change in the covariate. Our estimate in column (1) is that a one standard-deviation increase in cross-market covariance reduces the variance of destination house values by about 17 percent, with a standard error of 2.4 percent.

By including origin and destination dummies, we control for any origin- or destination-specific differences in the variance of house values that might be correlated with covariance.
In essence, we are comparing households who move to MSA B from an MSA that does not covary with it to those who move to MSA B from an MSA that does covary with it. Any factors that are specific to B are absorbed by the destination fixed effects. Likewise, we simultaneously compare households who move from MSA A to either high or low covariance destinations. Factors specific to A are absorbed by the origin fixed effects.

For example, if people who move from San Francisco have more variable (unobserved) wealth, it will be picked up by the San Francisco dummy and will not contaminate our estimates. Similarly, if the New York MSA happens to have a wider variety of house values than other MSAs, the New York fixed effect will absorb that. Instead, we rely on households who move from San Francisco to more highly covarying MSAs having lower variance of house values at their destinations than other households who moved out of San Francisco, and households who move to New York from more highly covarying markets having a lower variance of house values in New York than other households who moved to New York.

The same covariates in the second stage are also included in the first stage regression. The origin and destination fixed effects pick up differences in the mean house value across MSAs as well as average differences in housing demand among movers to and from each MSA. Meanwhile, the covariance term in the first stage picks up differences in the conditional mean that are correlated with covariance.

Column (2) repeats the conditional variance estimation, this time adding controls for household characteristics such as family size and the age, sex, citizenship status, race, education, and marital status of the household head. These covariates serve two purposes. In the first stage, the estimation of the conditional mean, the covariates control for differences in latent housing demand that are functions of observable household characteristics, the characteristics of the origin and destination cities, and the effect of covariance. In the second stage, the covariates control for differences in the composition of movers that might...
be correlated with the variance of housing demand. For example, if highly-educated households had more or less variability in housing demand and were more or less likely to move between covarying MSAs, our estimate would be biased in the absence of the controls. With the addition of the household controls, the estimated covariance coefficient shrinks to -0.127, with a standard error of 0.019.

In column (3) we add current household income and its square to the set of covariates, as proxies for lifetime income. While current income is probably endogenously determined by households, we include it to make sure that our estimated relationship between covariance and variance of house values does not simply reflect sorting of households of different incomes into different locations. The coefficient of interest shrinks in magnitude, to about 10 percent, but remains quite precisely estimated. We also find that the volatility of housing demand increases with current income for most households, although the quadratic term captures the fact that it eventually declines at very high levels of income.

In column (4), we attempt to isolate the direct hedging effect of covariance. As discussed in Section 3.2.4, one potential confounding factor is that households may alter their initial consumption and investment choices in response to their anticipated covariances. That can induce an independent, second-order effect on the amount or volatility of wealth a household faces when purchasing a home after a move. It is worth emphasizing that this is not a statistical bias per se: Our approach does estimate the effect of covariance on the variance of housing demand. Rather, in column (4) we are interested in seeing how much of the effect is due directly to the hedging property of home owning versus the other channels by which covariance might operate.

To decompose these mechanisms, we make use of the notion that pre-move housing demand and savings decisions are based on an *ex ante* expected covariance of house prices between the origin city and all MSAs the household might move to. But *ex post* housing demand, after the move, depends only on the covariance of house prices between the
origin MSA and the MSA the household ended up moving to. By including the *ex ante* expected covariance as a covariate, we can control for the effect of the non-hedge channels on housing demand and the volatility of wealth, while still identifying the hedging mechanism through our usual covariance variable.

Following Sinai and Souleles (2009), we compute the expected covariance for each household as the weighted average covariance from the origin MSA to all other MSAs, where the weights are the imputed probability that household moves between each MSA pair. Using their city of origin and the industry of employment of the household head, we calculate the household’s probability of moving to each possible destination city as the rate of moving in each MSA pair x industry cell in the IPUMS. We then construct a weighted covariance using these probabilities as weights, under the assumption that households expectation of the probability of moving to a given city is the same as the actual probability for their origin-industry cell\(^2\) In column (4), we see that controlling for the indirect mechanisms barely changes our estimated effect of covariance (to -0.110) and that the estimated coefficient on expected covariance is not statistically distinguishable from zero at any conventional level of significance.

Estimating a linear model of the effect of covariance in house prices on the variance of housing spending masks the fact that there is a larger effect at high covariances than at low covariances. We estimate the nonlinear relationship using a generalized additive model (GAM) that specifies that the conditional mean of the dependent variable comprises the sum of a set of nonlinear functions, one for each covariate. The procedure estimates splines that “penalize” likelihood function for additional degrees of freedom; this helps to avoid over-fitting (Hastie and Tibshirani 1990, Wood 2006). Wood’s (2006) recent technical innovation allows for the estimation of a GAM using automated cross validation methods

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22Separating the expected covariance from the realized covariance relies on the assumption that households’ true expected covariances are better proxied by our weighted average measure than the covariance they realize across their actual origin and destination.
to choose the penalty parameters for the spline.

We estimate a model that includes age and income as continuous covariates, using splines, as well as the usual fixed effects from the models above. As in our more parametric versions, we estimate the model in two steps, first fitting the mean as a function of these covariates and then running another GAM with the log squared residuals on the left side and the same covariates on the right. The curve relating covariance and the variance of post-move housing spending, along with a 95 percent confidence interval, is shown in Figure 3.2. For covariances below the mean, which is standardized to zero, the curve is less steeply sloped than for those above the mean. On average, the slope above the mean is about -0.14, while the slope below it is just -0.06. Because of the long right tail of the distribution, about two-thirds of the sample have covariances below the mean. Households with a covariance in the 99th percentile of our sample have 40 percent less variance in destination housing spending than comparable households with little or no covariance between origin and destination. This nonlinearity is not surprising since our theoretical result, Equation 3.6, prescribes a linear effect only through the use of a Taylor approximation.

### 3.5.2 Within City Pair Identification

The identification in Table 3.3 requires that there are no unobservable differences in the variance of housing demand among movers between MSA pairs that happen to be correlated with differences in the covariance in house prices between the MSAs in the pair, conditional on origin and destination MSA fixed effects. We can relax that requirement by making use of equation 3.6, which predicts that covariance should decrease volatility by the most when the household owned more housing in the origin.

Empirically, we can compare the variance among movers who were likely to own large

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23To focus the plots on the dense part of the covariance distribution, we drop the top and bottom 1 percent of our sample by covariance. This has little effect on the estimated curves, precisely because they are estimated flexibly.
houses to the variance among those who were likely to own smaller houses to see if covariance has a larger dampening effect for the former group. A simple way to test for this effect is to follow Equation 3.6 literally and interact covariance with factors that shift \( q_{t-1} \). We try two strategies: First, we interact covariance with the (standardized) ratio of household income to origin median house price, since higher-income households or those in lower-priced areas should own more housing, all else equal.\(^{24}\) Second, we run a regression of log house values on a set of household covariates and location fixed effects and predict the quantity of housing a household was expected to own in the origin given its particular covariate values and fixed effect. We likewise interact this predicted housing consumption variable with covariance and include it in the conditional variance estimates.

One caveat to this approach is that households with different income or wealth should also have different marginal elasticities of demand \((q_P(\bar{w}, \bar{P}) \text{ and } q_w(\bar{w}, \bar{P}))\) in Equation 3.6. For example, richer households should be less impacted by a one-dollar increase in house prices. Differences in these elasticities would also lead to different effects of covariance, since they multiply the covariance term. That said, we expect any changes in marginal demand elasticities to be second-order relative to changes in actual demand.

In columns (1) and (2) of Table 3.4, we find that higher covariance in house prices between an MSA pair reduces the variance in house values more for households who have high incomes relative to origin house prices or high predicted \( q_{t-1} \). In column (1), we see that a one standard deviation increase in income increases the effect of covariance by about half relative to the mean, with a coefficient of -0.076 on the interaction term. The controls include the full set of household demographics, origin and destination MSA fixed effects, and our proxy for the ex ante expected covariance, paralleling column (4) of Table 3.3. The interaction is highly statistically significant, since the standard error is just 0.012. Similarly,

\(^{24}\)In principle we would like to use pre-move incomes or wealth, but these are not reported in our data. Consequently, we use post-move income as a proxy for pre-move income.
in column (2) a one standard deviation increase in predicted house size substantially and statistically significantly amplifies the effect of covariance, by 4.1 percentage points. These results match our predictions based on Equation 3.6.

Since both income and predicted $q_t$ vary at the household level, we can also include MSA-pair fixed effects to control for unobservable differences across MSA pairs and test whether covariance has a larger dampening effect for high $q_{t-1}$ movers within a given MSA pair. This comes at the cost of not being able to estimate the main effect of covariance, since it varies only by MSA pair. The estimated effects in columns (3) and (4) are smaller than in (1) and (2), respectively. The coefficient on the interaction with income declines by about half (-0.029) but remains statistically significant at the 5 percent level. In column (4), we find only a very small negative effect of predicted house size on the effect of covariance on housing consumption variance; this coefficient is appreciably smaller than its standard error. Nonetheless, taken as a whole the evidence supports the idea that households who we expect had larger houses before moving have bigger reductions in variance in the destination.

### 3.5.3 Ex Post Probability of Owning

Covariance could also affect households’ tenure choice in the destination, as discussed in Section 3.4.1. In this subsection, we test the tenure choice/covariance relationship by estimating a linear probability model, with an indicator for whether a household owns or rents its house as the dependent variable, and the same sets of covariates as in the conditional variance estimates.\(^{25}\) The estimation sample contains approximately twice as many households as our sample of homeowners.

\(^{25}\)We have also estimated probit models; the results are very similar. We prefer the linear probability model because fixed effect probit estimates are not necessarily consistent when the number of observations within each group are fixed. This is the “incidental parameters” problem. Another alternative candidate model, fixed effects logit, can only accommodate a single set of fixed effects, where we have several. Including additional sets of dummy variables reintroduces the issue of incidental parameters (Wooldridge 2002, pp. 491-492).
The baseline results are reported in Table 3.5. In column (1) the only controls are origin and destination fixed effects. We find that households who faced higher covariances — that is, had a better hedge against destination house prices — are substantially more likely to own their homes after moving. A one standard deviation increase in covariance raises the probability of owning by about 3.0 percentage points (with a standard error of 0.3 percent). In our sample, the average homeownership rate is 50 percent, so 3 percentage points corresponds to a 6 percent increase.\footnote{The homeownership rate in the selected sample of inter-MSA migrants is substantially lower than the national homeownership rate because renters are much more likely to move than homeowners.}

Columns (2) through (4) sequentially add the full vector of household controls that were used previously in Table 3.3. The estimated coefficient on covariance is not affected much, and ranges from 0.030 to 0.025.

Table 3.6 repeats the strategy from Table 3.4 of interacting covariance with factors that shift pre-move housing quantity \( q_{t-1} \). With or without origin x destination fixed effects, we find small and statistically insignificant effects of the income-origin price ratio on the covariance hedging effect. Large predicted house sizes do have an effect, with a one standard deviation increase in predicted \( q_{t-1} \) increasing the effect of covariance by about 1 percentage point, with a standard error of .2 percentage points. All told, the covariance hedge appears to work not only by reducing the variance of subsequent housing consumption for home owners but also by increasing the probability that a household will be able to afford a home at the destination.

### 3.6 Magnitudes

In this subsection, we demonstrate the scale of the covariance hedge by computing the predicted reduction in variance across several groups and parts of the covariance distribution. We find that the hedge is strongest for households who are likely to own larger homes and households who move cities at the high end of the covariance distribution. The first col-
umn of Table 3.7 uses our estimates to calculate the percentage effect of a one standard deviation increase in covariance on post-move housing consumption variance. In addition to the average effect across our entire sample, taken from column (4) of Table 3.3, we also calculate the effect for households with high or low incomes relative to prices in the origin, using column (1) of Table 3.4. Households in the high-income group, which we define as having an income-origin price ratio at the 90th percentile, were more likely to own a larger home before moving and thus get a larger average benefit from higher covariance, of about 18 percent for each standard deviation. Conversely, the low-income group, at the 10th percentile of the income-origin price ratio, gets just a 7 percent reduction.

Since a standard deviation of covariance is not an especially intuitive measure, it is perhaps more useful to compare the strength of the hedge at different points in the covariance distribution. Because the distribution has a long right tail, as seen in Figure 3.1, similar percentile increases in covariance have a larger effect at the top of the distribution than at the bottom. For example, for the average household, moving between cities at the 5th percentile of covariance versus a city pair at the median reduces variance by just 6 percent. Moving between the 95th percentile city pair versus the median, on the other hand, reduces variance by 24 percent. Much of this effect is concentrated at the top of the distribution: A household with a covariance at the 95th percentile gets an 18 percent reduction in variance relative to a household at the 75th percentile.

The same pattern holds for the high- and low-income groups. The strongest predicted covariance hedge is for high income households at the top of the covariance distribution, who have just 40 percent the variance in post-move consumption relative to high income households at the median of covariance. Meanwhile, low-income households in the lower half of the covariance distribution get much less benefit from the hedge. A household with

27For ease of explanation, we do not use the nonlinear estimates from the GAM described above and shown in Figure 3.2. The patterns described here would be even more stark if we did.
low income relative to prices in their origin city who moves between cities at the median of covariance experiences just 4 percent lower variance than a household who moves between cities at the 5th percentile.

### 3.7 Conclusion

In this paper, we examine the empirical link between cross-market house price covariance and variance in subsequent housing consumption. Theory suggests that higher covariance should hedge the volatility of housing consumption since changes in home owners’ wealth would offset changes in the costliness of housing. Prior research has demonstrated that households respond prospectively to a potential hedge by being more likely to own their houses and to spend more on housing when the potential hedge is stronger. This paper shows that the hedge works.

Empirically identifying whether home ownership successfully reduces the volatility of housing consumption is challenging because adjusting housing consumption is a low-frequency event, and in our data we observe neither the shock to housing costs nor the household’s latent housing demand. We surmount these difficulties by applying a conditional variance estimation technique that is novel in the consumption smoothing literature. In essence, we use the variance of housing spending across a cross-section of households as an estimate of the variance of housing spending that a single household would experience across different states of the world.

This strategy works because we can condition on household level observable and MSA-level unobservable characteristics, so each household varies only by the (unobserved) shock to housing costs. Differences in the variance of housing spending among movers to a destination can thus be related to differences faced by those households in the covariance in house prices between their origins and destinations. We also examine whether the variance
of housing consumption for households who move between a given pair of MSAs responds more to covariance for those households that theory predicts would be more sensitive to it, that is, households who were likely to own larger homes (and thus have a bigger hedge) before they moved.

Our estimates show that home ownership significantly reduces the variance in housing spending for households that move between covarying MSAs. A one standard deviation increase in covariance, holding all else constant, reduces the average variance of housing spending by 10 to 17 percent, depending on the specification. This average estimate masks considerable nonlinearity and heterogeneity across groups. Allowing the estimated coefficient to vary nonlinearly with the level of covariance by using a generalized additive model, we find that for households with covariance above the mean, a one standard deviation change in covariance would reduce the variance of housing spending by 14 percent whereas households with below-mean covariance enjoy just a 6 percent reduction. The effect is especially sizable for wealthy households (20 percent) as well as those who are particularly likely to have owned a large home before moving.

We find additional evidence consistent with the model. On average, households who face higher covariances tend to spend less on housing after a move because of the convexity of the housing demand function. They also are more likely to purchase a house in the destination since they are better protected against unexpected changes in house prices.

Finally, we show that the hedge can be especially valuable for certain households, particularly those who own large homes and move across cities whose prices covary strongly. We find that for such households, covariance can reduce the variance of post-move housing spending by more than 40 percent relative to otherwise identical households who move across cities with covariance at the median. The variance in housing consumption is reduced even more because greater covariance raises the odds that a household can afford to own a home after a move. Conversely, the hedging benefit is weakest for low-income
households, who do not own much if any housing, and households who move across city
pairs that do not covary much.

This natural hedge provided by home owning can help explain some facts that the con-
ventional wisdom finds surprising. For example, the measured marginal propensity to con-
sume out of housing capital gains might be low, as found by Calomiris, Longhofer and
Miles (2009), Attanasio, Blow, Hamilton and Leicester (2009) and Campbell and Cocco
(2007), because increases in housing wealth are spent on commensurately higher housing
costs. As another example, while insurance markets have arisen to mitigate most other
major sources of consumption uncertainty — health care, long-term care, or even col-
lege tuition costs — markets to insure against house price uncertainty have not taken off
(Shiller 2008). Our results suggest that simply owning a house provides valuable insurance
against housing costs in future cities, obviating some of the need for a separate financial
product. Finally, higher covariance in house prices may mitigate not only changes in hous-
ing consumption after a move, but changes in non-housing consumption as well.

3.8 Mathematical Appendix

We first demonstrate that \( q_w (c_t, q_t) = \frac{\partial q_t}{\partial w_t} \) is positive under standard assumptions, namely
that housing and consumption are complimentary or at least sufficiently non-substitutable
and that the return on the financial asset \( r_{t+1} \) exceeds the return on housing \( \pi_{t+1} \) in expec-
tation. Denote the Euler equations 3.3 and 3.4 by \( A \) and \( B \), respectively. Totally differenti-
tiating the Euler equations and applying Cramer’s rule indicates that we can determine the
sign of \( \frac{\partial q_t}{\partial w_t} \) by examining the sign of \( -A_w B_s + A_s B_w \), where \( A_w \) is the partial of the first
Euler equation with respect to \( w_t \), and so on.
\[ \text{sign} \left( \frac{\partial q_t}{\partial w_t} \right) = \text{sign} \left( -A_w B_s + A_s B_w \right) \]

\[ = \text{sign} \left( u_{cc} (c_t, q_t) P_t^{mt} \right) \cdot \beta E \left[ u_{cc} (c_{t+1}, q_{t+1}) \left( (1 + r_{t+1}) \left( r_{t+1} - \pi_{t+1}^{mt} \right) \right) \right] \]

\[ - u_{qc} (c_t, q_t) \beta E \left[ u_{cc} (c_{t+1}, q_{t+1}) (1 + r_{t+1})^2 \right] \]

Since \( u_{cc} (\cdot) \) is negative by assumption, the first compound term is positive so long as \( E \left[ r_{t+1} - \pi_{t+1}^{mt} \right] > 0 \), which must be true under a no-arbitrage condition since part of the return on housing is consumed in-kind as a service flow. The second term must simply be sufficiently non-negative so as not to exceed the first term, which will be true as long as \( u_{qc} \) is positive — housing and consumption are complementary — or sufficiently non-negative.

Next, we show that \( q_P (c_t, q_t) = \frac{\partial q_t}{\partial P_t^{mt}} \) is negative, holding future prices constant. Following the same procedure as above, we totally differentiate the Euler equations and apply Cramer’s rule to yield

\[ \text{sign} \left( \frac{\partial q_t}{\partial P_t^{mt}} \right) = \text{sign} \left( -A_P B_s + A_s B_P \right) \]

\[ = \text{sign} \left( -u_{cc} (c_t, q_t) q_t P_t^{mt} \right) \]

\[ \cdot \beta E \left[ u_{cc} (c_{t+1}, q_{t+1}) \left( (1 + r_{t+1}) \left( r_{t+1} - \pi_{t+1}^{mt} \right) \right) \right] \]

\[ + u_{qc} (c_t, q_t) q_t \beta E \left[ u_{cc} (c_{t+1}, q_{t+1}) (1 + r_{t+1})^2 \right] \]

\[ + u_c (c_t, q_t) \left( u_{cc} (c_t, q_t) + \beta E \left[ u_{cc} (c_{t+1}, q_{t+1}) (1 + r_{t+1})^2 \right] \right) \]

This expression is quite similar to the previous one, except it has the opposite sign and includes an additional term that incorporates \( u_c (c_t, q_t) \), the first derivative of the utility function with respect to consumption. This final term is unambiguously negative, so the

\[ ^{28} \text{It is natural to hold future prices constant because a transitory increase in price today should be accompanied by a decrease in the expected return.} \]
requirements for housing demand to be decreasing in current prices are even weaker than for it to be increasing in wealth. This is not surprising, since it would be quite strange if the model implied that housing were a Giffen good.

Finally, we derive the conditions under which \( \frac{\partial q_t}{\partial \text{Cov}[P^m_{t+1},P^{m+1}_{t+1}]} \) is positive or negative. This is key to understanding how pre-move housing purchases endogenously respond to expected covariance between pre- and post-move house prices. Again totally differentiating the Euler equations and applying Cramer’s rule, we get

\[
sign\left( \frac{\partial q_t}{\partial \text{Cov}[P^m_{t+1},P^{m+1}_{t+1}]} \right) = sign \left( -A_{Cov}B_s + A_sB_{Cov} \right) \quad (3.10)
\]

Since \( B_s = u_{cc}(c_{t+1},q_{t+1}) + \beta E \left[ u_{cc}(c_{t+1},q_{t+1}) (1 + r_{t+1})^2 \right] \) is negative, the sign of the first term in Equation 3.10 depends on the sign of \( A_{Cov} \), the partial derivative of the first Euler equation with respect to covariance. This in turn depends on the cross-partial of \( u_c(c_{t+1},q_{t+1}) P^m_{t+1} \) with respect to pre- and post-move house prices; if this cross-partial is positive, then the expectation of the expression is increasing in covariance.

\[
\frac{\partial^2 u_c(c_{t+1},q_{t+1}) P^m_{t+1}}{\partial P^m_t \partial P^m_{t+1}} = -u_{cc}(c_{t+1},q_{t+1}) q_{t+1} - u_{ccc}(c_{t+1},q_{t+1}) q_t q_{t+1}
\]

The first term here is positive, while the second is negative if the agent is a precautionary saver. This illustrates the tension between the direct effect of covariance on the riskiness of housing investment and the indirect effect via the precautionary saving motive. If the direct effect dominates, than \( A_{Cov} \) is positive and the first term on the right side in Equation 3.10 is positive.

Turning to the second term on the right side of Equation 3.10 \( A_s B_{Cov} \), we can see that \( A_s \) is negative as long as housing and consumption are not too substitutable. So the sign of
this expression depends on $B_{Cov}$, which in turn depends on whether

$$\frac{\partial^2 u_c (c_{t+1}, q_{t+1})}{\partial P_{\bar{m}t} \partial P_{\bar{m}t+1}} = -u_{ecc} (c_{t+1}, q_{t+1}) q_t q_{t+1}$$

is positive or negative. If the agent is a precautionary saver, the preceding cross-partial is negative, implying that the expectation of the expression is decreasing in covariance, so that $B_{Cov}$ is negative. Importantly, this tends to offset the precautionary saving channel from the first expression of the right side of Equation 3.10, which confirms that the direct effect of higher covariance making housing less risky is likely to dominate. This means pre-move housing investment is increasing in covariance, which is in accord with the findings of Sinai and Souleles (2009), Han (2008), and Han (2010).
Figure 3.1: Covariance Histogram

Histogram of Standardized Covariance at Household Level

Histogram of covariance at the household level, excluding the top and bottom 1 percent. Covariance is standardized to have mean zero and standard deviation one.
Solid curve is a penalized regression spline. The regression includes the full set of discrete household covariates detailed in the text, as well as splines in income, age and (prior) expected covariance. Dashed curves show the 95% confidence interval. Covariance is standardized to have mean zero and standard deviation one.
Table 3.1: A Highly Stylized Example

<table>
<thead>
<tr>
<th>Case</th>
<th>If house prices in city B changed by:</th>
<th>...house prices in city A must have changed by:</th>
<th>...and housing consumption in period 2 would change by:</th>
</tr>
</thead>
<tbody>
<tr>
<td>Perfect covariance</td>
<td>+20%</td>
<td>+20%</td>
<td>0%</td>
</tr>
<tr>
<td></td>
<td>−20%</td>
<td>−20%</td>
<td>−5%</td>
</tr>
<tr>
<td>Perfect negative covariance</td>
<td>+20%</td>
<td>−20%</td>
<td>−40%</td>
</tr>
<tr>
<td></td>
<td>−20%</td>
<td>+20%</td>
<td>+40%</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>+35%</td>
</tr>
</tbody>
</table>

Notes: Households move from City A to City B. \( \epsilon_w \) and \( \epsilon_p \) are the wealth and destination house price elasticites of housing demand, respectively. See text for further details.

 Relative to consumption with no price changes.
### Table 3.2: Summary Statistics, By Sample

<table>
<thead>
<tr>
<th></th>
<th>All Homeowners Mean</th>
<th>Migrants Mean</th>
<th>Estimation Sample Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>House value</td>
<td>147,173</td>
<td>207,043</td>
<td>227,795</td>
<td>175,591</td>
</tr>
<tr>
<td>Household income</td>
<td>64,277</td>
<td>87,397</td>
<td>96,425</td>
<td>82,870</td>
</tr>
<tr>
<td>Age of head</td>
<td>53.76</td>
<td>45.72</td>
<td>44.97</td>
<td>13.75</td>
</tr>
<tr>
<td>Female head</td>
<td>0.28</td>
<td>0.23</td>
<td>0.22</td>
<td>0.42</td>
</tr>
<tr>
<td>Married</td>
<td>0.67</td>
<td>0.75</td>
<td>0.77</td>
<td>0.42</td>
</tr>
<tr>
<td>Family size</td>
<td>2.60</td>
<td>2.75</td>
<td>2.82</td>
<td>1.40</td>
</tr>
<tr>
<td>Black</td>
<td>0.07</td>
<td>0.06</td>
<td>0.06</td>
<td>0.24</td>
</tr>
<tr>
<td>College-educated</td>
<td>0.33</td>
<td>0.58</td>
<td>0.61</td>
<td>0.49</td>
</tr>
<tr>
<td>Covariance (levels)</td>
<td></td>
<td></td>
<td>324 mil.</td>
<td>584 mil.</td>
</tr>
<tr>
<td>Covariance (% changes)</td>
<td></td>
<td></td>
<td>0.0019</td>
<td>0.0028</td>
</tr>
<tr>
<td>Income / median origin price</td>
<td></td>
<td></td>
<td>0.6604</td>
<td>0.633</td>
</tr>
<tr>
<td>N</td>
<td>3,330,743</td>
<td>146,012</td>
<td>100,851</td>
<td>181</td>
</tr>
</tbody>
</table>

Summary statistics for progressively smaller subsets of the 2000 Census PUMS. Individual-specific data are reported for the first person listed on the Census form, whom we call the “head”. “Migrants” includes only those households who reported moving across MSAs in the past five years. The “Estimation Sample” drops all households for whom we do not have covariance or other data, or who are severe outliers with respect to the reported price-income ratio. See text for further details.
Table 3.3: Baseline Percentage Effect of Covariance on Housing Expenditure Variance

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariance (standardized)</td>
<td>-0.173</td>
<td>-0.127</td>
<td>-0.103</td>
<td>-0.110</td>
</tr>
<tr>
<td>Household income ($1000’s)</td>
<td></td>
<td>0.011</td>
<td>0.011</td>
<td></td>
</tr>
<tr>
<td>Household income -squared</td>
<td></td>
<td>-0.000009</td>
<td>-0.000009</td>
<td></td>
</tr>
<tr>
<td>Exp. Covariance (standardized)</td>
<td></td>
<td></td>
<td>0.023</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>100851</td>
<td>100851</td>
<td>100851</td>
<td>100851</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.099</td>
<td>0.135</td>
<td>0.162</td>
<td>0.162</td>
</tr>
</tbody>
</table>

Origin FE
Destination FE
Household controls

Standard errors are bootstrapped by origin x destination cluster using 500 replications to account for two-step estimation of conditional variance. Covariance and expected covariance are standardized to have mean zero and standard deviation one. Expected covariance is imputed based on origin and industry. Household controls include age and age-squared as well as indicator variables for sex of household head, family size, marital status, citizenship, race, English language abilities, and education.
Table 3.4: Interacted Percentage Effect of Covariance on Housing Expenditure Variance

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariance (standardized)</td>
<td>-0.125</td>
<td>-0.134</td>
<td>(0.020)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Covariance x Income / Origin Price</td>
<td>-0.076</td>
<td>-0.029</td>
<td>(0.012)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Covariance x Predicted House Size</td>
<td>-0.041</td>
<td>-0.005</td>
<td>(0.010)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Observations</td>
<td>100851</td>
<td>100851</td>
<td>96350</td>
<td>96350</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.161</td>
<td>0.161</td>
<td>0.280</td>
<td>0.284</td>
</tr>
<tr>
<td>Origin FE</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Destination FE</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Orig.*Dest. FE</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Household controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Exp. Cov. control</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Income controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Standard errors are bootstrapped by origin x destination cluster using 500 replications to account for two-step estimation of conditional variance. Covariance is standardized to have mean zero and standard deviation one. Predicted house size is the fitted value from a regression of log house price on a full set of household covariates. Income-origin price ratio and predicted house size are standardized to have mean zero and standard deviation one in the interaction terms. Expected covariance is imputed based on origin and industry. Household controls include age and age-squared as well as indicator variables for sex of household head, family size, marital status, citizenship, race, English language abilities, and education. Income controls comprise a linear and a quadratic term.
Table 3.5: Baseline Effect of Covariance on Ex Post Probability of Owning

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariance (standardized)</td>
<td>0.030</td>
<td>0.027</td>
<td>0.026</td>
<td>0.025</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Household income ($1000's)</td>
<td>0.003</td>
<td>0.003</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Household income -squared</td>
<td></td>
<td>-0.000005</td>
<td>-0.000005</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.000000)</td>
<td>(0.000000)</td>
<td></td>
</tr>
<tr>
<td>Exp. Covariance (standardized)</td>
<td></td>
<td></td>
<td>0.009</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.004)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>207472</td>
<td>207472</td>
<td>207472</td>
<td>199297</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.057</td>
<td>0.267</td>
<td>0.298</td>
<td>0.297</td>
</tr>
<tr>
<td>Origin FE</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Destination FE</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Household controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
</tbody>
</table>

Linear probability model. Standard errors clustered at the origin x destination level. Covariance and expected covariance are standardized to have mean zero and standard deviation one. Expected covariance is imputed based on origin and industry. Household controls include age and age-squared as well as indicator variables for sex of household head, family size, marital status, citizenship, race, English language abilities, and education.
Table 3.6: Interacted Effect of Covariance on Ex Post Probability of Owning

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Covariance</td>
<td>0.025</td>
<td>0.025</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(standardized)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Covariance x Income / Origin Price</td>
<td>0.001</td>
<td>0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Covariance x Predicted House Size</td>
<td>0.010</td>
<td>0.011</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>199297</td>
<td>199297</td>
<td>199297</td>
<td>199297</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.297</td>
<td>0.297</td>
<td>0.364</td>
<td>0.365</td>
</tr>
<tr>
<td>Origin FE</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Destination FE</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Orig.*Dest. FE</td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Household controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Exp. Cov. control</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Income controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Standard errors clustered at the origin x destination level. Covariance is standardized to have mean zero and standard deviation one. Predicted house size is the fitted value from a regression of log house price on a full set of household covariates. Income-origin price ratio and predicted house size are standardized to have mean zero and standard deviation one in the interaction terms. Expected covariance is imputed based on origin and industry. Household controls include age and age-squared as well as indicator variables for sex of household head, family size, marital status, citizenship, race, English language abilities, and education. Income controls comprise a linear and a quadratic term.
Table 3.7: Magnitudes

<table>
<thead>
<tr>
<th></th>
<th>Effect of 1 SD Increase in Covariance</th>
<th>Effect of Shift Across Percentiles of Covariance Distribution</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>5% → 50%</td>
</tr>
<tr>
<td>Average Household</td>
<td>-10%</td>
<td>-6%</td>
</tr>
<tr>
<td>High Income Households</td>
<td>-18%</td>
<td>-10%</td>
</tr>
<tr>
<td>Low Income Households</td>
<td>-7%</td>
<td>-4%</td>
</tr>
</tbody>
</table>

We define the high- and low-income groups as households near the 90th and 10th percentiles of household income divided by origin median price, as a proxy for quantity of housing owned in the origin. The difference in the effect of covariance in the income groups comes from the income-origin price ratio interaction term in column (1) of Table 3.4. See text for further details.
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