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Essays in Household Finance and Housing Economics

Abstract
With the wake of the United States financial crisis in 2008, policymakers and academics have begun to reevaluate the nature and impact of household financial decisions. While standard economic theory assumes individuals are fully rational, the devastation of the crisis suggests households may be subject to systematic biases that can have significant effects on the economy. Chapter 1 asks whether consumer sentiment has an impact on asset prices, particularly during the boom and bust of housing prices that instigated the most recent financial crisis. Empirically identifying a link between sentiment and prices is challenging, however, as measures of investor beliefs are difficult to construct. This paper develops the first measures of sentiment across local housing markets by quantifying the tone in local housing newspaper articles. The sentiment index forecasts both the boom and bust of housing prices by more than two years, and can predict over 70 percent of the variation in national housing prices above and beyond economic fundamentals. Chapter 2 then asks whether households can time the own versus rent decision successfully and generate profitable savings. Using 29 years of historical data, this essay creates robust measures of the costs of owning and renting and evaluates whether owning or renting was less expensive ex-post across 39 metropolitan areas in the United States. We find that households can potentially time their homeownership profitably and can save as much as 50 percent of annual rent costs using a few simple trading rules. Chapter 3 addresses whether the lack of household financial literacy has significant consequences for household wealth. We find that an overwhelming majority of households lack basic financial skills and that financial literacy appears to have a significant effect on wealth above and beyond other observed factors. Our results suggest that improving financial literacy could have large positive effects on wealth accumulation.

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ESSAYS IN HOUSEHOLD FINANCE AND HOUSING ECONOMICS

Cindy K. Soo

A DISSERTATION

in

Applied Economics

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

Degree of Doctor of Philosophy

2013

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To Mom and Dad,

who have provided me with incredible support and love.

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ABSTRACT

ESSAYS IN HOUSEHOLD FINANCE AND HOUSING ECONOMICS

Cindy K. Soo

Olivia S. Mitchell

With the wake of the United States financial crisis in 2008, policymakers and academics have begun to reevaluate the nature and impact of household financial decisions. While standard economic theory assumes individuals are fully rational, the devastation of the crisis suggests households may be subject to systematic biases that can have significant effects on the economy. Chapter 1 asks whether consumer sentiment has an impact on asset prices, particularly during the boom and bust of housing prices that instigated the most recent financial crisis. Empirically identifying a link between sentiment and prices is challenging, however, as measures of investor beliefs are difficult to construct. This paper develops the first measures of sentiment across local housing markets by quantifying the tone in local housing newspaper articles. The sentiment index forecasts both the boom and bust of housing prices by more than two years, and can predict over 70 percent of the variation in national housing prices above and beyond economic fundamentals. Chapter 2 then asks whether households can time the own versus rent decision successfully and generate profitable savings. Using 29 years of historical data, this essay creates robust measures of the costs of owning and renting and evaluates whether owning or renting was less expensive ex-post across 39 metropolitan areas in the United States. We find that households can potentially time their homeownership profitably and can save as much as 50 percent of annual rent costs using a few simple trading rules. Chapter 3 addresses whether the lack of household financial literacy has significant consequences for household wealth. We find that an overwhelming majority of households lack basic financial skills and that financial literacy appears to have a significant effect on wealth above and beyond other observed factors. Our results suggest that improving financial literacy could have large positive effects on wealth accumulation.

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CHAPTER 1

QUANTIFYING ANIMAL SPIRITS: NEWS MEDIA AND SENTIMENT IN THE HOUSING MARKET*

Cindy K. Soo

The Wharton School, University of Pennsylvania

Abstract

Sentiment or "animal spirits" has long been posited as an important determinant of asset prices, but measures of sentiment are difficult to construct and often confounded by asset fundamentals. This paper provides a first empirical test of the role of sentiment in the run-up and crash of housing prices that instigated the great financial crisis of 2008. I develop the first measures of sentiment across local housing markets by quantifying the positive and negative tone of housing news in local newspaper articles. I find that my housing sentiment index forecasts the boom and bust pattern of house prices at a two year lead, and can predict over 70 percent of the variation in aggregate house price growth. Consistent with theories of investor sentiment, I find that my sentiment index not only predicts price variation but also patterns in trading volume. Estimated effects of sentiment are robust to an extensive list of observed controls including lagged fundamentals, lagged price growth, subprime lending patterns, and news content over typically unobserved variables. To address potential bias from latent fundamentals, I develop instruments from a subset of weekend and narrative articles that newspapers use to cater to sentiment but are plausibly exogenous to news on fundamentals. Estimates remain robust to instrumental variable estimation, suggesting bias from unobserved fundamentals is minimal.

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1 Introduction

Sentiment, broadly defined as the psychology behind investor beliefs, has long been posited as an important determinant of asset price variation (Keynes (1936); Shiller (1990); Kothari and Shanken (1997); Baker and Wurgler (2002); Shiller (2005)). However, identifying an empirical link between sentiment and prices presents two major challenges. First, beliefs are by definition unobservable and therefore not straightforward to quantify. Second, it is difficult to separate effects of sentiment from underlying economic fundamentals. If fundamentals jointly determine sentiment and asset prices, then an empirical correlation between a proxy for sentiment and prices may reflect effects from latent fundamentals rather than the role of sentiment (Baker and Wurgler (2006)).

The goal of this paper is to quantify the role of investor sentiment in asset price formation and address both of these challenges in novel ways. I use the run-up and crash of U.S. housing prices from 2000 to 2011 as my laboratory to examine the role of sentiment. This is an important and useful setting for several reasons. First, housing is a significant sector of the economy. Over two-thirds of U.S households own a home and invest the majority of their portfolio in real estate (Tracy et al. (1999); Nakajima (2005)). The housing crash also greatly impacted the financial sector, as banks and financial institutions held significant investments in mortgage-backed securities and other housing related assets. Second, the housing market provides greater power for identifying potential effects of sentiment. Unlike the stock market, which is dominated by large institutional investors, housing is primarily traded by individual buyers who are likely more subject to sentiment. Finally, the recent housing cycle is an important setting to examine the effect of sentiment because standard economic explanations for the housing boom have so far been difficult to reconcile empirically. Observed fundamentals that accounted for nearly 70 percent of the variation in national house price growth from 1987 to 2000, explain less than 10 percent of the variation from 2000 to 2011 (Lai and Van Order (2010)). While there was much discussion of the potential role of sentiment, empirical evidence of this theory has been limited and largely anecdotal.

This paper provides the first measures and empirical test of sentiment in the housing market. I measure sentiment by capturing the qualitative tone of housing news from local newspapers. Specifically, I calculate the difference between the share of positive and negative words across newspaper articles each month. I construct sentiment indices corresponding to each of the 20 city markets covered by the Case-Shiller home price index. This methodology builds on work from Tetlock (2007) and a growing number of studies that construct proxies for sentiment in the stock market with media coverage. This strategy is also motivated by literature on asset price bubbles that claims the media reflects sentiment through an incentive to cater to readers' preferences over a particular asset (Kindleberger (1978); Galbraith (1990); Shiller (2005)). I present a simple theoretical model that formalizes these arguments and illustrates how news media may relate to sentiment.

I find that my sentiment index forecasts the boom and bust trend of housing prices by more than a two year lead. Figure 1 shows that aggregate sentiment increases rapidly and peaks in 2004, well before the peak of national house prices in mid-2006. This pattern is also evident across cities. Cities that experienced dramatic rises and declines in house prices are preceded by similar cycles in sentiment, whereas cities with milder price changes are led by more subdued sentiment growth. Furthermore, I find that my sentiment measure can explain over 70 percent additional variation in national house price movements above and beyond observed fundamentals. This is significant as prior studies have found standard fundamental determinants to account for only a limited fraction of house price variation after 2000.[1] Nonetheless, interpreting these effects as sentiment is limited without a validation of media sentiment as a reflection of investor beliefs.

External validations of sentiment proxies are naturally difficult to provide since investor beliefs are unobservable (Baker et al. (2012a)). In this paper, I validate my measure of sentiment by comparing it with surveys of investor expectations in the housing market. I find that my sentiment measure is highly correlated with housing market confidence indexes from the Survey of Consumers and the National Association of Home Builders. In particular, home buyer survey confidence also peaks in 2004, reflecting similar timing to trends in my composite index. Case et al. (2012) implement annual surveys of home buyer expectations and similarly find that long term expectations peak in 2004, well ahead of house prices. These surveys are otherwise limited in frequency and geographic scope, but reaffirm the overall time-varying trends in my sentiment indices.

[1] For example, Glaeser et al. (2010) find that lower real interest rates can explain only one-fifth of the rise in house prices from 1996 to 2006. He et al. (2012) examine the role of liquidity in the housing boom, and find that their model can account for approximately one-fifth of house price run up from 1996 to 2006.

Still, all of these measures may potentially capture variation in fundamentals. I first address this by controlling for an exhaustive sequence of fundamental determinants of house prices. I find that the predictive power of sentiment on house prices not only remains robust in significance, but also in magnitude. The stability of the estimates suggests that bias from unobservable factors is less likely. I find that estimates also remain stable to the inclusion of additional controls for subprime lending trends. While not considered a typical housing fundamental, subprime credit exhibited unprecedented expansion with the growth of house prices in many cities (Mian and Sufi (2009); Demyanyk and Van Hemert (2011); Goetzmann et al. (2012)). The richness of my news dataset also allows me to control for the content of news articles directly. News may report on harder-to-quantify fundamentals that I do not observe. Thus, I control for the share of positive minus negative words in any article that directly mentions a fundamental in its text and find that this does not affect my results. Furthermore, I find that sentiment not only predicts house price variation but also patterns in transaction volume. This result is consistent with existing theories and empirical studies of investor sentiment (Odean (1998, 1999); Scheinkman and Xiong (2003); Barber and Odean (2000, 2008)). Interestingly, sentiment leads volume first and is followed by prices another year later. This evidence supports a hypothesis that search frictions in the housing market likely induce lags between changes in sentiment, housing transactions, and prices.

While these results are highly suggestive, the positive association between my sentiment index and house prices may still be driven by latent fundamentals. I present two candidate instruments for sentiment by isolating a subset of housing news articles that cater to reader sentiment but are plausibly exogenous to news on fundamentals. The first is my measure of sentiment calculated only over housing articles published over the weekend. Weekend articles tend to cater to readers who have preferences for lighter content, and are arguably exogenous to news on fundamentals since official press releases on economic data can only occur on a weekday. The second proposed instrument is my measure of sentiment calculated only over narrative housing news articles. Narratives cater to sentiment through a human interest appeal, and are plausibly exogenous to fundamentals because they consist of anecdotal stories rather than actual information. Of course, the validity of these instruments relies on the assumption that information on fundamentals is not being reported on or somehow related through these subset of news articles. I acknowledge and test for a number of possible violations of this assumption, and find that results are consistent with the exclusion restriction. Given this, I show that the predictive power of sentiment remains robust both in significance and magnitude even after instrumenting for sentiment.

This paper provides evidence that sentiment may have a significant effect on house prices, and challenges standard explanations of the housing boom and bust that rely solely on fundamentals. The results of this paper suggest that if a fundamental drove house prices during this period, then it would also have had to drive expectations at a two year lead to prices both nationally and across cities. Furthermore, to be consistent with the empirical data, this fundamental would fail to explain prices from 1987 to 2000 but suddenly begin to drive expectations and prices differently from 2000 to 2011. This paper does not advocate that fundamentals did not play any role, but that the evidence suggests sentiment played an economically important role as well.

These findings complement a number of empirical studies that attempt to quantify sentiment and provide evidence for its effect on asset prices (Edmans et al. (2007); Baker and Wurgler (2006, 2007); Baker et al. (2012a); Baker and Stein (2004); Greenwood and Nagel (2009); Barber et al. (2009); Brown and Cliff (2005)). At the same time, the evidence in this paper relates to a large body of work that explores determinants and consequences of the last housing boom and bust (Piskorski et al. (2010); Avery and Brevoort (2010); Haughwout et al. (2011); Bhutta (2009); Bayer et al. (2011); Glaeser et al. (2008); Gerardi et al. (2008); Ho and Pennington-Cross (2008)). This paper also generally relates to a larger literature that explores housing price dynamics and more specifically to studies that explore the role of expectations in the housing market (Genesove and Mayer (2001); Piazzesi and Schneider (2009); Goetzmann et al. (2012); Arce and López-Salido (2011); Burnside et al. (2011); Favilukis et al. (2010)). Finally, this paper contributes to research that links media coverage to trading activity and shows that media sentiment can be used to predict asset prices beyond stock market applications (Tetlock (2007); Tetlock et al. (2008); Tetlock (2011); Antweiler and Frank (2004); Barber and Loeffler (1993); Dougal et al. (2012); Dyck and Zingales (2003); Engelberg (2008); Engelberg and Parsons (2011); Garcia (2012); Gurun and Butler (2012)).

Section 2 presents a model that describes the relationship between news, sentiment, and prices. Section 3 describes how I construct my database of newspaper articles and set of observed fundamentals. Section 4 details how the sentiment index is calculated. Section 5 and 6 present the main empirical and instrumental variable results respectively. Section 7 concludes and discusses potential avenues for future work.

2 Theoretical Motivation

In this section, I present a simple theoretical framework that illustrates the potential relationship between the news media, investor sentiment, and housing prices. I specifically measure sentiment with news because prominent literature on bubbles and panics commonly stress that the news media has an important relationship with investor beliefs (Kindleberger (1978); Galbraith (1990); Shiller (2005)). They argue that newspapers have a demand-side incentive to cater to reader preferences, and will spin news according to readers' opinion over assets they own. Economic models of media slant make similar arguments in the context of readers' political preferences. Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006) assume that readers have a disutility for news that is inconsistent with their beliefs, citing psychology literature that show people have a tendency to favor information that confirms their priors.[2] Indeed, Gentzkow and Shapiro (2010) find empirical evidence that readers have a preference for news consistent with their beliefs and news outlets respond accordingly. This framework adapts models of investor sentiment (De Long et al. (1990a); Copeland (1976); Hong and Stein (1999)) and models of media slant (Gentzkow and Shapiro (2010); Mullainathan and Shleifer (2005)) to show how news relates to investor sentiment and asset prices.
Agents: I assume there are two types of agents in the economy: fully rational traders and imperfectly rational optimists that have a preference for news that confirms their priors. Agents are otherwise identical in utility maximization and risk aversion parameters. In each period t, the fraction of optimistic traders are present in the economy each period at measure μ_t, and fully rational agents are present in the economy at measure $(1-\mu_t)$. All agents have constant absolute risk aversion where γ denotes the common coefficient of risk aversion. Thus, the allocation to the risky asset is unaffected by the accumulation of wealth. For simplicity, I assume there is no consumption decision, no labor

[2] This tendency is called confirmatory bias in the psychology literature (Lord (1979); Yariv (2002)).

supply decision, and no bequest. The resources agents have to invest are completely exogenous. In each period, agents choose an optimal allocation of housing, X_t , to maximize the following:

$$\max_{H_t} E[-e^{-2\gamma W_{t+1}}]$$

subject to the budget constraint:

$$W_{t+1} = W_t(1 + r_f(1-\tau)) + X_t[P_{t+1} + D_{t+1} - P_t(\delta_t + m_t + (1-\tau_t)(1 + r_f + \pi_t)]$$

where W_t represents wealth in period t. Agents allocate wealth between a risk-free asset that guarantees a risk-free rate of $r_f > 0$ each period and a risky asset of housing that pays dividends, D_t, in the form of housing services each period. Housing is in supply quantity Q_t each period, and the risk-free asset is in perfectly elastic supply. The price of housing stock is denoted by P_t. I assume housing depreciates at rate δ_t, requires maintenance and repairs at a fraction of house value m_t, and incurs property tax liabilities at rate π_t. Furthermore, all investors must pay a marginal income tax of τ_t, but may deduct property taxes from taxable income and otherwise borrow or lend at the risk-free rate r_f. This represents the user cost of housing as formalized by Poterba (1984). For ease of notation going forward, let $\omega_t = \delta_t + m_t + (1-\tau_t)(1 + r_f + \pi_t)$.

Maximizing expected utility over X_t yields the following optimal demand function for housing:[3]

$$X_t = \frac{EP_{t+1} + D_{t+1} - P_t\omega_t}{2\gamma E\sigma^2_{P_{t+1}}}. \tag{1}$$

Since this is just a linear demand function, for simplicity let the above be represented by:[4]

$$X_t = \alpha_t - \omega P_t \tag{2}$$

[3]With normally distributed returns, maximizing the above is the same as maximizing mean-variance utility. I rewrite the agents problem such that they maximize the following expected utility each period: $EU = E[W_{t+1}] - \gamma\sigma^2_{W_{t+1}} = W_t(1+r_f)(1-\tau_t) + X_t[E_tP_{t+1} + D_{t+1} - \omega_tP_t] - X_t\gamma E_t\sigma^2_{P_{t+1}}$, where $\sigma^2_{W_{t+1}}$ is the one-period ahead variance of wealth and σ^2_{Pt+1} is the one period ahead variance of price. This follows the set up in De Long et al. (1990a).

[4] where $\alpha = \frac{EP_{t+1}+D_{t+1}}{2\gamma E\sigma^2_{P_{t+1}}}$ and $\omega = \frac{\omega_t}{2\gamma E\sigma^2_{P_{t+1}}}$.

Rational traders demand housing according to equation (1), but I assume optimists overestimate the expected price of housing relative to rational traders by an additional positive parameter θ.[5] Thus relative to rational traders, optimists shift their demand curves upward by an additional θ.

$$X_t^{Opt} = \alpha_t + \theta - \omega P_t \tag{3}$$

Newspapers: I also assume that optimistic investors have a preference for news that confirms their positive beliefs. Gentzkow and Shapiro (2007) model this preference by assuming readers have a quadratic disutility for news that conflicts with their priors, and derive an equation for newspaper readership approximately equal to $a - (S_n - S_i)^2$ where a is a constant, S_{nt} is slant reported by newspaper n, and S_{it} is the overall level of sentiment in city i and period t. In this framework, the overall level of sentiment in the economy is equal to the fraction of optimists, μ_t, multiplied by their level of optimism, θ. Thus, $S_{it} = \mu_t\theta$, and the optimal level of news slant that maximizes a newspaper's readership is equal to:

$$S_{nt}^* = S_{it} = \mu_t\theta \tag{4}$$

Thus news slant, or the sentiment in news, directly reflects the overall level of reader sentiment.

Equilibrium Price: Given the presence of μ_t optimists and $(1-\mu_t)$ rational traders, equilibrium is characterized by setting demand equal to supply, $(1-\mu_t)(\alpha - \omega P_t) + \mu_t(a + \theta - \omega P_t) = Q_t$. Thus the equilibrium price equals:

$$P_t = \frac{(\alpha_t + \mu_t\theta - Q_t)}{\omega} \tag{5}$$

Equation 5 reveals that investor sentiment has a positive association with prices ($\frac{dP_t)}{d\mu_t\theta} > 0$). Using equation 4, we can rewrite equation 5 in terms of news sentiment:

$$P_t = \frac{(\alpha_t + S_{nt}^* - Q_t)}{\omega} \tag{6}$$

Then the price change from t to $t+1$ can be expressed by:

[5] Conversely, this framework could also apply to a set of pessimists who underestimate the expected price of housing by a negative parameter θ.

$$\triangle P_{t+1} = \frac{1}{\omega}[(\triangle \alpha_{t+1}) + (\triangle S^*_{nt+1}) - (\triangle Q_{t+1})] \tag{7}$$

where $\triangle P_{t+1} = P_{t+1} - P_t$. Thus Equation (7) predicts that changes in news sentiment ($\triangle S^*_{nt+1}$) are positively associated with changes in prices (P_{t+1}). Positive fundamentals such as dividends, D_t, will also drive prices up, while increasing costs and housing stock will have dampening effect on prices. If there are no optimists in the market ($\mu_t = 0$) or sentiment remains unchanged, then prices will equal $P_t = \frac{(\alpha - Q_t)}{\omega}$ and are only moved by changes in fundamentals and rational expectations in α, β, and Q_t.

Examining the effect of sentiment in the housing market allows me to analyze not only the time-varying effects of sentiment but also the cross-sectional effect of sentiment across different local housing markets. Let $\triangle P_{it} = P_{it} - P_{it-1}$ be the change in prices in city i and $\triangle P_{jt}$ represent the changing prices in city j. The difference in house price changes across cities can be written as:

$$\triangle P_{it} - \triangle P_{jt} = \frac{1}{\omega}[(\triangle \alpha_{it} - \triangle \alpha_{jt}) + (\triangle S^*_{i,nt} - \triangle S^*_{j,nt}) - (\triangle Q_{it} - \triangle Q_{jt})] \tag{8}$$

Equation 8 shows that if the price increase from $t-1$ to t is greater in city i than in city j, then this is due to either a greater increase in components in $\triangle \alpha_{it}$ or in investor sentiment (proxied by news sentiment $\triangle S^*_{i,nt}$).

Trading Volume. Increasing sentiment driven by the rising demand from optimists in the economy has further implications for trading volume in each housing market. Suppose the fraction of optimists increases from t to $t+1$ such that $\mu_{t+1} > \mu_t$. Trading volume, V_{t+1}, is then equal to the additional demand for housing from the fraction of optimists period to period:[6]

$$\begin{aligned} V_{t+1} &= \mu_{t+1} X^{Opt}_{t+1} - \mu_t X^{Opt}_t \\ &= \frac{1}{\omega}(S_{nt+1} - S_{nt})(\alpha - Q) \end{aligned} \tag{9}$$

Equation 9 illustrates that as sentiment increases, trading volume will be pushed upward. The greater the demand from optimists is relative to the previous period, the greater the volume of

[6] I assume that α and Q stay constant here to make the effect of sentiment clear.

trades. This framework predicts that positive changes in sentiment should lead to increases in trading volume.

Lagged Effect. The above framework assumes that news only reflects investor sentiment. However, Shiller (2005) argues that news media can simultaneously fuel sentiment if readers misperceive optimism in the news for real information about fundamentals the housing market. Housing, in particular, is a widely held household investment by individual buyers. Thus the average housing investor is likely less financially sophisticated than the typical stock market investor. Survey evidence shows that a majority of Americans do suffer from surprisingly low levels of financial literacy (Lusardi and Mitchell (2007a,b)). Even more sophisticated investors may find it difficult to process quantitative data on market fundamentals. Indeed, Engelberg (2008) provides empirical evidence from earnings announcements that qualitative information on positive fundamentals is especially difficult to process. News slant can make it difficult for readers to separate true information from sentiment, and can subsequently affect trading behaviors. Empirical studies on political media slant show that the media has been able to shift public opinion and voting behavior (DellaVigna and Kaplan (2007); Gerber et al. (2009)). Engelberg and Parsons (2011) show that different local media coverage of the stock market drives different trading outcomes across markets. If this is the case, then news sentiment in period t can also drive investor sentiment in future periods, $\mu_{t+1}\theta$, and prices would be positively associated with both contemporaneous and lagged values of news sentiment, S_{nt} and S_{nt-k}.

Furthermore, this framework also assumes that transactions in the housing market are immediate and costless. The transaction process of buying a home is by no means immediate, and the search process for a home can actually take several months. Thus there can be several lags between a change in sentiment and its effect on prices, and potentially no contemporaneous effect at all. If news slant does feed sentiment, then this can also take some time to diffuse and spread across investors.[7] Thus I consider the effect of both contemporaneous and lagged effects of sentiment in my empirical estimations.

[7] Hong and Stein (1999)model a gradual diffusion of news where only a fraction of traders receive innovations about dividends in each period.

3 Data Description

3.1 Newspaper Articles

My approach to measuring sentiment requires the text of newspaper articles covering the housing market. My source for news articles is Factiva.com, a comprehensive online database of newspapers.[8] Factiva categorizes its articles by subject, and provides a code that identifies articles that discuss local real estate markets. This code is determined by a propriety algorithm that remains objective across all newspapers and years. This subject code covers new and existing home sales, housing affordability indices, and housing price indices as well as supply side indicators on housing starts, building permits, housing approvals, and construction spending. Routine real estate property listings are not included. Wire-service articles are also generally excluded, as syndicated stories cannot be redistributed and typically do not appear in the Factiva database. This exclusion is actually preferable to capturing the *local* sentiment unique to each city. Wire-service articles are typically those that cover topics of more general national interest, supplied to local newspapers by large media companies such as the Associated Press. Excluding such articles ensures each city's sentiment measure is only based on news articles written by local staff writers. To that end, I also exclude any additional republished or duplicate news stories from other news outlets.[9]

I download all newspaper articles covering the housing market between January 2000 and August 2011 from the major newspaper publication in each of the following 20 cities: Atlanta, Boston, Charlotte, Chicago, Cleveland, Dallas, Denver, Detroit, Las Vegas, Los Angeles, Miami, Minneapolis, New York, Phoenix, Portland, San Francisco, San Diego, Seattle, Tampa, and Washington, D.C. I retrieve a total of 19,620 articles.

I then apply a second automated script to parse information from each article. I not only extract the text of the articles, but also useful information on the the date, headline, author, section, and copyright. My database contains each individual word of an article with its corresponding date,

[8] Other similar newspaper databases are Lexis Nexis and NewsBank. Factiva.com arguably has the most comprehensive coverage.

[9] I do not, however, exclude stories that are written by local staff writers but may comment on the housing market of other cities. While an article may comment on other cities, publication of these articles may be in response to a *local* interest in reading housing news. In a follow up paper, I provide evidence that suggests news mentions of other cities is a mechanism through which a contagion of sentiment is spread.

word position, author, and originating newspaper. My final dataset consists of a total 15,295,393 words. I then implement a final script that produces counts of positive and negative words and total words across housing articles by city and month.

Table 1 summarizes some descriptive statistics on the collected articles by city. Most cities have one major newspaper that dominates the news market, with the exception of Boston, Detroit, and Los Angeles, which have two. Some Associated Press articles remain in the sample, but make up less than 6 percent of the collected articles. Approximately 20 percent of the articles are found in the front or "A" section of the newspapers. Additionally, 20 percent are found in a special real estate section. Furthermore, over 30 percent of the articles are published in local news or regional editions of the newspaper. Otherwise, the majority of articles are reported in a general news or business section.

3.2 Housing Fundamentals and Additional Variables

The goal of this paper is to identify an effect of sentiment on house prices. However if housing market fundamentals also affect my news sentiment proxy, then estimating an effect of sentiment on house prices will suffer from omitted variable bias. In particular, a positive shock to fundamentals may simultaneously drive both sentiment and prices upward, biasing coefficient estimates upward. Thus, controlling for these fundamentals is key to identification. Since the true model of house prices is unknown, I apply a "kitchen sink" approach and assemble as many housing market inputs and ouputs that may account for the variation in house prices.

Rents. The "fundamental value" of an asset typically refers to its present discounted value of future cash flow. As noted in Section 2, the model assumes housing pays dividends in the form of rental services. I acquire measures of monthly rents from two sources: REIS and the Bureau of Labor Statistics (BLS). REIS provides average asking rents on rental units with common characteristics with single family homes. REIS reports monthly data on actual rental values which I normalize to match price indexes (100=January 2000). I also obtain residential rents from the Consumer Price Index Housing Survey implemented by the BLS. The BLS reports rents of primary residences as a part of the shelter component of the consumer price index. I include the BLS measure of rents as a robustness check and report the results using REIS rental indices.

Supply. I measure changes in housing supply using data on building permits and housing starts for the U.S. Census Bureau. Housing starts are the total new privately owned housing units started each month. Building permits are those authorized for new privately owned housing units in each city. I also include a measure of supply elasticity developed by Saiz (2010) with the Wharton Residential Land Use Regulatory Index (WRLURI) created by Gyourko et al. (2008).

Employment and Unemployment. A number of models highlight the importance of labor market variables on housing demand (Roback (1982); Rosen (1979); Nakajima (2011); Mankiw and Weil (1989)). I attain monthly employment levels and local unemployment rates by city from the BLS. I also test various measures of employment such as civilian labor force, or employment rates by particular sector, age, and industry.

Population and Income. I attain measures of income and population growth by city from the Bureau of Economic Analysis (BEA). I also use income data on loan applicants from the Home Mortgage Disclosure Act (HMDA). HMDA requires lending institutions file reports on all mortgage applications, and thus provides an exceptional profile of the pool of potential home buyers.

Interest Rates. A large focus of the debate over the housing crisis has been on the role of low real interest rates and availability of easy credit. Theory shows that low interest rates should lead to increased housing demand and higher prices (Himmelberg et al. (2005); Mayer and Sinai (2009); Taylor (2009)). I include measures of both real and nominal interest rates relevant to home buyers. I use the national 30-year conventional mortgage rate from the Federal Reserve Board. Following Himmelberg et al. (2005), I calculate real interest rates by subtracting the Livingston Survey 10-year expected inflation rate from the 10-year Treasury bond rate. The standard user cost formula of housing suggests a 10-year rate, rather than a short-term rate, is more sensible when approximating the duration of mortgages. I also include measures of the the 10-year treasury bill rate and the 6-month London Interbank Offered Rate (LIBOR).

Subprime Lending and Leverage. Studies also hypothesize that the availability of credit should boost housing demand and prices are likely more sensitive in cities where homeowners are highly leveraged (Stein (1995); Lamont and Stein (1999)). Thus, I attain loan-to-value ratios come from a comprehensive new micro dataset provided by DataQuick[10], an industry data provider (Ferreira et al. (2010)). DataQuick provides detailed transaction level data on over 23 million arms length housing transaction from 1993 to 2009. Loan-to-value ratios include the total amount of mortgage debt including not only the primary but also any debt up to three loans taken to finance the home. This dataset covers transactions cover 16 cities in my sample. I also use the percent of subprime mortgages as calculated by Ferreira and Gyourko (2012). The share of subprime loans in a city is the share of loans issued by any of the top twenty subprime lenders ranked by the publication *Inside Mortgage Finance*.

Housing Prices and Volume. I measure home prices for each city from 2000 to 2011 with monthly indexes calculated by Standard & Poor's/Case-Shiller home price index. I use their composite-20 home price index to measure aggregate prices. The S&P/Case-Shiller price indices estimate price changes with repeat sales to control for the changing quality of houses being sold through time. The overall average price index over all twenty cities is 147.3, with the highest, 280.9, occurring in Miami December 2006 and the lowest hitting 67.68 in Detroit the March of 2010. The Case-Shiller Composite 20 index aggregates prices of all 20 major metropolitan areas into composite index and has a slightly higher mean of 157.2 with less variance over time. As a further robustness check, I also test quarterly home price indices calculated by the Federal Housing Finance Agency (FHFA). Since DataQuick covers transaction level data across cities, I also calculate the volume of transactions as an additional dependent variable. This dataset covers transactions for most of cities in my sample and is available monthly.

4 Measuring Sentiment in the News

4.1 Textual Analysis of News Articles

I capture news sentiment through a textual analysis of newspaper articles. Textual analysis is a increasingly popular methodology used to quantify the tone and sentiment in financial documents.[11] For example, a number of finance and accounting studies have applied textual analysis techniques

[10]Data provided by DataQuick Information Systems, Inc. www.dataquick.com.

[11]Alternative labels for textual analysis are content analysis, natural language processing, or information retrieval.

to capture the tone of earnings announcements, investor chat rooms, corporate 10-K reports, IPO prospectuses, and newspaper articles (Engelberg (2008); Antweiler and Frank (2004); Li (2006); Loughran and Mcdonald (2011); Tetlock (2007); Jegadeesh and Wu (2011); Hanley and Hoberg (2010); Kothari et al. (2009); Feldman and Segal (2008); Henry (2008)). Many of these papers have linked the sentiment of these documents to outcomes such as firm earnings, stock returns, and trading volume. Tetlock (2007), one of the most well known of these papers, quantifies the negative tone of the popular Wall Street Journal newspaper column "Abreast the Market." His results support the tone of news as as robust proxy for stock market sentiment.

I apply the most standard methodology employed by this literature, which quantifies the raw frequency of positive and negative words in a text. These papers typically identify words as positive or negative based on an external word list. External word lists are preferred because they are predetermined and less vulnerable to subjectivity from the author. A number of previous papers start with general positive or negative word lists provided by *Harvard IV-4 Psychological Dictionary*. Existing studies have found, however, that these general tonal lists can contain irrelevant words and lead to noisy measures (Tetlock et al. (2008)). For example, Engelberg (2008) points out words on the general Harvard positive list such as *company* or *shares* have limited relevance in capturing positive tone and can unintentionally capture other effects in finance applications. Indeed, several papers have specifically found limited use for the general Harvard positive list (Tetlock (2007); Engelberg (2008); Kothari et al. (2009)). A recent study by Loughran and Mcdonald (2011) shows that the noise introduced by the general Harvard negative word list can also be substantial and argues that word lists should be discipline-specific to reduce measurement error.

To balance these concerns, I still use a predetermined list from the Harvard IV-4 dictionary to reduce subjectivity, but choose one that specifically reflects how the media spins excitement over asset markets. Shiller (2008) asserts that "the media weave stories around price movements, and when those movements are upward, the media tend to embellish and legitimize 'new era' stories with extra attention and detail." He argues that the media employs superlatives that emphasize price increases and upward movements. For example, a news article may describe markets as "*skyrocketing,*" "*soaring*," "*booming*" or "*heating up*." For this reason, I use the Harvard IV-4 lists *Increase* and *Rise*, words associated with increasing outlook and rising movement.[12] Nonetheless, these lists still include a few words such as *people* and *renaissance* that are clearly irrelevant and would result in obvious misclassifications. I manually remove these words, but simultaneously expand the remaining words with their dictionary synonyms.[13] For example, *skyrocket* is a synonym of *soar*, but not included in the original Harvard lists. I exclude synonyms that correspond to an alternative definition of the original word. Following Loughran and Mcdonald (2011), I also expand the list with inflections and tenses that retain the original meaning of each word. Thus counts for the root word *skyrocket*, for example, also include *skyrockets, skyrocketed,* and *skyrocketing*. The original Harvard IV-4 lists include 136 words and the expanded list, including inflections and synonyms, contains 403 words. Table 2 reports a sample of positive words and their corresponding word counts. I repeat the above process to create negative word lists using the converse Harvard IV-4 lists *Decrease* and *Fall*.

4.2 Calculating the Sentiment Index

Using an automated script, I generate counts of positive words by city and month. I calculate the fraction of positive words in city i and month t by simply dividing the number of positive words by the total number of words each month. The share of positive words is represented by:

$$Pos_{it} = \frac{\#positivewords}{\#totalwords}_{it} \tag{10}$$

An alternative method is to calculate the share of positive words in each individual article and then average across articles; I try both methods and they do not make a difference in values. To be conservative, I focus my analysis and report my results based on the leading text of an article. An article may intend to express a negative tone with the first half of its text, but contain a number of positive words in the latter half. Thus, tabulating word counts over the full text can potentially overestimate the share of positive words. Nevertheless, the share of positive words based on the full text of the articles is highly correlated with the share based on the leading text.

Still, positive words in a text may be simultaneously surrounded by a number of negative

[12] These lists can be found at http://www.wjh.harvard.edu/∼inquirer/Increas.html and http://www.wjh.harvard.edu/∼inquirer/rise.html.

[13] My dictionary source for synonyms is *Rogets 21st Century Thesaurus, 3rd Edition*.

words. I address this issue by subtracting the share of negative words from the share of positive words. I define the fraction of negative words by the analogous expression:

$$Neg_{it} = \frac{\#negativewords}{\#totalwords}_{it} \tag{11}$$

and define the housing news sentiment index by:

$$S_{it} = Pos_{it} - Neg_{it} \tag{12}$$

where i and t denote the city and month respectively. I additionally adjust both negative and positive word counts for negation using the terms: *no, not, none, neither, never, nobody*. I consider a word negated if it is preceded within five words by one of these negation terms.[14] Finally, I apply a backwards 3-month moving average to smooth the series and reduce noise.[15] The window for each reporting month is based on data for that month and the preceding two months. This mirrors the same 3-month moving average used to calculate the S&P/Case-Shiller home price indices. In addition, I apply the same normalized weights used to create the Case-Shiller Composite-20 home price index to create an analogous Composite-20 housing sentiment index.

I create a number of alternate versions of the baseline index sentiment index for robustness. For example, I calculate a version of the index that uses the full, rather than just the leading, text of the articles. I also construct a version that accounts for not only the tone of news, but also the frequency of housing articles published each month. Loughran and Mcdonald (2011) also suggest a "term-weighted" index that adjusts for the commonality and frequency of a word across documents. I find that the results remain robust to these alternative versions. Details on alternate versions and their correlations with the baseline index are available in the 7.

[14] Loughran and Mcdonald (2011) apply the same strategy except with a preceding word distance of three words. Textual analysis studies in the computer science field use a preceding distance of five words, so I opt for the wider window.

[15] Baker et al. (2012b)suggest a 36-month backward moving average to smooth a monthly series of an economic policy uncertainty index.

4.3 Validating Sentiment Index Patterns

Figure 3 plots my composite-20 housing news sentiment index with the Case-Shiller composite-20 housing price index across time. My housing news sentiment index exhibits a striking boom and bust pattern, and appears to forecast the rise and fall of aggregate housing prices by more than two years. My sentiment index peaks in January 2004, while the housing price index peaks 30 months (2.5 years) later in July 2006. This aggregate pattern is driven by similar patterns across in individual cities. Figure 2 plots individual sentiment indexes across time for a sample of six cities. As in the composite index, cities such as Las Vegas and Phoenix that experienced large swings in house prices were preceded by similar swings in news sentiment. Conversely, cities with more moderate increases in housing prices such as Atlanta and Minneapolis, do not appear to have clear trending patterns in news sentiment. Plots for all cities are available in Figure A.1.

One concern might be that these patterns reflect some coincidental manifestation of text across newspaper articles. While Figure 2 shows that the pattern of sentiment varies across cities, it is possible that the boom and bust pattern of words is common across all subjects and not necessarily specific to housing. To address this issue, I collect a random sample of articles that cover any subject or topic. I then compute a "random" sentiment index using the same methodology I used to create my housing sentiment index. If my index really reflects sentiment in the housing market, then we would not expect to see the same pattern arise from a random set of news articles. Figure 3 reveals that the random index is a relatively flat line, and does not exhibit any discernible trend. This suggests that the sentiment index is at least specific to housing news.

Validating the sentiment index as a proxy for investor beliefs is naturally more challenging. By definition, beliefs are unobservable, but there exist some surveys that ask investors about the housing market. Existing survey measures are limited in frequency or geographic variation, but can be used to validate overall trends in my composite sentiment index. The Survey of Consumers (SOC) run by the University of Michigan and Reuters surveys a nationally representative sample of 500 individuals each month on their attitudes toward personal finances, business conditions, and buying conditions. One of these questions refers to the buying conditions in the housing market. Specifically, the SOC asks consumers, "Generally speaking, do you think now is a good or bad time to buy a house?" Respondents answer "yes," "no," or "do not know." Figure 4 plots the percentage of respondents that answered "yes" across time. This simple question on home buyer confidence reveals a strikingly similar pattern to my composite-20 housing sentiment index. The percentage of positive home buyers also peaks well before housing prices, by more than a two year lead. Surveyed home buyer confidence actually appears to lead housing news sentiment slightly, from two to six months. This lead is consistent with a theory that news sentiment responds to consumer sentiment in the market. Interestingly, the increase in survey confidence is also followed by a similar increase in news sentiment in 2008. Both of the increases occur before the temporary rebound of the housing market in 2009, but fall again afterwards.

Case and Shiller (2003) implement even more detailed surveys of home buyer behaviors and provide more detailed perspective on investor expectations. They directly ask respondents how much they expect their house price to grow over the next ten years. Answers in 2003 revealed astonishingly high expectations; with respondents expecting prices to rise an average of 11 to 13 percent annually. Case et al. (2012) recently updated these surveys each year from 2003 to 2012. Their survey covers just four suburban areas, but the similarity in timing of sentiment across the same cities in my dataset is significant. They find that long-term expectations of home buyers also peak in 2004, the same time as my sentiment index.

Panel B in Figure 4 further plots my sentiment index with an index of home builder confidence constructed by the National Association of Home Builders (NAHB). The NAHB implements a monthly survey of their members, asking builders and developers to rate the current market conditions of the sale of new homes, the prospective market conditions in the next 6 months, and the expected volume of new home buyers. The NAHB index weights these answers into one index to represent an aggregate builders' opinion of housing market conditions. Figure 4 shows that builder confidence index in the housing market declined significantly at similar timing to my sentiment index. Builder confidence peaks in 2005, suggesting a slight lag to home buyer confidence. My sentiment index highly correlates with survey measures of housing market confidence in both trends and timing, suggesting that news sentiment does reflect investor beliefs over the housing market. Still, both survey and news sentiment may still be driven by changes in fundamentals. I address effects from both observed and unobserved fundamentals in the following sections.

5 Does Sentiment Reflect Changes in Observed Fundamentals?

5.1 Sentiment Effects on House Price Growth

In this section I test the empirical predictions of the effect of sentiment on prices in Section 2 and analyze whether the results reflect variation in observed fundamentals. I first test the predicted effect of sentiment on prices across time using the composite index. I approximate Equation 7 with the following estimating equation:

$$\Delta p_t = \alpha_0 + \sum_{k=0}^{K} b_k L^k \Delta s_{nt} + \gamma \Delta x_t + \delta_m + \nu_t \tag{13}$$

where a lowercase letter represents a log operator ($p_t = lnP_t$) and Δ denotes the first difference such that $\Delta p_t = lnP_t - lnP_{t-1}$. L^k is a lag operator such that lags $L^k \Delta s_n t = lnS_{n,t-k} - lnS_{n,t-k-1}$. Vector x_t controls for changes in observable fundamentals that drive housing prices over time. House price growth may generally coincide with increased home buying in particular seasons of the year (such as the summer), so I include a set of monthly fixed effects, δ_m, to control for price changes due to seasonality. I assume the error term ν_t is heteroskedastic across time and serially correlated, and calculate Newey and West (1987) standard errors that are robust to heteroskedasticity and autocorrelation up to twelve lags.

Taking log differences provides a convenient approximation of growth period, but also addresses concerns of nonstationarity. Serial correlation in house prices have been well documented (Case and Shiller (1989, 1990)). Estimates will still be consistent if prices and sentiment are serially correlated, as long as this correlation weakens over time.[16] However if both prices and sentiment are nonstationary and contain unit roots, then a regression of Equation 8 could result in a significant estimate of sentiment even if the series are completely unrelated. First differencing also has an additional benefit of removing any linear time trend in price levels. For estimates to be consis-

[16]In other words, to ensure that prices and sentiment are stationary and weakly dependent, weak dependence is generally defined as occurring when the correlation between observations x_t and x_{t+h} of a series approaches zero "sufficiently quickly" as $h \to \infty$.

tent, I also impose an assumption that the error term v_t is uncorrelated with fundamentals and both contemporaneous and lagged values of news sentiment. Making this assumption is useful because it does not require that the error term be independent from future values of news sentiment. This is important because it does not rule out feedback from prices onto future values of news sentiment. In particular, newspapers may put a positive spin on news by emphasizing certain past price increases over others.

The effect of sentiment on prices is captured by the coefficients b_k. Each individual coefficient b_k represents the effect of the one-time change in sentiment growth in period $t-k$ on the equilibrium price growth in time t. Conceptually, the lagged coefficients b_k represent the lagged adjustment path of prices to sentiment.[17] As noted in the last section, Figures 1 reveals that composite sentiment peaks in 2004, suggesting a lag structure of nearly three years. Ultimately, I am interested in the accumulated effect of sentiment on prices, represented by the sum of the coefficients, $\sum_{k=0}^{K} b_k$ For ease of notation going forward, let $\beta = \sum_{k=0}^{K} b_k$.

Table 4 tests the the hypothesis that $\beta > 0$ against the null that $H_o : \beta = 0$. If news sentiment simply reflects price movements or information about fundamentals that is already in prices, then β will not be significantly different than zero. Column (1) estimates equation 13 without any control variables. The first row reports the total accumulated effect of sentiment, β, on the current t monthly growth in prices. The subsequent rows groups the summed lagged effect of sentiment by years. The estimated coefficient describes the proportional relationship between the percentage change in lagged sentiment and prices. An estimated coefficient equal to one would indicate that monthly price and lagged sentiment growth have a one-to-one relationship. Estimates show that a one percent appreciation in the sum of lagged sentiment is associated with a monthly price appreciation of approximately 0.8 percentage points. This is significant relative to the mean of monthly housing price appreciation across this period of 25 basis points.

Nonetheless, the estimated effect of sentiment may still be due to changes in fundamentals. For example, if news sentiment reports on a fundamental not yet incorporated into prices, then β

[17] It is important to note that all estimations rely on assumptions over a particular lag structure on the data. I select this structure using a number of standard model selection criteria, but each has its acknowledged benefits and drawbacks. In addition, the lag structure restricts my estimation sample period. Since my measures for sentiment being in January 2000, my estimation evaluates prices beginning in 2003.

may still be greater than zero but biased upwards. To address this concern, columns (1) through (6) add an increasing number of fundamental controls to the specification. I add each of the fundamental controls sequentially to test the stability of β. Column (2) controls for rental growth, column (3) adds variables for real interest rates and 30-year mortgage rates, and column (4) adds housing supply variables including new housing starts and building permits. Column (5) controls for additional labor market variables for employment, unemployment, and changing labor force, while column (6) includes controls for changing population and income. I do not present the individual coefficients for each control variable as they are not the primary interest of my analysis, but the coefficients are either generally in the right direction or not significantly different than zero. Estimates of β remains remarkably robust with the inclusion of each additional control and decline neither in significance nor magnitude. As argued by a number of previous studies, the stability of my estimates to the sequential addition of controls suggests bias from unobserved factors is less likely (Altonji et al. (2005); Angrist and Krueger (1999)).

Figure 5 plots the predicted prices first using only fundamentals, and then using sentiment. The plot shows that sentiment growth is able to fit both the boom and subsequent bust of prices. In contrast, fundamentals explain a portion of the boom, but are not able to fit the subsequent bust in prices. Consistent with prior studies, observed fundamentals are not able to explain much of the variation in prices on their own. The adjusted R^2 from running a regression with fundamental controls only is 0.10.[18] Adding in lagged sentiment explains an additional 75 percent of the variation in price growth, increasing the R^2 to 0.85. From 2004 to 2006, aggregate housing prices increased by 33 percent. Observed fundamental controls account for approximately 9 percentage points, while sentiment explains an additional 24 percentage points.

Column (7) adds in monthly fixed effects to control for any seasonal variation in housing prices. The magnitude of β actually increases by 10 basis points. Alternatively, the effect of sentiment could simply be capturing a linear time trend in house price changes. Column (8) shows that controlling for a simple linear time trend does reduce the magnitude of β somewhat, but estimates

[18]However, these same fundamentals were able to explain a significant variation in prices historically. As detailed in the next section, running a regression with the same fundamentals prior to this period (from 1987 to 2000) results in an adjusted R^2 of 0.69.

remain positive and significant. Further examination reveals that the coefficient estimate on the linear time trend (not shown) is negative, fitting the bust of the housing prices rather than the boom. Sentiment still largely accounts for the run-up in aggregate house prices.

Column (9) applies a specification that includes lagged measures of fundamentals. Search frictions in the housing market could also potentially affect the immediate effect of fundamentals (Wheaton (1990); Stein (1995); Krainer (2001)). Not all lags can be included due to high collinearity among fundamentals, but I select as many lags as possible with the same model selection criteria used to select the lag structure of sentiment. The effect of sentiment again remains positive, significant, and robust in magnitude. Column (10) reveals that the only variable able to drive down the magnitude of β are lagged measures of the price growth itself. This is not surprising as the predictability of house prices has been well documented (Case and Shiller (1989); Cutler et al. (1990)). Still, coefficient estimates of sentiment growth remain positive. In the following panel estimation, the predictive effect of sentiment remains both positive and significant beyond lagged price growth.

Still, estimations in Table 4 are limited to a small number of observations ($N = 94$) and only accounts for variation in aggregate price growth. Table 5 utilizes the full panel dataset and tests whether sentiment has an effect on prices across cities. I estimate this effect with the following regressions:

$$\triangle p_{it} = \alpha_0 + \beta L^k \Delta s_{n,it} + \gamma \Delta x_{it} + \delta_m + c_i + \nu_{it} \tag{14}$$

where i denotes each city. In some specifications I also control for unobserved heterogeneity across cities with city dummies, c_i. I assume errors are heteroskedastic across time and serially correlated within city, and cluster Newey and West (1987) standard errors by city assuming auto-correlation up to twelve lags. The number of observations between Columns (1) and (2) of Table 5 vary slightly since I do not have rental data for Las Vegas, but I do include Vegas when I estimate the effect of sentiment without controlling for fundamentals. Also, rental data is only available through October 2009 for most of cities. Column 1 has more observations since my sentiment indexes are available through August 2011. Some newspapers do have gaps in coverage by Factiva at various points in time, and thus are missing sentiment measures for those months.

Column (1) estimates regression 14 without any additional controls. Estimates of β are even larger in magnitude than in the aggregate specification, with an estimated coefficient for β of 1.12. Adding in fundamentals sequentially between columns (1) and (2) does not change the magnitude or significance of the results, and including all fundamentals actually increases the total effect of sentiment slightly to 1.22. The robustness of this estimates confirms the stability of β from the composite estimation, and further reduces concerns of that bias from unobserved fundamentals.

Column (3) of Table 5 adds city fixed effects to the specification. Trading behavior in different markets may have particular characteristics that affect the differences in house price movements across different cities. Some cities may have inherently higher or lower house price levels (for example, New York may have high house prices due to particular characteristics of its location, financial center, etc.) that corresponds to innately optimistic newspapers. Transforming prices into growth terms normalizes fixed differences in house price levels across cities. Nonetheless, some markets also may also have coincidentally higher house price and news sentiment changes. Including city fixed effects removes any differences in house price appreciation due to time-invariant unobservable characteristics. The estimated effect of sentiment actually increases in magnitude after controlling for city fixed effects. This suggests that a large part of the predicted effect of sentiment can be attributed to its effect on price growth across time.

Columns (4) and (5) add month and year fixed effects. Adding just month fixed effects does not affect the results, estimates do not appear to be driven by seasonality. Including both month and year fixed effects drops the estimated coefficient by about half the magnitude. This drop in magnitude reflects the common trends in price growth across markets. The most recent boom of housing markets was notable because it was appeared to be a coordinated movement across many markets. Nonetheless, even with month and year dummies, the sentiment index still has a positive and significant predictive effect on price appreciation both statistically and economically. The coefficient implies that a one percent increase in accumulated sentiment growth predicts a 0.6 percentage change in price growth (monthly). This is still large compared to the average monthly house price growth of 16 basis points across cities during this period. Column (6) alternatively controls for a linear time trend, which drives down the magnitude slightly from column (4). As in the aggregate estimates, the coefficient on the linear time trend is negative, fitting the bust of prices in many places but not the boom.

In column (7), I add lagged fundamentals and find that the magnitude of the effect declines slightly to 0.87, but is still positive and economically significant. Column (8) of Table 5 separately tests whether sentiment has any predictive effect from price growth above and beyond lagged prices. While the β drops to 30 basis points, the estimated effect of sentiment remains positive and significant. As in the aggregate specification, most of the explanatory power of lagged price growth comes from the first few lags (Δp_{t-1}). Lagged prices beyond the preceding year do not have much predictive power for future prices, whereas sentiment growth leads prices by more than two years.

Estimating over the whole sample period conceals whether the results are driven by the boom or bust period housing prices, or both. In columns (9) and (10), I split the sample and estimates the effect of sentiment on prices separately for each time period. Column (9) estimates equation 14 with data before July 2006, and Column (10) runs the regression with data July 2006 and afterwards. Concurrent with plots in Panel B of Figure 4, I find that sentiment predicts *both* the boom and bust of housing prices across cities. Estimated effects are positive, significant, and large in magnitude, while the magnitude of β is slightly larger for the bust than the boom. This is consistent with the observation that not all cities experienced a rise in housing prices, but a majority experienced a subsequent bust.

5.1.1 Subprime Conditions

One concern for the results in Table 4 and 5 is that estimates could instead reflect a spurious correlation between news and the rise in the availability of credit and subprime lending patterns. The extraordinary rise in house prices from 2000-2005 was also accompanied by an unprecedented expansion of mortgage credit, particularly in the subprime market (Mian and Sufi (2009); Glaeser et al. (2010)). Easing lending standards and rising approval rates opened homebuying to a new set of consumers, which potentially allowed a new group of homebuyers to shift aggregate demand and drive up house price growth (Keys et al. (2010, 2012); Mian et al. (2010)).[19] Mian and Sufi (2009)

[19] Other papers that explore subprime lending explanations and the role of mortgage securitization in the housing crisis are Bajari et al. (2008); Danis and Pennington-Cross (2008); Demyanyk and Van Hemert (2011); Gerardi et al. (2008); Goetzmann et al. (2012); Mayer and Pence (2008); Mayer et al. (2010); Haughwout and Tracy (2009) Adelino et al. (2009); Campbell et al. (2011); Foote et al. (2008); Mayer et al. (2009); Mian and Sufi (2009); Mian et al. (2010);

show that lending to subprime zip codes grew rapidly from 2002 to 2005, and sharply fell as house prices declined. Thus if news simply documents the rise and fall in subprime lending, then not controlling for these patterns may misrepresent the effect of β.

I address this possibility by including additional controls for credit and subprime lending in Table 7. Column (1) in Table 7 adds controls for the changes in the six-month London Interbank Offered Rate (LIBOR). Estimations in Tables 4 and 5 already include changes in overall the real interest rate and 30-year mortgage rate, but many adjustable-rate subprime mortgages were set at an initial fixed rate for the first two years and then indexed to changes in the LIBOR six-month rate (Mayer et al. (2009); Gerardi et al. (2008)). Column (1) includes the full set of controls from column (5) in Table 5, including fundamentals, lagged fundamentals, month and city fixed effects. Including changes in the 6-month LIBOR rate has no effect on the results, and the estimated effect of sentiment is still positive and significant. The estimate also remains robust in magnitude compared to estimates in column (5), Table 5.

Column (2) additionally controls for the fraction of subprime mortgages and average loan-to-value ration in each city. I do not have measures for subprime lending and applicant income for Atlanta, Charlotte, Dallas, and Minneapolis. Thus, regressions in columns (2)-(5) only include data from 16 cities. Additionally, measures of subprime lending, loan-to-value, and applicant income are only available through 2008. Thus, estimations in columns 2-5 are limited to five years of data (2003-2008), and restricted to observations where both data on subprime lending and sentiment indexes are available. Nonetheless including trends of subprime lending and loan-to-value ratios does not significantly change the results. The estimated effect of sentiment on price growth declines slightly, but by less than 5 basis points. In column (5), I include additional measures of income, but specific to those reported by mortgage applicants. The effect of sentiment is again remarkably robust. β decreases slightly by 5 basis points, but remains positive and significant in magnitude. Only including additional lags of the subprime variables reduces estimates of β more substantially, but estimated effect of sentiment remains economically significant.

Piskorski et al. (2010).

5.2 Sentiment Effects on Housing Trading Volume

Existing theories of sentiment also links sentiment to trading volume (Harrison and Kreps (1978); De Long et al. (1990b)). For example, Baker and Stein (2004) reason that when limits to arbitrage are very costly, optimistic investors are more likely to trade and drive up volume. Scheinkman and Xiong (2003) and Odean (1998) make related arguments based on overconfident investors. The model similarly provides testable empirical predictions for housing sentiment and trading volume. Equations 9 suggests a relationship between changes in sentiment and trading volume levels. Thus, I estimate the effect of sentiment on trading volume in the housing market with the following specification:

$$v_{it} = \varphi_0 + \kappa L^k \Delta s_{n,it} + \Delta x_{it} + \delta_m + c_i + \xi_{it} \tag{15}$$

where v_{it} represents the de-trended log volume of housing transactions in each month t . I measure trading volume in de-trended log levels to address concerns of nonstationarity in levels of volume in the housing market. I follow a de-trending methodology applied to volume in Campbell et al. (1993). I also control for all observed fundamentals, quarterly fixed effects, and city fixed effects, and lagged fundamentals. As in equation 13, κ represents the sum of coefficients for all lags of sentiment.

Figure 6 plots the composite-20 housing sentiment index and volume of housing transactions over time. I construct a composite measure of transaction volume by aggregating the number of transactions in each city and weighting each measure with the normalized weights used to calculate the composite-20 Case-Shiller home price index. Figure 6 shows that sentiment not only forecasts the pattern in prices, but also foreshadows a rise and fall in volume. Interestingly, volume appears to peak before prices. The plot shows that volume begins to drop at the end of 2005, while prices do not begin to decline until July 2006. Sentiment thus still precedes volume by approximately a 18 months (1.5 years). This pattern provides a potential explanation for the long lead in sentiment to prices. Figure 6 suggests that sentiment moves first and leads to housing transactions in the following year, and this increased trading activity shows up in housing prices another year later.

Table 7 presents the results for regression 15. I select a model that includes $K = 18$ lags i.e.

a year and six months. Note that my volume data ends in July 2009 so that my sample period is shorter than in my estimations for prices. Columns (1)-(3) estimate the effect of sentiment on the composite-20 measure of transaction volume, and Columns (4)-(6) estimates over the panel dataset across cities. Consistent with predictions in Equation 9, the growth in sentiment has a positive association with increases in transaction volume levels. Columns (1) and (4) runs the regression with any additional controls. Sentiment growth has a positive and significant accumulated effect on trading volume both in the composite and panel data. Specifically, a one percent increase across monthly lags of sentiment growth leads to a 4.7 and 3.5 percent increase in the volume of housing transactions in the composite and panel regressions respectively.

As in our regressions above, a primary concern is that this positive effect instead reflects positive changes from fundamentals. Thus, Columns (2)-(3) and (5)-(6) include the same set of housing fundamentals used to explain housing prices as well as month and city fixed effects. In the composite regressions, the estimated coefficient for κ remains robust to the inclusion of fundamentals in x_{it}, and further increases in magnitude after controlling for month fixed effects. In the panel regressions, including fundamentals, lagged fundamentals, month, city fixed effects reduces the magnitude of the κ in the panel regressions, but the effect of sentiment growth on volume remains positive and significant. Column (6) shows that a one percent positive appreciation in lagged sentiment leads to a 1.6 percent increase in transaction volume after controlling for lagged fundamentals. This is still well above the mean of detrended log volume (-.02). These results are consistent with empirical evidence that connects investor sentiment to trading volume (Barber and Odean (2000, 2008); Odean (1999)). The correlation between volume and prices has also been previously documented in the housing market (Stein (1995)). Genesove and Mayer (1997) provide empirical evidence that behavioral biases such as loss aversion might explain positive price-volume correlations in the housing market.

6 Does Sentiment Reflect Changes in Unobserved Fundamentals?

The previous section shows that sentiment, proxied by the tone of news, has a predictive effect for house price growth and transaction volume above and beyond a number of observed housing fundamentals. In this section I address whether this effect instead reflects effects from unobserved fundamentals. As noted in the previous section, the robustness of the estimates to the inclusion of each additional control is already strongly suggestive that bias from unobservables is less likely. Furthermore, the lead in sentiment growth to prices suggests that prices move in response to sentiment and not the reverse. One might be worried that these indexes actually overlap since Case-Shiller home price index is reported using housing transactions from previous months. However news sentiment leads prices by more than two years, and the Case-Shiller home price index is calculated over transactions from the current month and the previous two months. Even if there is some further delay in reported transactions, news sentiment peaks at such a significant year lead that it very unlikely due to some mechanical delay in the reporting of prices. Still, prediction does not eliminate the possibility that news is reporting information on unobserved fundamentals not yet incorporated into prices. Search frictions in the housing market could delay the effect of both sentiment and fundamentals on price growth.

If the housing sentiment index is affected by unobserved fundamentals, estimates of sentiment in Tables 4 and 5 may be potentially biased. The extent of this bias depends on whether x_{it} includes the key set of fundamentals that drive house price growth. If only minor fundamentals are missing, then estimates may still be biased but only minimally. I can assess whether my observed vector x_{it} appears to miss any important housing fundamentals by testing whether it explains prices well during periods where sentiment is not suspected to be a factor. Table 8 splits the sample into two periods, pre- and post-2000, and estimates the effect on prices with fundamentals alone. If x_{it} sufficiently controls for important determinants of housing prices, then these variables should explain changes in price growth during the "pre-bubble" period, i.e. before 2000. The adjusted R^2 in column 1 shows that fundamentals explain almost 70 percent of the variation in composite housing prices before January 2000. I use the composite-10 price index since the composite-20 index is only available starting in 2000. Similar to the composite-20 index, the Case-Shiller Composite-10 home price index is a weighted average of ten major U.S. cities., which includes Boston, Chicago, Denver, Las Vegas, Los Angeles, Miami, New York, San Diego, San Francisco, and Washington, D.C. In contrast, the same fundamentals explain very little of the change in prices after 2000 with an adjusted R^2 equal to only 0.092. Columns 3 and 4 similarly show that fundamentals have greater explanatory power for housing prices across cities prior to 2000. Local fundamentals do explain at least 23 percent of the variation in prices after 2000, but are able to explain 1.55 times more prior to 2000. Fundamentals are more significant in cities that did not experience rapid growth in prices. These results suggest that if that my news sentiment index is affected by articles on unobserved fundamentals, then bias from these variables are at least minimal.

Still, the housing sentiment index may be contaminated by news reports on unobserved fundamentals. I exploit the richness of my data to isolate any articles that discuss housing fundamentals and partial out their effect directly. I identify any article that mentions words related to housing fundamentals using stem words such as "unemployment", "mortgage rates", or "taxes." Tetlock et al. (2008) employ a similar strategy to identify news articles that discuss firm fundamentals. The advantage of this strategy is that I can identify articles that discuss fundamentals that I both observe and do not observe. I then directly control for fraction of the positive minus negative words in these news stories that mention fundamentals in my estimations. If information on fundamentals from these articles subsequently drive prices, then controlling for words in these articles should drive down the significance and magnitude of the results in Section 5.

Table 9 show that the estimated effects of sentiment on price growth remain robust to controlling for news content over fundamentals. I create individual measures of these "media fundamentals" and evaluate their effect on prices separately. I control for all lags of these measures as well as all observed controls. Columns (1) through (7) adds a control for articles discussing each housing fundamental to test the stability of β. Column (2) shows that the estimate drops after controlling for news articles discussing credit conditions, but the remains stable with the addition of remaining media fundamentals. Column 2 reports an estimated coefficient for the accumulated effect of sentiment approximately equal to 0.5, an almost one-to-two proportional relationship between lagged sentiment changes and monthly price growth. The estimated effect of positive news sentiment remains significant, positive, and large in magnitude.

6.1 Weekend and Narrative News Content

Results in Section 5 show that sentiment predicts price growth at a significant lead of more than two years, and estimated effects remain highly robust to the sequential addition of observed controls. The observed set of fundamentals explains a significant amount of variation in price growth prior to 2000, suggesting it is unlikely effects are due to a key omitted fundamental after 2000. In addition, the estimated effect does not appear to be driven by articles that discuss fundamentals in its text. To narrow the identifying variation further, I isolate two subsamples of the news articles that cater to reader sentiment but are less likely to be affected by information on fundamentals.

The first set I isolate are those articles that are published on the weekend. Weekend articles are likely correlated with sentiment because it must cater to readers who prefer content lighter in nature. Indeed, research on newspaper readership shows that lighter readers are concentrated on the weekend. The Readership Institute of Northwestern University conducted a survey of 37,000 newspaper readers in 2000 and found that readership is highest on Friday, Saturday and Sunday, driven by the greater proportion of "light" readers on the weekend. Light readers are those who spend fewer than 16 minutes reading the newspaper a week, whereas heavy readers pay attention to the news every day. Furthermore, the survey reports that these readers appear to be light readers of all news alternatives, including television news, magazines, and internet websites.[20] Thus these readers are more likely to be those who are more subject to sentiment and demand articles that cater to their preferences. This is consistent with why Saturday and Sunday editions of newspapers typically include additional sections, such as entertainment and sports, in order to draw readers who are more subject to sentiment. At the same time, weekend news articles are less likely to reflect information on fundamentals as market news tends to be reported during the business week. Furthermore, any press releases on fundamentals data can only occur on a working weekday. Thus, news stories on the weekend are more likely to be exogenous to official news reports on fundamentals. Because my dataset includes the exact date of each story, I am able to identify the exact day of the week each article is published. Thus, I create a weekend instrument that only analyzes the tone of articles that occur on Friday, Saturday, and Sunday.

[20] Survey reports can be found at http://www.readership.org/reports.asp

Examining a smaller subset of articles allows me to run further falsification tests on the assumption that they are less likely to reflect news on fundamentals. For example, one concern might be that news releases on fundamentals are increasingly released on Friday and then reported over the weekend. If this is the case than the increase of positive or negative words on the weekend may be the result of increasing news releases concentrated at the end of the week. To test this possibility, I compile a dataset of of all the press release dates on various housing fundamentals. Specifically, I organize the schedule of press releases from the Bureau of Labor Statistics (BLS) and regional data from the Census. Table 10 reports the correlation of the weekend instrument with the percentage of news reports released on Friday. The first row reports the correlation of all BLS news releases and the subsequent rows reports the correlation with regional and employment releases. Column 2 reveals that the correlation with each are very low, suggesting the weekend instrument is not simply reporting news occurring on Friday. The last two rows examine the correlation with Census releases on new residential construction and sales. The weekend instrument is also uncorrelated with the percentage of these releases occurring on Friday.

Another concern might be that news on fundamentals are reported during the working week, but then summarized over the weekend. One way to address this issue is to control for the pattern of positive and negative words that occur during the weekday. If weekday articles contain information on fundamentals, then controlling for this content should address concerns that weekend content is actually a proxy or response to weekday information. I control for the fraction of positive minus negative words in weekday articles in both the first stage regressions Table 11 and instrumental variable (IV) results in 12. A captured effect of sentiment is then narrowed to the differential variation between weekend and weekday news.

I create an additional instrument that from the narrative articles in my sample. A narrative article refers to one that narrates a story or account of events around particular individuals. Narrative writing is also a particular writing strategy through which newspapers can reflect sentiment and capture readers' attention. The Readership Institute Survey reports that readers have high preference for "people-centered news" or articles about local ordinary people. The study particularly encourages newspapers to increase readership through this "approach to story-writing" and finds that it is how a story is written that matters more for reader satisfaction. At the same time, narrative articles contain anecdotal stories, but tend to offer no actual data or news on fundamentals in the market. The above narrative expresses an obvious optimistic view over the housing market, but contains no actual news on any particular fundamentals. Thus, trends in news slant across narrative articles are correlated with sentiment but plausibly exogenous from any actual news on fundamentals.

I identify narrative articles by locating those that discuss individual people. I isolate any article that includes a name from name lists from the Social Security Administration (SSA) and the Census. The Social Security publishes a list of the 200 most popular first names of the 2000s. I create a list of last names with the top 1000 most frequently occurring surnames in the 2000 census. I then define an article as narrative if it discusses any of these names in its first paragraph. I exclude any articles that match a quoted statement by an individual in case these are cited statements from various experts. I then analyze the share of positive and negative words in just the identified "narrative" articles in my sample.

I then use share of positive and negative words over the smaller sample of weekend and narrative articles as instruments for my overall measure of sentiment. These instruments are only valid if they are sufficiently correlated with the housing sentiment index. I directly test the first-stage relevance between sentiment and each of my instruments with the following first-stage regression:

$$\triangle s_{n,it} = a_0 + \lambda \triangle z_{it} + \eta \triangle x_{it} + \delta_m + c_i + u_{it} \tag{16}$$

where z represents the log of the candidate instrument. Columns 1 and 2 in Table 11 confirms that changes in both the weekend and narrative instruments are positively and significantly correlated with positive news sentiment. I test the strength of both instruments and report the F-statistics in bold at the bottom of Table 11. The weekend instrument is stronger than the narrative instrument, but both instruments have more than sufficient strength, with F-statistics well above the benchmark of 10.

Table 12 presents the second-stage results of instrumenting for positive news sentiment. Column (1) presents the original ordinary least squares estimates with all controls from estimating Equation 14. Columns (2) and (3) reports the results instrumenting for sentiment using the weekend and narrative index respectively. The estimated effect for sentiment on price growth remains positive, significant, and robust in magnitude. Instrumenting sentiment with the weekend instrument actually increases the magnitude of the estimated effect of sentiment on price growth substantially. While our main concern is addressing upward bias, noise from sentiment measures likely biases standard ordinary least squares estimates downward. Estimates remain robust in magnitude after instrumenting with the narrative index, though do not increase.

7 Conclusion

This paper presents evidence that sentiment has a significant effect on housing prices, particularly during the boom and bust from 2000 to 2011. While there has been much discussion and interest in the role of mass psychology or "animal spirits" in the most recent housing crisis, empirical support for this argument has been limited due to the lack of sentiment measures for the housing market. This paper provides the first measures of sentiment across local housing markets by capturing the tone of local housing news across 20 major city newspapers.

I find that sentiment forecasts the boom and bust of housing markets by a significant lead, peaking two years before house prices began to decline in 2006. Results show that sentiment growth is positively associated with future price growth, and is able to explain a significant amount of variation in the price changes above and beyond fundamentals. In particular, the housing sentiment index is able to explain an additional 70 percent of the variation in national house prices beyond observed fundamentals. Further evidence suggests these estimates are unlikely driven by latent fundamentals. Estimates are significantly robust to the inclusion of an exhaustive list of controls and remain robust to a novel instrumental variable strategy.

The findings of this paper have several potential implications. The evidence suggests that sentiment has an important effect on asset prices, and raises questions over how behavioral factors interact in economic contexts. Expectations and fundamentals likely have a more complex relationship, for example, perhaps where individuals systematically overestimate a positive shock from lower interest rates or increases in credit supply. Indeed, studies on financial literacy suggest that many investors are not able to appropriately compound interest or account for inflation (Lusardi and Mitchell (2007b)). Brunnermeier and Julliard (2008) find supportive evidence that particularly links money illusion to the run-up in housing prices. Furthermore, the ability of news to forecast price movements suggests measures of market sentiment may be useful indicators to monitor empirically. The central finding of this paper, however, highlights that sentiment plays an important role on aggregate economic outcomes and suggests it deserves greater attention in future work.

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Appendix

A.1 Sentiment Index Robustness and Alternate Versions

Leading v. Full Text. The primary sentiment index used in this paper is the share of positive minus negative words calculated over the leading text of housing articles each city-month. I create a number of alternate versions of the baseline sentiment index for robustness. Table A.1 compares the effect of sentiment on house price growth using different versions of the housing sentiment index. Column (1) first presents the results using the baseline index, $Pos_{it} - Neg_{it}$. Column (2) similarly applies the share of positive minus negative words, but calculated using the full text of housing articles. Using the full rather than the leading text has no significant effect on the results, in precision or magnitude. The bottom panel of Table A.1 reports the correlations of each alternative with the baseline index, and shows that the full text version of the index is highly correlated with the baseline.

News Intensity. Excitement over the housing market may be evident in not only the tone of news articles, but also by how many articles cover the housing market each month. A newspaper can cater to reader sentiment through both the slant and frequency of its housing news articles. Thus to capture this dimension, I interact the baseline index with the share of housing articles published by a newspaper each month. Specifically, this version can be represented by:

$$(Pos_{it} - Neg_{it}) * \frac{\text{\# Housing Articles}}{\text{\# Total Articles}}_{it}$$

The share of housing articles is equal to the number of housing articles divided by the total number of news articles (in any subject) in city i and month t. Column (3) shows that this version also has no effect on the results, and is highly correlated with the baseline.

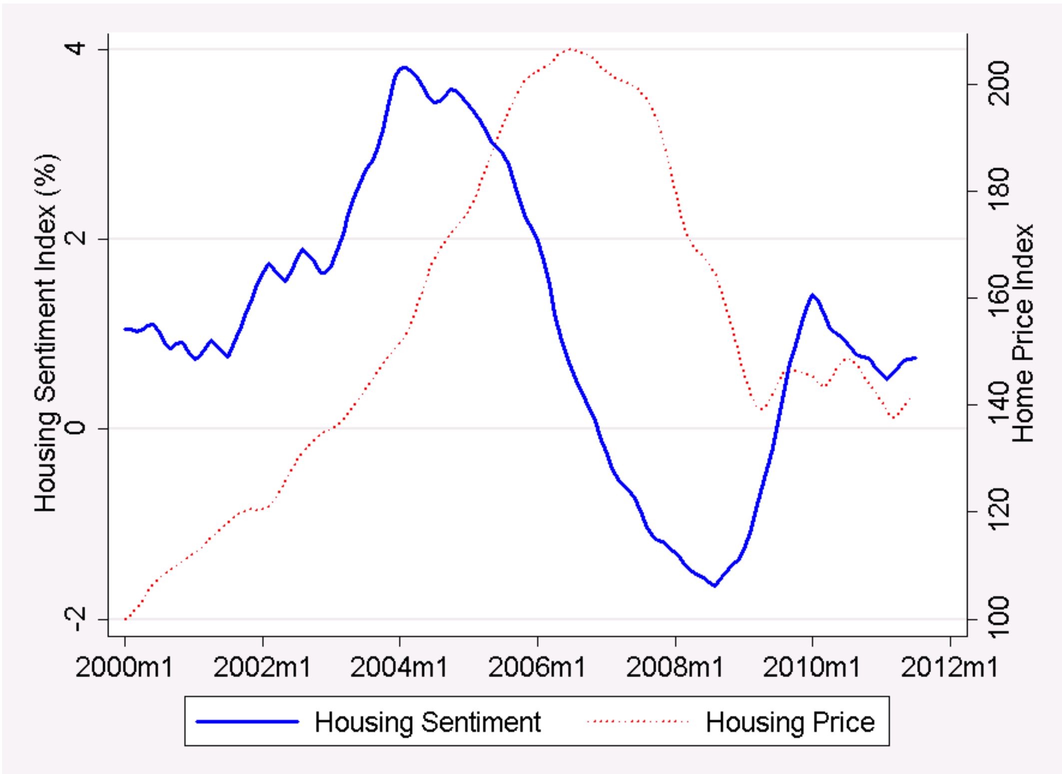
Positive v. Negative Index. Another informative robustness check is to separate the effect of positive and negative words. If the baseline index is appropriately capturing sentiment, we might expect the growth in the share of positive words to have a positive association with prices while the share of negative words should have a negative association with house prices. Indeed, columns (4) and (6) shows that the effect of just positive words is positive while negative words has an opposing negative effect. The baseline index has a greater predictive effect for house prices than just positive or negative words alone, but both still have a significant effect on house price growth individually.

Term Weighted Index. Loughran and Mcdonald (2011) also propose an index that weights each word in an article using the term-weighting formula:

$$w_{kj} = \frac{1 + log tf_{ij}}{1 + log(a)} log(\frac{N}{df_i})$$

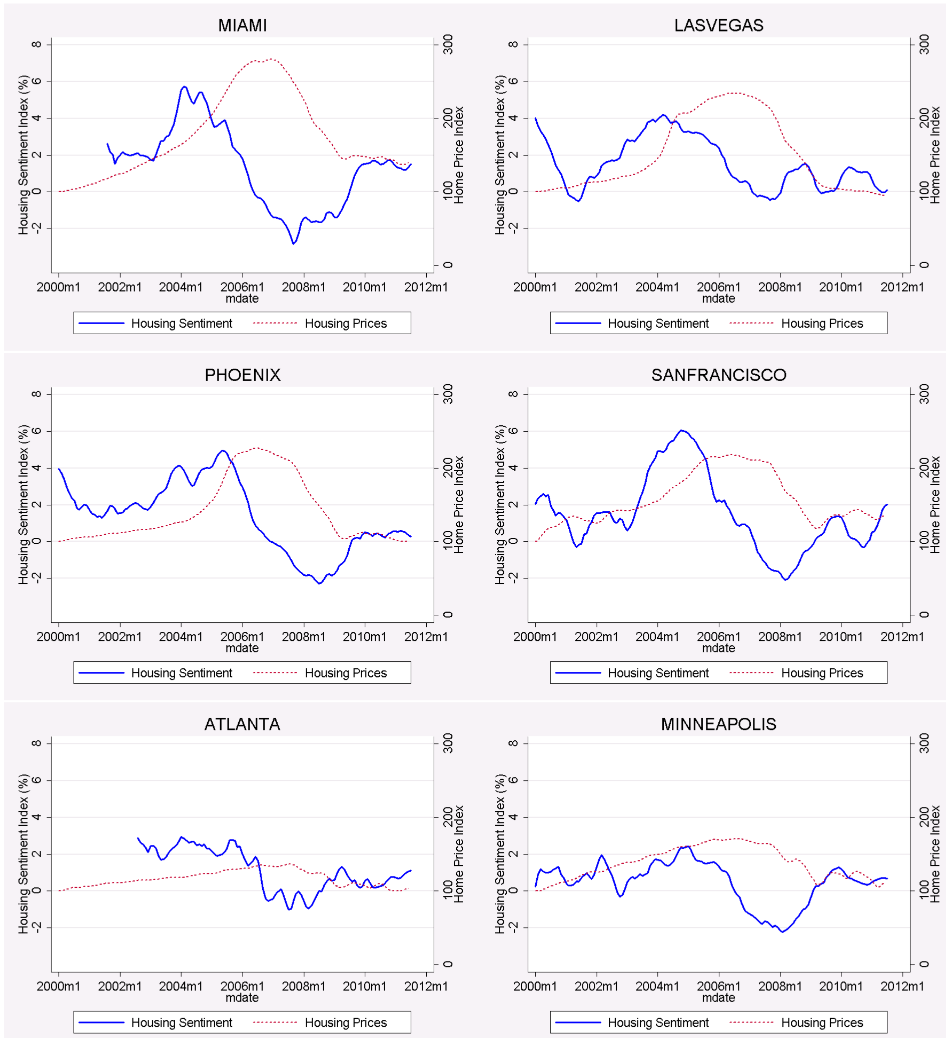
where N represents the total number of articles in the sample, df_i, the number of articles containing at least one occurrence of the i^{th} word, tf_{ij} the raw count of the i^{th} word in the j^{th} document, and a the total number of positive words in the article. The first term accounts for the frequency of the term within each article but also applies a log transformation to attenuate the impact of high frequency words. For example, the word *soar* may appear 32,000 times in our sample while the word *skyrocket* only appears 10 times, but this does not mean *soar* is necessarily 3200 times more important than the word *skyrocket*. The second term measures the importance of the term across documents by dividing the total number of documents in the sample by the number of documents containing the particular term. Thus the word *soar* will receive a high weight based on the first term, but if it is a common word that appears in more than 90 percent of articles, then the second term will decrease the first term by more than 90 percent. I apply this weighting formula to the share of positive words and test to see this has a significant effect on the results in Column (5) of Table A.1. The results show that term-weighted share of positive words has an almost identical impact on house price growth as the non-weighted positive index.

FIGURE 1: COMPOSITE-20 HOUSING SENTIMENT AND CASE-SHILLER HOME PRICE INDEX



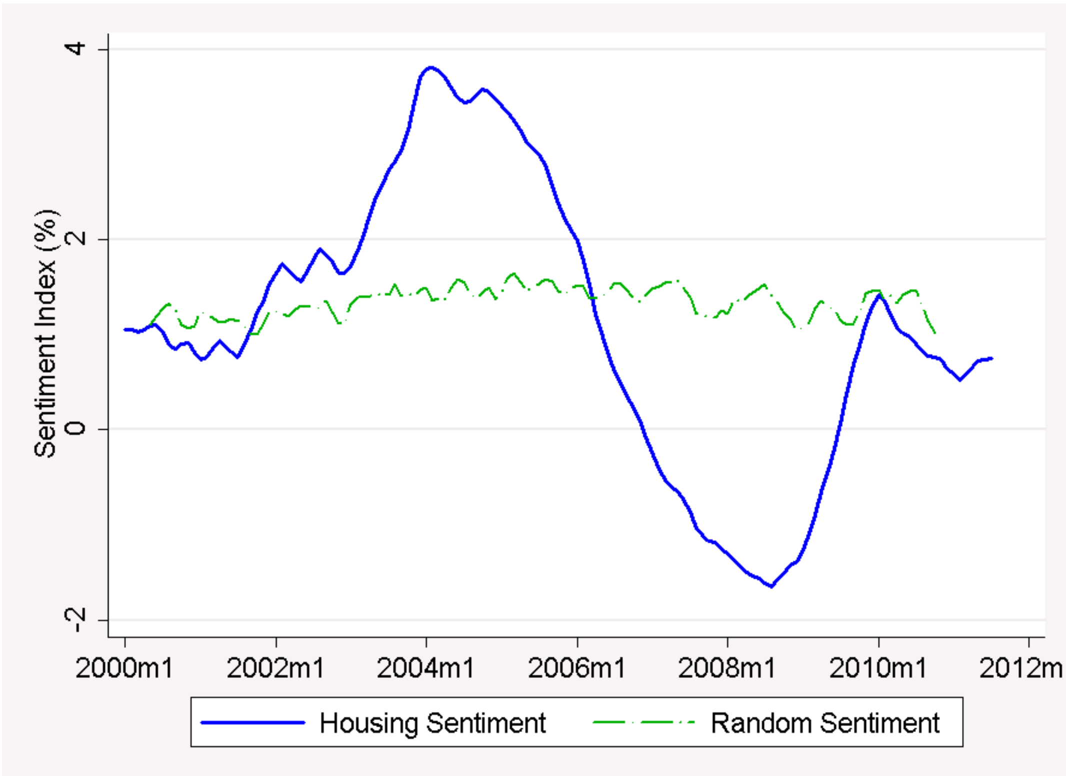
Note: This figure plots the composite-20 sentiment index and the composite-20 Case-Shiller housing price index. Lines are smoothed for seasonal variation and noise with a 6-month backward and forward moving average. Housing prices and sentiment are calculated using a 3-month backward moving average in empirical estimations.

FIGURE 2: HOUSING SENTIMENT AND CASE-SHILLER HOME PRICE INDEXES BY CITY



Note: Figure 2 plots the housing sentiment index and housing price indexes for individual cities. Lines are smoothed for seasonal variation and noise with a 6-month backward and forward moving average. Housing prices and sentiment are calculated using a 3-month backward moving average in empirical estimations.

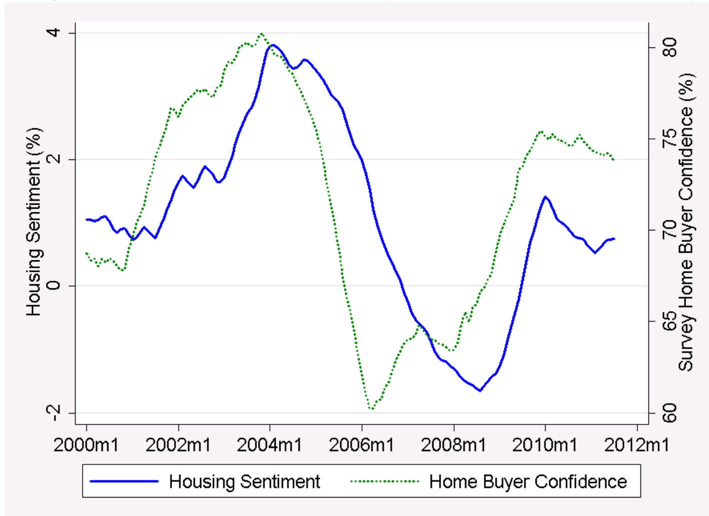
FIGURE 3: RANDOM SENTIMENT PLACEBO TEST



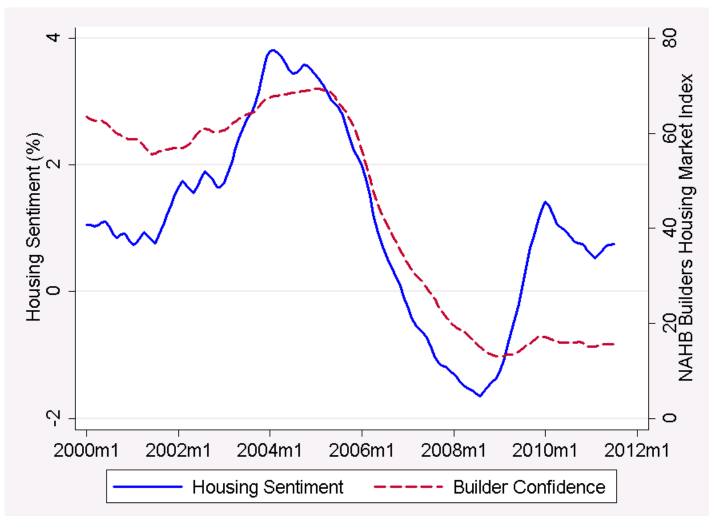
Note: Figure 3 presents evidence that the pattern of positive minus negative words is specific to housing articles. "Housing Sentiment" is the share of positive minus negative words calculated over newspaper articles that cover the housing market. "Random" is the share of positive minus negative words across a random sample of articles of any subject each city-month. As seen in the plot, random sentiment generally remains relatively flat and does not exhibit the same boom and bust pattern as housing sentiment. Lines are smoothed for seasonal variation and noise with a 6-month backward and forward moving average.

FIGURE 4: VALIDATING SENTIMENT AGAINST SURVEYS OF HOUSING MARKET CONFIDENCE

Panel A. Housing Sentiment Index and Survey of Consumers Home Buyer Confidence

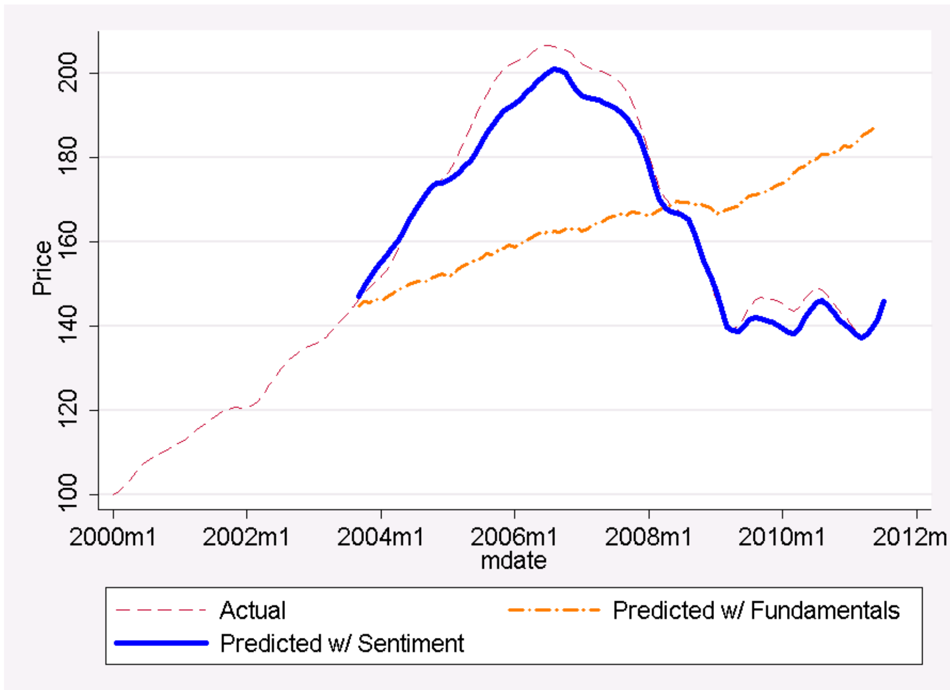


Panel B. Housing Sentiment Index and National Association of Home Builders Confidence Index



Note: Panel A plots the composite-20 housing sentiment index with a national survey of home buyer confidence. The Survey of Consumers surveys a nationally representative sample of 500 consumers and asks whether they think it is a good time to buy a home. Consumers answer "Yes/No/Don't Know." The green dashed line represents the percentage of those surveyed who answered "Yes." Panel B plots the composite housing sentiment index with a national survey of members of home builder confidence. The National Association Home Builders asks members of their association each month to rate the current market conditions of the sale of new homes, the prospective market conditions in the next 6 months, and the expected volume of new home buyers. The NAHB index weights these answers into one index to represent an aggregate builders' opinion of housing market conditions. The timing the sentiment index coincides with survey measures of confidence, suggesting that it is reflecting investor beliefs over the housing market. Lines are smoothed for seasonal variation and noise with a 6-month backward and forward moving average.

FIGURE 5: PREDICTING HOUSE PRICE GROWTH WITH SENTIMENT INDEX V. FUNDAMENTALS



Note: Figure 7 plots observed composite-20 prices and predicted prices. The dashed line represents prices predicted with contemporaneous. fundamentals alone. The solid line plots prices predicted with positive sentiment only. The picture illustrates that sentiment can explain a significant variation in prices. More importantly, sentiment fits the prediction to the timing of the boom and bust, whereas fundamentals only predict a linear projection of prices.

FIGURE 6: COMPOSITE-20 HOUSING SENTIMENT INDEX AND TRANSACTION VOLUME

Note: Figure 8 plots a composite-20 volume of housing transactions and my housing sentiment index. Data for transaction volume comes from DataQuick. I calculate a composite-20 measure of volume using the same weights used to create the Case-Shiller Composite-20 Home price Index. Lines are smoothed for seasonal variation and noise with a 6-month backward and forward moving average.

TABLE 1: DESCRIPTIVE STATISTICS FOR NEWSPAPER HOUSING ARTICLES

	Newspaper Publication	# Articles	AP	A-section	Real Estate	Local	General
All Cities		19,620	6%	19%	20%	28%	45%
Atlanta	The Atlanta Journal-Constitution	647	0	24	13	29	60
Boston	Boston Herald/Boston Globe	966	3	23	15	24	43
Charlotte	The Observer	556	14	23	28	17	33
Chicago	Chicago Tribune	1,965	8	79	66	13	27
Cleveland	The Plain Dealer	303	1	18	13	20	62
DC	The Washington Post	1,171	6	13	38	27	24
Dallas	The Dallas Morning News	1,294	0	3	0	74	22
Denver	The Denver Post	432	1	13	0	11	83
Detroit	Detroit News/Detroit Free Press	624	5	48	23	10	55
LA	LA Times/LA Daily News	3,579	5	17	14	18	69
Las Vegas	Las Vegas Review-Journal	588	0	15	0	4	92
Miami	The Miami Herald	678	7	27	11	14	51
Minneapolis	Star Tribune	625	1	17	20	1	79
NYC	New York Times	1,372	4	19	33	17	42
Phoenix	The Arizona Republic	1,921	0	19	5	52	29
Portland	The Oregonian	509	2	18	16	35	42
San Diego	The San Diego Union-Tribune	1,086	7	14	26	16	52
San Francisco	The San Francisco Chronicle	530	0	27	8	8	81
Seattle	The Seattle Times	398	29	25	36	5	59
Tampa	Tampa Tribune	376	0	30	2	43	41

Note: Table 1 lists each city, its corresponding newspaper, and descriptive statistics for my sample of housing news articles. My source for housing news articles is Factiva.com, which provides a subject code to identify articles that cover housing market news. My sample covers articles from January 2000 to August 2011. "AP" lists the percent of articles that are credited to the Associated Press. "A-section" refers to the percent of articles located in the front or "A" section of the newspaper. "Real Estate" is the percent of articles that were published in a special real estate section of the newspaper. "Local News" refers to those articles listed in the metropolitan or any specific regional news section of the newspaper. Most of the articles are found in a general news or business news section of the newspaper. It is possible for one article to show up in more than one category. For example, if an article is in the real estate section of the regional edition of the newspaper than it would show up in both columns 6 and 7. Thus, the percents will not necessarily add up to 100 percent for each city.

TABLE 2: SAMPLE POSITIVE WORDS AND WORD COUNTS

word	% of Total PosWord Count	Freq.
BOOM	3.24	959
BOOST	1.17	348
BRIGHT	0.36	106
EXCEED	0.33	98
EXTEND	0.52	154
GOOD	2.29	678
GREAT	0.69	203
HEAT	3.1	917
HOPE	0.69	205
JUMP	2.67	790
LEAP	0.49	145
POSITIVE	0.3	89
SHOOT	0.44	130
SIZZLE	0.48	143
SKYROCKET	0.34	101
SOAR	2.23	660
SPIKE	0.32	96
SPRINGING	0.49	145
STRONG	2.4	711
SURGE	1.91	565

Note: This base list of positive words are from the word lists *Increas* and *Rise* word lists in the Harvard IV-4 Psychological Dictionary. I use these lists to maintain the objectivity of a predetermined list, but also reflect how the media spins excitement over asset markets. Shiller (2008) in particular argues that the media expresses a positive slant through superlatives that emphasize price increases and upward movements. I then expand the original word list with synonyms, alternate tenses, and inflections. I also eliminate obvious misclassifications. The original Harvard list consisted of 136 words while the extended Inc-NEW list contains 403 words. This table presents a sample of words and their corresponding word counts.

TABLE 3: SUMMARY STATISTICS – SENTIMENT, PRICES, VOLUME, AND FUNDAMENTALS

	Obs.	Mean	Std.Dev.	Min	Max
Housing Sentiment Indexes :					
Composite-20	139	1.093	1.716	-3.152	4.435
Cities	2515	1.102	2.606	-10.355	9.979
Case-Shiller Housing Price Indexes:					
Composite-20	138	154.451	31.077	100.000	206.520
Cities	2760	144.543	40.856	64.030	280.870
Volume of Housing Transactions:					
Composite-20	114	5326.172	1580.046	2538.817	9797.987
City	2046	4584.942	2762.715	160.000	21809.000
Fundamentals:					
Real Interest Rate	138	1.833	0.812	-0.010	4.020
30-yr Mortgage Rate	138	6.087	0.933	4.230	8.520
LIBOR 6-month Rate	138	3.001	2.006	0.400	7.000
Rental Index	2242	113.317	12.255	89.661	154.958
Unemployment Rate	2760	6.074	2.461	2.100	16.600
Employment (Thousands)	2760	2161.150	1917.453	158.500	8757.600
Housing Starts	2760	1259.820	1099.021	49.000	6291.000
Building Permits	2760	1894.101	1534.131	57.000	20802.000
Log Population	2400	15.201	0.635	14.109	16.764
Log Income	2400	18.882	0.699	17.554	20.755
Average Loan-to-Value	1872	0.743	0.099	0.331	0.882
Share of Subprime Lending (in Amt)	1872	12.937	7.247	0.000	34.963
Log Loan Applicant Income	1728	11.450	0.265	10.939	12.192

Note: Housing sentiment indices in this table are the difference between the share of positive and negative words each city-month ($pos - neg/total$), see Section 4 for full details on how the index is calculated. Data for the sentiment indices go through July 2011, Case-Shiller home prices are reported with a two-month lag so are available through June 2011, volume of housing transactions are provided by DataQuick through June 2009, and rent is available from REIS through October 2009. Composite-20 versions of the housing sentiment index and transaction volume are calculated using the same normalized weights used to calculate the Composite-20 Case-Shiller index. There are some gaps in newspaper coverage in the data, thus data for housing sentiment indices are not completely balanced. The index can only be calculated for months where newspaper coverage is available in the data, thus some cities are missing sentiment index data in months where the newspaper was not covered by Factiva. Details on the sources of the housing fundamentals are available in Section 3.

TABLE 4: SENTIMENT PREDICTS NATIONAL HOUSE PRICE APPRECIATION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sum of Lagged Sentiment	0.756***	0.763***	0.760***	0.760***	0.760***	0.763***	0.838***	0.696***	0.932***	0.120
	(0.080)	(0.076)	(0.074)	(0.077)	(0.079)	(0.078)	(0.125)	(0.163)	(0.206)	(0.073)
Year 1 Lags ($L^1 + ... + L^{12}$)	0.213***	0.184***	0.189***	0.189***	0.186***	0.185***	0.193***	0.169***	0.273***	0.021
	(0.018)	(0.026)	(0.025)	(0.026)	(0.024)	(0.025)	(0.029)	(0.039)	(0.071)	(0.021)
Year 2 Lags ($L^{13} + ... + L^{24}$)	0.101***	0.087***	0.089***	0.089***	0.088***	0.087***	0.095***	0.100***	0.164***	0.012
	(0.030)	(0.027)	(0.024)	(0.024)	(0.023)	(0.024)	(0.025)	(0.028)	(0.042)	(0.011)
Year 3 Lags ($L^{25} + ... + L^{43}$)	0.442***	0.493***	0.482***	0.482***	0.486***	0.491***	0.550***	0.427***	0.495**	0.087
	(0.051)	(0.049)	(0.051)	(0.053)	(0.056)	(0.058)	(0.103)	(0.139)	(0.176)	(0.052)
Rents	.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Interest Rate Variables	.	.	✓	✓	✓	✓	✓	✓	✓	✓
Labor Market Variables	.	.	.	✓	✓	✓	✓	✓	✓	✓
Housing Supply	.	.	.	.	✓	✓	✓	✓	✓	✓
Population and Income	.	.	.	.	.	✓	✓	✓	✓	✓
Month Fixed Effects	.	.	.	.	.	.	✓	✓	✓	✓
Linear Time Trend	.	.	.	.	.	.	.	✓	.	.
Lagged Fundamentals	.	.	.	.	.	.	.	.	✓	.
Lagged Price Growth	.	.	.	.	.	.	.	.	.	✓
Observations	94	94	94	94	94	94	94	94	94	94
Adjusted R^2	0.854	0.863	0.866	0.86	0.858	0.852	0.835	0.838	0.841	0.974

Note: * 10% significance, ** 5% level, *** 1% level. Newey and West (1987) standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. L^k denotes the lag $t-k$. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The rows below break down total sum of the monthly lags of sentiment by lagged years. "Year 1 Lags" equals the sum of lagged sentiment from L^1 to L^{12}, "Year 2 Lags" is the sum of lags L^{13} to L^{24}, "Year 3 Lags" is the sum from lags L^{25} to L^{43}.. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. The lag structure is chosen through a joint F-test. Additional lags after L^{43} does not affect the results.

TABLE 5: SENTIMENT PREDICTS CITY HOUSE PRICE APPRECIATION (PANEL)

									BOOM	BUST
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sum of Lagged Sentiment	1.120***	1.223***	1.342***	1.276***	0.670***	1.015***	0.846***	0.266***	0.814***	1.035***
	(0.120)	(0.131)	(0.127)	(0.125)	(0.170)	(0.171)	(0.144)	(0.048)	(0.309)	(0.153)
Year 1 Lags ($L^1 + ... + L^{12}$)	0.248***	0.282***	0.323***	0.294***	0.156***	0.257***	0.200***	0.068***	0.289***	0.206***
	(0.034)	(0.042)	(0.039)	(0.037)	(0.035)	(0.039)	(0.041)	(0.013)	(0.085)	(0.036)
Year 2 Lags ($L^{13} + ... + L^{24}$)	0.311***	0.440***	0.479***	0.462***	0.226***	0.366***	0.314***	0.093***	0.249**	0.371***
	(0.041)	(0.050)	(0.048)	(0.047)	(0.058)	(0.065)	(0.051)	(0.018)	(0.112)	(0.056)
Year 3 Lags ($L^{25} + ... + L^{43}$)	0.560***	0.501***	0.540***	0.521***	0.287***	0.392***	0.332***	0.105***	0.275*	0.458***
	(0.065)	(0.083)	(0.070)	(0.069)	(0.098)	(0.089)	(0.085)	(0.024)	(0.144)	(0.083)
Fundamentals	.	✓	✓	✓	✓	✓	✓	✓	✓	✓
City Fixed Effects	.	.	✓	✓	✓	✓	✓	✓	✓	✓
Month Fixed Effects	.	.	.	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	.	.	.	.	✓	.	.	.	.	.
Linear Time Trend	.	.	.	.	.	✓	.	.	.	.
Lagged Fundamentals	.	.	.	.	.	.	✓	.	.	.
Lagged Price Growth	.	.	.	.	.	.	.	✓	.	.
Observations	1564	1106	1106	1106	1106	1106	1106	1106	399	707
Adjusted R^2	0.287	0.466	0.517	0.582	0.634	0.589	0.669	0.863	0.471	0.576

Note: * 10% significance, ** 5% level, *** 1% level. This table estimates the effect of sentiment across cities. The number of observations decline from columns (1) to (2) because fundamentals are only available through 2009 while sentiment indices are available through July 2011. Newey and West (1987) standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. L^k denotes the lag $t-k$. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The rows below break down total sum of the monthly lags of sentiment by lagged years. "Year 1 Lags" equals the sum of lagged sentiment from L^1 to L^{12}, "Year 2 Lags" is the sum of lags L^{13} to L^{24}, "Year 3 Lags" is the sum from lags L^{25} to L^{43}.. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing.

TABLE 6: SENTIMENT PREDICTS CITY HOUSE PRICES BEYOND SUBPRIME LENDING TRENDS

	(1)	(2)	(3)	(4)	(5)
Sum of Lagged Sentiment	0.830***	0.778***	0.769***	0.692***	0.421***
	(0.148)	(0.162)	(0.161)	(0.154)	(0.135)
Year 1 Lags $(L^1 + ... + L^{12})$	0.193***	0.213***	0.212***	0.224***	0.144***
	(0.043)	(0.049)	(0.049)	(0.045)	(0.035)
Year 2 Lags $(L^{13} + ... + L^{24})$	0.304***	0.290***	0.287***	0.263***	0.169***
	(0.052)	(0.057)	(0.057)	(0.051)	(0.049)
Year 3 Lags $(L^{25} + ... + L^{43})$	0.333***	0.275***	0.270***	0.204**	0.108
	(0.088)	(0.098)	(0.097)	(0.097)	(0.08)
Month Fixed Effects	✓	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓	✓
Fundamentals	✓	✓	✓	✓	✓
Lagged Fundamentals	✓	✓	✓	✓	✓
LIBOR 6-month rate	✓	✓	✓	✓	✓
% Subprime Loans	.	✓	✓	✓	✓
Loan-To-Value	.		✓	✓	✓
Loan Applicant Income	.		.	✓	✓
Lagged Credit Variables	.		.	.	✓
Observations	1106	876	876	771	771
Adjusted R^2	0.667	0.707	0.709	0.735	0.793

Note: * 10% significance, ** 5% level, *** 1% level. This table estimates the effect of sentiment across cities. The number of observations decline from columns (1) to (2) because data for % of subprime loans are only available for 16 cities in the sample and only through September 2009, observations further decline because loan applicant income from the HMDA database are only available through 2008.Newey and West (1987) standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. L^k denotes the lag $t-k$. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The rows below break down total sum of the monthly lags of sentiment by lagged years. "Year 1 Lags" equals the sum of lagged sentiment from L^1 to L^{12}, "Year 2 Lags" is the sum of lags L^{13} to L^{24}, "Year 3 Lags" is the sum from lags L^{25} to L^{43}.. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. The lag structure is chosen through a standard joint F-test. Including additional lags after L^{43} does not affect the results. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing.

TABLE 7: SENTIMENT PREDICTS THE VOLUME OF HOUSING TRANSACTIONS

	Composite			Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of Lagged Sentiment	4.674***	4.909***	5.531***	3.658***	2.709***	1.479**
	(1.189)	(1.337)	(1.915)	(0.678)	(0.668)	(0.594)
Year 1 Lags ($L^1 + ... + L^{12}$)	3.555***	3.938***	4.13**	2.957***	2.355***	1.381***
	(0.890)	(1.012)	(1.681)	(0.556)	(0.532)	(0.486)
Year 2 Lags ($L^{13} + ... + L^{18}$)	1.119**	0.971	1.401	0.701***	0.354*	0.098
	(0.505)	(0.874)	(0.893)	(0.208)	(0.206)	(0.153)
Rents	.	✓	✓	.	✓	✓
Interest Rate Variables	.	✓	✓	.	✓	✓
Labor Market Variables	.	✓	✓	.	✓	✓
Housing Supply	.	✓	✓	.	✓	✓
Population and Income	.	✓	✓	.	✓	✓
Month Fixed Effects	.	✓	✓	.	✓	✓
City Fixed Effects	.	.	.	.	✓	✓
Lagged Fundamentals	.	.	✓	.	.	✓
Observations	96	96	96	1578	1481	1437
Adjusted R^2	0.430	0.613	0.595	0.068	0.261	0.507

Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. This tables estimates the effect of sentiment on detrended log volume. I use detrended log volume to address non stationarity concerns, and detrend volume following Campbell et al. (1993). Specifically, I subtract the one year backward moving average. Newey and West (1987) standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. L^k denotes the lag $t-k$. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The rows below break down total sum of the monthly lags of sentiment by lagged years. "Year 1 Lags" equals the sum of lagged sentiment from L^1 to L^{12}, "Year 2 Lags" is the sum of lags L^{13} to L^{24}, "Year 3 Lags" is the sum from lags L^{25} to L^{43}.. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. The lag structure is chosen through a standard joint F-test. Including additional lags after L^{43} does not affect the results. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing.

TABLE 8: EXPLANATORY POWER OF OBSERVED FUNDAMENTALS PRE- AND POST-2000

	Composite-10		Panel	
	Pre-2000	Post-2000	Pre-2000	Post-2000
	(1)	(2)	(4)	(5)
Rents	1.424***	0.373	0.840***	0.365**
	(0.166)	(0.704)	(0.110)	(0.179)
Interest Rate Variables	✓	✓	✓	✓
Labor Market Variables	✓	✓	✓	✓
Housing Supply	✓	✓	✓	✓
Population and Income	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓
City Fixed Effects	.	.	✓	✓
Observations	119	118	2136	2241
Adjusted R^2	**0.693**	**0.092**	**0.363**	**0.234**

Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and autocorrelation up to 12 lags are in parentheses. L^k denotes the lag $t-k$. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing. This table shows that they key set of fundamentals explain prices much better prior to the suspected bubble period, post-2000. For example, the R^2 in column 1 shows that the key set of fundamentals is able to explain nearly 70 percent of the variation in aggregate price growth prior to 2000. After 2000, however, this same set of fundamentals explains very little of the variation in price growth with an adjusted $R^2 = 0.09$. This suggests that the main set of results at least incorporate the key set of fundamentals that typically explain housing price growth, and that price movements post-2000 must be due to some other variable. Thus, sentiment estimates in the main results are less likely driven by bias from an unobserved fundamental.

TABLE 9: IS SENTIMENT DRIVEN BY NEWS STORIES ON UNOBSERVED FUNDAMENTALS?

	Dep Var: Housing Price Growth, t=monthly						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sum of Lagged Sentiment	0.826***	0.540**	0.636***	0.572***	0.589***	0.530**	0.581**
	(0.148)	(0.212)	(0.208)	(0.21)	(0.215)	(0.256)	(0.279)
Media Rents	✓	✓	✓	✓	✓	✓	✓
Media Credit Conditions	.	✓	✓	✓	✓	✓	✓
Media Labor Market Conditions	.	.	✓	✓	✓	✓	✓
Media Housing Supply	.	.	.	✓	✓	✓	✓
Media User Costs	.	.	.	.	✓	✓	✓
Media Demographics	.	.	.	.	.	✓	✓
Media Local GDP & Inflation	.	.	.	.	.	.	✓
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Fundamentals	✓	✓	✓	✓	✓	✓	✓
Lagged Fundamentals	✓	✓	✓	✓	✓	✓	✓
Observations	1094	1094	1093	1093	1093	1093	1093
Adjusted R^2	0.678	0.683	0.717	0.717	0.717	0.718	0.723

Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing. This table directly controls for news content over fundamentals by identifying any news article that mentions a particular fundamental in its text. The variable "Media Rents", for example, is the share of positive minus negative words in any articles that mention any word related to "rents" in its text. This table shows that controlling for articles that mention fundamentals has minimal effect on the estimated effect of sentiment on house prices.

TABLE 10: CORRELATION OF WEEKEND INSTRUMENT WITH FRIDAY NEWS RELEASES

% of Releases on Friday	Correlation with Weekend Instrument
All BLS	0.07
Any Metro or Regional	-0.01
County Employment	-0.04
Regional Employment	-0.05
Metro Area Employment	0.00
CPI	-0.02
PPI	0.14
New Residential Construction	-0.02
New Residential Sales	-0.01

Note: This table test for a possible violation of the exclusion restriction for the weekend instrument. The validity of the weekend instrument relies on the assumption that no news on fundamentals is being released over the weekend. One possible violation of this assumption is that news is increasingly released on Friday and therefore reported over the weekend. I put together a database of the schedule of economic data releases from the BLS and the Census. This table shows that the fraction released on Friday is uncorrelated with the share of positive minus negative words over the weekend. The first column lists the types of press releases, including all releases by the Bureau of Labor Statistics, any release on metropolitan or regional specific fundamentals, release on employment, measures of inflation, and housing specific fundamentals from the Census. The second column reports the simple correlation between the fraction of these releases that occur on Friday with the weekend instrument.

TABLE 11: WEEKEND AND NARRATIVE INSTRUMENTS FOR SENTIMENT, FIRST-STAGE

	Dep Var: Sentiment Growth, t=monthly	
	Weekend	Narrative
Instrument	0.458***	0.208***
	(0.100)	(0.031)
Weekday News Tone	✓	.
Rents	✓	✓
Interest Rate Variables	✓	✓
Labor Market Variables	✓	✓
Housing Supply Variables	✓	✓
Population and Income	✓	✓
Month Fixed Effects	✓	✓
City Fixed Effects	✓	✓
Lagged Fundamentals	✓	✓
F-statistic	**233.776**	**46.089**
Observations	1856	1856
Adjusted R^2	0.663	0.108

Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing. This table reports the first-stage estimates of sentiment on the weekend and narrative instruments. The bottom panel reports the F-statistic for the instruments in bold to test for instrument strength. Both instruments are sufficiently relevant to the housing sentiment index, with F-statistics well above the benchmark rule of 10.

TABLE 12: PREDICTING PRICE GROWTH USING POSITIVE SENTIMENT, IV RESULTS

	OLS	Weekend IV	Narrative IV
	(1)	(2)	(3)
Sum of Lagged Sentiment	0.837***	1.247***	0.805**
	(0.096)	(0.217)	(0.382)
Year 1 Lags (L1+...+L12)	0.18***	0.305	0.01
	(0.035)	(0.187)	(0.304)
Year 2 Lags (L13+...+L24)	0.294***	0.500***	0.47**
	(0.039)	(0.153)	(0.215)
Year 3 Lags (L25+...+L43)	0.363***	0.441***	0.326
	(0.042)	(0.091)	(0.201)
Weekday News Tone	.	✓	.
Month Fixed Effects	✓	✓	✓
City Fixed Effects	✓	✓	✓
Fundamentals	✓	✓	✓
Lagged Fundamentals	✓	✓	✓
Observations	1106	1106	1106
Adjusted R^2	0.669	0.647	0.648

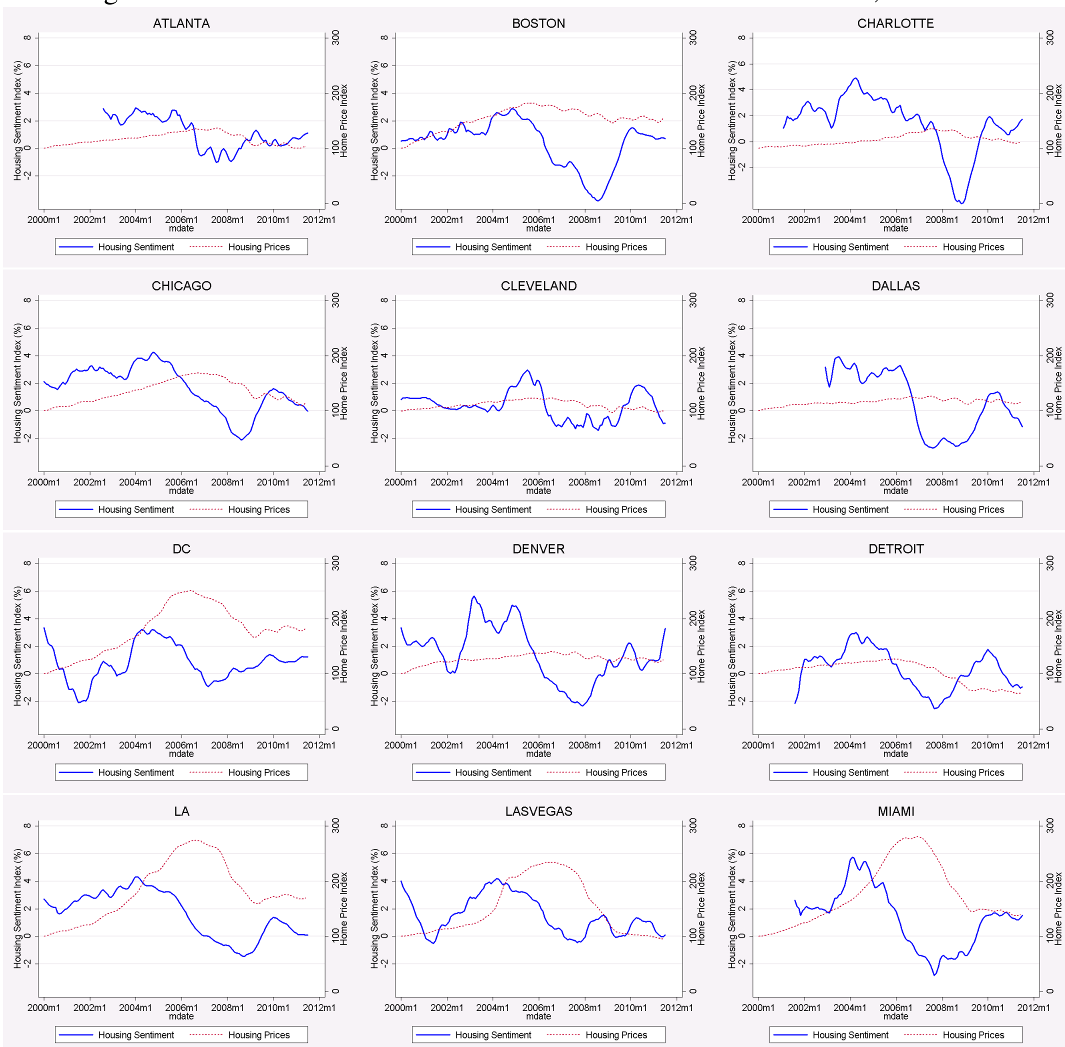
Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing. This table presents the original OLS estimates in column (1), and the instrumental variable estimates using the weekend and narrative instruments in columns (2) and (3) respectively. The estimated effect of sentiment remains robust to both instrumental variable strategies, suggesting bias from unobserved factors in the original estimates are less likely.

TABLE A.1: COMPARING EFFECT OF ALTERNATIVE SENTIMENT INDICES

	Dep Var: Housing Price Growth, t=monthly					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pos-Neg	Pos-Neg	Pos-Neg*	Positive	Positive	Negative
	(baseline)	*(full text)*	*% housing articles*		*(term-weighted)*	
Sum of Lagged Sentiment	0.846***	0.803***	0.802***	0.264***	0.277**	-0.349***
	(0.144)	(0.137)	(0.149)	(0.075)	(0.108)	(0.069)
Fundamentals	✓	✓	✓	✓	✓	✓
City Fixed Effects	✓	✓	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓	✓	✓
Lagged Fundamentals	✓	✓	✓	✓	✓	✓
Correlation with *baseline*	1.00	0.784	0.655	0.674	0.510	-0.666
Observations	1106	1106	1106	1106	1106	1106
Adjusted R^2	0.669	0.694	0.696	0.624	0.615	0.662

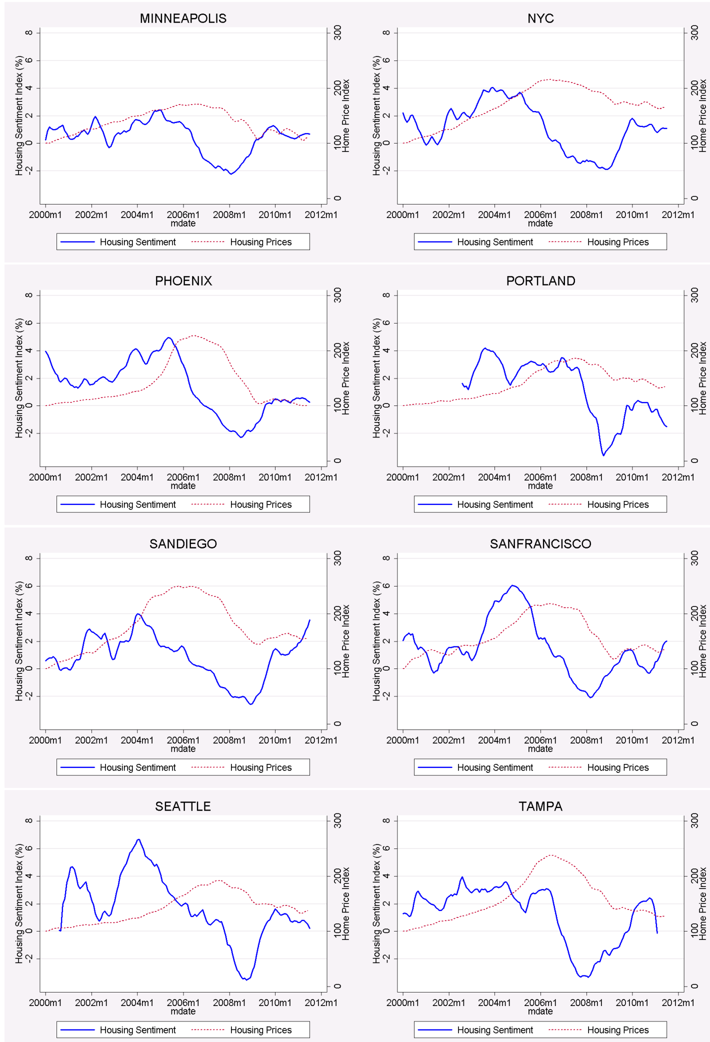
Note: * 10% significance, ** 5% level, *** 1% level. Newey-West standard errors that are robust to heteroskedasticity and auto-correlation up to 12 lags are in parentheses. Sum of Lagged Sentiment sums all the coefficient estimates of current and lagged sentiment growth together. The corresponding standard errors for the linear combination of estimates are reported in parentheses below. Estimates of lagged logged sentiment measure the impact of a one percent increase in the monthly growth of sentiment on the monthly growth in prices i.e. monthly capital appreciation on housing. This table compares the effect of alternate versions of the sentiment index on house prices and shows that results are qualitatively the same. “Pos-Neg” represents the difference between the share of positive and negative words in the leading text of housing articles each city-month. Pos-Neg *(full text)* is the same index calculated over the full text of the articles. Column (3) adds another dimension of sentiment by interacting the baseline index with the fraction of all newspaper articles that cover the housing market. This index accounts for both the tone of newspaper articles and the number of articles published on housing. Columns (4)-(6) considers the effect of positive and negative sentiment separately. Column (4) uses just the share of positive words. Column (5) calculates a “term-weighted” positive index, which add weights for the commonality and frequency of a word across documents (Loughran and Mcdonald (2011)). “Negative” is the share of negative words across articles each month.

Figure A.1: HOUSING SENTIMENT INDEX AND HOUSING PRICES, BY CITY



Note: See Figure 2 Notes.

Figure A.1: HOUSING SENTIMENT INDEX AND HOUSING PRICES, BY CITY (CONT'D)



Note: See Figure 2 Notes.

CHAPTER 2

TIMING THE HOUSING MARKET*

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Abstract

We create reliable measures of the cost of owning and the cost of renting that enable us to compare the level of rents and ownership costs across MSAs. We show that households can predict whether renting or owning will end up being less expensive ex post. This exercise is more robust than trying to predict house price changes or housing returns because much of that uncertainty is inframarginal in the optimal own/rent decision, which depends only on the which tenure mode is cheaper. We show that households can profitably time the home ownership decision. Using several simple trading rules, we estimate that households can save as much as 50 percent of annual rental costs over a five-year period by timing the decision of when to buy a home. The potential savings varies across cities.

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1 Introduction

Pioneering work by Case and Shiller (1989) has documented significant predictability in the house price changes, such that a change in a given year predicts the same direction in the following year. From an asset pricing perspective, this predictability suggests an opportunity for investors to time the market and develop a profitable trading strategy. Despite the predictability of the housing market, recent studies find that empirical forecasts of house prices lack sufficient precision for investors arbitrage in practice (Glaeser and Gyourko, 2010). Still, the predictability of housing prices may be useful for the average homebuyer. For the typical household investor, the primary concern in facing the housing market is whether own versus rent. The average household makes a decision of when to own or rent in order to obtain housing at the lowest possible cost. Thus, timing the market for the household rests on the relative costs of owning versus renting.

In this paper, we investigate whether the predictability of house price changes has any relevance for the household decision to own versus rent. Since the total cost of housing depends on the realization of the sale price of the house, predictability of house prices suggests that the costs of owning may also be forecastable (Hendershott and Van Order 1987; Poterba 1984). Nevertheless, the costs of rent may vary with the costs of owning such that the relative decision to own versus rent is still difficult to predict. Furthermore Poterba (1984) shows that if the housing market is in equilibrium, the marginal cost of owning should equal the cost of rent in steady-state.

In order to reliably compare the costs of owning and renting, we first pin down the relative *level* of rents and house prices. Using extensive sets of both proprietary and Census household level data, we match housing and rental values across housing and buyer characteristics including the number of bedrooms, age of structure, and income quartiles. With that data, we estimate the relative cost of owning and renting across 39 different metropolitan statistical areas (MSAs) from 1980 to 2009.

We document a significant amount of predictability in the binary decision for the household to own versus rent relevant for the typical household. While theory suggests that the costs of owning and renting should be equal in equilibrium, we find that the relative costs of owning and renting are forecastable year to year. On average, if it is better to rent (own) in a given year and city, it is better to rent (own) respectively the next year. Overall, in many cities owning is generally cheaper than renting, though the difference in costs varies in magnitude across locations.

We then develop several trading strategies for the binary own versus rent decision and compare the expected savings of using these strategies against less sophisticated trading rules. We find that a simple trading rule of choosing to own when it was cheaper to own in the previous year saves 13 percent of annual rent relative to always owning. A household can benefit even more relative to a simple strategy of always renting, saving more than 40 percent on average. While the amount of savings from this rule varies across cities, it is consistently positive. In some cities in the South, for example, it is almost always better to own than rent such that switching based on the trading rule produces minimal gains. In other cities, particularly those with more momentum in their house price changes, the savings from switching relative to always owning is much larger. While a more sophisticated rule based on a regression model performs better, it produces minimal savings above and beyond the simpler benchmark.

We further extend our analysis to a multi-year holding period. Prior literature often focuses on the housing returns of a 1-year holding period, but the horizon for investing in a home is likely much longer for many households. We find that the difference in the cost of owning and renting is still predictable over longer horizons. For example, if it is better for an investor with a horizon of five years to own in a given year, then it is still likely better for him to own if he enters the market in the following year. We find that a first-time homebuyer gains the most savings if she waits to buy until she observes that is cheaper to own in the previous year. Otherwise, the she should continue to rent if it was cheaper to rent in the prior year. This simple strategy can save as much as 50 percent of annual housing costs over a 5-year period.

In contrast to prior literature, we find that the tenure choice between owning and renting may be profitably timed. In a number of cities – but far from all – the savings from switching tenure mode following our trading rules is large enough to offset even sizeable transaction costs. The key is that the relative costs of owning and renting are so persistent that households do not need to change tenure mode very often to take advantage of them. Like Case and Shiller (1989), we find that first-time homebuyers can save money by following a trading rule to decide whether to defer home buying for a year. However, we also show that by making the own/rent decision serially, a household that decides to continue to rent will have the option the following year to continue to rent some more. In most cities, we find that simple strategy can approach the lowest cost timing for buying a home.

Our paper also contributes to several literatures besides the study of housing market efficiency. We find that rental and owner-occupied housing markets do not appear to be in equilibrium (Diaz and Luengo-Prado 2008, Glaeser and Gyourko 2007, Verbrugge 2009). Our work also has implications for the recent literature on housing risk that emphasizes the choice between owning and renting (Han 2008, Sinai and Souleles 2005, Ortalo-Magne and Rady 2002).

The remainder of the paper is organized as follows: Section 2 details the methodology and data used to calculate our measures of the cost of owning and cost of renting. Section 3 presents the main results based on the 1-year costs of investing or renting a home. Section 4 develops a profitable trading strategy based on these results. In section 5, we extend our analysis across greater horizons. Section 6 analyzes our results across different tax brackets, and the final section concludes.

2 Measuring the Cost of Owning and Renting

2.1 Rent to Price Ratios

To compare the relative costs of owning versus renting houses, we need estimates of the *levels* of house prices and rents for an otherwise-equivalent residence. This presents several empirical challenges. First, owned units and rental units may differ in physical and economic characteristics, and it is rare for researchers to observe rents and prices for the same unit (Glaeser and Gyourko, 2010). Second, commonly available house price indexes and rent indexes report only the growth in normalized prices or rents. Instead, data that can match the level of house price and rental values are only observed at occasional points in time. Furthermore, if one uses a growth index to extrapolate prices or rents from one point in time to another, the extrapolated levels of prices or rents will not match observed levels in the years when they overlap.

A number of prior studies have employed varying strategies to match house prices and rents for comparable residences and convert house price and rent indexes into levels, (Davis et al. 2008; Meese and Wallace, 1994; Cutts et al. 2005; Gallin 2004; Crone et al. 2004; Smith and Smith 2006). Our approach is to apply a wide variety of methods to obtain a distribution of possible house price and rent series. We estimate owning and rental costs in each city and year for each possible price and rent series. This approach enables us to construct a rough estimate of the uncertainty around the relative costs of owning and renting that is due to measuring prices and rents with error.[1]

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[1] Prior studies typically commit to one approach. Meese and Wallace (1994) and Cutts et al. (2005) derive a rent-price ratio for specific metro at one or two points in time. Other studies such as Gallin (2004) calculated a relative rent to price ratio using just growth rates of prices and rents while ignoring the level. Crone et al. (2004) uses the American Housing Survey to create a biennial estimate of the national rent-price ratio, though not by city. Davis et al. (2008) use a hedonic method to obtain predicted rents and housing values for a set of owners and renters respectively.

To implement this approach, we extract household-level micro data on the level of house values and rents values from the 1980, 1990, and 2000 U.S. Decennial Census. The Census data consists of a nationally representative sample of five percent of U.S. households and reports rents paid by renters, the market value of housing reported by owners, and household income and demographic variables. These surveys also include detailed data on house characteristics.[2]

Using this data, we impute rental values to homeowners and house values to renters. We match rental and owner-occupied units based on the following characteristics: number of bedrooms -- including one, two, and three and more bedroom units; age of structure – whether built within the last 0-10 years, 11-20 years, 21-30 years, or 31 years or older; and buyer and renter income quartiles. For homeowners, we impute the average rent of rental units in the same cell. Similarly, we impute the average house price of owner-occupied homes to rental units in the same cell. We construct cells based on the three categories alone and in combination, and for each year separately, yielding 42 different imputations.

We also employ a hedonic method to calculate corresponding housing and rental values based on housing characteristics. Specifically, we regress housing values on housing characteristics and imputed predicted house values for the units rented by renters. Conversely, we regress rental values on a set of housing characteristics and use the estimates to predict rents for homeowners as in Davis et al. 2008. We do this separately for each Census year, for another six imputations.

We obtain average annual rental values by MSA from a proprietary source, REIS.[3] Using these values, we create three estimates of rent to price ratios: (a) using matched rental and

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[2] We drop any observations where the residence lacks complete plumbing, a kitchen, phone, or heat. We also exclude group quarters and non-family units.

[3] For robustness, we also used rent values from the Bureau of Labor Statistics' rent index and found little difference.

housing values from the Census; (b) using REIS rental values and matched Census housing prices; and using (c) REIS rents and a calculated REIS housing value based on the Census rent to price ratio.[4] We calculate these rent-to-price ratios in each given Census base year, and then extrapolate the levels from each base year using normalized indices. We extrapolate house prices using annual growth rates for 39 MSAs from the Federal Housing Finance Agency's constant-quality repeat sales index from 1980 to 2010. We calculate MSA-level rent growth rates using annual rental values from REIS. In the end, we obtain over 200 different estimated rent to price ratios per MSA per year, 1980-2009.

2.2 Cost of Owning and Cost of Renting

The annual cost of renting, CR, is rent, R_t, observed directly. The annual cost of owning, however, must be calculated. Following prior literature, we apply the user cost formula to estimate the annual cost of owning.[5] Nonetheless in contrast to prior work, we are interested in the <u>realization</u> of the user cost rather than the expectation. Thus we substitute actual house price appreciation, π_t, into the traditional user cost formula in place of the expected capital gain, $E[\pi_t]$. Our estimate of the ex-post observed annual cost of owning is:

$$CO = P_t\Big(\left(1-\tau_{mtg}\right)(LTV)r_t + \left(1-\tau_y\right)(1-LTV)r_t + m_t + \left(1-\tau_{\tau_p}\right)\tau_p - (1-\tau_{CG})\pi_t\Big) \qquad (1)$$

where r_t denotes the nominal interest rate, LTV is the loan-to-value ratio, τ_p is equal to the property tax rate, and m_t is the rate of maintenance and depreciation. Mortgage interest is tax-deductible at rate τ_{mtg}, property taxes are deductible at rate τ_{τ_p}, dividend and interest income is taxed at rate τ_y, and housing capital gains is taxed at the rate τ_{CG}.

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[4] i.e. Predicted REIS housing value = REIS rent * (Census Price/Census Rent)

[5] For examples, see Hendershott and Slemrod (1983), Poterba (1984), Poterba and Sinai (2008).

We do not observe a household's actual mortgage rate, so we approximate the household's mortgage interest rate r_t with the contemporaneous 30-year fixed mortgage rate from the Federal Reserve Board. Glaeser, Gottlieb, and Gyourko (2012) emphasize that households have a one-sided option to refinance to lower mortgage rates, which implies that our average proxy mortgage rate will be higher than the true average mortgage rate, and our calculations will err on the side of overestimating the cost of owning. The opportunity cost of equity, $P_t(1 - LTV)r_t$, is a latent variable. We follow the prior literature by assuming that a household's equivalent-risk alternative investment is to lend at the mortgage interest rate, r_t.

Following Himmelberg, Mayer, and Sinai (2005), property tax rates are allowed to vary by MSA using the estimates reported in Emrath (2002); and maintenance and depreciation are assumed to total 2.5 percent per year as in Harding, Rosenthal, and Sirmans (2007). We assume a loan-to-value ratio of 80 percent, the typical cutoff most lenders require to separate high versus low risk borrowers and meet Fannie Mae and Freddie Mac underwriting guidelines.[6]

One significant difference between the user cost formula in equation (1) and that used in Poterba (1984), Himmelberg, Mayer, and Sinai (2005), and others, is that we do not include a risk premium for owning, usually denoted b. The prior literature typically assumes a risk premium of 2.0 percent.[7] However, recent work by Sinai and Souleles (2005, 2013) and Paciorek and Sinai (2012) implies that when comparing owning with renting – rather than considering owning in a vacuum – the net risk premium could be zero for households on the margin of home buying and negative for inframarginal homeowners. Additionally, owning may be more or less expensive than renting because of differences in risk, and so we seek to estimate the cost difference rather than assume a risk premium.

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[6] Our results are not sensitive to this assumption since τ_{mtg} and τ_y are usually very close. We have replicated our analysis with LTV=0.69, the average for young households according to the 2004 Survey of Consumer Finances as reported in Poterba and Sinai (20YY), and LTV=0.387, which is the average for all households, and obtain very similar estimates.

[7] See Campbell et al (2009) for estimates of a time-varying risk premium.

Across longer horizons, we estimate the cost of owning using a more general formula:

$$CO_{t,t+k} = P_t - \frac{P_{t+k}}{\Pi_{j=t+1}^{t+k}(1+\delta_j)} + \sum_{i=t+1}^{t+k} \frac{(\tau_p + m)P_i}{\Pi_{j=t+1}^{i}(1+\delta_j)} \qquad (2)$$

where $\delta_j = LTV(1-\tau_{mtg})r_j + (1-LTV)(1-\tau_y)r_j$. Similarly, the cost of renting from year *t* to year *t+k* is equal to the sum of discounted rents from year *t* to *t+k* with values being discounted starting year *t+1*. We calculate the annual cost of renting over longer horizons with the following equation:

$$CR_{t,t+k} = \sum_{i=t+1}^{t+k} \frac{R_i}{\Pi_{j=t+1}^{i}(1+(1-\tau_y)r_j)} \qquad (3)$$

For our baseline tables, we will set the income and deduction tax rates equal to the national average federal income rate and state average state income tax rate as reported by the National Bureau of Economic Research. We acknowledge income tax rates vary across households within a housing market, and estimate the effects of varying the tax rate in Section 4. We will assume that the tax rate on housing capital gains, τ_{CG}, is always zero.[8]

Following the discussion in Section 2.1, we do not estimate the rent, R_t, or house price, P_t, with certainty. Instead, we observe $\widetilde{CR_{kt}} = CR_t + \mu_{kt}$ and $\widetilde{CO_{kt}} = CO_t + \omega_{kt}$ where the error terms represent noise in the process of extrapolating from rent and house price growth to the levels of those variables.

3 Predictability of Cost of Owning and Renting

We first estimate the relative costs of owning and renting assuming that a household is agnostic about future costs. In such case, the fraction of the time in the past that owning turned

[8] This is a common and reasonable assumption. For most of our sample period, married households enjoyed a capital gains tax exclusion of $500,000 ($250,000 for singles) on their primary residence. In the early to mid-1980s, the exclusion amount was lower and limited to older households. However, younger households could defer capital gains taxes by purchasing a more expensive house. In both cases, the effective tax rate typically was zero. In practice, the capital gains tax on housing collects little revenue. See Cunningham and Engelhardt (2008) and Shan (2011).

out to be less expensive than renting is the best predictor of whether owning will be less expensive than renting in the future.

Using the rent and price series developed in Section 2, we calculate the distribution of the difference between the costs of owning and renting for each of the sample 39 MSAs each year.[9] We treat each index as a separate observation; thus MSAs with less of a consensus across estimation methods will exhibit more variance in their relative costs of owning versus renting measures within a year.

The first column of Table 1 reports one statistic from the estimated distribution: the probability that the cost of owning is less than the cost of renting ($\widetilde{CO} < \widetilde{CR}$) across all measures and years by city. Overall, owning a house ends up being less expensive than renting during our sample period across all cities. Nevertheless, the odds vary considerably across MSAs. The first row reports the average across all MSAs, with the average in any given city in subsequent rows. The probability that the cost of owning was less than renting is about 65 percent across all cities and never less than 50 percent in any individual MSA. Owning was particularly dominant in Charlotte (it was cheaper 84 percent of the time), Atlanta (75 percent), and St. Louis (74 percent). It was least dominant in San Diego (51 percent), Seattle (53 percent), and Los Angeles (55 percent).

In Table 1, we also report the estimated saving from owning a home versus renting. We find that the average cost of owning over our sample period was less than the average rent across all MSAs. The first row of column 2 shows that, averaged across all MSAs, it was nearly 30 percent cheaper per year to own rather than to rent.[10] Ex-post savings from owning ranged from

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[9] We replicate all of our results restricting our rent and price series to 2 bedroom units, where rent and owned units have the most overlap. We find no significant difference in results. Comparable tables evaluating 2 bedroom units only are available in Appendix Tables 4-7.

[10] Savings are expressed as a percentage of average annual rent.

approximately 11 percent of annual rent (Cleveland) to 56 percent (Boston).[11] Those MSAs where owning was more likely to be cheaper than renting do not necessarily generate the greatest savings from owning, however. For example, owning was cheaper than renting 74 percent of the time in Columbus, Ohio, but generated an average savings of just 21 percent of rent. By contrast, owning in San Jose was cheaper than renting just 55 percent of the time, but generated a net savings of 53 percent of average rent.

The reason for this pattern is that the savings from renting when renting is cheaper may be bigger or smaller than the savings from owning when owning is cheaper. This reduces the correlation between whether owning is cheaper than renting and how much cheaper it is. In theory – though this does not happen in our data – renting could provide such a large savings during the fraction of the time that renting is cheaper than owning that it could swamp the savings from owning during the majority of the time when owning is less expensive. Instead, we see that, for a homeowner, the savings from owning when it was less expensive would have outweighed the losses from not renting when renting was cheaper, with the degree varying across MSAs. This pattern can be seen in columns 3 and 4, which calculate the average $\widetilde{CO} - \widetilde{CR}$ (expressed as a percentage of average annual rent) when it is cheaper to rent and cheaper to own respectively. On average, when the cost of owning is greater than the rent, it is approximately 81 percent more expensive than renting. When it is cheaper to own, owning costs about 74 percent less than renting. Across cities, the average owning premium ranged from 30 percent (Atlanta) to 156 percent (San Jose) in years when it was cheaper to rent ex post, and the average owning discount ranged from 37 percent (Columbus) to 188 percent (San Jose) when it is cheaper to own. (A discount of more than 100 percent can arise when the capital gain from owning a home for a year outstrips the annual carrying cost, yielding a profit.) Cities like

[11] We obtain qualitatively similar results when we express the savings as a percentage of house price, as in Case and Shiller (1989). Differences arise due to variation across MSAs in their price-to-rent ratios. The savings in high P/R MSAs appear somewhat larger relative to annual rent and somewhat smaller relative to purchase price.

Columbus had very similar savings from owning (37 percent) or renting (40 percent) when the respective mode was cheaper. San Francisco, by contrast, had a larger gap between owning (186 percent savings) and renting (165 percent savings). In a handful of cities – Austin, Indianapolis, and San Diego – the savings from renting in a year when renting was cheaper is much larger than the savings from owning in the years when owning was cheaper, yet owning was still cheaper on average because there were sufficiently few years where renting is the less expensive mode.

Overall, the results in Table 1 suggest that a household that planned never to move – so moving costs are not an issue – would save money, on average, in any MSA by always owning. Nonetheless, the wide dispersion in some MSAs between the average savings from owning when owning was cheaper, and the average savings from renting when renting was cheaper suggests that if a household could predict which tenure would be less expensive it could save money relative to always-owning or choosing its tenure mode randomly.

Predictability of house price changes suggests that the relative cost of owning and renting might also be forecastable, since owning costs decline when house price growth is greater. Indeed, a lengthy literature has found that house prices exhibit short-run momentum and long-run mean reversion, (Glaeser and Gyourko 2010). Still, predictability of the relative cost of owning and renting does not necessarily follow from predictability of house price changes because rents also vary over time. A negative correlation between house price changes and rent would make the relative cost of owning and renting less predictable than house prices alone, but a positive correlation would make the relative cost more predictable.

In addition, the binary rent/own prediction is different than the continuous cost-of-owning vs. rent forecast. In some ranges of the relative costs of owning and renting – say, when owning is much more expensive than renting – errors in the forecast of the relative cost of owning or renting would not affect which option is less expensive. In those ranges, when errors in estimated owning or renting costs are inframarginal, predictions of whether owning or renting is cheaper could be more robust than forecasts of the differences in cost. Conversely, when renting and owning costs are close, small forecasting errors can induce large swings in the rent versus own prediction.

Table 2 asks if, in a MSA in a particular year, owning is more or less expensive than renting, does it have predictive power for whether owning or renting is cheaper the next year? We estimate the following logit forecasting regression:

$$I(\widetilde{CO} - \widetilde{CR} > 0)_{i,t} = \beta_0 + \beta_1 I\left(\widetilde{CO} - \widetilde{CR} > 0\right)_{i,t-1} + \varepsilon_t \qquad (4)$$

The function $I(\widetilde{CO} - \widetilde{CR} > 0)_{i,t}$ equals one if the annualized cost of owning exceeds rent in city *i* and year *t*. The unit of observation is a city × year × price/rent estimation method. We cluster the standard errors by city × year to account for the correlation across estimation methods.[12]

We find that, in all cities in our sample, the lagged relative-cost indicator is positively correlated with the subsequent year's indicator for whether renting is cheaper. That is, years when renting is cheaper are more often than not followed by years when renting is cheaper. Likewise, if owning is cheaper, it is more likely to be cheaper again the next year. The first column of Table 2 pools the MSAs and controls for MSA fixed effects, thus using variation over time and within-MSA to identify β_1. The estimated logit coefficient of 2.4 (standard error of 0.11) corresponds to a 11 percent increase in the odds that owning will be more expensive than renting if it was more expensive than renting the prior year. The second column adds year dummies, so the identifying variation is due to changes in whether an MSA's cost of owning is above or below the rent for an equivalent property, after controlling for national changes in that

[12] Since CO incorporates the change in house prices, measurement error in P_{t-1} could potentially affect both the right- and left-hand-sides of the regression equation, inducing correlated measurement error. However, instrumenting for $I\left(\widetilde{CO} - \widetilde{CR} > 0\right)_{i,t-1}$ with $I\left(\widetilde{CO} - \widetilde{CR} > 0\right)_{i,t-2}$ had little effect on the estimates in columns 1 and 2 of Table 2. The MSA-specific coefficients in column 3 were affected more, but the qualitative results are the same. See Appendix Table 2.

relationship. The estimated logit coefficient, 2.5 (0.15), barely changes.[13] The lagged indicator variable $I(\widetilde{CO} - \widetilde{CR} > 0)_{i,t-1}$ contains the bulk of the MSA-level information, whether time-varying or not.

The last column of Table 2 estimates a separate logit regression, and thus a separate β_1, for each MSA. This column shows that in all MSAs, the lagged indicator for whether renting was cheaper than owning is positively correlated with renting being cheaper than owning the subsequent year. The greatest persistence is in Detroit (3.9 with a standard error of 0.07), where years when owning is above-average more expensive than renting are 50 percent more likely to repeat. The lowest persistence is found in Baltimore (1.3 with a standard error of 0.05), and is estimated to be just 3.7 percent.

One way to assess the quality of the model's fit is to examine the percent of the time the model correctly predicts the relative expense of owning versus renting. Results appear in Table 3, where each of the columns matches the corresponding specifications from Table 2. For the binary decision to own or rent a home for one year, our results show that on average the forecast is correct approximately 80 percent of the time and ranges from a high of 93 percent in San Diego to a low of 73 percent in Phoenix.

The reason for the model's fit can be seen in Figures 1 and 2. We estimate $(\widetilde{CO} - \widetilde{CR})_{i,t} = \gamma_0 + \gamma_1(\widetilde{CO} - \widetilde{CR})_{i,t-1} + \varepsilon_t$ and plot the actual ex post difference between the cost of owning and the rent versus the cost difference predicted by the model, for the entire distribution of cost of rent measures across all MSAs (in Figure 1) and selected cities (in Figure 2). Because this model predicts the serial correlation in cost differences, larger positive gaps in the cost of owning and renting are further to the northeast in the figures, whereas bigger negative gaps are further to the southwest. For the country as a whole, as well as in the selected cities, there is a

[13] Estimated coefficients on the MSA fixed effects are available from the authors, but are almost always statistically insignificant.

reasonably strong positive relationship between the actual and predicted cost difference. Nevertheless there is some noise, especially in a subset of cities. A perfect fit would lie along the 45-degree line. Instead, the predictions deviate from that.

Households have an easier job, however, as reflected by the specification in equation (6). They do not need to forecast how much more (or less) expensive owning might be than renting, but rather they need only to predict whether owning will be more or less expensive than renting. In Figures 1 and 2, when a point is in the top right or bottom left quadrants, the model gets that prediction right. Only in the northwest and southeast quadrants is the forecast in error. For example, in Figure 1, there is a point where the predicted cost of owning is only slightly less than the cost of renting, but the ex post realization was that the actual cost of owning was much less than rent. From the household's perspective, though, the model got it exactly right: owning was forecasted to be cheaper than renting and, in hindsight, it was. Accordingly, the corresponding data point lies in the bottom-left quadrant.

There are a couple of factors that can make it more reliable to predict whether to own or rent. In some cities, like Boston in Figure 2, the predicted relative costs of owning and renting are close to the actual, especially in the region around the origin where accuracy is especially important. In other cities, such as Philadelphia, owning tends to be cheaper most of the time, so the forecast almost always prescribes owning, and therefore it almost always is right.

4 Trading Strategy

While observed differences in the cost of owning and renting are predictable year to year, can households exploit this predictability to time the purchase of their homes? We estimate the possible savings from using past information to influence future home buying/renting decisions. We consider two approaches. First, we allow households to switch between owning and renting as many times as they want. Naturally, the savings from switching to the lowest-cost tenure mode will be offset – or, as prior research has typically found, overwhelmed – by the transactions costs of switching.[14] Second, we consider the savings possible when making just one rent-to-own transition, so the transactions cost is essentially sunk. The primary decision that typically falls into this category that of a first-time homebuyer determining whether to purchase a house in the current year or rent for one more year. We will evaluate the potential savings from optimizing that one-year decision in this section. It will turn out that a large benefit to timing the rent/own decision is that continuing to rent for one more year extends the option to rent again. We will show in the next section that the option to continue renting has been valuable historically.

We consider a household who is indifferent between owning and renting outside of the costs captured by our measures and has a one-year horizon for holding a home. We simulate several possible tenure choice rules and evaluate them based on how close they are to the lowest-possible-cost of housing, which comes from always correctly picking whether owning or renting is cheaper. The difference in the costs is reported in Table 4 and is expressed as a percentage of the average annual rent; a lower-cost strategy will exhibit a lower cost premium over the optimal tenure choices.

We first simulate a simple trading rule: buy if the model reported in column 2 of Table 3 predicts a household should own, and otherwise rent. We label this the "regression-based trading strategy" in Column 1 of Table 4. On average, over the 1980-2009 period, households would have paid just 6 percent of annual rent per year more than the lowest-cost-option. The cost premium varies across cities, ranging from just 1.6 percent in Atlanta to 17 percent in Houston.[15]

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[14] The institutional features of the real estate market can impose significant transaction costs. Given the heterogeneity in both property types and individual preferences, households often face high search frictions in buying a home (Wheaton, 1990; Stein, 1995; Krainer, 2001). Selling a home also typically involves hiring a broker and paying commission fees. Households also likely incur substantial pecuniary and non-pecuniary moving costs from switching from owning to renting.

[15] This savings is based on in-sample fit of the prediction model. We obtain very similar results when we predict out-of-sample by repeatedly fitting the regression model over rolling 10-year periods, always

The savings from the regression-based trading strategy can be seen by comparing its cost premium to a more naïve strategy, such as always owning (in column 3) or always renting (in column 5). Both naïve strategies are on average more expensive than the regression-based strategy in all cities. Across all MSAs, the cost premium for always owning averages almost 22 percent of annual rent and for always-renting the premium is almost 50 percent of annual rent. (This result follows from Table 1, which showed that on average it was cheaper to own than rent during this time period.) On average, then, the regression-based strategy saved more than 15 percent of rent annually vs. always owning and more than 40 percent of rent relative to always renting. The magnitude of the difference varies by MSA; always-owning is fairly close in cost to the optimal tenure choice in Atlanta and Charlotte, and so the regression-based strategy does not do much better. However, always-owning is especially expensive in Boston, Fort Lauderdale, Los Angeles, Palm Beach, Sacramento, and San Jose, so the regression-based strategy provides especially large savings in those cities.

Since many households may not be able to generate own/rent forecasts from a logit regression model, we consider an alternative strategy where a household simply chooses to own if owning was cheaper than renting in the prior year, and otherwise chooses to rent. For most cities, this simple strategy performs comparably to the more complex regression analysis. A comparison of columns 7 and 1 shows that, on average across all MSAs, the regression-based strategy saves just over 3 percent of rent more than the simple "last year" strategy. Only in a few of the most volatile housing markets – Los Angeles, Miami, San Diego, San Francisco, San Jose, and Tampa – does the regression-based strategy save considerably more than the simple "last-year" strategy. In a handful of housing markets – Austin, Dallas, Houston, and Seattle – the last-year strategy actually saved slightly more on average over the 1980-2009 period than the regression-based strategy.

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predicting the first post-sample year. However, this procedure limits our usable sample period, so we report use the in-sample prediction as our base case.

The even-numbered columns of Table 4 contain the standard deviations of the corresponding savings estimates. Recall that there are two sources of error in our estimates: Year-to-year variation from the average, and estimation of the levels of prices and rents. The standard deviations reported in this table include both sources. Column 2, which corresponds to the regression-based prediction, includes a third source of error: The prediction error from the model in Equation 4. In the standard deviations reported in Table 4, we assume that ε_t from Equation 4 is independent of the other errors listed above. The standard deviations in Table 4 reflect the certainty of the potential savings – trading strategies that are more consistently close to lowest-possible-cost optimum will have the lowest standard deviations. Thus, it is unsurprising that we find that the trading strategies and cities that are closest to the least-cost optimum also generally exhibit the lowest standard deviations.

The reason why one can save money trading on the serial correlation in housing costs may be seen in Figure 3, which plots the distribution of cost of owning minus cost of rent measures for a sample of cities from 1980-2009. The shaded areas represent the saving from renting, when the number is positive, or owning, when the number is negative. The optimal tenure mode would be to rent when the shaded area is above the x-axis and own when it is below. Deviations from that strategy pay the shaded area in the year of a "mistake." For example, the average cost of owning is less than the average cost of renting when the shaded area below the x-axis is larger than the area above the x-axis.

The top panel shows that in cities such as Boston, Los Angeles, and New York, the difference between the cost of owning and rent is volatile across the sample period. These cities have experienced long periods where either renting or owning was less expensive, year after year. During those periods, whether owning or renting was cheaper the prior year is a strong predictor of owning or renting being cheaper the following year. Occasionally there is a regime switch and the other tenure mode becomes less expensive, but the difference in the cost is usually quite small in the transition period. That is, a city is unlikely to go from one year where owning is significantly cheaper than renting to a subsequent year where owning is wildly more expensive than renting. Hence while the decision rule of "rent if renting was cheaper than owning last year" is wrong every so often, choosing the wrong tenure mode in that year is unlikely to be a costly mistake. The saving in the meantime from profiting from the persistence in relative own/rent costs easily outweighs the loss from being wrong once or twice a decade.

In other MSAs, $\widetilde{CO} - \widetilde{CR}$ remains relatively stable over time. The bottom panel of Figure 3 provides some examples, such as Chicago, Cleveland, and Denver. In fact in cities such as Atlanta and Charlotte, it is almost always cheaper to own from 1980 to 2009. In these cities, households almost never need to switch tenure mode to save money. However, the differences in annual cost between owning and renting are very slight.

Of course, the savings from predicting whether renting or owning would be cheaper and switching tenure mode to take advantage of cost differences comes at a cost – every move incurs transaction costs. These costs include paying realtors, searching for a new residence, and the pecuniary, time and psychic costs of moving. Indeed, the classic literature on the predictability of house prices concludes that transaction costs in the housing market are too high to profitably arbitrage house prices by buying and selling homes. However, the savings from arbitraging the own-rent gap may be larger than the returns on trading on house prices alone. In addition, those savings differ across cities, so a switching-tenure-mode strategy might make financial sense in some cities but not in others.

We find that at reasonable levels of transaction costs, a switching tenure mode based on the trading strategies described earlier, saves money on net relative to always-renting but not relative to always-owning in most cities. For each of our trading strategies – the regression-based strategy and the last-year strategy – we compute the break-even level of transaction costs as a percentage of the price of the house. Specifically, we compute the savings from the trading strategy over our sample period relative to a strategy of picking one tenure mode – renting or owning – and never moving. We express the savings as a percent of house price and divide it by the fraction of years that a household would have to move in order to implement the strategy. We choose to express the savings in terms of a break-even level of transaction costs rather than netting out an assumed rate of costs because transaction costs might vary across households. However, a 10 percent transaction cost (6 percent realtor fees and 4 percent other costs) is a commonly used threshold.

The results are reported in Table 5. The first two columns report the break-even level of transaction costs that could be covered by the regression-based strategy and the latter two columns correspond to the simple last-year strategy. In each pair of columns, the counterfactual in the first column is a strategy of always owning and in the second column is always renting. On average (the first row), the trading strategies save enough relative to always-owning that transaction costs could be between 5 and 6 percent of the average house price. This is not enough savings to cover even standard realtors' fees (which are typically 6 percent of the purchase price). However, relative to always-renting, a tenure mode-switching strategy saves enough to cover transaction costs of 15 to 17 percent of house price on average, enough to cover any reasonable assumption of transaction cost.[16]

The breakeven level of transaction cost varies across MSAs due to differences in the efficacy of the trading strategies and the number of moves implementation would require. Even relative to always owning, where an active strategy typically does save enough to cover transaction costs, households in Detroit, Los Angeles, Sacramento, and San Diego could afford double-digit trading costs and still come out ahead. Furthermore relative to always-renting,

[16] Due to the momentum in house prices seen in Figure 3, the trading strategies do not require very many switches (the average number of switches is four to five over a 29 year sample period). Thus, households can afford higher transaction costs because they do not incur them very often.

where forecasting usually provides a considerable benefit, it would still not be worthwhile for typical households in Memphis, Nashville, and perhaps Seattle to bother switching.

5 Predictability by Horizon

Thus far we have followed prior studies by focusing on the one-year cost of owning or renting a home. Most households, however, intend to live in a home for longer than a year. Thus this section extends our analyses the costs of owning and renting for longer horizons. Using the formula detailed in equations 2 and 3, we first calculate the costs of owning and cost of renting a home for five years (*k=5*) and ten years (*k=10*) across the variety of methodologies and property types described in Section 2. We then estimate the predictability of the relative cost of owning and renting over these longer horizons. Next, we extend our trading rule to multi-year horizons and show that when owning was cheaper over the prior five- or ten-year period, it predicts that owning is less expensive than renting over the subsequent period. Finally, we consider the case of chaining together sequential one-year rent/own decisions for households with multiple-year horizons.

Analogous to our analysis for the one-year costs, we perform the following logit regression of the binary decision to own or rent for the five and ten year horizon:

$$I(\widetilde{CO} - \widetilde{CR})_{i,t}^{k} = \delta_0 + \delta_1 I(\widetilde{CO} - \widetilde{CR})_{i,t-k}^{k} + \nu_t \qquad (8)$$

where $I(\widetilde{CO} - \widetilde{CR})_{i,t}^{k}$ is an indicator variable that equals 1 if the cost of owning exceeds the cost of renting ($\widetilde{CO} - \widetilde{CR} > 0$) over the next k years in a given city i and starting year t, and 0 otherwise. For example, $I(\widetilde{CO} - \widetilde{CR})_{i,1990}^{5} = 0$ means that it was cheaper for a household to own from 1990 to 1995, rather than rent during that five-year period. We use the *t-k* lag of the own/rent decision to predict the binary decision in current year *t,* since this is the most recent lag over which a household would have information about costs at a *k* horizon. For example, if a household with a five-year horizon was deciding whether to buy or rent in 1990, the most recent observable costs for a five-year tenure would start in 1985. Thus $I(\widetilde{CO}-\widetilde{CR})^{k}_{i,t-k}$ indicates whether it was ex-post cheaper for the household to own or rent for the next k years starting in year t-k.

Table 6 reports the estimated values of δ_1 for horizons of both five and ten years. Columns 1 and 2 perform the above binary regression for the five-year horizon, first on the pooled sample with city effects, and then with both city and year fixed effects. Columns 3 and 4 implement the same respective specifications for the ten-year tenure horizon.[17] Our results reveals that pooled estimates are still on average significant at longer horizons, but at smaller magnitudes than for the 1-year horizon. The magnitude of the coefficient estimate for the 5-year horizon is a fifth of the estimate for the 1-year horizon (0.5 versus 2.5). The pooled estimate of δ_1 for the 10-year horizon is slightly greater in magnitude than the five-year at approximately 0.93, though still less than half of the one-year horizon estimate. The exponent of the ten-year horizon estimate suggests that when it is cheaper to rent in the current year, it is likely to be cheaper in the following year by odds of approximately 2.53 to 1. Nonetheless, it is important to note that the forecasts for these longer horizons are estimated over smaller sample periods. Since our sample period ends in 2009, the final observable costs of a five and ten-year tenure occur in 2004 and 1999 respectively.

Using the estimated coefficients from Table 5, we forecast whether it was better to own or rent each year from 1980 given a five- and ten-year tenure horizon. We then compare the actual versus predicted results and calculate how often the predicted binary decision was correct. The binary prediction indicates whether a household with a five-year tenure horizon should start owning or renting in a given city and year, and we consider a forecast to be "correct" if it was indeed cheaper to own or rent throughout the next five years. Table 7 presents the share of years the in-sample prediction correctly forecasts the binary decision to own or rent by city. Each

[17] Though not shown, results are very similar if we exclude time and city fixed effects all together.

column corresponds to the respective specifications from Table 5. Column 1 shows that on average, from 1980 to 2004 the predicted decision from the pooled regression forecasts the own or rent decision correctly 82 percent of the time for a household with a five-year horizon. Column 3 shows that this predictability is even greater at the ten-year horizon, forecasting the binary decision correctly 93 percent of the time. This is driven by the fact it is more often better to own with a ten-year horizon. In some cities such as Atlanta, for example, it was cheaper to rent less than 3 percent of the time across the observed sample period and distribution of cost measures. Overall, the results show that if it was cheaper to own in a current year, it will be even more likely be cheaper to own again in the following year for households with longer horizons.

We then use these predictions to simulate a simple trading rule: buy if the above results indicate that it is cheaper to own for the next five (or ten) years, and otherwise rent. This trading strategy assumes that a household would have to buy or rent throughout the entire residence spell. As a performance benchmark, we estimate housing costs assuming that a household had perfect foresight and perfectly timed its shift from renting to owning (or owning to renting). We limit the household to just one switching of tenure mode to hold transactions costs essentially constant. For example, for a household with a five-year horizon, it may be ex-post less expensive to rent for the first two years of its tenure and then own a home for the remaining three years. Thus, for each MSA and year of our sample period, we calculate the least expensive tenure choice across our distribution of cost measures. We then use this option as a benchmark to compare the performance of different trading strategies.

The first column of Table 8 indicates how close to the perfect-foresight optimum a household with a five-year horizon could have gotten by using the regression-based tenure choice criteria over the 1980-2009 period. On average, the difference in cost would have equaled about six percent of annual rent. In many MSAs, the regression-based model was within five percent of rent of the optimum strategy. In other MSAs, such as Austin and San

Antonio Texas, annual ownership costs under this rule exceeded the optimum by more than 16 percent of annual rent on average. For most cities, however, always-owning (column 3) or always-renting (column 5), performs significantly worse: for example, the cost of always-owning in Boston exceeded the optimal strategy each year by about 23 percent of annual rent. Always-renting is worse, wasting about 52 percent of annual rent. Using the regression-based strategy in Boston would have moved a household within seven percent of annual rent of the perfect-foresight ideal.

Column 7 evaluates the "last year" strategy of owning over the next five years, if owning was cheaper over the prior five years. In most cities, this strategy unsurprisingly performed like the average of the always-own and always-rent approaches. Still, in a handful of cities where owning was almost always dominant – Atlanta, Charlotte, Cincinnati, Columbus, Kansas City, Sacramento, and Washington DC – this strategy did very well.

However, it may be unrealistic to think that a renter would commit to renting for 5 years. (Conversely, homeownership is generally an absorbing state.) Instead, a more realistic trading rule is one that says a household should own if owning was less expensive the prior year, and then continue to own for the remainder of its horizon. However, if renting is cheaper, the household should rent for another year and then reevaluate. If owning turns out to have been cheaper, the household should then own. But if not, it should rent again and reevaluate for another year. In this way, the household ends up renting for its entire horizon only if renting was cheaper than owning for each of the first *k-1* years.

This strategy compares very favorably to the perfect-foresight ideal and almost always does better than the regression-based strategy. Column 9 reports the average cost relative to optimal timing as a percentage of average annual rent. On average, the year-at-a-time strategy costs just five percent of annual rent more than the best-case housing cost. That is close to the regression-based cost from column 1.

A large portion of the savings from this strategy comes from the option to delay the switch from renting to owning for several years. That savings can be seen by comparing columns 7 and 9. Column 7's cost premium to the lowest-cost strategy assumes the household commits upfront to either renting for 5 years or owning for 5 years. In column 9, the household either owns for 5 years, or rents for one year and then reevaluates. The difference between the two columns, then, is due to the option to have an intermediate number of years of renting before entering into a home owning commitment. The savings is considerable – the average cost premium in column 7 is more than three times as large as the one in column 9. Indeed, the year-at-a-time strategy always is less expensive than the last-year strategy, and the greatest benefit accrues in the cities with cyclical housing markets such as San Francisco or Boston, where the commitment strategy does particularly poorly.

Table 9 evaluates the same trading strategies for households with a ten-year horizon. We define the predicted strategy based on the results for the ten-year horizon in Table 6, and similarly define the naïve strategy as owning if it was cheaper to own over the last ten years. Results for the 10-year horizon are qualitatively the same as the 5-year horizon, but the savings differences between trading strategies are wider in magnitude.

6 Effect of different tax rates

One of the more significant determinants of the relative cost of owning and renting is a household's tax bracket. From equations 2 and 3, holding the housing constant, rents do not vary with a household's tax rate but the annual cost of owning rises as the tax rate declines. This implies that the difference between the cost of owning and rent is greater for low income, low tax-rate households. The precise details of the U.S. tax treatment can be found in Poterba (1991) and Poterba and Sinai (2008).

In Table 10, we expand our analysis to consider how the own/rent balance varies across the income distribution. We obtain average tax rates on mortgage interest, property taxes, and income at different points in each city's income distribution. Using household-level micro data from the 1980, 1990, 2000, and 2010 Decennial Censuses, we compute the average income of households whose house values are between zero and 75 percent of the median house value in their city (tier 1), 75-100 percent of the median house value (tier 2), 100-125 percent of the median house value (tier 3), and more than 125 percent of the median house value (tier 4). It is worth underscoring that tiers have different meanings in different cities. For example, households in in San Francisco have higher average incomes than households in the same tier in Columbus, Ohio. For each household, we obtain the family structure from the Census and impute mortgage interest and opportunity cost of capital using the current 30-year mortgage interest rate. We linearly interpolate the households' characteristics incomes for each intervening year and use the National Bureau of Economic Research's TAXSIM tax model to impute state and federal taxes.

Table 10 reports the probability that owning is cheaper than renting for five categories of households: Four categories corresponding to each of the house value tiers, and one where we assume zero tax rates. Households in the highest house value tier in each city generally will find that owning is cheaper than renting (in column 5). However, it is a toss-up in Palm Beach and renting is slightly cheaper on average in San Diego. By contrast, a household with zero tax rates, and thus no tax subsidy to owner-occupied housing, would find that renting is less expensive most of the time in most cities (in column 1). In Milwaukee, for example, in the absence of taxes owning is cheaper than renting just one-third of the years between 1980 and 2009. (For the highest tier households there, owning is cheaper than renting nearly 68 percent of the time.) In the exceptions -- Austin, Charlotte, Detroit, Houston, Kansas City, Memphis, Miami, Nashville, Phoenix, Pittsburgh, San Antonio, and St. Louis – owning is cheaper than renting barely more than 50 percent in the absence of taxes, except for San Antonio where it is 59 percent.

The intervening columns vary on whether owning has typically been cheaper than renting or not, depending on the sensitivity of owning costs to the tax rate and on how the tax rates change across tiers. In some cities, like Austin, owning is always more likely to be cheaper than renting. In some, like Palm Beach, owning is typically more expensive in all tiers. Other cities, like Baltimore, exhibit large differences between the lowest tier (owning is cheaper 41 percent of the time) and the highest tier (owning is cheaper 65 percent of the time). By contrast, San Francisco ranges little, from 48 percent (lowest tier) to 54 percent (highest tier).

Whether owning or renting is cheaper on average due to taxes does not influence the efficacy of the owning/renting strategy – it just changes the frequency of the time that owning or renting are chosen. We have replicated Table 4 for the various income tiers and even in the bottom tier, which is the most different from our baseline results, the savings from the last-year and regression-based strategies are not affected much.[18] On average across all MSAs, the last-year strategy is just 7.9 percent of annual rent more expensive than the lowest-possible-cost housing (versus 8.1 in Table 4) and the regression-based strategy is just 5.9 percent more expensive. (The same as in Table 4.) We do see some differences across MSAs, but they are slight and for every MSA where the trading strategy is slightly less efficient, there is another where it is slightly more effective.

7 Conclusion

We examine 30 years of data from 39 metropolitan areas to see if households could successfully time the housing market when deciding whether to own or rent their homes. We find several home buying strategies that would have saved households money. First, simply

[18] Savings for the lowest income tier can be found in Table 11.

choosing homeownership when in the previous year owning was cheaper than renting saves 13 percent of annual rent on average relative to always-owning and 40 percent on average relative to always being a renter. The potential savings varies across cities, but it is always positive. While we can improve upon this simple rule of thumb with a regression model to predict whether to own or rent, in most cities the gains from the additional complexity are small.

This approach also extends to the first-time buyer with a multi-year holding period. If a household is otherwise indifferent between renting and owning, it should buy if owning was cheaper than renting the prior year. However, if it was not, it should rent another year. Not only does that save the difference between rent and ownership costs for that year, it preserves the option to keep renting. In other words, if the first-time homebuyer defers ownership for a year, it can defer it again if conditions warrant. Indeed, we find that if renting is chosen, the household should again evaluate whether to own or rent based on the relative cost in the prior year. In this manner, a first-time homebuyer will not buy a house until owning is finally cheaper than renting. We find that this option to keep renting is quite valuable and in many cities this strategy approaches the lowest-possible cost timing for becoming an owner.

The intuition behind these results is straightforward. It is by now well-established that house prices exhibit short-run persistence and long-run mean reversion. We find that statistical pattern is not undone by corresponding changes in rent. From the perspective of a potential homebuyer, this means that a year when owning is cheaper than renting tend to be followed by another year when owning is cheaper than renting. Thus, owning being less expensive than renting is a good signal to buy. When owning finally becomes more expensive than renting, it is time to sell. However, due to the slow-changing nature of house prices, owning initially is not much more expensive than renting, so the cost of getting the transition year wrong is low. Finally, because owning typically is cheaper than renting for many years in a row, not many moves are needed to arbitrage relative costs of owning and renting. Thus, even in the presence of high moving costs, it can be worthwhile in some cities to switch tenure mode.

This analysis emphasizes the importance of persistence in house price growth and the covariance of house price changes and rents in timing the home buying decision. Indeed, the differences between our results and those in other papers are due to these factors. For example, Glaeser and Gyourko (2010) conclude that waiting a year to own incurs considerable house price risk because the purchase price of the next year's housing is uncertain. However, they calibrate their model assuming no persistence in house prices; that persistence dampens the exposure to house price changes and increases the benefit from waiting a year. In the four cities in their data, Case and Shiller (1989) exploit the serial correlation in house price changes and find a benefit to waiting a year to buy if their model does not predict housing excess returns to be positive. However, they do not consider the additional benefit from potentially renting for multiple years.

In addition, we establish two empirical patterns that future research should try to explain. First, housing markets do not seem to be in equilibrium either with rents and ownership costs being equal ex post, or in the Poterba (1984) sense that *expected* annual housing costs equal rents. It is hard to reconcile the fact that in many cities owning is typically cheaper than renting, and the expected cost difference is positive, with an equilibrium that equates user cost and rent (Verbrugge, 2008). Second, the degree and frequency to which expected user cost is below rent varies across cities.

These patterns deserve investigation. Possible explanations include taxes – which we find to have a big effect on the relative costs of owning and renting.[19] Since tax rates are heterogenous across the population, the tax rate of the marginal homebuyer may differ across

[19] See also Diaz and Luengo-Prado (2008).

cities. Other possible explanations include differences in liquidity constraints, transaction costs, and risk.

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TABLE 1. SUMMARY STATISTICS OF RELATIVE COSTS OF OWNING V. RENTING

	Pr(CO>CR)	Average CO-CR (as % of CR)		
	Overall	Overall	if CO>CR	if CO<CR
All MSAs	65.1	-29.2	81.3	-73.7
Atlanta	74.8	-31.0	29.5	-46.3
Austin	69.6	-21.8	106.3	-61.4
Baltimore	64.8	-38.6	59.7	-84.0
Boston	57.5	-56.1	115.1	-155.0
Charlotte	83.8	-40.0	31.7	-49.8
Chicago	63.0	-14.3	69.2	-54.7
Cincinnati	68.4	-14.2	52.0	-38.2
Cleveland	64.7	-11.8	55.0	-40.2
Columbus	74.2	-21.1	40.7	-37.2
Dallas	63.4	-11.3	58.9	-39.6
Denver	57.0	-20.0	61.7	-71.3
Detroit	67.0	-19.7	88.5	-60.7
Fort Lauderdale	64.0	-36.8	99.7	-92.6
Houston	67.2	-15.2	91.9	-54.2
Indianapolis	71.9	-21.4	38.9	-38.2
Kansas City	74.2	-28.0	56.2	-49.0
Los Angeles	54.7	-45.2	154.1	-184.3
Memphis	71.9	-20.2	68.1	-45.8
Miami	65.0	-34.8	98.6	-98.4
Milwaukee	68.5	-15.2	60.3	-43.4
Minneapolis	70.2	-30.2	57.9	-60.8
Nashville	68.5	-26.5	44.8	-54.2
New York	58.5	-52.7	115.6	-137.2
Orlando	67.8	-35.5	72.9	-74.1
Palm Beach	55.5	-28.0	87.3	-97.8
Philadelphia	64.2	-40.5	51.9	-80.9
Phoenix	66.2	-33.1	109.4	-91.7
Pittsburgh	75.1	-25.2	57.9	-46.4
Portland	60.8	-31.6	71.9	-93.7
Richmond	70.0	-36.1	34.3	-59.8
Sacramento	55.9	-29.2	155.7	-146.7
San Antonio	69.6	-24.3	91.7	-61.1
San Diego	50.6	-34.1	132.7	-176.5
San Francisco	55.9	-49.9	164.6	-185.7
San Jose	54.6	-53.4	155.0	-187.6
Seattle	52.8	-20.2	84.2	-107.7
St. Louis	73.6	-29.7	38.9	-48.7
Tampa	65.4	-35.7	69.5	-79.2
Washington, D.C.	59.7	-48.5	80.9	-118.7

Note: The cost of owning is denoted by “CO”, and the cost of renting “CR”. Pr(CO>CR) is the probability that the owning is cheaper than renting. “Overall" is the average CO-CR across all cost measures and years in a given city. Cost measures are calculated by equation (4), assuming a loan-to-value constant of 0.8 and national average federal and state income tax for all tax rates.

TABLE 2. PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
All MSAs	2.38	2.52	
	(0.11)	(0.15)	
Atlanta			3.00
			(0.07)
Austin			2.18
			(0.06)
Baltimore			1.25
			(0.05)
Boston			2.73
			(0.06)
Charlotte			2.56
			(0.07)
Chicago			2.07
			(0.05)
Cincinnati			3.52
			(0.07)
Cleveland			3.07
			(0.06)
Columbus			2.85
			(0.06)
Dallas			3.15
			(0.06)
Denver			3.73
			(0.07)
Detroit			3.87
			(0.07)
Fort Lauderdale			1.46
			(0.05)
Houston			2.53
			(0.06)
Indianapolis			2.36
			(0.06)
Kansas City			2.58
			(0.06)
Los Angeles			2.67
			(0.06)
Memphis			1.77
			(0.06)
Miami			2.09
			(0.05)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.22	0.39	
Obs	314496	314496	8064

Note: Dependent variable is $I(CO\text{-}CR)_t$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

TABLE 2. (CONT'D) PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON

	Coef/SE of $(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
Milwaukee			2.98
			(0.06)
Minneapolis			2.18
			(0.06)
Nashville			1.83
			(0.05)
New York			3.16
			(0.06)
Orlando			2.04
			(0.05)
Palm Beach			1.76
			(0.05)
Philadelphia			1.68
			(0.05)
Phoenix			1.66
			(0.05)
Pittsburgh			1.45
			(0.06)
Portland			3.15
			(0.06)
Richmond			1.70
			(0.05)
Sacramento			2.90
			(0.06)
San Antonio			2.11
			(0.05)
San Diego			3.48
			(0.06)
San Francisco			2.10
			(0.05)
San Jose			1.63
			(0.05)
Seattle			2.72
			(0.06)
St. Louis			3.32
			(0.07)
Tampa			1.52
			(0.05)
Washington, D.C.			2.18
			(0.05)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.22	0.39	
Obs	314496	314496	8064

Note: Dependent variable is $I(CO\text{-}CR)_t$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

TABLE 3. FORECASTING THE 1-YEAR HORIZON OWN VERSUS RENT DECISION

	% of Time Forecast is Correct		
	(1)	(2)	(3)
All MSAs	78.7	82.5	78.8
Atlanta	84.9	87.6	84.9
Austin	77.6	76.9	77.6
Baltimore	67.6	74.6	67.6
Boston	79.7	86.9	79.7
Charlotte	85.9	84.8	85.9
Chicago	75.4	82.8	75.4
Cincinnati	86.8	90.3	86.8
Cleveland	83.5	85.8	83.5
Columbus	83.7	84.5	83.7
Dallas	83.5	79.0	83.5
Denver	86.5	87.4	86.5
Detroit	88.5	86.7	88.5
Fort Lauderdale	69.4	74.7	69.4
Houston	79.7	73.8	79.7
Indianapolis	79.6	80.6	79.6
Kansas City	82.5	84.0	82.5
Los Angeles	79.0	92.4	79.0
Memphis	75.7	78.4	75.7
Miami	75.5	79.8	75.5
Milwaukee	83.6	86.4	83.6
Minneapolis	78.5	80.8	78.5
Nashville	75.0	75.8	75.0
New York	83.2	89.5	83.2
Orlando	75.9	84.5	75.9
Palm Beach	70.8	79.7	70.8
Philadelphia	71.8	77.7	71.8
Phoenix	71.9	73.1	71.9
Pittsburgh	74.7	82.1	77.7
Portland	83.6	85.3	83.6
Richmond	74.4	82.9	74.4
Sacramento	81.1	83.0	81.1
San Antonio	76.8	76.9	76.8
San Diego	85.1	92.8	85.1
San Francisco	74.5	80.9	74.5
San Jose	69.4	85.7	69.4
Seattle	79.7	76.3	79.7
St. Louis	86.7	86.8	86.7
Tampa	71.2	79.8	71.2
Washington, D.C.	75.4	87.0	75.4
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Obs	314496	314496	8064

Note: This table calculates the percent of the time the predicted binary rent v. own decision was "correct", i.e. when it was actually better to own or rent. The columns correspond to the specifications in Table 2.

TABLE 4. ANNUAL SAVINGS FROM LEAST COSTLY BENCHMARK (% OF CR), 1980-2009, 1-YEAR

	v. Regression-Based		v. Always Owning		v. Always Renting		v. Last-Year Strategy	
	Savings	SD	Savings	SD	Savings	SD	Savings	SD
All MSAs	5.9	18.2	21.5	41.7	48.5	58.1	8.1	21.4
Atlanta	1.6	7.1	5.9	13.4	37.0	36.2	2.4	7.9
Austin	7.3	19.4	25.0	61.0	46.7	50.1	5.7	15.3
Baltimore	5.4	12.9	15.9	30.1	48.4	64.0	8.7	16.7
Boston	5.1	22.1	29.4	44.6	68.5	82.8	8.7	25.3
Charlotte	3.5	12.9	4.0	13.6	45.4	37.0	3.5	12.1
Chicago	2.4	8.7	18.3	35.4	29.9	33.8	4.2	9.9
Cincinnati	0.9	5.2	12.9	27.4	26.1	26.0	1.8	6.3
Cleveland	1.9	7.9	15.1	28.4	26.1	28.2	2.9	8.9
Columbus	4.1	16.7	8.0	20.8	27.9	26.4	4.4	16.3
Dallas	5.0	14.6	18.8	39.4	31.3	36.7	3.9	12.4
Denver	2.2	9.3	19.5	31.1	36.0	46.1	2.3	7.9
Detroit	4.4	18.5	27.2	53.1	49.3	53.3	4.3	18.5
Fort Lauderdale	7.3	17.7	30.4	73.5	68.9	91.3	9.3	19.1
Houston	17.0	38.1	28.7	50.5	46.5	46.6	11.4	31.4
Indianapolis	5.5	20.3	8.8	22.3	31.0	32.6	6.0	20.1
Kansas City	8.3	30.3	12.7	33.1	44.2	41.0	8.6	30.0
Los Angeles	2.1	10.1	41.7	68.6	71.5	90.7	8.4	21.3
Memphis	14.7	47.1	16.3	43.0	37.7	41.0	15.6	46.8
Miami	6.4	17.9	30.4	74.8	63.6	81.8	11.9	31.5
Milwaukee	2.4	9.5	14.9	31.3	28.7	29.3	3.2	10.0
Minneapolis	4.9	14.5	13.4	27.4	40.6	42.0	5.4	13.8
Nashville	7.5	19.4	11.9	24.1	36.9	35.7	7.7	18.6
New York	4.1	15.9	29.0	43.7	68.5	78.3	6.5	18.0
Orlando	2.7	10.0	20.2	49.9	57.5	84.2	10.1	30.5
Palm Beach	4.4	12.2	32.4	67.3	59.9	89.6	9.1	19.2
Philadelphia	7.7	19.9	15.5	27.4	55.5	63.3	9.8	20.9
Phoenix	14.0	30.5	29.9	60.6	61.0	92.5	15.5	32.6
Pittsburgh	6.8	24.1	13.3	35.9	42.0	41.1	10.9	27.1
Portland	4.4	14.5	21.6	35.8	47.0	56.6	4.5	12.8
Richmond	2.6	8.9	8.4	17.2	43.5	50.0	5.5	12.1
Sacramento	8.5	26.6	42.2	67.1	62.6	84.2	10.0	28.0
San Antonio	16.7	45.9	27.2	58.3	57.2	62.0	17.5	46.0
San Diego	1.7	7.9	40.4	58.8	62.9	80.8	8.9	24.7
San Francisco	5.9	16.0	35.3	49.9	62.8	80.1	12.8	30.3
San Jose	5.0	17.0	36.6	54.4	68.9	91.6	20.5	44.4
Seattle	9.3	23.4	25.6	42.4	39.1	66.2	7.2	19.1
St. Louis	2.3	10.0	8.7	20.9	39.5	36.4	2.3	8.8
Tampa	9.8	32.2	21.8	49.6	60.1	78.9	17.1	41.7
Washington, D.C.	3.8	12.9	22.7	41.5	61.4	78.5	7.8	17.5

Note: Each column reports the expected money saved from following the least costly benchmark (correctly forecasting) against the indicated trading strategy. "Regression-Based " indicates a trading strategy in which one rents or buys given the predicted strategy from specification (2) in tables 1 and 2. Always own and always rent is to apply a strategy of always owning or renting regardless of MSA or year. “Last-Year” refers to a strategy where one just owns or rent based on whether it was observed to be cheaper to own or rent in the previous year.

TABLE 5. TRANSACTION COST PER RENT-OWN SWITCH BEFORE ELIMINATE SAVINGS, 1980-2009

	Transaction Cost Per Switch (as % of P)			
	Regression-Based v. Always Owning	Regression-Based v. Always Renting	Last-Year v. Always Owning	Last-Year v. Always Renting
All MSAs	5.8	16.9	4.7	14.8
Atlanta	1.8	14.8	1.8	17.6
Austin	9.6	21.2	7.0	14.6
Baltimore	3.1	12.3	1.6	8.8
Boston	11.5	27.9	7.5	20.2
Charlotte	0.2	21.1	0.2	25.5
Chicago	7.5	15.6	3.4	7.7
Cincinnati	6.4	16.6	5.0	13.7
Cleveland	6.7	15.9	4.2	10.5
Columbus	1.9	11.3	1.6	10.4
Dallas	5.8	10.6	6.7	11.9
Denver	10.1	19.0	9.8	18.4
Detroit	13.5	28.8	13.6	28.9
Fort Lauderdale	5.8	14.1	5.1	13.1
Houston	2.9	9.2	5.5	12.8
Indianapolis	1.7	11.8	1.1	8.4
Kansas City	0.6	13.6	0.5	15.1
Los Angeles	16.0	27.5	11.8	21.8
Memphis	-0.8	7.9	-1.0	7.1
Miami	7.8	17.6	5.6	14.6
Milwaukee	5.9	13.7	4.8	11.5
Minneapolis	2.4	12.3	2.2	12.1
Nashville	0.3	8.5	0.3	9.5
New York	11.3	29.6	9.7	27.1
Orlando	6.1	18.5	3.1	14.1
Palm Beach	8.5	15.9	5.9	12.2
Philadelphia	2.5	14.7	1.5	11.7
Phoenix	4.2	11.7	3.8	11.3
Pittsburgh	0.7	25.3	-0.9	9.4
Portland	6.8	19.6	6.6	19.2
Richmond	2.1	17.4	0.6	11.5
Sacramento	8.5	13.5	12.6	20.5
San Antonio	4.1	15.7	3.1	12.4
San Diego	13.2	21.7	15.0	26.9
San Francisco	7.4	14.6	6.4	14.5
San Jose	12.5	24.2	3.9	11.1
Seattle	2.6	6.8	4.8	11.6
St. Louis	2.7	18.9	3.1	22.0
Tampa	3.8	18.2	0.8	11.0
Washington, D.C.	7.3	21.7	4.5	15.8

Note: Each column reports average transaction cost each rent/own switch would have to equal in order completely eliminate savings from the predicted strategy versus the more naïve strategy in each column.. "Predicted" indicates a trading strategy in which one rents or buys given the predicted strategy from specification (2) in tables 1 and 2. Always own and always rent is to apply a strategy of always owning or renting regardless of MSA or year. “Last-year” strategy is to buy or rent based on whether it was better to buy or rent in previous year.

TABLE 6. PREDICTING THE 5-YEAR AND 10-YEAR HORIZON OWN VERSUS RENT DECISION

	Coef/SE of $I(CO\text{-}CR)_{t-1}$			
	5-Year Horizon		10-Year Horizon	
	(1)	(2)	(3)	(4)
(Pooled)				
All MSAs	0.45	0.48	0.74	0.94
	(0.13)	(0.15)	(0.25)	(0.23)
(MSA Dummies)				
Atlanta	-1.61	-0.59	-4.79	-5.66
	(0.36)	(0.39)	(0.56)	(0.71)
Austin	-1.40	-0.41	-4.79	-5.77
	(0.49)	(0.68)	(0.39)	(0.93)
Baltimore	-1.24	-0.05	-1.88	-2.38
	(0.40)	(0.48)	(0.58)	(0.60)
Boston	-0.37	1.11	-2.77	-3.21
	(0.44)	(0.43)	(1.05)	(0.88)
Charlotte	-2.46	-1.53	-3.75	-4.54
	(0.35)	(0.39)	(0.23)	(0.62)
Chicago	-1.57	-0.48	-2.56	-3.38
	(0.37)	(0.42)	(0.45)	(0.58)
Cincinnati	-2.23	-1.30	-2.80	-3.77
	(0.24)	(0.38)	(0.21)	(0.91)
Cleveland	-1.75	-0.75	-1.78	-2.41
	(0.35)	(0.55)	(0.29)	(1.03)
Columbus	-2.08	-1.13	-2.55	-3.35
	(0.29)	(0.43)	(0.23)	(0.88)
Dallas	-0.90	0.31	-3.15	-3.99
	(0.36)	(0.47)	(0.35)	(0.69)
Denver	-1.06	0.04	-5.58	-6.70
	(0.42)	(0.65)	(0.83)	(1.24)
Detroit	-1.59	-0.57	-1.94	-2.58
	(0.51)	(0.71)	(0.67)	(1.47)
Fort Lauderdale	-1.68	-0.68	-3.88	-4.80
	(0.31)	(0.33)	(0.69)	(0.70)
Houston	-1.56	-0.59	-3.88	-4.90
	(0.37)	(0.51)	(0.32)	(0.74)
Indianapolis	-1.75	-0.74	-2.05	-2.71
	(0.18)	(0.37)	(0.15)	(0.82)
Kansas City	-2.03	-1.10	-4.80	-5.87
	(0.31)	(0.36)	(0.61)	(0.79)
Los Angeles	-0.86	0.45	-1.07	-1.33
	(0.49)	(0.52)	(0.77)	(0.78)
Memphis	-1.42	-0.37	-2.74	-3.55
	(0.29)	(0.40)	(0.16)	(0.76)
Miami	-2.46	-1.59	-4.77	-5.89
	(0.33)	(0.42)	(0.69)	(0.72)
MSA Fixed Effects	YES	YES	YES	YES
Time Fixed Effects		YES		YES
Pseudo R^2	0.06	0.24	0.13	0.29
Obs	224640	224640	112320	112320

Note: Dependent variable is $I(CO\text{-}CR)_{t-k}$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Each column performs pooled regressions with different specifications.

TABLE 6 (CONT'D). PREDICTING THE 5-YEAR AND 10-YEAR HORIZON OWN VERSUS RENT DECISION

	Coef/SE of $I(CO\text{-}CR)_{t-1}$			
	5-Year Horizon		10-Year Horizon	
	(1)	(2)	(3)	(4)
(MSA Dummies)				
Milwaukee	-2.65	-1.77	-3.49	-4.59
	(0.41)	(0.54)	(0.36)	(0.97)
Minneapolis	-1.82	-0.86	-7.55	-8.58
	(0.40)	(0.48)	(0.65)	(0.84)
Nashville	-1.49	-0.46	-3.77	-4.68
	(0.43)	(0.53)	(0.19)	(0.74)
New York	-0.52	0.90	-1.93	-2.29
	(0.41)	(0.41)	(0.75)	(0.67)
Orlando	-1.47	-0.43	-3.56	-4.38
	(0.29)	(0.35)	(0.50)	(0.63)
Palm Beach	-1.08	0.09	-3.21	-4.04
	(0.32)	(0.31)	(0.66)	(0.68)
Philadelphia	-0.99	0.27	-1.64	-2.04
	(0.41)	(0.47)	(0.59)	(0.61)
Phoenix	-1.32	-0.29	-5.41	-6.40
	(0.45)	(0.57)	(0.69)	(0.72)
Pittsburgh	-2.19	-1.21	-2.19	-2.88
	(0.22)	(0.39)	(0.15)	(0.72)
Portland	-3.16	-2.35	-6.11	-7.40
	(0.46)	(0.65)	(0.68)	(1.16)
Richmond	-2.02	-1.02	-3.22	-4.05
	(0.34)	(0.36)	(0.49)	(0.60)
Sacramento	-1.03	0.23	-1.52	-1.92
	(0.52)	(0.61)	(0.68)	(0.67)
San Antonio	-1.49	-0.51	-3.76	-4.72
	(0.45)	(0.62)	(0.27)	(0.78)
San Diego	-0.90	0.39	-1.99	-2.44
	(0.50)	(0.54)	(0.83)	(0.74)
San Francisco	-0.91	0.36	-2.64	-3.19
	(0.49)	(0.49)	(1.09)	(0.96)
San Jose	-1.18	0.01	-3.10	-3.73
	(0.52)	(0.54)	(1.03)	(0.89)
Seattle	-1.77	-0.73	-2.75	-3.69
	(0.57)	(0.63)	(0.41)	(0.70)
St. Louis	-1.77	-0.77	-3.76	-4.62
	(0.32)	(0.34)	(0.45)	(0.61)
Tampa	-1.57	-0.56	-4.21	-5.08
	(0.32)	(0.37)	(0.55)	(0.63)
Washington, D.C.	-0.96	0.32	-1.87	-2.33
	(0.45)	(0.52)	(0.72)	(0.66)
MSA Fixed Effects	YES	YES	YES	YES
Time Fixed Effects		YES		YES
Pseudo R^2	0.06	0.24	0.13	0.29
Obs	224640	224640	112320	112320

Note: Dependent variable is $I(CO\text{-}CR)_{t-k}$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Each column performs pooled regressions with different specifications.

TABLE 7. FORECASTING THE 5-YEAR AND 10-YEAR HORIZON OWN VERSUS RENT DECISION

	% of Time Forecast is Correct			
	5-Year Horizon		10-Year Horizon	
	(1)	(2)	(3)	(4)
All MSAs	78.2	82.1	92.2	92.9
Atlanta	77.6	68.5	98.5	98.5
Austin	74.5	77.7	84.4	87.0
Baltimore	42.4	84.4	91.4	91.4
Boston	91.7	91.7	97.5	97.5
Charlotte	80.8	80.8	91.6	91.6
Chicago	89.2	89.2	92.8	92.8
Cincinnati	83.8	83.8	83.4	75.3
Cleveland	88.1	88.1	91.6	91.6
Columbus	66.0	70.5	92.4	92.4
Dallas	70.3	60.0	99.4	99.4
Denver	82.0	82.0	86.5	82.8
Detroit	82.0	83.0	97.1	97.1
Fort Lauderdale	79.3	77.3	96.5	96.5
Houston	83.9	83.9	86.6	85.7
Indianapolis	87.0	87.0	98.9	98.9
Kansas City	66.3	75.4	69.2	89.2
Los Angeles	78.2	80.6	91.7	91.7
Memphis	91.0	91.0	98.8	98.8
Miami	92.6	92.6	96.3	96.3
Milwaukee	84.6	84.6	99.9	99.9
Minneapolis	79.6	80.3	96.9	96.9
Nashville	58.3	90.3	82.5	88.4
New York	78.6	82.6	95.9	95.9
Orlando	70.3	87.3	93.8	93.8
Palm Beach	69.8	73.8	80.9	85.5
Philadelphia	75.8	81.3	99.3	99.3
Phoenix	88.9	88.9	88.3	86.4
Pittsburgh	95.3	95.3	99.7	99.7
Portland	87.0	87.0	95.3	95.3
Richmond	69.3	60.1	79.7	86.8
Sacramento	78.3	70.3	96.0	96.0
San Antonio	67.0	80.2	85.0	85.6
San Diego	67.6	91.8	91.7	91.7
San Francisco	73.7	87.1	95.1	95.1
San Jose	82.8	82.8	93.1	93.1
Seattle	84.1	84.1	97.0	97.0
St. Louis	80.6	82.7	97.9	97.9
Tampa	68.8	82.3	84.3	90.8
Washington, D.C.	80.6	81.8	97.9	95.0
MSA Fixed Effects	YES	YES	YES	YES
Time Fixed Effects		YES		YES
Obs	224640	224640	112320	112320

Notes: See Table 3 notes.

TABLE 8. AVERAGE ANNUAL SAVINGS FROM LEAST COSTLY BENCHMARK, 5-YEAR HORIZON, 1980-2009

	Annual Savings from Least Costly Tenure Choice (as % of CR)				
	v. Regression -Based	v. Always Owning	v. Always Renting	v. Last-Year Strategy	v. 1-Year Strategy
All MSAs	6.4	7.8	54.1	17.9	4.7
Atlanta	2.9	3.2	42.1	6.7	2.4
Austin	23.7	19.1	47.5	28.5	8.9
Baltimore	4.3	5.3	57.3	18.2	4.7
Boston	6.9	23.0	51.7	40.0	11.1
Charlotte	1.1	1.1	46.4	3.1	1.0
Chicago	2.4	2.4	32.7	11.0	2.3
Cincinnati	1.2	1.2	32.3	4.9	1.4
Cleveland	2.5	2.5	31.6	6.7	2.4
Columbus	1.4	1.4	31.7	2.9	1.3
Dallas	11.1	14.9	34.9	11.2	4.6
Denver	21.0	10.9	49.0	19.0	2.4
Detroit	7.8	7.8	58.1	19.1	5.7
Fort Lauderdale	3.0	3.3	82.2	6.3	3.8
Houston	8.6	7.9	50.3	9.8	2.7
Indianapolis	2.3	2.3	31.0	2.6	1.6
Kansas City	2.5	2.5	53.1	4.8	1.5
Los Angeles	16.1	23.0	84.7	53.2	11.2
Memphis	3.8	4.4	33.6	5.0	3.2
Miami	1.6	1.6	79.4	6.7	3.4
Milwaukee	0.8	0.8	36.3	6.4	1.1
Minneapolis	2.3	2.3	51.7	4.0	2.0
Nashville	5.1	5.4	36.2	12.6	3.7
New York	3.9	22.4	54.2	32.8	9.3
Orlando	3.3	4.2	66.6	5.4	2.6
Palm Beach	2.1	6.6	72.3	10.3	5.8
Philadelphia	7.9	9.0	53.7	19.2	4.7
Phoenix	6.2	8.1	65.7	12.2	5.4
Pittsburgh	1.7	1.7	43.5	4.3	2.9
Portland	2.1	2.1	63.7	22.2	2.0
Richmond	2.0	2.0	53.1	5.2	2.5
Sacramento	26.3	16.4	77.5	57.8	10.0
San Antonio	16.8	12.8	54.9	15.4	9.8
San Diego	11.5	18.4	80.2	56.3	10.5
San Francisco	7.2	17.4	68.2	55.9	9.9
San Jose	8.9	15.7	70.8	51.7	11.6
Seattle	5.3	5.3	48.1	29.0	4.8
St. Louis	2.7	2.7	42.6	4.1	1.3
Tampa	3.3	3.7	69.6	4.9	2.5
Washington, D.C.	5.2	9.4	69.6	28.5	6.3

Note: Each column reports the expected money saved if a household was able to follow the least costly tenure choice perfectly against the indicated trading strategy. "Regression-Based" indicates a trading strategy in which one rents or buys given the predicted strategy from specification (2) in Table 5. Always own and always rent is to apply a strategy of always owning or renting regardless of MSA or year. "1-year strategy" refers to a decision rule in which a household decides to start owning for the rest of the tenure choice horizon if the observed 1-year cost of owning was cheaper than the cost of rent in the previous year. If not, then the household will continue to rent until the 1-year costs indicate it was cheaper to own in the prior year.

TABLE 9. AVERAGE ANNUAL SAVINGS FROM LEAST COSTLY BENCHMARK WITH 10-YEAR HORIZON, 1980-2009

	Annual Savings from Least Costly Tenure Choice (as % of CR)				
	v. Regression-Based	v. Always Owning	v. Always Renting	v. Last-Year Strategy	v. 1-Year Strategy
All MSAs	2.1	5.7	50.2	15.1	3.9
Atlanta	0.2	1.8	45.5	6.2	1.8
Austin	0.1	9.9	48.1	30.2	5.1
Baltimore	2.2	4.8	43.8	10.9	4.4
Boston	5.0	17.9	53.0	34.3	9.3
Charlotte	0.1	0.7	46.5	0.5	0.8
Chicago	0.9	1.6	28.1	1.8	1.7
Cincinnati	0.3	0.7	33.2	2.7	1.0
Cleveland	2.0	1.3	31.7	2.2	1.6
Columbus	0.4	0.8	32.7	1.0	0.9
Dallas	0.8	9.3	34.4	23.7	4.4
Denver	0.1	5.5	58.8	31.9	0.7
Detroit	2.2	2.1	59.8	4.1	2.4
Fort Lauderdale	0.7	2.2	74.1	18.1	3.0
Houston	0.5	4.5	48.5	24.7	3.2
Indianapolis	0.9	1.7	30.9	0.6	1.5
Kansas City	0.1	1.2	56.9	8.5	0.8
Los Angeles	8.7	21.9	62.9	43.5	13.0
Memphis	0.6	2.7	33.5	4.3	2.5
Miami	0.5	1.0	71.2	12.4	2.7
Milwaukee	0.2	0.3	35.5	3.2	0.6
Minneapolis	0.1	0.7	57.0	9.7	0.6
Nashville	0.3	3.0	36.2	5.1	2.6
New York	5.2	20.4	50.2	36.0	11.2
Orlando	0.7	3.2	58.1	18.0	2.3
Palm Beach	1.3	4.7	62.7	28.0	4.9
Philadelphia	2.6	7.3	40.3	13.4	5.3
Phoenix	0.2	4.2	62.4	28.2	3.1
Pittsburgh	1.0	1.3	40.1	0.9	2.0
Portland	0.3	1.4	61.4	8.9	1.2
Richmond	0.5	1.5	45.7	4.6	2.0
Sacramento	8.3	13.5	59.0	19.4	7.4
San Antonio	0.3	5.9	48.1	24.9	6.1
San Diego	10.3	17.3	71.0	33.3	9.8
San Francisco	9.0	16.6	64.3	27.1	9.7
San Jose	7.7	13.3	69.0	16.8	8.0
Seattle	2.2	3.1	38.4	3.4	3.7
St. Louis	0.2	1.6	42.4	4.5	1.2
Tampa	0.5	2.6	65.3	19.0	2.4
Washington, D.C.	4.1	9.0	56.4	20.8	6.8

Note: This table reports the same as Table 8 except for the 10-year horizon. Each column reports the expected money saved if a household was able to follow the least costly tenure choice perfectly against the indicated trading strategy.

TABLE 10. PROBABILITY OWNING IS CHEAPER BY INCOME TIER, 1-YEAR HORIZON

	PR(CO<CR)				
	Zero Tax Baseline	Low House Value Tier	Tier 2	Tier 3	High House Value Tier
All MSAs	45.8	47.5	51.6	54.6	61.9
Atlanta	45.7	47.4	58.4	64.7	74.9
Austin	50.3	51.4	56.1	59.3	65.6
Baltimore	39.7	41.1	49.9	56.2	64.6
Boston	47.4	49.1	51.7	53.3	56.2
Charlotte	54.3	56.1	61.4	66.5	80.7
Chicago	38.7	40.2	47.1	54.5	62.8
Cincinnati	36.1	39.4	43.7	48.0	62.0
Cleveland	35.1	36.3	39.2	43.3	57.3
Columbus	35.9	37.5	42.3	48.8	65.2
Dallas	41.7	42.9	46.6	50.0	59.0
Denver	39.0	40.9	46.5	49.3	56.5
Detroit	51.1	52.1	55.1	57.6	64.5
Fort Lauderdale	49.9	50.5	52.2	54.0	59.8
Houston	56.9	57.6	57.8	58.8	62.6
Indianapolis	42.4	44.3	47.1	50.9	64.2
Kansas City	53.5	55.0	59.2	63.6	73.2
Los Angeles	45.9	47.4	49.1	50.4	53.2
Memphis	52.3	53.3	56.2	58.1	68.9
Miami	52.2	52.7	53.5	55.1	61.0
Milwaukee	33.7	35.8	48.4	57.3	67.7
Minneapolis	41.9	45.3	54.9	61.4	69.9
Nashville	53.3	54.3	56.5	58.3	64.2
New York	47.4	49.7	53.3	54.3	57.4
Orlando	48.0	48.8	52.3	54.4	62.2
Palm Beach	42.7	43.4	44.9	45.5	50.0
Philadelphia	47.2	48.1	51.5	54.4	61.9
Phoenix	51.7	54.1	57.9	59.8	65.2
Pittsburgh	53.0	54.4	55.0	56.5	64.9
Portland	45.4	46.6	52.0	52.8	58.4
Richmond	46.1	47.7	54.6	59.0	69.4
Sacramento	42.1	42.7	46.2	48.1	51.8
San Antonio	59.3	59.5	60.3	60.7	63.8
San Diego	45.4	46.1	48.0	48.6	49.6
San Francisco	43.2	47.6	50.7	52.0	53.8
San Jose	41.0	48.0	52.4	53.1	54.5
Seattle	38.6	39.2	43.4	47.1	51.5
St. Louis	50.2	51.6	55.4	59.5	70.2
Tampa	47.1	47.5	48.7	50.2	57.1
Washington, D.C.	40.7	46.5	54.3	55.8	58.2

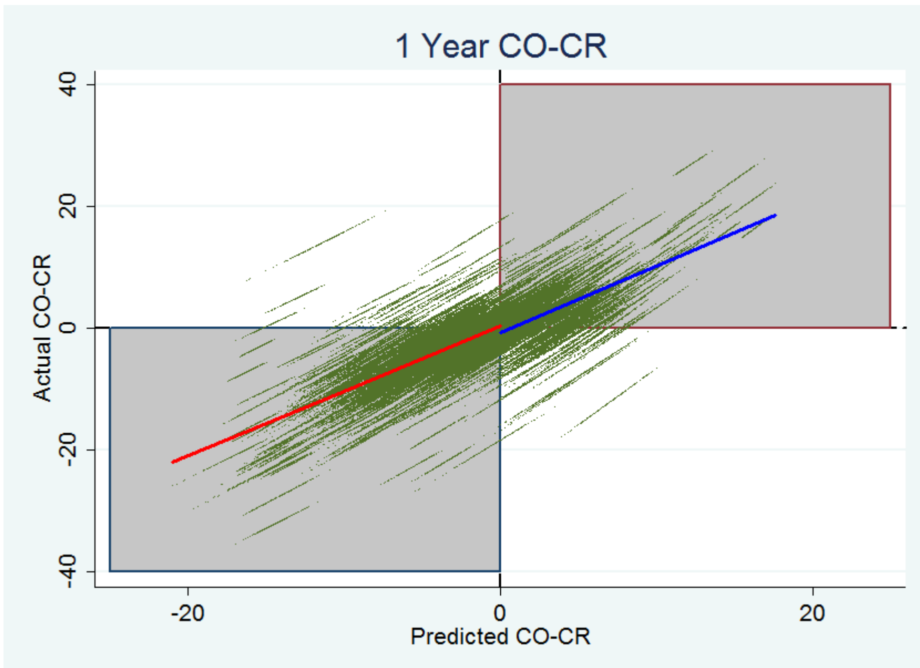
Note: This table reports the probability owning is cheaper than renting by house value tiers. “Zero Tax Baseline” is the PR(CO<CR) assuming no taxes or deductions. “Low House Value Tier” corresponds to households with house values to 0.75*median house value or lower, Tier 2 households have house values between the low and the median, Tier 3 house values are between median and 1.25*median, and “High House Value Tier” are those above 1.25*median house value. We assume loan-to-value equals 0.8 in these calculations.

TABLE 11. AVERAGE ANNUAL SAVINGS FROM LEAST COSTLY BENCHMARK WITH 1-YEAR HORIZON, LOWEST INCOME TIER, 1980-2009

	Annual Savings from Least Costly Tenure Choice (as % of CR)							
	v. Predicted		v. Always Owning		v. Always Renting		v. Last-Year Strategy	
	Savings	SD	Savings	SD	Savings	SD	Savings	SD
All MSAs	5.9	17.8	35.8	52.8	31.6	49.1	7.9	21.6
Atlanta	1.4	6.8	20.2	26.7	18.1	27.6	2.8	9.9
Austin	11.4	23.4	36.2	68.4	29.1	41.9	8.3	17.6
Baltimore	2.6	10.4	32.3	40.9	31.3	56.4	5.9	15.9
Boston	6.5	22.0	41.1	52.8	53.3	72.0	8.6	22.8
Charlotte	3.8	12.5	15.5	27.9	21.8	29.0	4.0	11.6
Chicago	3.0	10.1	34.4	47.4	14.7	23.8	3.9	11.4
Cincinnati	2.0	7.5	30.2	43.7	8.9	16.0	2.4	7.8
Cleveland	4.1	11.9	34.1	45.2	8.9	17.1	4.7	12.4
Columbus	2.5	9.1	25.1	35.6	8.6	16.1	2.5	8.9
Dallas	3.5	11.7	33.2	48.0	17.5	28.8	2.7	10.5
Denver	2.5	9.1	34.1	41.8	21.0	35.6	1.7	7.8
Detroit	3.6	13.8	41.5	65.1	29.1	41.6	3.1	13.2
Fort Lauderdale	7.0	20.7	42.4	79.0	53.0	83.7	10.2	22.9
Houston	9.5	26.4	40.8	63.9	30.1	36.4	10.6	27.3
Indianapolis	5.3	21.1	21.9	34.8	13.7	22.7	6.4	21.6
Kansas City	4.1	20.6	24.8	45.8	24.5	32.1	4.0	20.6
Los Angeles	2.0	8.9	58.0	76.5	55.6	81.9	7.2	22.7
Memphis	12.2	39.8	27.0	53.7	21.8	32.6	12.3	39.8
Miami	9.5	22.5	42.4	80.7	47.9	75.4	15.9	36.1
Milwaukee	2.3	8.7	34.9	47.8	9.6	19.0	2.2	9.1
Minneapolis	2.0	8.5	28.3	38.8	21.1	31.9	3.6	11.6
Nashville	6.6	17.7	22.7	37.0	20.9	27.1	7.6	18.1
New York	6.7	21.7	43.5	55.4	50.1	65.4	7.9	23.0
Orlando	3.3	10.7	32.0	55.3	41.4	78.7	9.7	32.4
Palm Beach	4.7	14.7	46.7	72.5	46.3	82.6	9.9	23.1
Philadelphia	5.1	14.9	29.3	39.4	37.1	53.2	6.6	16.2
Phoenix	11.1	32.3	41.6	69.1	42.0	86.0	12.4	32.7
Pittsburgh	7.8	21.1	25.5	50.0	22.7	32.1	10.4	23.6
Portland	8.0	24.4	41.9	55.1	27.9	45.4	5.4	17.9
Richmond	4.1	12.1	24.1	32.2	25.8	42.6	5.2	12.5
Sacramento	9.4	25.5	62.9	76.3	45.7	73.4	10.9	33.7
San Antonio	22.9	57.4	38.6	68.7	39.4	50.8	23.3	57.4
San Diego	2.2	10.6	59.8	69.7	46.9	69.9	9.1	28.6
San Francisco	4.8	14.7	47.7	59.0	51.3	72.6	12.9	34.6
San Jose	9.4	24.9	48.9	63.3	56.2	85.7	23.5	50.0
Seattle	7.6	19.5	40.9	52.7	28.2	58.7	8.0	20.7
St. Louis	1.9	7.9	21.2	34.5	19.8	27.2	1.8	7.7
Tampa	8.7	27.7	34.7	56.8	44.7	70.9	12.9	32.9
Washington, D.C.	3.8	12.1	35.9	49.5	48.1	71.0	5.8	15.5

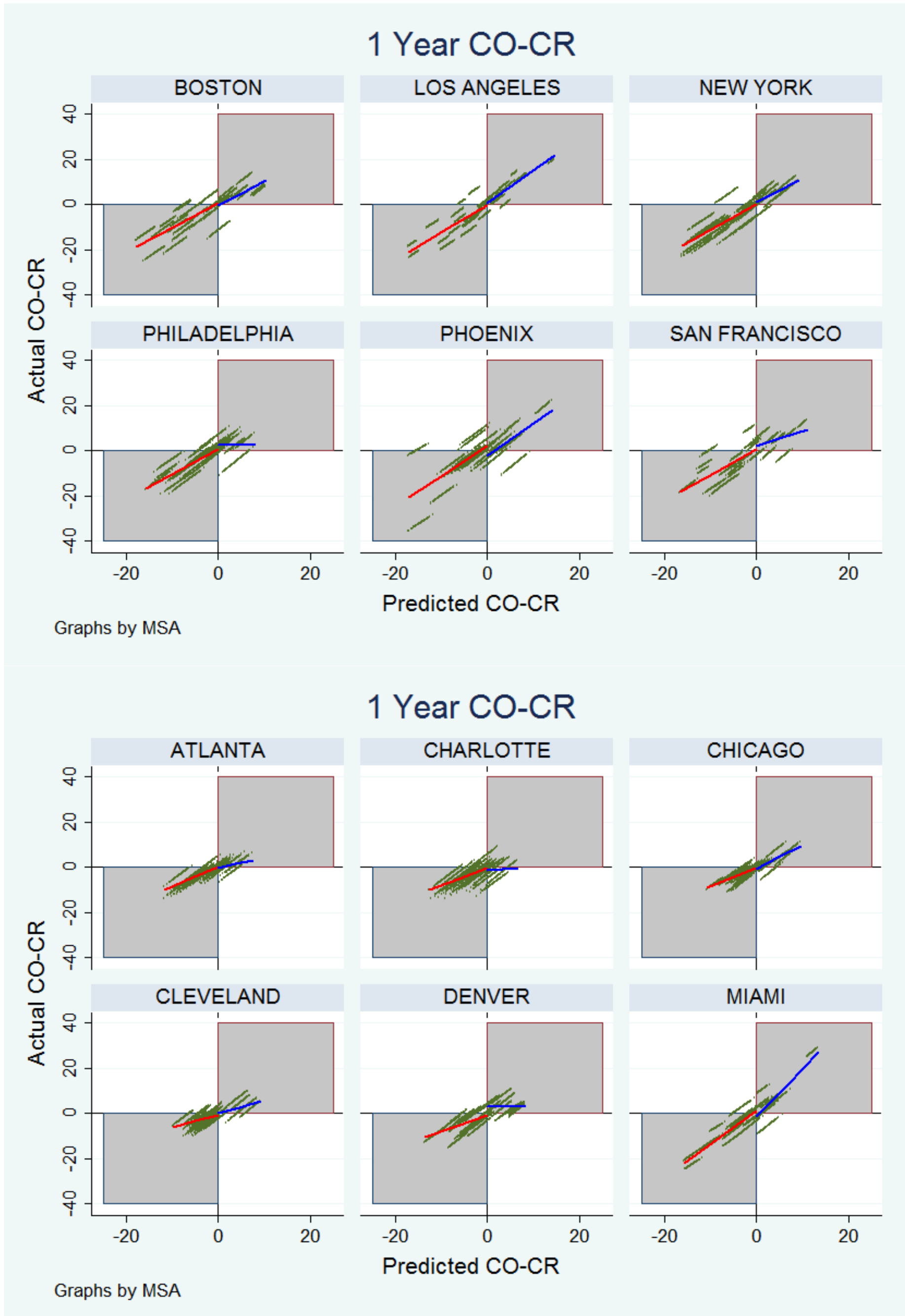
Note: Each column reports the expected money saved if a household was able to follow the least costly tenure choice perfectly against the indicated trading strategy, but only for the lowest income tier.

FIGURE 1. ACTUAL V. PREDICTED COST OF OWNING MINUS COST OF RENTING



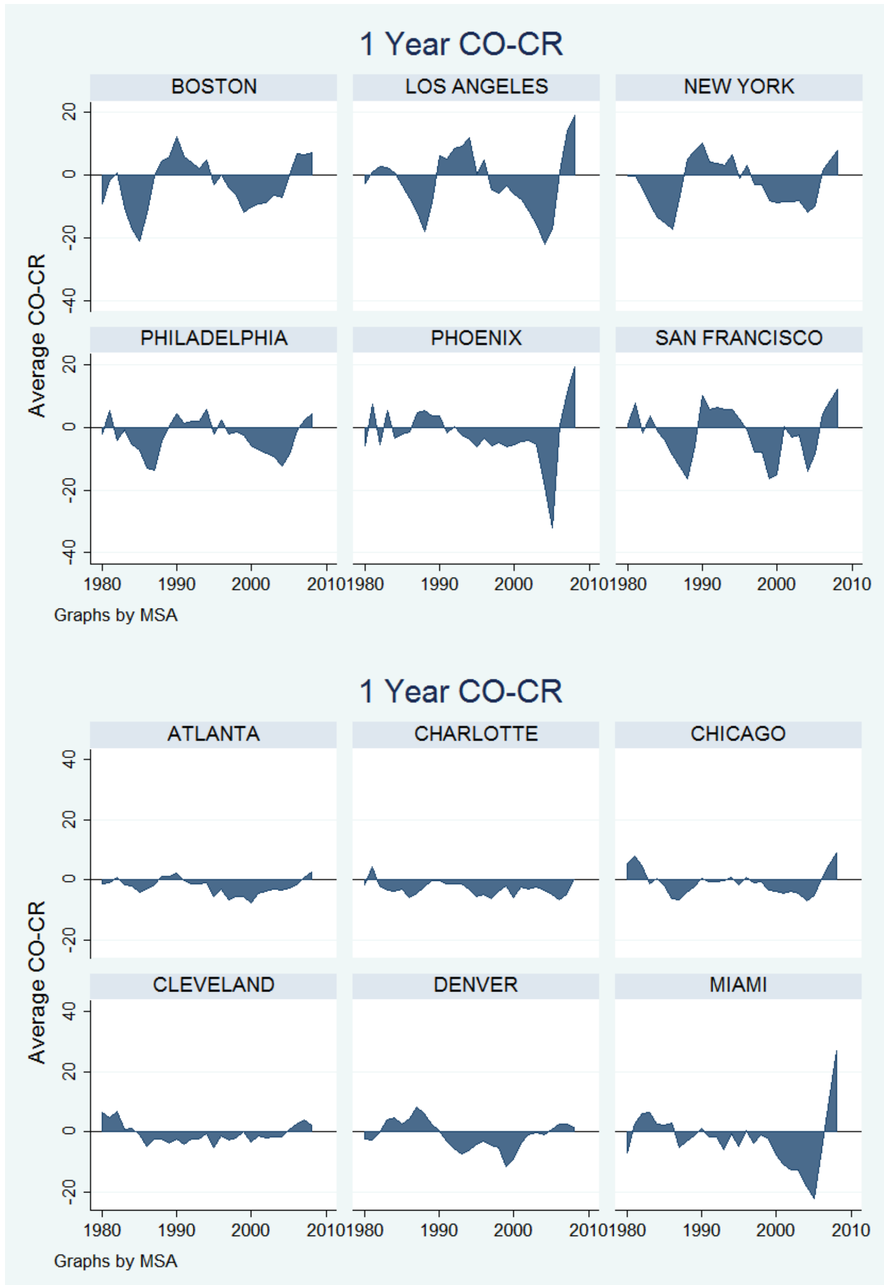
Note: This figure plots the actual CO-CR against the predicted CO-CR across all MSAs and cost measures.

FIGURE 2. ACTUAL V. PREDICTED COST OF OWNING MINUS COST OF RENTING BY CITY



Note: This figure plots the actual CO-CR against the predicted CO-CR for a select sample of cities.

FIGURE 3. COST OF OWNING MINUS COST OF RENTING, 1980-2009



Note: This figure plots the average cost of owning minus cost of renting (CO-CR) for a 1-year horizon across a select sample of cities. The red dashed lined indicates where the cost of owning equals the cost of rent.

APPENDIX TABLE 1. PREDICTING CONTINUOUS CO-CR, 1-YEAR HORIZON

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
All MSAs	0.70	0.64	
	(0.04)	(0.04)	
Atlanta			0.84
			(0.01)
Austin			0.71
			(0.01)
Baltimore			0.78
			(0.01)
Boston			0.79
			(0.01)
Charlotte			0.69
			(0.01)
Chicago			0.82
			(0.01)
Cincinnati			0.78
			(0.01)
Cleveland			0.71
			(0.01)
Columbus			0.63
			(0.01)
Dallas			0.81
			(0.01)
Denver			0.86
			(0.01)
Detroit			0.87
			(0.01)
Fort Lauderdale			0.82
			(0.01)
Houston			0.64
			(0.01)
Indianapolis			0.54
			(0.01)
Kansas City			0.53
			(0.01)
Los Angeles			0.82
			(0.01)
Memphis			0.03
			(0.01)
Miami			0.90
			(0.01)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
R^2	0.44	0.61	
Obs	314496	314496	8064

Note: Dependent variable is $(CO\text{-}CR)_t$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

APPENDIX TABLE 1 (CONT'D). PREDICTING CONTINUOUS CO-CR, 1-YEAR HORIZON

	Coef/SE of $(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
Milwaukee			0.67
			(0.01)
Minneapolis			0.84
			(0.01)
Nashville			0.45
			(0.01)
New York			0.83
			(0.01)
Orlando			0.72
			(0.01)
Palm Beach			0.84
			(0.01)
Philadelphia			0.70
			(0.01)
Phoenix			0.53
			(0.01)
Pittsburgh			0.40
			(0.01)
Portland			0.76
			(0.01)
Richmond			0.75
			(0.01)
Sacramento			0.77
			(0.01)
San Antonio			0.30
			(0.01)
San Diego			0.82
			(0.01)
San Francisco			0.66
			(0.01)
San Jose			0.51
			(0.01)
Seattle			0.45
			(0.01)
St. Louis			0.81
			(0.01)
Tampa			0.61
			(0.01)
Washington, D.C.			0.79
			(0.01)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
R^2	0.44	0.61	
Obs	314496	314496	8064

Note: Dependent variable is $(CO\text{-}CR)_t$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

APPENDIX TABLE 2. PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON, IV RESULTS

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
All MSAs	2.17 (0.09)	2.55 (0.11)	
Atlanta			1.72 (0.07)
Austin			1.83 (0.05)
Baltimore			2.18 (0.03)
Boston			2.15 (0.04)
Charlotte			2.21 (0.08)
Chicago			2.14 (0.04)
Cincinnati			2.44 (0.05)
Cleveland			2.41 (0.04)
Columbus			2.50 (0.05)
Dallas			2.51 (0.04)
Denver			2.27 (0.05)
Detroit			2.53 (0.05)
Fort Lauderdale			2.24 (0.03)
Houston			2.66 (0.03)
Indianapolis			2.38 (0.05)
Kansas City			2.63 (0.05)
Los Angeles			1.52 (0.06)
Memphis			1.93 (0.07)
Miami			2.11 (0.05)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.22	0.39	
Obs	314496	314496	8064

Note: Dependent variable is $I(CO\text{-}CR)_t$. This table instrument $I(CO\text{-}CR)_{t-1}$ with $I(CO\text{-}CR)_{t-2}$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

APPENDIX TABLE 2 (CONT'D). PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON, IV RESULTS

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
Milwaukee			2.29
			(0.05)
Minneapolis			2.39
			(0.05)
Nashville			1.86
			(0.07)
New York			2.02
			(0.05)
Orlando			1.82
			(0.06)
Palm Beach			2.28
			(0.02)
Philadelphia			2.28
			(0.03)
Phoenix			2.36
			(0.03)
Pittsburgh			2.04
			(0.06)
Portland			2.04
			(0.05)
Richmond			2.26
			(0.03)
Sacramento			1.39
			(0.06)
San Antonio			2.19
			(0.04)
San Diego			1.36
			(0.07)
San Francisco			1.77
			(0.05)
San Jose			1.29
			(0.08)
Seattle			1.43
			(0.07)
St. Louis			1.57
			(0.07)
Tampa			2.42
			(0.02)
Washington, D.C.			1.94
			(0.05)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.22	0.39	
Obs	314496	314496	8064

Note: Dependent variable is $I(CO\text{-}CR)_t$. This table instrument $I(CO\text{-}CR)_{t-1}$ with $I(CO\text{-}CR)_{t-2}$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city.

APPENDIX TABLE 3. PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON, 2 BEDROOM

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
All MSAs	2.38	2.24	
	(0.13)	(0.18)	
Atlanta			2.57
			(0.19)
Austin			2.96
			(0.13)
Baltimore			0.73
			(0.11)
Boston			3.20
			(0.12)
Charlotte			0.74
			(0.44)
Chicago			2.43
			(0.13)
Cincinnati			4.07
			(0.17)
Cleveland			4.07
			(0.16)
Columbus			2.35
			(0.17)
Dallas			3.06
			(0.14)
Denver			3.68
			(0.13)
Detroit			3.39
			(0.14)
Fort Lauderdale			1.63
			(0.11)
Houston			3.06
			(0.13)
Indianapolis			2.70
			(0.18)
Kansas City			1.69
			(0.18)
Los Angeles			2.45
			(0.11)
Memphis			1.56
			(0.17)
Miami			2.72
			(0.12)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.24	0.42	
Obs	81432	81432	2088

Note: The cost of owning is denoted by "CO", and the cost of renting "CR". Pr(CO>CR) is the probability that the owning is cheaper than renting. "Overall" is the average CO-CR across all cost measures and years in a given city. Cost measures are calculated by equation (4), assuming a loan-to-value constant of 0.8 and national average federal and state income tax for all tax rates. The calculations in this table are restricted to 2 bedroom units only.

APPENDIX TABLE 3 (CONT'D). PREDICTING BINARY RENT V. OWN DECISION, 1-YEAR HORIZON, 2 BEDROOM

	Coef/SE of $I(CO\text{-}CR)_{t-1}$		
	(1)	(2)	(3)
Milwaukee			1.63
			(0.14)
Minneapolis			1.80
			(0.10)
Nashville			1.20
			(0.11)
New York			1.62
			(0.11)
Orlando			2.31
			(0.18)
Palm Beach			3.07
			(0.12)
Philadelphia			1.47
			(0.19)
Phoenix			2.47
			(0.11)
Pittsburgh			2.55
			(0.12)
Portland			3.29
			(0.12)
Richmond			2.12
			(0.10)
Sacramento			1.75
			(0.10)
San Antonio			2.44
			(0.11)
San Diego			2.54
			(0.18)
San Francisco			0.98
			(0.13)
San Jose			1.91
			(0.11)
Seattle			2.44
			(0.11)
St. Louis			2.56
			(0.18)
Tampa			0.98
			(0.13)
Washington, D.C.			2.06
			(0.11)
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Pseudo R^2	0.24	0.42	
Obs	81432	81432	2088

Note: Dependent variable is $(CO\text{-}CR)_t$. Standard errors are clustered by MSA-year and in parentheses. Each unit of observation is for a given MSA, year, method, and type of home. Columns 1 and 2 perform pooled regressions with different specifications, while column 3 performs individual regressions by city. The calculations in this table are restricted to 2 bedroom units only.

APPENDIX TABLE 4. FORECASTING THE 1-YEAR HORIZON OWN VERSUS RENT DECISION, 2 BEDROOM

	% of Time Forecast is Correct		
	(1)	(2)	(3)
All MSAs	82.8	84.9	83.2
Atlanta	90.1	90.8	90.1
Austin	85.4	81.5	85.4
Baltimore	68.0	77.2	73.3
Boston	83.6	86.7	83.6
Charlotte	95.9	95.7	95.9
Chicago	82.3	88.9	82.3
Cincinnati	91.5	92.6	91.5
Cleveland	91.2	91.4	91.2
Columbus	88.8	88.8	88.8
Dallas	86.0	80.2	86.0
Denver	86.7	84.9	86.7
Detroit	87.7	89.9	87.7
Fort Lauderdale	73.9	76.0	73.9
Houston	84.5	67.7	84.5
Indianapolis	90.2	91.4	90.2
Kansas City	91.1	88.8	91.1
Los Angeles	77.4	91.5	77.4
Memphis	90.0	87.2	90.0
Miami	81.7	83.6	81.7
Milwaukee	86.0	86.5	86.0
Minneapolis	81.1	84.9	81.3
Nashville	80.5	78.1	82.7
New York	82.8	86.2	82.8
Orlando	80.8	87.8	82.5
Palm Beach	73.0	78.0	73.0
Philadelphia	71.5	80.3	73.1
Phoenix	72.8	73.9	72.8
Pittsburgh	92.5	92.2	92.5
Portland	83.6	85.4	83.6
Richmond	89.6	89.1	89.6
Sacramento	78.1	82.7	78.1
San Antonio	82.3	78.9	82.3
San Diego	83.9	92.6	83.9
San Francisco	74.9	80.7	74.9
San Jose	70.8	87.7	70.8
Seattle	78.1	76.3	78.1
St. Louis	90.3	89.8	90.3
Tampa	74.9	78.7	81.2
Washington, D.C.	74.3	87.0	74.3
MSA Fixed Effects	YES	YES	
Time Fixed Effects		YES	
Obs	81432	81432	2088

Note: This table calculates the percent of the time the predicted binary rent v. own decision was “correct”, i.e. when it was actually better to own or rent. The columns correspond to the specifications in Table 2. The calculations in this table are restricted to 2 bedroom units only.

APPENDIX TABLE 5. AVERAGE ANNUAL SAVINGS FROM LEAST COSTLY BENCHMARK WITH 1-YEAR HORIZON, 2 BEDROOM

	Annual Savings from Least Costly Tenure Choice (as % of CR)							
	v. Regression-Based		v. Always Owning		v. Always Renting		v. Last-Year Strategy	
	Savings	SD	Savings	SD	Savings	SD	Savings	SD
All MSAs	4.5	14.3	13.2	29.3	49.8	49.5	6.1	17.5
Atlanta	0.8	4.3	1.0	4.9	45.8	31.9	1.1	4.8
Austin	6.5	20.5	16.7	46.1	47.1	43.2	3.5	13.2
Baltimore	3.3	9.1	8.6	20.4	47.7	54.0	6.9	13.7
Boston	4.0	15.7	19.8	32.5	61.3	70.1	6.6	19.5
Charlotte	0.9	6.0	0.9	6.0	57.6	29.3	2.2	9.8
Chicago	1.3	6.6	11.0	25.0	31.8	30.0	3.0	9.4
Cincinnati	0.7	4.3	5.5	16.5	34.1	24.0	1.4	6.7
Cleveland	0.9	4.6	7.0	16.6	32.5	25.7	1.1	5.0
Columbus	3.0	12.3	2.8	12.0	37.0	23.3	3.1	12.4
Dallas	5.2	15.2	9.2	26.4	39.7	34.8	3.3	12.0
Denver	2.3	8.4	11.8	21.5	34.8	40.3	1.9	7.7
Detroit	4.2	18.7	15.4	35.0	56.7	49.2	5.0	19.5
Fort Lauderdale	5.0	12.8	20.3	56.6	64.8	75.5	6.3	15.6
Houston	20.7	37.6	17.6	33.9	51.8	42.4	8.9	27.9
Indianapolis	2.4	11.6	2.7	11.8	42.0	28.8	2.8	12.0
Kansas City	6.0	22.2	5.7	21.0	54.7	36.5	6.9	22.7
Los Angeles	1.8	8.5	31.1	53.6	60.5	74.5	6.9	17.4
Memphis	6.8	26.9	8.1	28.8	47.3	36.2	12.5	39.0
Miami	4.1	14.2	20.9	58.2	57.5	67.3	8.2	24.9
Milwaukee	2.6	9.6	8.0	20.6	34.0	26.8	2.9	10.4
Minneapolis	3.8	12.4	7.7	18.8	41.2	35.6	4.5	12.7
Nashville	5.4	16.5	4.6	13.1	42.8	32.6	5.2	16.2
New York	5.2	17.4	21.9	34.2	57.6	64.7	5.2	14.7
Orlando	1.9	7.9	11.0	36.0	61.9	70.2	7.3	23.0
Palm Beach	3.5	9.4	20.9	51.9	55.3	74.3	6.4	14.8
Philadelphia	5.8	16.7	7.1	15.7	56.5	55.3	8.7	18.9
Phoenix	11.3	23.9	21.0	46.1	55.5	75.4	13.5	27.7
Pittsburgh	2.2	10.7	5.7	21.4	54.9	36.1	6.4	23.9
Portland	3.0	10.7	13.6	25.5	43.9	48.6	3.7	11.3
Richmond	1.8	7.5	1.9	7.8	48.4	42.7	2.9	9.7
Sacramento	6.5	21.9	30.1	51.1	56.0	70.4	8.6	22.1
San Antonio	11.4	30.9	16.2	40.8	63.6	54.7	10.4	30.6
San Diego	1.1	5.6	29.0	45.2	54.5	67.5	7.0	19.7
San Francisco	4.3	11.5	26.8	39.0	52.5	65.6	10.1	24.0
San Jose	3.3	12.2	26.6	41.9	59.6	76.0	14.6	32.9
Seattle	6.7	18.5	16.5	31.5	36.0	55.5	5.5	15.1
St. Louis	2.4	10.4	2.3	9.5	49.0	31.1	2.5	10.5
Tampa	11.9	34.7	12.4	36.2	59.7	66.1	13.5	35.2
Washington, D.C.	2.0	8.5	14.4	30.8	56.2	66.0	6.6	15.5

Note: Each column reports the expected money saved from following the least costly benchmarket (correctly forecasting) against the indicated trading strategy. "Regression-Based " indicates a trading strategy in which one rents or buys given the predicted strategy from specification (2) in tables 1 and 2. Always own and always rent is to apply a strategy of always owning or renting regardless of MSA or year. “Last-Year” refers to a strategy where one just owns or rent based on whether it was observed to be cheaper to own or rent in the previous year. The calculations in this table are restricted to 2 bedroom units only.

CHAPTER 3

FINANCIAL LITERACY, SCHOOLING, AND WEALTH ACCUMULATION*

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Abstract

Financial literacy and schooling attainment have been linked to household wealth accumulation. Yet prior findings may be biased due to noisy measures of financial literacy and schooling, as well as unobserved factors such as ability, intelligence, and motivation that could enhance financial literacy and schooling but also directly affect wealth accumulation. Here we use a new household dataset and an instrumental variables approach to isolate the causal effects of financial literacy and schooling on wealth accumulation. While financial literacy and schooling attainment are both strongly positively associated with wealth outcomes in linear regression models, our approach reveals even stronger and larger effects of financial literacy on wealth. It also indicates no significant positive effects of schooling attainment conditional on financial literacy in a linear specification, but positive effects when interacted with financial literacy. Estimated impacts are substantial enough to suggest that investments in financial literacy could have large positive payoffs.

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1 Introduction

Traditional economic theory posits that forward-looking individuals maximize expected lifetime utility using economic information to accumulate and then decumulate wealth effectively over their lifetimes. Yet survey evidence reveals that fewer than half of U.S. workers have even attempted to estimate how much money they might need in retirement, and many older adults face significant retirement saving shortfalls (Lusardi and Mitchell 2007a and b; Mitchell and Moore 1998; Scholz, Seshadri, and Khitatrakun 2006). Numerous economic explanations for these phenomena have been suggested including dispersion in discount rates, risk aversion, and credit constraints, but the empirical literature exploring such factors thus far has been unable to account for much of the observed differentials in wealth (Bernheim, Skinner, and Weinberg, 2001; Barsky, Juster, Kimball, and Shapiro, 1997).

The present study seeks to evaluate whether people who find it difficult to understand their financial environment are also less likely to accumulate wealth. Specifically, we examine the links between financial literacy, by which we mean the ability to process economic information and make informed decisions about household finances, and wealth accumulation and pension contributions. Previous studies have reported strong correlations between financial literacy and asset accumulation as well as retirement planning.[1] These findings have prompted policymakers to support efforts to enhance household wealth accumulation and welfare through increasing financial literacy. For instance, the U.S. President's Advisory Council on Financial Literacy recently stated that (PACFL, 2008, np): "While the crisis has many causes, it is undeniable that financial illiteracy is one of the root causes... Sadly, far too many Americans do not have the basic financial skills necessary to develop and maintain a budget, to understand

[1] For instance, Hilgert, Hogarth, and Beverley (2003) show that more financially knowledgeable US respondents are also more likely to engage in a wide range of recommended financial practices; Lusardi and Mitchell (2007a, b) find that more financially literate elderly U.S. respondents are also more likely to plan, to succeed in planning, and to invest in complex assets; and Campbell (2006) reports that more educated Swedish households also diversify their portfolios more efficiently. Cole, Sampson, and Zia (2009) find that the financially more literate are more likely to have bank accounts in India and Indonesia.

credit, to understand investment vehicles, or to take advantage of our banking system. It is essential to provide basic financial education that allows people to better navigate an economic crisis such as this one." Similarly, the Organization for Economic Cooperation and Development (OECD nd) has recently launched a major initiative to "identify individuals who are most in need of financial education and the best ways to improve that education."

Despite these and other enthusiastic endorsements for programs to boost financial literacy, questions have been raised about whether these associations reflect causality (Lusardi and Mitchell, 2008, 2010). For example, individuals who fail to save also may be financially illiterate due to some underlying and usually unobservable factor such as impatience, making it difficult to assess whether boosting financial education would, in fact, enhance household wealth accumulation. Moreover, in simple bivariate associations of financial literacy with wealth, financial literacy might be proxying, in part, for other factors such as schooling attainment. Empirical measures of financial literacy are also likely to have considerable measurement error that, *ceteris paribus,* is likely to bias standard estimates of the impacts of financial literacy towards zero. Instrumental variable (IV) estimates in principle can control for both the unobserved variable and the random measurement error biases, and schooling attainment can be included in the same specification to control for the possibility that financial literacy proxies for schooling. To our knowledge, however, no studies have yet used IV methods to estimate the impact of financial literacy and schooling attainment on wealth, as we do here.[2]

[2] Some studies have looked at related issues using IV methods. For instance, Lusardi and Mitchell (2007a) test the possible causal effect of wealth on financial planning using changes in regional housing prices as an instrument for wealth, but they limit their study to older respondents in the U.S. Health and Retirement Study and do not consider the possible impact of financial literacy on wealth as we do in this study. Bernheim, Garrett, and Maki (2001), Cole and Shastry (2009), and Lusardi and Mitchell (2009) investigate how changes in U.S. schooling laws and state mandates requiring schools to offer financial literacy relate to financial market participation, but these studies do not focus on wealth accumulation as we do here. Ameriks, Caplin, and Leahy (2003) explore instruments for planning by U.S. respondents but they are silent on the role of financial literacy.

In what follows, we draw on a unique microeconomic dataset, the Chilean Social Protection Survey, to explore how financial literacy and schooling attainment influence wealth.[3] This dataset includes extensive information on household wealth as well as individual and household characteristics for a representative sample of prime-age adults, permitting us to evaluate the effects of financial literacy using a richer range of ages and schooling than heretofore available.[4] Using a set of plausibly exogenous instrumental variables that satisfy critical diagnostic tests to isolate the causal effects of financial literacy and schooling attainment on wealth, we show that both financial literacy and schooling attainment are positively associated with wealth outcomes. Moreover, our IV estimates indicate even stronger effects of financial literacy on wealth than suggested by OLS models, while the opposite is true for schooling in linear specifications; interactive specifications imply that both schooling and financial literacy have significant positive effects.

Our results are relevant for financial educational policy in that we find that improved financial literacy can make a significant difference for financial behavior, even after controlling for schooling. This rigorous analysis of the impact of financial literacy on wealth accumulation should be useful in informing governments and their policy advisers around the world, as they consider new initiatives for financial education.[5]

2 Empirical Framework

Several prior studies have shown that financial literacy and schooling are significantly correlated with positive financial behavior, but few have controlled for (usually) unobserved

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[3] The Social Protection Survey is described at www.microdatos.cl/interior_areasMT.php?id_s=2&id_ss=2&id_proy=1

[4] Ameriks et al. (2003) examine highly-educated TIAA-CREF survey participants; Lusardi and Mitchell (2007a) use Health and Retirement Study respondents over age 50. In contrast, the dataset we use below is a nationally representative sample of men age 24-65 and women age 24-60.

[5] For instance the World Bank and the Russian Federation have recently announced a multi-million dollar, multi-year collaborative to improve financial literacy in low- and middle-income countries (see http://web.worldbank.org/WBSITE/EXTERNAL/TOPICS/EXTEDUCATION/0,,contentMDK:21936796~menuPK:2643854~pagePK:64020865~piPK:149114~theSitePK:282386,00.html)

factors such as risk aversion, self-esteem, innate ability, intelligence, and motivation that may shape the relationship between financial literacy and financial behaviors.[6] For this reason, it is difficult to conclude, based on the scientific evidence, that improvements in financial literacy actually enhance financial planning and saving, or whether, instead, wealth and financial literacy are both the result of some other unobserved factors. For this reason, analyses that do not control for such unobserved factors may be vulnerable to biases in the estimated effects of schooling and financial literacy on financial wellbeing. Moreover, empirical indicators of schooling and financial literacy are noisy measures, and as is well-known, random measurement error in right-side variables tends to bias their coefficient estimates towards zero. Estimates of noise-to-signal ratios for schooling attainment are often about 10 percent (Ashenfelter and Krueger 1994; Behrman, Rosenzweig, and Taubman 1994), producing a bias towards zero of almost that magnitude. Measures of financial literacy are likely subject to greater measurement errors, and thus, greater biases. Instrumental variable estimates are one way to eliminate the attenuation bias towards zero due to measurement error.

Our goal is to assess whether wealth accruals could be enhanced with greater financial literacy and schooling. Suppose the true relationship between financial literacy, schooling, and wealth could be described for the *ith* person as:

$$W_i = \beta_0 + \beta_1 FL_i + \beta_2 S_i + \beta_3 FL_i * S_i + \beta_4 C_i + \beta_5 E_i + \varepsilon_i \quad , \qquad (1)$$

where wealth W_i depends linearly on financial literacy FL_i, schooling S_i, their interaction $FL_i *S_i$, other observed individual characteristics C_i, unobserved individual characteristics E_i, and unobserved random shocks ε_i. We include in relation (1) both linear terms in FL_i and S_i and their interaction. We include the interaction because it is possible that the effects of financial literacy *FL* depend on the level of schooling S_i and *vice versa*. This coefficient of this interaction term

[6] Both Lusardi (2003) and Ameriks et al. (2003) use IV strategies, but they focus on financial planning rather than financial literacy.

may be positive if financial literacy FL_i and schooling S_i are complements and reinforce each other or negative if they are substitutes in determination of wealth accumulation. Below we consider three variants of relation (1):

(1a) only linear terms,[7]
(1b) both the linear and interaction terms, and
(1c) only the interaction term.

Equation (1) posits that there are no other endogenous variables beyond financial literacy and schooling that directly determine wealth. For example, the time one devotes to schoolwork and how that time is divided between arithmetic and other topics might affect wealth, but our assumption is that such effects are indirect via financial literacy and schooling. Likewise, there could be other behavioral channels through which FL_i and S_i affect W_i. For instance, part of the effects on wealth may work through choosing to contribute more to pensions, or by increasing understanding of business news and market predictions. Estimating equation (1) does not illuminate such possibilities, though formulations similar to equation (1) but using some saving pathway as the dependent variable could illuminate the roles of FL_i and S_i in determining the relevant mechanism. In what follows, we offer analysis of two such pathways, the density of pension contributions and whether the individual attempted to calculate money needed for retirement.

We further posit that financial literacy and schooling are determined by observed personal characteristics C_i* (that may overlap with C_i), some factors in C* and X_i that affect learning and schooling but do not directly affect W_i , unobserved individual characteristics $E_{i,}$ and error terms u_i and v_i:[8]

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7 We also consider two sub-variants of the linear case with only financial literacy or only schooling.

8 We also could include another equation parallel to (2) and (3) for the interaction, but since the points made here for the case in which FL_i and S_i enter in equation (1a) only linearly carry over to the case with the interaction, we limit this discussion to the simpler case in which they only enter linearly. We have written equations (2) and (3) as if FL_i and S_i have the same determinants except for u_i and v_i, which are likely to be correlated (perhaps perfectly correlated). This is the usual setup in household models if decisions regarding FL_i and S_i are made at the same time – in principle, all concurrent decisions are made in

$$FL_i = \eta_0 + \eta_1 C_i^* + \eta_2 X_i + \eta_3 E_i + u_i \qquad (2)$$

$$S_i = \alpha_0 + \alpha_1 C_i^* + \alpha_2 X_i + \alpha_3 E_i + v_i \qquad (3)$$

In general, for consistent estimates of the coefficients of interest, ordinary least squares (OLS) regression requires that the covariance between the disturbance terms in equations (1) and (2) and (1) and (3) be zero: that is, there can be no unobserved factors that are correlated with financial literacy or schooling but also affect the outcome of interest W_i. Nevertheless, the unobserved individual factors vector E_i appears in the compound disturbance terms for all relations, implying that OLS estimates are likely to suffer from omitted variable bias. The direction of the bias depends on whether the true values of β_5 and η_3 (and similarly, of β_5 and α_3) have the same or the opposite signs. For example, if the unobserved factor is ability that positively affects financial literacy and also directly positively affects wealth, both β_5 and η_3 are positive and OLS estimates of β_1 are biased upward, overestimating the magnitude and significance of financial literacy as a determinant of wealth. Conversely, if some unobserved factor such as innate caution produced greater investment in financial literacy, but *ceteris paribus* reduced wealth due to too great caution in investment behavior, OLS estimates of β_1 are biased downward, underestimating the magnitude and significance of financial literacy as a determinant of wealth accumulation. In addition to the possibility of such omitted variable biases, financial literacy and schooling measures are potentially subject to measurement error as noted above, which would tend to bias OLS estimates towards zero.

A similar point holds with regard to estimates that include *only* FL_i or only S_i – one at at a time – if the true relation is actually equation (1a) with both entering linearly.[9] Equations (2) and

light of all the variables that determine household behaviors – though, of course, the coefficients could differ and some may not be significantly different from zero. If decisions are made at different times, the right-side determinants in equations (2) and (3) may differ; for example, some expectations that determine the earlier decision could be replaced by realized outcomes that occurred prior to the later decision. Our microeconomic dataset, like most, does not permit empirical representations of such possibilities.

[9] Some other endogenous variable Y_i might also be included in equation (1) but our maintained hypothesis for our estimates, as in other instrumental variable estimates, if that this is not the case.

(3) show that it is highly likely that FL_i and S_i are correlated because their determinants are basically the same.[10] Accordingly, if the true relation is equation (1a) with both FL_i and S_i entering linearly but analysts include *either* FL_i *or* S_i, the coefficient estimate for the included variable is biased because it is correlated with the excluded factor.

To handle this problem, we use an IV approach with robust standard errors to estimate the three variants of equation (1) in light of (2) and (3), seeking to isolate the causal effects of financial literacy and schooling, and to control for random measurement errors. Good instruments are ones that are sufficiently correlated with financial literacy FL_i and with schooling S_i, but that are independent of unobserved effects in equation (1) determining wealth. The X_i vector and elements of the C_i^* vector excluded from C_i in equations (2) and (3) refer to such instruments. For the IV estimation, we begin by estimating the "first stage" determinants of financial literacy and schooling in (2) and (3); next we use these estimates to predict financial literacy and schooling and employ them in the "second stage" estimate of equation (1). Note that with the above assumptions, predicted financial literacy and predicted schooling are independent of the compound error term in (1). Therefore, if equation (1) is the true relation, the IV or two-stage least squares procedure leads to consistent estimates of β_1 and β_2.

In what follows, we utilize a set of plausible instruments and diagnostic tests to determine whether our instruments are (a) sufficiently strong (using F tests for excluded instruments, Angrist-Pische multivariate F tests for excluded instruments, and the Kleibergen-Paap weak identification tests), and (b) independent of the second-stage compound disturbance term ($\beta_4 E_i$ + ε_i) using the Hansen J statistic overidentification test.[11] Our candidate instruments, on which we

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[10] It is possible but highly unlikely in such household models that the coefficients of the variables in equations (2) and (3) differ so that financial literacy and schooling are orthogonal.

[11] There recently has been what Stock (2010) calls a "transformation" in econometric tools for making inferences, including development of some of the diagnostic tools that we use here(see Stock (2010) and the references therein). As is well-known, the J statistic only tests the overidentifying restrictions, not the exogeneity of all the first-stage instruments (e.g., Stock and Watson 2007, Wooldridge 2002). As also is well known (e.g. Wooldridge 2002), the failure to reject the null in overidentification tests may be because

elaborate below, include (1) age-related factors such as governmental policies and macroeconomic conditions, (2) family background, and (3) personality traits. We find that many of these candidates are good by conventional criteria. Nevertheless, some are insufficiently strong predictors of the endogenous FL_i and S_i right-side variables, and some are not independent of the second-stage disturbance term. Therefore, arguably, this latter group should be included as controls in the second-stage relation (i.e., in the vector C_i in relation 1).

3 Data and Descriptive Statistics

Our primary data source is the Social Protection Survey (Encuesta de Protecion Social, EPS) administered by us in collaboration with the Microdata Center of the University of Chile (Arenas et al., 2008; Bravo et al. 2004, 2006, 2010). This survey is comparable to the U.S. Health and Retirement Study (HRS) that provides a nationally-representative stratified random survey on respondents over the age of 50, covering, *inter alia,* their wealth, schooling, financial literacy, work history, childhood background, and selected personality traits. In contrast to the HRS, however, the EPS covers all adults, not just respondents over age 50. In what follows, we limit our attention to 13,054 prime-age respondents surveyed in 2006, namely men age 24-65 and women age 24-60 (since in Chile the legal retirement age is 60 for women but 65 years for men). As noted below, we also have linked these data to some information on policies, markets and macroeconomic conditions at critical junctures in respondents' lives.

Wealth and Pension Contribution Outcomes: Our outcomes of interest are components of net wealth, drawing on four EPS measures summarized in Table 1 (wealth in US$2006):

- *Pension wealth* averages $38,600 or 54 percent of total net wealth, though with considerable variance across respondents and about a quarter (25 percent) of respondents have zero pension wealth. In 1981, the Chilean government terminated

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the test has low power for detecting the endogeneity of some of the instruments. As discussed below, however, in our case, the overidentification test does have power to reject a number of candidate instruments.

the old insolvent pay-as-you-go retirement system and replaced it with a national, mandatory defined-contribution scheme known as the AFP system (Mitchell, Todd, and Bravo, 2008). This reform required all new formal sector employees to contribute at least 10 percent of their salaries to one of several licensed defined contribution pension funds.[12] We believe that pension wealth is likely to be relatively accurately reported in Chile because respondents receive annual statements from the government summarizing their defined contribution pension system accruals.

- *Net housing wealth* averages \$22,100 or 31 percent of total wealth, again with considerable variance across respondents (though with a standard deviation only about half as large as for pension wealth despite a greater range); about a quarter (26 percent) of respondents report none and 1 percent report negative net housing wealth. We calculate net housing wealth based on self-reported data on market values (either for sale or for rent) minus estimated mortgage debt. Our measure of housing wealth is probably noisier than our measure of pension wealth and some of the other wealth components.
- *Other net wealth* averages \$10,600 or 15 percent of total net wealth, with greater variance across respondents than either pension or housing wealth but again about a quarter (25 percent) of respondents report zero and more (31 percent) report negative values. We calculated other net wealth by summing self-reported business wealth, agricultural assets, other real estate assets, and financial investments and subtracting all forms of household debt. This other net wealth measure probably also is a noisier than the measure of pension wealth.
- *Total net wealth* averages \$71,500, with greater variance and greater range than the other wealth measures just described. Total net wealth is the sum of the three components above.

In addition to these wealth measures, we also explore two possible channels via which financial literacy and schooling might affect particularly pension wealth. The first is the "*density of pension contributions*." This concept refers to the fraction of months each individual contributed to the pension system, from age 18 to the survey date, and therefore is indicative of

[12] Those who started working prior to 1980 could elect to join the new scheme or remain covered by the previous system.

how attached the worker is to the pension saving system. We derive this measure by tracking respondent self-reports of the number of months they worked in covered jobs over time and contributed to a pension fund, compared to the number of months when they could have contributed. On average respondents report that they contributed to their pension almost half the time they were eligible to do so, though there is again wide dispersion over the sample.[13] About 10 percent of individuals contributed all of the available time, while 17 percent report they never contributed. The second channel that we explore is a retirement planning indicator of whether the individual has attempted to calculate the money he or she needs for retirement. The survey question for this retirement planning variable is as follows:

- *Have you attempted to calculate the money needed in order to retire? [yes/no]*

We create a dummy indicator in which 1 indicates a yes response, and 0 represents a negative response.

<u>Explanatory Variables: Schooling and Financial Literacy</u>*:* Our key explanatory variables are schooling attainment and financial literacy. *Schooling attainment* is measured in a fairly conventional manner (e.g., Bravo, Mukhopadhyay, and Todd 2010), with primary school referring to grades 1-8, secondary school to grades 9-13, and post-secondary school to grades beyond that, to a maximum of 20. The average schooling attainment in our sample (see Table 2) is 10.4 grades, with a standard deviation of 3.9 grades. Only about one percent of the respondents have no schooling, and about the same fraction has the maximum of 20 years.

Financial literacy is measured using a rich set of 12 questions. The first three 'core' questions cover basic economics and finance including an understanding of risk and simple interest; the second more 'sophisticated' set of three pertains to more elaborate financial concepts; and a third set of six covers knowledge of retirement system rules including the legal retirement age and how to calculate AFP pension benefits.

[13] Our density estimates conform to those reported in Arenas et al. (2008).

The "core" first three financial literacy queries were developed and implemented in the HRS (Lusardi and Mitchell, 2007c); they have also been adopted by several other international surveys. They are as follows:

- *If the chance of catching an illness is 10 percent, how many people out of 1000 would get the illness?*
- *If 5 people share winning lottery tickets and the total prize is 2 Million pesos, how much would each receive?*
- *Assume that you have $100 in a savings account and the interest rate you earn on this money is 2 percent a year. If you keep this money in the account for 5 years, how much would you have after 5 years? [more than $120, exactly $120, less than $120]*

The second more sophisticated set of three questions has also been fielded in a special HRS module (Lusardi and Mitchell, 2009) intending to measure more complex concepts such as compound interest, inflation, and risk diversification. The specific questions are:

- *Assume that you have $200 in a savings account, and the interest rate that you earn on these savings is 10 percent a year. How much would you have in the account after 2 years? [exact number]*
- *Assume that you have $100 in a savings account and the interest rate that you earn on these savings is 1 percent a year. Inflation is 2 percent a year. After one year, if you withdraw the money from the savings account you could buy more/less/the same?*
- *T/F: Buying shares in one company is less risky than buying shares from many different companies with the same money.*

The third set of questions is specific to the EPS, and it touches on some of the key aspects of the Chilean retirement system focusing on the mandatory contribution rate, the legal retirement age for women and men, how pension benefits are computed in the defined contribution system, whether people are aware of the government's welfare benefit for the elderly, and whether people know they can contribute to the Voluntary Pension system even when they are not in covered-sector jobs. The specific wording of these questions is:

- *Do you know what percentage of income is (has been or would be) deducted monthly for pension system contributions? [yes/no]*
- *Do you know the legal retirement age for women? [60]*
- *Do you know the legal retirement age for men? [65]*
- *Do you know how to calculate pensions in the AFP? [yes, by balance of individual account and other elements such as age of retirement]*

- *Do you know there is a minimum state guaranteed old age pension for people aged 65 and over? [yes/no]*
- *Have you heard of the Voluntary Pension Savings system introduced in 2002? [yes/no]*

Table 3 lists all 12 financial literacy questions along with a summary of how the individuals in our sample answered them. As is clear from Column 1, only half of the respondents knew the correct answer to the core questions (1-3), and fewer knew the sophisticated financial literacy questions (4-6). While people did score relatively well on the risk diversification question, they could have been guessing as only a true/false response was required.[14] Patterns are more variable for questions regarding knowledge of pension system benefit rules and provisions: most knew the legal retirement ages, but only about one-third knew the mandatory contribution rates and only 10 percent could say how benefits are computed. About half the sample knew about both the guaranteed minimum benefit and the Voluntary Savings plan.

Previous authors have measured financial literacy by selecting one or two key questions and reporting whether respondents answered each one correctly (Lusardi and Mitchell, 2007a). With such a rich set of financial literacy measures available in the EPS, however, it is inefficient to limit ourselves to a question or two; instead, we seek to use all the information contained in the dozen questions. A conventional way to aggregate responses would be to assign one point to each question answered correctly and calculate an overall percentage correct score. Yet this approach has the disadvantage of weighting each question equally and hence does not allow distinctions among questions either in difficulty or information.

A more sophisticated approach to measuring financial literacy employs a weighted scoring mechanism called PRIDIT, first designed to deal with difficult-to-observe outcomes where indicator variables that proxy for the dependent variable are binary or categorical. For example, Brockett et al. (2002) use the approach to assess insurance fraud, where investigators use several indicator variables (such as whether an individual had time gaps between medical

[14] This pattern is similar to that reported for India and higher than for Indonesia (Cole, Sampson, and Zia 2009).

treatments or experienced many hospital visits) to assess whether a given claim might be fraudulent. PRIDIT has also been used in the health economics field to evaluate hospital care, where indicators of quality are used to generate a 'best' or most informative quality index (Lieberthal, 2008).

In what follows, we use the PRIDIT approach to develop financial literacy scores and highlight which questions are the most informative indicators of financial literacy.[15] This approach involves a two-step weighting scheme, where the first step links each individual's responses on particular questions to others' responses to the same question. One goal is to determine which questions are more difficult – ones that few people answer correctly – and then it gives more credit to particularly difficult questions that few people can answer correctly. A simple aggregation would simply assign zero credit for an incorrect answer and a full point for each correct answer; by contrast, PRIDIT applies a negative penalty for an incorrect answer and a greater penalty for a question that more of the population answers correctly. As an example, a small fraction of the sample answered question 4 correctly (Table 3, Column 1), so question 4 is considered a difficult question. Consequently, answering question 4 correctly is assigned a greater reward, while answering it incorrectly results in only a relatively small penalty. Unlike simple integer scoring, this method captures the degree and direction to which an individual's response stands out relative to the population.

The second PRIDIT step applies a principal components analysis to take into account correlations across questions.[16] The resulting PRIDIT scores indicate how financially literate an individual is in relation to the average population and to specific questions asked. Questions tend to be informative, *ceteris paribus,* the less they are correlated with other questions. The bivariate

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[15] A related approach was implemented in Mitchell et al. (2008) in an analysis of pension switching patterns.

[16] Specifically, we calculate the first principal component vector for each of the 12 questions and the eigenvalue of the first principal component. The eigenvector with the largest eigenvalue captures more of the variance in the data than any other eigenvector. Using these values, we then calculate a weight for each question that gives more weight to questions that are more informative on financial literacy.

correlations are suggestive though not conclusive in this regard, because correlations of the answers to a question with a linear combination of the answers to other questions may differ from the bivariate correlations. The bivariate correlations among the correct answers to the questions vary considerably, from 0.04 (for the correlations between question 4 and questions 8, 9, and 11) to 0.63 (for the correlation between questions 8 and 9). Also the mean correlations of each question with the other 11 questions vary considerably, with those for questions 4 and 10 only about half of those for questions 1, 2 and 12 (third column from right in Table 3). By this criterion, in isolation, questions 4 and 10 seem to be relatively important. But this is not the only criterion. Questions also tend to be more important on average, *ceteris paribus,* if the proportions correct are closer to one half, rather than almost zero or almost one. The intuition for this is clear by considering the extremes: questions for which the proportion correct is zero or one provide no information because the answers are the same for everyone, whereas questions for which the proportion correct is close to zero or close to one provide substantial information to distinguish among those in the tails of the distribution. However, if the distribution of the underlying latent variable for true financial literacy is normal, relatively few individuals will be in the tails of the distribution, versus in the middle. By this criterion, questions 4 and 10 are relatively unimportant, particularly in comparison to the three 'core' HRS questions (1-3) and questions 6, 11 and 12 (penultimate column in Table 3).

The last column of Table 3 reports PRIDIT weights for each question that are indicative of how "informative" a given question is regarding the underlying latent financial literacy variable, relative to other questions based on both criteria. The 'core' HRS financial literacy questions receive the greatest weight compared to the other financial literacy questions included in the EPS. Next most informative are the queries on pension system knowledge (e.g. question 7 "Do you know what percentage of income is deducted for monthly pension system contributions?" and question 12 "Have you heard of the Voluntary Pension Savings system introduced in 2002?"). Despite being most informative by the criterion of being least correlated with other questions, question 4 "Assume that you have $200 in a savings account, and the interest rate that you earn on these savings is 10 percent a year. How much would you have in the account after 2 years?" and question 10 "Do you know how to calculate pensions in the AFP?") have the smallest PRIDIT weights because of the second criteria discussed in the previous paragraph (i.e., proportions correct close to zero).

The PRIDIT score thus computed is highly correlated with a simple percentage correct tally, and results using either type of aggregation are very similar. Nevertheless, we favor the PRIDIT approach as it incorporates additional information about the relative difficulty of each question and value-added of each question, and we use it in estimates presented below.

Control Variables. Demographic controls included in our specification for equation (1) include *Age* in a quadratic form to account for the typical hump-shaped life-cycle pattern of wealth accumulation. The mean age of our respondents is 43 years, with a standard deviation of 11 years. We also control on the variable *Male*, a dichotomous variable to allow for shifts on average between wealth accumulations for men versus women. Just over half (52 percent) of our respondents are male.

We do not include in the set of controls any variables likely to be determined in part by schooling and financial literacy, and hence possibly affect wealth, such as marital status and current residence.[17] We do include as controls some of the candidate instruments that do not satisfy the second condition for a good instrument, independence of the disturbance term in equation (1), which are apparently correlated with factors that have direct effects on wealth

[17] We adopt this approach because we are interested in the gross effect of schooling and financial literacy, not net of effects through such behaviors as marital status and current residence. Moreover, if we were to include such variables it would be necessary to treat them as endogenous, but it is difficult to increase the number of endogenous variables beyond the two on which we focus. For this reason, our approach thus assumes that these are among the channels through which schooling and financial literacy work to affect wealth. (Below we explore the robustness of our estimates to the inclusion of such factors in the second-stage estimates, but without treating them as behaviorally-determined.)

accumulation in addition to any effects that work through schooling and financial literacy. We indicate which variables these are in our discussion of the results below.

Candidate First-Stage Instruments*:* As is generally the case, we cannot identify good instruments *a priori*, only possible candidate instruments that might predict schooling and financial literacy well, while not being correlated with the second-stage disturbance. Even experiments that directly affect schooling and financial literacy might not be good instruments if they have weak effects on schooling and financial literacy (and therefore do not satisfy the first condition), or if they affect wealth directly through some other channel than schooling and financial literacy (and therefore do not satisfy the second condition). In what follows, we consider as three broad sets of candidate instruments: *Age-dependent* variables, *Family Background* factors, and *Respondent Personality* traits. We describe each in turn.

For the *Age-dependent variables,* we include factors indicative of where the respondents attended primary school as children, how old they were when an innovative national voucher program was implemented by the government in 1981, what macroeconomic conditions were when they were of an age to have been making marginal schooling and labor market entry decisions, and what pension marketing practices prevailed when they were of an age to have completed initial job searches and to have settled in more permanent positions. These four variables are as follows:

- *Primary School in Urban Area:* In Chile, as in many countries, urban primary schools on average tend to be better and have a wider range of options, which may lead to more learning relevant for financial literacy and greater schooling attainment. Chile is a fairly urban country and 81 percent of the respondents did attend primary school in urban areas.
- *School Voucher Exposure (years of school age under voucher system):* In 1981, the Chilean government adopted a national school voucher system for primary and secondary school. Anyone turning age 18 prior to 1981 therefore had no exposure, whereas younger individuals had varying numbers of years of exposure to the new school voucher program. We posit that this exogenous policy change may have had significant effects on

individual schooling attainment and financial literacy. At the same time, the introduction of school vouchers could also have had direct effects on wealth accumulation through increasing schooling quality, beyond direct effects on financial literacy and schooling attainment. For instance, Bravo, Mukhopadhyay, and Todd (2010) report that this schooling reform improved schooling quality and resulted in subsequent higher labor market earnings for adults exposed to the voucher system when they were children. Our respondents averaged 2.2 years of exposure to the voucher system when they were of primary school age and 1.8 years of exposure to the voucher system when they were of secondary school age, but with a fair amount of variance among respondents depending on when they were born. In fact, a substantial majority of our respondents (73 percent) had no exposure to the school voucher system at all due to having been older than age 18 at the time of the reform.

- *Macroeconomic conditions around the time of the school-leaving/labor-market-entry decision:* It is also likely that the state of the macroeconomy around the age respondents made school-leaving and labor market entry decisions influenced both their schooling attainment and financial literacy. For this reason we control for the *unemployment rate* in the Santiago metropolitan area at the time the individual was age 16, since these rates (but not national rates) are available for a sufficiently long time period and a large fraction of the population lives in the capital city.
- *Pension marketing activities around end of early adult job search:* We also posit that AFP marketing agents and expenditures early in a respondent's work life could increase financial literacy, by enhancing awareness of wealth accumulation in general and of pensions in particular. Accordingly, we measure the number of marketing agents and AFP marking expenditures around the time the individual completed initial labor market search and settled down in more permanent employment, around age 24. But such AFP marketing activities might also have direct effects on wealth accumulation in addition to indirect effects through financial literacy (or possibly schooling, though most respondents completed their schooling prior to age 24), a pathway we test below. In fact, there was substantial variation in the number of AFP marketing agents and marketing expenditures across respondent birth cohorts; at the same time, almost 40 percent of respondents were older than 24 before the AFP system was implemented, so for them marketing activities around this age were zero.

We posit that these four conditions are unlikely to have been affected by conscious decisions by either the respondents when they were young, or their families, to increase respondents' subsequent wealth levels. That is, we assume that respondents' parents did not move to urban areas when the children were in primary school for reasons correlated with the respondents' later wealth accumulations, and that neither the respondents nor their parents could affect national schooling voucher policies, macroeconomic conditions, or AFP marketing. Nevertheless, some of these variables might not satisfy the second condition for good instruments, as we note above and test in the empirical work.

For the *Family Background Variables*, it is well-known that there are strong empirical links between family background and schooling attainment, and family background is included among instruments in some previous studies where schooling attainment is a right-side explanatory variable.[18] We argue that a similar association exists with financial literacy (though there is no literature to date on the topic), and accordingly family background should meet the first condition for a good financial literacy instrument as well. Nevertheless, it seems *a priori* plausible that family background could also proxy for factors such as intergenerationally correlated ability endowments via channels other than schooling and financial literacy that directly affect wealth.[19] Accordingly, we include indicators of family background in our set of candidate instruments, but we test whether they satisfy the second condition for being good instruments. The specific family background indicators we include are:

- *Paternal and Material Schooling Attainment:* These averaged 7.2 and 6.6 grades, respectively, indicating considerable intergenerational increases in schooling attainment given the respondents' average of 10.4 grades of schooling completed.

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[18] See Hanushek and Welch (2006), as well as studies mentioned in the next note and the citations therein.

[19] For example, studies of the impact of maternal schooling on child schooling find that significantly positive associations become much smaller or even reversed in sign if estimation techniques using twins data, adopted children, or policy changes are used to control for unobserved intergenerationally-correlated endowments such as ability (e.g., Behrman and Rosenzweig 2002, 2005; Black, Devereux, and Salvanes 2005; Plug, 2004).

- *Poor Economic Background when Child:* Some eight percent of respondents characterized their childhood family economic background as poor.
- *Respondent Worked when Under 15 Years of Age:* Child labor generally is associated with poorer family backgrounds; in our sample; 7 percent of respondents reported that they had started to work when younger than 15 years of age.

*Respondent Personality Trait*s are enduring individual characteristics that generally reflect genetic endowments and earlier life experience rather than states that change over fairly short time periods for adults. McCrae and Costa (1990), for example, report that both many longitudinal studies following the same individuals over time and cross-sectional comparisons across different age groups show a high degree of stability in personality traits during adulthood.[20] Heritability variance decompositions using twins data typically attribute half (and sometimes much more) of the overall variance of personality traits to genetic variation, and the remaining variance is mostly due to early life experience. [21] For this reason, we posit that some personality traits observed in our data are relatively stable and may have significant effects on schooling and financial literacy. Of course they may also have direct effects on wealth accumulation in addition to indirect effects through schooling and financial literacy, and thus they could violate the second condition for good instruments, something that we also test below. The specific variables we use from the EPS are as follows:

[20] For some other examples see Kahnemann (1999:14) who argues that, with respect to wellbeing or happiness, "each individual may be on a personal treadmill that tends to restore well-being to a predetermined setpoint after each change of circumstances." Csikszentmihalyi and Jeremy (2003: 185–186) conclude that "chance events like personal tragedies, illness, or sudden strokes of good fortune may drastically affect the level of happiness, but apparently these effects do not last long." And Costa et al. (1987: 54) report that "objective circumstances appear to be limited in the magnitude, scope, and particularly duration of their effects on psychological well-being, which, in the long run, is likely to reflect instead stable characteristics of the individual." Easterlin (2005) reviews the psychological literature with respect to this "set-point theory" of happiness.

[21]Lykken and Tellegen (1996) report that variation in the well-being component of the Multidimensional Personality Questionnaire for twins in the Minnesota Twin Registry is primarily associated with genetic variation; that is, genetic effects account for up to 80 percent of the variance in happiness indicators obtained by averaging repeated measures of well-being. Moreover, socioeconomic status, schooling, family income, marital status, and religious commitment do not account for more than three percent of the variance in these averaged measures of well-being. In another example, Bouchard and McGue (2003) summarize the estimated heritabilities for the "Big Five" personality traits to be about one-half.

– *Risk Aversion*, referring to a respondent's reluctance to accept a risky but possible more rewarding alternative versus a choice with a more certain but lower expected payoff. This is measured using a dichotomous variable for a positive answer for Alternative A to the following question:[22]

> *Suppose that you, as the only source of household income, have to choose between the following two jobs. What alternative would you choose in [this] situation?*
> ***Alternative A**. A fixed income job that is stable for life.*
> ***Alternative B**. A job where you have the same possibility of earning double or only three quarters of your income for the rest of your life.*

By this measure almost two-thirds (65 percent) of our sample is risk averse.[23]

– *Self Esteem* is used by psychologists to refer to an individual's overall evaluation of his or her own worth (Mruk 2006). For empirical research, this is usually assessed with a self-report inventory such as the Rosenberg Self-Esteem Scale that usually uses a 10-question battery scored on a four-point response system that requires participants to indicate their level of agreement with a series of statements about themselves. The 2006 EPS applied a version of the Rosenberg test as follows:

> *Finally, we ask about the level of agreement or disagreement with the following statements: [Scale: 1. Strongly Disagree, 2. Disagree, 3. Agree, 4. Strongly Agree]*
> *1. I feel that I am a valuable person, at least with respect to others*
> *2. I feel that I have a number of good qualities*
> *3. I definitely tend to think that I am a failure*
> *4. I can do things as well as other people*
> *5. I do not feel that I have much to be proud of*
> *6. I have a positive attitude about myself*
> *7. All in all, I am happy with myself*
> *8. I would like to have more self-respect*
> *9. I sometimes feel useless*
> *10. I sometimes feel that I am good for nothing.*

In our analysis, we focus on a measure of *positive self-esteem* defined as the sum of the answers to questions 1, 2, 4, 6 and 7, as well as a measure of *negative self-esteem* defined as the sum of the answers to questions 3, 5, 8, 9 and 10.[24] Both sums range from 1 to 20, with a mean

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[22] This is third question in a series of three alternative pairs, where the previous two indicate riskier options for Alternative B.

[23] For other recent studies linking risk aversion and economic behavior, see Dohmen et al. (2010 a, b), Eckell et al. (2005); and Guiso et al. (2005, 2008). Borghans, Duckworth, Heckman and ter Weel (2008) provide another recent discussion about personality traits and economics.

[24] We also considered a combined index defined as *positive self-esteem – negative self-esteem*, but the two separate indices have greater predictive power so we include them separately in our estimates.

for *positive self-esteem* of 16.1 and for *negative self-esteem* of 10.5. Each of the five respective components in each sum is weighted equally, so we also investigate whether any of the components has significantly different effects than the sums.

Finally we also include a vector of *other respondent characteristics* for robustness tests in some alternative specifications. We do not include these in our basic estimates because many could argue they are endogenous, including residence in the Santiago metropolitan area at the time of the survey (38 percent), self-reported bad (6 percent) or good health (69 percent), being never married (23 percent) or married at the time of the survey (66 percent), being a household head (56 percent) or spouse of household head (24 percent) at the survey date.

4 Ordinary Least Squares Estimates

Since most prior studies have used ordinary least squares (OLS) models that did not treat schooling and financial literacy as behaviorally determined or imprecisely measured due to random measurement errors, we also begin with OLS estimates to describe the associations among schooling, financial literacy, and the wealth, pension density and retirement planning outcomes. Results appear in Table 4 (control variables are identical to those used later in IV models so estimates can be compared across results).

Table 4 here

Panel I of Table 4 reports estimated coefficients for a specification that includes only the PRIDIT index of financial literacy and excludes schooling (equation 1a-subvariant 1). Results indicate that the PRIDIT index is positive and strongly statistically significant in all four wealth equations, the density of pension contributions, and the probability of calculating the money needed for retirement. Moreover, the estimates are quantitatively important, implying that a 0.2 standard deviation increase in the PRIDIT index (taken from Table 2) is associated with an average \$4,000 increase in net wealth, or almost 6 percent increase in mean net wealth. The largest response is for pension wealth ($2,200), with other wealth ($1,000) and housing wealth ($800) less than half as large. A 0.2 standard deviation increase in PRIDIT is also associated with an average increase of 1.4 percent in the density of pension contributions and 0.8 percent increase in the probability of calculating the money needed for retirement.

Panel II of Table 4 provides coefficient estimates for an OLS specification that includes schooling as an explanatory variable but excludes the PRIDIT financial literacy measure (equation 1a-subvariant 2). Schooling coefficient estimates are positive and highly significant for all four wealth measures, the density of pension contributions, and retirement planning. Moreover they are substantial and even somewhat larger than the PRIDIT effects in that they imply that that a 0.2 standard deviation increase in schooling (taken from Table 2) is associated with an average $5,900 increase in net wealth, or 8.3 percent of mean net wealth. The largest component of this overall wealth increment again is pension wealth ($3,000), with other wealth ($1,600) and housing wealth ($1,300) taken together almost as large. A 0.2 standard deviation increase in schooling is associated with an average increase of 1.4 percent in the density of pension contributions and 0.6 percent increase in the probability of calculating money needed for retirement.

Yet the PRIDIT financial literacy index and schooling are significantly positively correlated (r=0.51), so their coefficients estimates are anticipated to change when both are included in the same regression. Indeed this is the case, as is shown in Panel III of Table 4 (equation 1a), where the PRIDIT coefficient estimates drop by a quarter for the density of pension contributions to two-thirds for housing wealth, with total wealth in between, dropping by half. The schooling coefficient estimates also decline, though by less, around 10 percent for housing wealth and 16 percent for total wealth. Consequently, one can conclude that including only one of these two explanatory variables in OLS regressions produces larger estimates for each, than if both are included. Nevertheless, when both are included, the associations remain significant and fairly large in magnitude for both.

As noted in the discussion of equation (1), having only linear terms for schooling and financial literacy is but one possible specification choice. Adding interactions between schooling and the PRIDIT financial literacy index in addition to the linear terms alters the results somewhat (Panel IV of Table 4, equation 1b). The linear schooling coefficient estimates remain about the same magnitudes as without the interaction term, and they are estimated with greater precision. Yet the linear PRIDIT coefficient estimates become negative and significant for three of the four wealth outcomes (of course, the total association of PRIDIT with the wealth outcomes for any schooling attainment must include the interaction with schooling attainment). For the density of pension contributions, in contrast, the coefficient estimates of both the PRIDIT and the schooling terms remain significantly positive and the coefficient estimates of the interaction is significantly negative. For the retirement planning variable, the coefficient estimates on both the PRIDIT and the schooling terms also remain significantly positive but the coefficient estimate of the interaction is insignificant.

If the specification of the effects of financial literacy and schooling is limited to only the PRIDIT-schooling interaction term (Panel V in Table 4, equation 1c), the coefficient estimates of the interaction are positive, significant, and fairly substantial for all of the wealth outcomes and for the density of pension contributions and retirement planning.

5 Instrumental Variable Estimates

As noted above, omitted variables and/or measurement error can bias measured OLS coefficients, so next we turn to instrumental variable (IV) estimates using the candidate instruments discussed above. Some of the candidate instruments – namely, years of exposure to school vouchers when of school age, AFP marketing efforts, family background, and risk aversion – do not appear to be independent of the second-stage disturbance term but do seem to affect wealth accumulation and the density of contribution directly, in addition to indirect effects through schooling and financial literacy. This result suggests that the Hansen J statistic has some power in identifying problematic candidate instruments, and we include all these variables as controls in results to follow (as well as in the OLS estimates discussed above). Our remaining instruments, discussed at the end of this paragraph and listed in Appendix Table B, work quite well. First, they predict both financial literacy and schooling well as is required by the first condition for good instruments. For instance, when financial literacy and schooling enter linearly, the F tests for excluded instruments respectively are 156.56 and 215.57 (prob > F = 0.0000 in both cases), the Angrist-Pischke multivariate F test of excluded instruments respectively are 17.19 and 24.58 (prob > F = 0.0000 in both cases), and the Kleibergen-Paap Wald F weak identification test statistic is 15.767, indicating between 5 percent and 10 percent maximal IV relative bias according to the Stock-Yogo weak ID test critical values. Second, they are independent of the second-stage disturbance term as required by the second condition for good instruments. The Hansen J statistic for the overidentification test of all instruments' p values are from 0.26 to 0.92 for the linear specification in equation 1a (see Table 5), indicating that our instruments are independent of the disturbance term in the second-stage relation.[25] The patterns of significant coefficient estimates are also plausible *a priori*: positive effects are recorded for having had primary schooling in an urban area and positive self-esteem, and negative effects of unemployment rates when age 16 and negative self-esteem (with some significant deviations from equal weighting for some of the components of esteem).[26] Interestingly, negative self-esteem is a much more important predictor of both financial literacy and schooling than is positive self-esteem.

[25] The p values also are satisfactory for the specification with only the PRIDIT measure of financial literacy in Panel I of Table 5. For the specification with only schooling in Panel II of Table 5 the p values for four of the outcomes are below 0.011. However if the true relation is equation (1a), this relation is actually mis-specified because financial literacy is excluded, which could account for this result.
[26] See Appendix B for a complete set of estimates.

When only the PRIDIT financial literacy index is included and instrumented (equation 1a-subvariant 1), the coefficient estimates are positive, significant, substantial, and twice to three times larger than the OLS estimates presented earlier (compare Panel I of Table 5 with Panel I of Table 4). When only schooling is included and instrumented (equation 1a-subvariant 2), the coefficient estimates are positive, significant, substantial, and from 16-84 percent larger than the comparable OLS estimates (Panel II of Table 5 versus Panel II of Table 4). But when we include *both* the instrumented schooling and the PRIDIT financial literacy variables (equation 1a), the schooling effects mostly become statistically insignificant and negative, whereas the PRIDIT effects are positive, significant, substantial and much larger than the OLS results (compare Panel III in Tables 4 and 5; the latter effects are 282-1775 percent larger for the PRIDIT variable) for all of the dependent variables except retirement planning. The effect of financial literacy on retirement planning is no longer statistically significant when we instrument both financial literacy and schooling.

This pattern suggests that, if equation (1a) is the true model, OLS coefficient estimates substantially understate the effect of financial literacy on wealth accumulation, due to random measurement error and omitted variable bias. This may be due to omitted factors in the OLS framework that are negatively associated with wealth accumulation but positively correlated with financial literacy. For example, as noted above, over-cautious individuals who invest more in financial literacy may be less successful in accumulating wealth.[27] If the IV estimates can be interpreted causally, as we argue is appropriate, these estimates mean that financial literacy is a powerful determinant of wealth and pension contributions. Specifically, they imply that a 0.2 standard deviation increase in the PRIDIT financial literacy score could, on average, raise net wealth by $13,800, broken down into about a $5,200 boost in pension wealth, a $1,600 rise in net

[27] Another example of such positive selection is offered by Finkelstein and McGarry (2006) who show that long-term health care insurance buyers tend to be healthier, which they interpret to indicate that cautious people buy insurance and take good care of their health.

housing wealth, and a gain of $6,900 in other wealth. The same 0.2 standard deviation increase in the PRIDIT financial literacy score would also boost the density of pension contributions by on average of 3 percent and the probability of calculating retirement money by an average of .5 percent. In other words, increased financial literacy can have relatively large payoffs in wealth, particularly pension and other wealth, and less so in terms of housing wealth.

Since the two sets of the HRS questions have also been introduced recently in other international surveys, we assess the marginal impacts of correct responses of the individual questions on each of the six outcomes of interest. Table 6 gives simulated impacts for the "core" as well as the "sophisticated" HRS questions, based on the linear specification of 0.2 standard deviation increases in correct responses to individual questions underlying the PRIDIT estimates. The findings suggest that knowing the correct answers to the HRS "core" questions has a particularly strong impact. It is also of interest that schooling has only a small and insignificant impact when both factors are instrumented. In other words, if the true model is equation (1a) for these outcomes, mis-specifying the relation by leaving out financial literacy (as Table 5, Panel II) leads to rather misleading estimates of the impact of schooling on household wealth accumulations.[28] Our preferred linear estimates (Table 5, Panel III) for these outcomes suggest that it is financial literacy that actually counts, rather than increasing general schooling attainment.[29]

Adding a PRIDIT-schooling interaction term in Panel IV of Table 5 (equation 1b) results in coefficient estimates for the interaction terms that are positive for all wealth components (though not for the density of pension contributions or retirement planning) and substantially

[28]As well as problems with the Hansen's J-test for overidentifying restrictions that suggests that the second condition for good instruments is not satisfied, as noted above.

[29] For other wealth, the schooling coefficient estimate is significant and fairly large but negative (which carries over at the 10% significance level for total wealth); this result is surprising and may be implausible. We conjecture that this negative schooling coefficient estimate may reflect some interaction with financial literacy that also probably underlies the relatively large coefficient estimate for financial literacy for this outcome.

more precisely estimated than the linear PRIDIT and schooling terms for three of the four wealth components. Indeed the interactions are the only variables that have significantly nonzero coefficient estimates for the wealth components at even the 0.10 level (and for total wealth at the 0.05 level), which suggests a specification that includes only the interaction between PRIDIT and schooling (as in Panel V of Table 5, equation 1c). The diagnostics for such estimates are good: the F test for excluded instruments[30] is 109.74 (prob > F = 0.0000) and the Kleibergen-Paap rk Wald F weak identification test statistic is 109.74, indicating substantially less than 5 percent maximal IV relative bias according to the Stock-Yogo weak IV test critical values. With regard to the second condition for good instruments, the Hansen J statistic for the overidentification test of all instruments p values are from 0.39 to 0.95 for the wealth components (last row of Table 5), suggesting that our instruments are independent of the disturbance term in the second-stage relation.[31] The root mean squared errors are somewhat smaller for the interactive specification in Panel V (equation 1c) than for the linear specifications for other wealth and total wealth (Panel III, equation 1b), but slightly larger for pension wealth, household wealth, the density of pension contributions, and retirement planning.

These findings indicate that a case can be made to favor the interactive over the linear specification for other wealth and total wealth, though the estimated effects for the interactive specification are, in any case, substantial for the wealth components and similar to those for the linear model in Panel III of Table 5 discussed above. They imply that a 0.2 standard deviation increase PRIDIT in the interactive format would induce a $11,600 increase in total net wealth, attributable to a $5,900 increase in pension wealth, a $3,600 increase in other wealth, and a $2,100 increase in housing wealth. These are somewhat bigger than the implied effects the same PRIDIT change in the linear model for pension wealth (13 percent bigger) and housing wealth

[30] In the one endogenous variable case, this also is the Angrist-Pischke multivariate F test of excluded instruments.
[31] For the density of contributions this test also is satisfactory with a probability of 0.21. For the financial planning indicator, however, it is less so with a probability of 0.06.

(by 28 percent), but substantially smaller for other wealth (48 percent smaller) and somewhat smaller for total wealth (by 16 percent).[32]

Finally, we assess how robust our estimates are to specification changes (results available on request). For example, given intergenerational correlations in schooling (significant correlations of 0.34 with fathers, 0.38 with mothers), including parental schooling as a control in the second-stage could change estimated impacts of respondent schooling attainment. Interestingly, we find no substantial changes in our variables of interest. Similarly, it might be thought that including the family background variables as second-stage controls could make a difference, but again there are no substantial changes in results. Finally, we allow for the possibility that respondent characteristics at the time of the survey, such as current urban residence, current health, marital status, household head/spouse, could enter the second stage; again the relevant coefficient estimates are robust.

6 Discussion and Implications

In this paper we use an instrumental variable approach to identify the impact of financial literacy and schooling on wealth accumulation and pension contribution patterns. Prior studies have linked financial literacy and schooling with positive financial outcomes, but they usually do not control for unobserved factors that might shape both financial literacy and schooling, as well as wealth outcomes, nor do they control for possible measurement error in financial literacy and schooling. Using an IV approach (and conditional on our specification assumptions), we have isolated the causal effects of financial literacy and schooling on wealth outcomes using plausibly exogenous variation of instruments available in the Chilean Social Protection Survey. Results for a nationally-representative sample of adults indicate that financial literacy and schooling

[32] The interactive estimates seem more plausible for other wealth and possibly total net wealth, because the relatively large positive coefficient estimate for financial literacy in the linear case (Table 5, Panel III) may compensate for the substantial significantly negative coefficient estimate for schooling.

attainment are both positively and significantly correlated with wealth, pension contributions, and retirement planning using OLS, while the IV estimates uncover an even stronger positive impact of financial literacy. They also indicate no significant positive effects of schooling attainment, conditional on financial literacy, in a linear specification, though the effect is positive when interacted with financial literacy.

There are several implications of our findings. First, prior studies using OLS models to estimate the effects of financial literacy and schooling are likely to be misleading due to measurement error and unobserved factors. IV estimates indicate that financial literacy is at least as important, if not more so, than schooling, in explaining variation in household wealth and pension contributions. Second, our improved estimates of the impact of financial literacy are economically meaningful and potentially quite important. Indeed, in our view they are substantial enough to imply that investments in financial literacy could well have high payoffs. Third, our estimates indicate that some components of financial literacy, such as the HRS "core" questions, are particularly important. This insight would not have been gained with the most representations of financial literacy (e.g., percentage correct) used in the previous literature, Fourth, our paper contributes to a growing body of research on the factors influencing peoples' attachment to financial markets. Households that build up more net wealth, particularly via the pension system, may be better able to smooth consumption in retirement and thus enhance risk-sharing and wellbeing in old age. Our finding that financial literacy enhances peoples' likelihood of contributing to their pension saving suggests that this is a valuable pathway by which improved financial literacy can build household net wealth.

In future work we hope to evaluate in more detail the costs as well as the benefits of enhancing financial literacy levels. Nevertheless, we view as very important the central finding of this paper that individuals, firms, and governments can enhance household wealth and wellbeing by investing in financial literacy.

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TABLE 1. SUMMARY STATISTICS FOR DEPENDENT VARIABLES.

	Mean	St. Dev.	Min.	Max.
Wealth components (2006 US$ 1000s)				
Pension Wealth	38.6	87.3	0	1076.8
Housing Wealth	22.1	43.7	-223	1395.8
Other Wealth	10.9	123	-553.8	11572
Total Wealth	71.5	166.1	-519.7	11985
Pension Density (%)	47.7	34.8	0	100
Calculated Retirement Money (%)	7.9	26.9	0	100

Note: Calculated from Chilean 2006 Social Protection Survey (EPS) on a sample of 13,054 respondents age 24-retirement age (60 for women, 65 for men) for whom key variables (including wealth outcomes, financial literacy, schooling) are available. See text.

TABLE 2. SUMMARY STATISTICS FOR RIGHT-SIDE VARIABLES AND CANDIDATE INSTRUMENTS.

	Mean	St. Dev.	Min.	Max.
Key Right-Side Variables				
Financial Literacy PRIDIT Score	0.6	7.7	-15.3	23.3
Schooling Attainment (Grades)	10.4	3.9	0	20
PRIDIT*Schooling Attainment	15	84.3	-259.5	466.4
Right-Side Demographic Controls				
Age (Years)	42.7	10.6	24	65
Male (%)	51.9	50	0	100
Candidate First-Stage Instruments				
I. Age-Related Variables				
Primary School Urban (%)	80.9	39.3	0	100
School Vouchers				
Primary Ages	2.2	3.2	0	8
Secondary Ages	1.8	1.9	0	4
Total	4	4.9	0	12
AFP Activities at Age 24				
Number of Agents (10^3)	6.2	6.9	0	23.7
Marketing Expenditures (P10^9)	2.2	2.7	0	10.1
No AFP Activities (%)	39.2	48.8	0	100
Macroeconomy at Age 16				
Unemployment Rate (%)	10.3	5.2	3	23.2
II. Family Background				
Father Schooling Attainment (Grades)	7.2	3.5	0	20
Mother Schooling Attainment (Grades)	6.6	3.4	0	20
Poor Economic Background (%)	8	27.1	0	100
Respondent Worked < 15 Y of Age (%)	6.9	25.4	0	100
III. Personality Traits				
Risk Aversion (%)	64.5	47.9	0	100
Positive Self Esteem (Sum)	16.1	2.5	5	20
Valuable Person	3.2	0.7	1	4
Number of Good Qualities	3.2	0.6	1	4
Can Do Well As Others	3.3	0.7	1	4
Positive Attitude about Self	3.2	0.7	1	4
Happy with Self	3.2	0.7	1	4
Negative Self Esteem (Sum)	10.5	2.6	5	20
Think I am a Failure	1.8	0.7	1	4
Not Much to be Proud of	2.1	0.9	1	4
Like More Self Respect	2.6	0.8	1	4
Sometimes Feel Useless	2.2	0.8	1	4
Feel Good for Nothing	1.9	0.8	1	4
Selected Current Characteristics				
Santiago Metropolitan Residence (%)	38.4	48.6	0	100
Health Bad (%)	6.4	24.5	0	100
Health Good (%)	68.7	46.4	0	100
Never Married (%)	22.8	41.9	0	100
Now Married (%)	65.7	47.5	0	100
Household Head (%)	55.5	49.7	0	100
Spouse of Household Head (%)	24.3	42.9	0	100

Note: For sample see Table 1.

TABLE 3. FINANCIAL LITERACY QUESTIONS: PERCENT CORRECT AND PRIDIT WEIGHTING SCHEME

	Question	Correct (%)	PRIDIT weights
Concepts of basic economics and finance			
	Main HRS questions:		
(1)	If the chance of catching an illness is 10%, how many people out of 1000 would get the illness?	50%	0.64
(2)	If 5 people share winning lottery tickets and the total prize is 2 Million pesos, how much would each receive?	44%	0.59
(3)	Assume that you have $100 in a savings account and the interest rate you earn on this money is 2% a year. If you keep this money in the account for 5 years, how much would you have after 5 years? [over $120, exactly $120, less than $120]	50%	0.59
	Additional questions:		
(4)	Assume that you have $200 in a savings account, and the interest rate that you earn on these savings is 10% a year. How much would you have in the account after 2 years? [exact number]	2%	0.29
(5)	Assume that you have $100 in a savings account and the interest rate that you earn on these savings is 1% a year. Inflation is 2% a year. After one year, if you withdraw the money from the savings account you could buy more/less/the same?	26%	0.42
(6)	T/F: Buying shares in one company is less risky than buying shares from many different companies with the same money.	46%	0.44
Knowledge of benefit rules and institutions			
(7)	Do you knows what percentage of income is (has been or would be) deducted monthly for pension system contributions? [yes/no]	37%	0.54
(8)	Do you know the legal retirement age for women? [60]	79%	0.44
(9)	Do you know the legal retirement age for men? [65]	84%	0.41
(10)	Do you know how to calculate pensions in the AFP? [yes, by balance of individual account and other elements such as age of retirement]	10%	0.37
(11)	Do you know there is a minimum state guaranteed old age pension for people aged 65 and over? [yes/no]	53%	0.42
(12)	Have you heard of the Voluntary Pension Savings system introduced in 2002? [yes/no]	55%	0.58

Note: For sample see Table 1. The PRIDIT financial literacy score is calculated using the 12 financial literacy questions in the 2006 EPS. Column 1 lists the % of people that answered the question correctly; Column 2 provides PRIDIT weights for each question (see text).

TABLE 4. OLS MODELS OF WEALTH AND PENSION DENSITY: PRIDIT ALONE, SCHOOLING ALONE, AND BOTH, PLUS INTERACTIONS

	Components of Wealth					
	Pension Wealth	Housing Wealth	Other Wealth	Total Net Wealth	Pension Density	Calculated Retirement Money
I. PRIDIT Index Alone						
PRIDIT	1.46	0.49	0.62	2.57	0.91	0.51
t	*12.48*	*8.43*	*5.82*	*14.24*	*25.24*	*14.72*
II. Schooling Alone						
Schooling	3.89	1.7	2.01	7.59	1.76	0.80
t	*14.52*	*12.92*	*5.78*	*15.59*	*21.89*	*9.93*
III. Both PRIDIT and Schooling						
PRIDIT	0.81	0.17	0.25	1.23	0.69	0.44
t	*6.61*	*2.82*	*2.58*	*6.91*	*16.98*	*11.37*
Schooling	3.07	1.53	1.75	6.35	1.06	0.36
t	*10.75*	*11.13*	*4.87*	*12.46*	*11.78*	*4.07*
IV. Linear and Interaction Effects						
PRIDIT	-0.32	-0.87	-1.17	-2.36	1.5	0.29
t	*-1.02*	*-4.9*	*-2.74*	*-3.92*	*14.95*	*3.01*
Schooling	3.14	1.59	1.84	6.56	1.01	0.37
t	*10.75*	*11.62*	*4.83*	*12.4*	*11.22*	*4.09*
PRIDIT*Schooling	0.1	0.1	0.13	0.33	-0.07	0.01
t	*3.56*	*5.54*	*3.3*	*5.83*	*-8.89*	*1.56*
V. Interaction Effect Alone						
PRIDIT*Schooling	0.13	0.05	0.07	0.25	0.06	0.04
t	*11.13*	*8.69*	*5.18*	*12.48*	*19.77*	*12.99*

Note: For sample size see Table 1. A complete set of coefficient estimates is provided in Appendix A. Pension density is included in the estimates as proportion between 0 and 1 but the coefficient estimate is multiplied by 100 in this table to be of the same magnitude as the other coefficient estimates. Other control variables include age, age squared, male, years of exposure to school vouchers when of school age, AFP marketing efforts, family background, and risk aversion. *"t"* refers to t-statistic.

TABLE 5. IV MODELS OF WEALTH AND PENSION DENSITY: PRIDIT ALONE, SCHOOLING ALONE, AND BOTH, PLUS INTERACTIONS

	Components of Wealth					
	Pension Wealth	Housing Wealth	Other Wealth	Total Net Wealth	Pension Density	Calculated Retirement Money
I. PRIDIT Index Alone						
PRIDIT	3.23	1.14	1.83	6.2	1.88	0.62
z	*9.18*	*7.46*	*4.47*	*10.31*	*15.15*	*5.58*
Hansen J p	0.35	0.40	0.09	0.14	0.96	0.29
II. Schooling Alone						
Schooling	5.55	1.98	2.66	10.18	3.23	1.12
z	*9.12*	*7.28*	*4.01*	*10.23*	*14.22*	*5.68*
Hansen J p	0.01	0.17	0.01	0.00	0.00	0.29
II. Both PRIDIT and Schooling						
PRIDIT	3.4	1.06	4.46	8.93	1.94	0.31
z	*3.23*	*1.9*	*3.64*	*4.77*	*4.92*	*0.90*
Schooling	-0.34	0.14	-5.05	-5.25	-0.12	0.58
z	*-0.18*	*0.14*	*-2.6*	*-1.68*	*-0.17*	*0.95*
Hansen J p	0.26	0.30	0.82	0.34	0.92	0.26
IV. Linear and Interaction Effects						
PRIDIT	-2.28	-1.51	0.28	-3.51	2.15	2.19
z	*-0.67*	*-1.01*	*0.09*	*-0.65*	*1.83*	*1.97*
Schooling	2.44	1.4	-3.01	0.82	-0.23	-0.34
z	*0.99*	*1.24*	*-1.22*	*0.21*	*-0.25*	*-0.41*
PRIDIT*Schooling	0.44	0.2	0.33	0.97	-0.02	-0.15
z	*1.75*	*1.7*	*1.26*	*2.36*	*-0.19*	*-1.80*
Hansen J p	0.41	0.39	0.86	0.92	0.85	0.5
V. Interaction Effect Alone						
PRIDIT*Schooling	0.34	0.12	0.2	0.66	0.19	0.06
z	*9.03*	*7.3*	*4.35*	*9.91*	*14.25*	*5.01*
Hansen J p	0.53	0.52	0.39	0.95	0.21	0.06

Notes: For sample size see Table 1. A complete set of coefficient estimates is provided in Appendix B. Pension density is included in the estimates as proportion between 0 and 1 but the coefficient estimate is multiplied by 100 in this table to be of the same magnitude as the other coefficient estimates. Other control variables include age, age squared, male, years of exposure to school vouchers when of school age, AFP marketing efforts, family background, and risk aversion. *"z"* refers to z-statistic; see text.

TABLE 6. PREDICTED CHANGE IN MEASURED OUTCOMES FOR CORRECT ANSWER TO FINANCIAL LITERACY QUESTION

		Pension Wealth ($000)	Housing Wealth ($000)	Other Wealth ($000)	Total Net Wealth ($000)	Pension Density (% pts)	Calculated Retirement Money (% pts)
Core HRS Questions:							
(1)	If the chance of catching an illness is 10%, how many people out of 1000 would get the illness?	1.2	0.4	1.6	3.1	0.7	0.11
(2)	If 5 people share winning lottery tickets and the total prize is 2 Million pesos, how much would each receive?	1.1	0.4	1.5	2.9	0.6	0.10
(3)	Assume that you have $100 in a savings account and the interest rate you earn on this money is 2% a year. If you keep this money in the account for 5 years, how much would you have after 5 years?	1.1	0.3	1.4	2.9	0.6	0.10
Sophisticated HRS:							
(4)	Assume that you have $200 in a savings account, and the interest rate that you earn on these savings is 10% a year. How much would you have in the account after 2 years?	0.6	0.2	0.7	1.5	0.3	0.05
(5)	Assume that you have $100 in a savings account and the interest rate that you earn on these savings is 1% a year. Inflation is 2% a year. After one year, if you withdraw the money from the savings account you could buy more/less/the same?	0.8	0.2	1.0	2.1	0.4	0.07
(6)	T/F: Buying shares in one company is less risky than buying shares from many different companies with the same money.	0.8	0.3	1.1	2.1	0.5	0.07

Note: The table presents simulated marginal effects of increasing the probability of answering each question correctly by a quarter standard deviation (Table 2 indicates mean responses) on outcomes, holding population responses to other questions constant.

APPENDIX A: COMPLETE SET OF OLS COEFFICIENT ESTIMATES FOR TABLE 4*

	Components of Wealth					
	Pension Wealth	Housing Wealth	Other Wealth	Total Net Wealth	Pension Density	Calculated Retirement Money
I. PRIDIT Index Alone						
PRIDIT	*1.46*	*0.49*	*0.62*	*2.57*	*0.91*	*0.51*
Male	*15.74*	*-3.18*	0.18	*12.95*	*19.22*	*2.27*
Age	*4.63*	-0.09	1.38	*5.52*	*2.36*	0.65
Age-squared	*-0.04*	0.01	-0.01	-0.04	*-0.02*	0.00
Risk averse	*-3.99*	-1.17	-3.43	*-8.18*	0.90	-0.14
Voucher exposure primary ages	*0.55*	0.27	0.38	1.15	*0.98*	0.02
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	5.13	1.10	5.09	10.84	*4.05*	0.13
Mother schooling attainment	0.52	*0.87*	*0.69*	*2.05*	0.00	-0.03
Father schooling attainment	*0.48*	*0.79*	*1.31*	*2.58*	-0.15	0.03
Poor economic background	-21.54	-1.14	0.76	*-22.18*	*-36.35*	*-2.17*
Worked when <15 years of age	*-4.84*	*-4.62*	*-7.20*	*-16.90*	*-5.33*	0.08
Constant	-90.09	3.46	-47.90	*-127.14*	*-22.22*	-11.80
II. Schooling Alone						
Schooling	*3.89*	*1.70*	2.01	*7.59*	*1.76*	*0.80*
Male	*19.70*	*-1.81*	1.86	*19.75*	*21.34*	*3.40*
Age	*4.02*	-0.21	1.25	*5.05*	*2.30*	0.64
Age-squared	*-0.03*	0.01	-0.01	-0.03	*-0.02*	0.00
Risk averse	-2.29	-0.62	-2.79	-5.71	*1.47*	0.15
Voucher exposure primary ages	0.42	0.22	0.32	0.96	*0.99*	0.03
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	4.01	0.75	4.70	9.46	*3.88*	0.08
Mother schooling attainment	-0.04	*0.61*	0.39	*0.97*	-0.18	-0.08
Father schooling attainment	0.16	*0.61*	1.10	*1.87*	*-0.22*	0.02
Poor economic background	*-19.68*	0.30	2.31	*-17.07*	*-36.27*	*-2.50*
Worked when <15 years of age	0.62	-1.88	-4.04	-5.30	*-3.22*	0.87
Constant	*-121.33*	-12.29	-66.67	*-200.30*	*-41.48*	-21.24
III. Both PRIDIT and Schooling						
PRIDIT	*0.81*	*0.17*	*0.25*	*1.23*	*0.69*	*0.44*
Schooling	*3.07*	*1.53*	*1.75*	*6.35*	*1.06*	*0.36*
Male	*18.02*	*-2.16*	1.35	*17.21*	*19.93*	*2.51*
Age	*3.97*	-0.22	1.23	*4.98*	*2.26*	0.62
Age-squared	*-0.03*	0.01	-0.01	-0.03	*-0.02*	0.00
Risk averse	*-2.62*	-0.69	-2.89	*-6.21*	*1.19*	-0.03
Voucher exposure primary ages	0.39	0.21	0.31	0.91	*0.96*	0.01
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	3.93	0.74	4.68	9.35	*3.82*	0.04
Mother schooling attainment	-0.05	*0.61*	0.39	*0.94*	*-0.19*	-0.09
Father schooling attainment	0.09	*0.60*	1.08	*1.77*	*-0.28*	-0.01
Poor economic background	*-18.28*	0.59	2.74	*-14.95*	*-35.09*	*-1.75*
Worked when <15 years of age	0.39	-1.93	-4.11	-5.65	*-3.42*	0.73
Constant	*-109.59*	-9.81	-63.05	*-182.45*	*-31.53*	-14.97
IV. Linear and Interaction Effects						
PRIDIT	-0.32	*-0.87*	*-1.17*	*-2.36*	*1.50*	*0.29*
Schooling	*3.14*	*1.59*	*1.84*	*6.56*	*1.01*	*0.37*
PRIDIT*Schooling	*0.10*	*0.10*	*0.13*	*0.33*	*-0.07*	0.01
Male	*17.98*	*-2.21*	1.29	*17.05*	*19.96*	*2.50*

Age	*3.94*	-0.25	1.19	*4.88*	*2.28*	0.62
Age-squared	*-0.03*	0.01	-0.01	-0.03	*-0.02*	0.00
Risk averse	-2.25	-0.35	-2.42	-5.02	0.92	0.02
Voucher exposure primary ages	0.35	0.18	0.27	0.79	*0.99*	0.00
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	4.13	0.92	4.93	9.97	*3.68*	0.07
Mother schooling attainment	-0.10	*0.56*	0.33	*0.79*	-0.16	-0.10
Father schooling attainment	0.05	*0.56*	1.03	*1.63*	*-0.24*	-0.02
Poor economic background	*-19.51*	-0.54	1.16	*-18.89*	*-34.18*	*-1.91*
Worked when <15 years of age	0.42	-1.90	-4.07	-5.54	*-3.44*	0.73
Constant	*-109.92*	-10.12	-63.48	*-183.52*	*-31.28*	-15.02
V. Interaction Effect Alone						
PRIDIT*Schooling	*0.13*	*0.05*	*0.07*	*0.25*	*0.06*	*0.04*
Male	*15.97*	*-3.33*	-0.04	*12.60*	*19.59*	*2.32*
Age	*4.22*	-0.11	1.35	*5.45*	*2.38*	0.65
Age-squared	*-0.04*	0.01	-0.01	*-0.04*	*-0.02*	0.00
Risk averse	*-3.13*	-1.00	-3.22	*-7.35*	*1.10*	0.01
Voucher exposure primary ages	0.47	0.25	0.35	1.06	*1.00*	0.01
Number of agents	0.00	0.00	0.00	0.00	*0.00*	0.00
No AFP activities	*4.95*	1.18	5.18	11.31	*4.30*	0.24
Mother schooling attainment	*0.46*	*0.84*	*0.64*	*1.94*	0.03	-0.03
Father schooling attainment	*0.46*	*0.76*	1.26	*2.47*	-0.10	0.03
Poor economic background	*-23.71*	-1.58	0.26	*-25.02*	*-37.96*	*-2.90*
Worked when <15 years of age	-5.33	*-4.55*	*-7.07*	*-16.95*	*-5.81*	-0.05
Constant	*-82.94*	3.99	-47.08	*-126.02*	*-23.91*	-12.08

Note: *italics indicate significance at 5% level or better. See also Table 4.

APPENDIX B: COMPLETE SET OF IV ESTIMATES FOR TABLE 5; FIRST-STAGE ESTIMATES AND DIAGNOSTICS, AND SECOND-STAGE ESTIMATES AND DIAGNOSTICS*

	Components of Wealth					
	Pension Wealth	Housing Wealth	Other Wealth	Total Net Wealth	Pension Density	Calculated Retirement Money
I. PRIDIT Index Alone						
PRIDIT	*3.23*	*1.14*	*1.83*	*6.20*	*1.88*	*0.62*
Male	*12.66*	*-4.30*	-1.91	*6.44*	*17.54*	*2.09*
Age	*4.32*	-0.22	1.12	*5.22*	*2.19*	0.63
Age-squared	*-0.04*	0.01	0.00	-0.04	*-0.02*	0.00
Risk averse	*-4.19*	-1.23	-3.56	*-8.99*	*0.79*	-0.15
Voucher exposure primary ages	0.38	0.20	0.27	0.86	0.89	0.01
Number of agents	0.00	0.00	0.00	0.00	*0.00*	0.00
No AFP activities	4.43	0.83	4.57	9.83	3.66	0.09
Mother schooling attainment	0.08	*0.71*	0.38	*1.18*	*-0.25*	-0.05
Father schooling attainment	0.01	*0.62*	0.99	*1.62*	*-0.40*	0.01
Poor economic background	-15.08	1.17	*5.18*	*-8.72*	*-32.83*	*-1.79*
Worked when <15 years of age	*-1.50*	*-3.67*	*-5.01*	*-10.17*	*-3.51*	0.28
Constant	-77.73	8.15	-38.71	*-108.28*	*-15.48*	-11.08
Hansen J p	0.35	0.40	0.09	0.14	0.96	0.29
II. Schooling Alone						
Schooling	*5.55*	*1.98*	*2.66*	*10.18*	*3.23*	*1.12*
Male	*19.95*	*-1.73*	2.08	*20.30*	*21.78*	*3.50*
Age	*4.22*	-0.25	1.12	*5.09*	*2.13*	0.61
Age-squared	*-0.03*	0.01	0.00	-0.03	*-0.02*	0.00
Risk averse	-2.21	-0.53	-2.58	-5.32	*1.95*	0.24
Voucher exposure primary ages	0.38	0.20	0.29	0.88	*0.89*	0.01
Number of agents	0.00	0.00	0.00	0.00	*0.00*	0.00
No AFP activities	3.98	0.66	4.47	9.11	*3.40*	-0.02
Mother schooling attainment	-0.38	*0.55*	0.24	0.41	*-0.51*	-0.16
Father schooling attainment	-0.14	*0.56*	0.99	1.42	*-0.49*	-0.03
Poor economic background	*-16.27*	0.81	*3.57*	*-11.90*	*-33.51*	*-1.89*

Worked when <15 years of age	4.51	-1.50	-2.67	0.34	0.00	1.57
Constant	*-140.83*	-14.26	-70.93	*-226.01*	*-52.21*	*-23.58*
Hansen J p	0.01	0.17	0.01	0.00	0.00	0.29
III. Both PRIDIT and Schooling						
PRIDIT	*3.40*	1.06	*4.46*	*8.93*	*1.94*	0.31
Schooling	-0.34	0.14	*-5.05*	-5.25	-0.12	0.58
Male	*12.25*	*-4.13*	*-8.01*	0.11	*17.39*	*2.79*
Age	*4.33*	-0.22	1.26	*5.36*	*2.19*	0.62
Age-squared	*-0.04*	0.01	-0.01	-0.04	*-0.02*	0.00
Risk averse	*-4.31*	-1.18	-5.33	*-10.83*	0.75	0.05
Voucher exposure primary ages	0.39	0.20	0.30	0.89	*0.89*	0.01
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	4.46	0.82	5.11	10.38	*3.67*	0.02
Mother schooling attainment	0.11	*0.70*	*0.88*	*1.69*	-0.23	-0.11
Father schooling attainment	0.03	*0.61*	1.21	*1.85*	*-0.40*	-0.02
Poor economic background	*-15.09*	1.18	*5.12*	*-8.79*	*-32.84*	*-1.78*
Worked when <15 years of age	-1.90	*-3.50*	*-11.08*	*-16.47*	*-3.66*	0.98
Constant	-74.03	6.58	16.60	-50.84	-14.12	-17.47
Hansen J p	0.26	0.30	0.82	0.34	0.92	0.26
IV. Linear and Interaction Effects						
PRIDIT	-2.28	-1.51	0.28	*-3.51*	2.15	*2.19*
Schooling	2.44	1.40	-3.01	0.82	-0.23	-0.34
PRIDIT*Schooling	0.44	0.20	0.33	0.97	-0.02	-0.15
Male	*14.32*	*-3.19*	*-6.48*	4.65	*17.31*	*2.11*
Age	*4.06*	-0.34	1.06	4.79	*2.20*	0.70
Age-squared	*-0.04*	0.01	-0.01	-0.03	*-0.02*	-0.01
Risk averse	-1.91	-0.09	-3.57	-5.57	0.66	-0.75
Voucher exposure primary ages	0.17	0.11	0.14	0.42	*0.90*	0.08
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	4.88	1.01	5.42	11.30	*3.66*	-0.12
Mother schooling attainment	-0.44	0.45	0.47	0.48	-0.21	0.07

Father schooling attainment	-0.38	0.43	0.91	0.96	*-0.38*	0.12
Poor economic background	*-18.83*	-0.52	2.36	*-17.00*	*-32.70*	-0.54
Worked when <15 years of age	2.09	-1.69	-8.14	-7.74	*-3.80*	-0.34
Constant	-99.93	-5.14	-2.47	-107.54	-13.18	-8.88
Hansen J p	0.41	0.39	0.86	0.92	0.85	0.50
V. Interaction Effect Alone						
PRIDIT*Schooling	*0.34*	*0.12*	*0.20*	*0.66*	*0.19*	*0.06*
Male	*11.71*	*-4.64*	-2.67	4.40	*17.11*	*2.03*
Age	*4.18*	-0.26	1.01	*4.93*	*2.12*	0.62
Age-squared	*-0.04*	0.01	0.00	-0.04	*-0.02*	0.00
Risk averse	-3.11	-0.85	-2.92	*-6.88*	*1.41*	0.04
Voucher exposure primary ages	0.23	0.15	0.17	0.55	*0.81*	-0.01
Number of agents	0.00	0.00	0.00	0.00	0.00	0.00
No AFP activities	4.97	1.02	4.86	10.85	*3.99*	0.20
Mother schooling attainment	-0.13	*0.64*	0.23	0.74	*-0.35*	-0.07
Father schooling attainment	-0.20	*0.54*	0.83	1.17	*-0.51*	-0.01
Poor economic background	*-18.00*	0.16	*3.83*	*-14.01*	*-34.69*	*-2.51*
Worked when <15 years of age	-1.02	*-3.49*	-4.51	-9.03	*-3.35*	0.24
Constant	-74.43	9.35	-35.96	*-101.04*	-14.03	-10.94
Hansen J p	0.53	0.52	0.39	0.95	0.21	0.06

Notes: also Table 4.
Source: Authors' computations.

APPENDIX B. FIRST STAGE ESTIMATES

	PRIDIT Index	Schooling	PRIDIT*Schooling
Male	*1.77*	*-0.25*	*19.87*
Age	0.07	*0.09*	1.22
Age-squared	0.00	*0.00*	-0.01
Risk averse	0.17	*-0.25*	-1.65
Voucher exposure primary ages	-0.04	0.03	0.13
Number of agents	0.00	0.00	0.00
No AFP activities	0.04	0.18	-1.28
Mother schooling attainment	*0.17*	*0.18*	*2.31*
Father schooling attainment	*0.19*	*0.13*	*2.50*
Poor economic background	*-2.96*	*-1.51*	*-20.16*
Worked when <15 years of age	*-1.41*	*-1.90*	*-15.13*
Primary school urban	*2.92*	*2.32*	*20.18*
Unemployment rate at age 16	*-6.21*	-1.32	*-58.02*
Think I am a Failure	0.08	*0.12*	1.87
Can Do Well As Others	*0.46*	*0.19*	4.05
Positive Attitude about Self	0.28	0.16	1.91
Like More Self Respect	0.04	*-0.14*	-2.06
Postive Self Esteem (Sum)	0.10	0.01	*1.36*
Negative Self Esteem (Sum)	*-0.58*	*-0.24*	*-6.04*
Constant	-3.25	*8.11*	-31.71
F test stat:	156.56	215.57	109.72
Angrist-Pischke multivariate F stat:			
III. Both PRIDIT and Schooling	17.19	24.58	
IV. Linear and Interaction Effects	2.32	19.05	3.16
Kleibergen-Paap Wald F stat:			
III. Both PRIDIT and Schooling	15.77		
IV. Linear and Interaction Effects	9.43		

Note: *italics indicate significance at 5% level or better
Source: Authors' computations.