

Wall Street and the Housing Bubble: Bad Incentives, Bad Models, or Bad Luck?

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PRELIMINARY

Abstract

We analyze whether mid-level managers in securitized finance were aware of the housing bubble in 2004-2006 using their personal home transaction data. We find little evidence of them timing the bubble or exercising caution in purchasing homes on average relative to uninformed control groups. On the other hand, we find that real estate lawyers, a sophisticated outside group, performed better in their home transactions than securitization managers. Our findings cast doubt on the popular “bad incentives” view of the recent financial crisis that Wall Street employees knowingly ignored warning signs of the housing bubble, as well as the “bad luck” view that the crisis was unpredictable by anyone. Instead, our analysis highlights distorted beliefs as a potentially important contributing factor to the crisis.

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In the aftermath of the recent financial crisis, the role played by Wall Street during the housing bubble that preceded the crisis has emerged as one of the focal points in numerous post-crisis debates. A popular view posits that moral hazard caused Wall Street employees to ignore clear warning signs about the presence of an unprecedented housing bubble and the imminent risk of the bubble bursting. According to the Financial Crisis Inquiry Report (2011) of the Financial Crisis Inquiry Commission formed by the U.S. Congress:

“In the decade preceding the collapse, there were many signs that house prices were inflated, that lending practices had spun out of control, that too many homeowners were taking on mortgages and debt they could ill afford, and that risks to the financial system were growing unchecked. Alarm bells were clanging inside financial institutions, regulatory offices, consumer service organizations, state law enforcement agencies, and corporations throughout America, as well as in neighborhoods across the country. Many knowledgeable executives saw trouble and managed to avoid the train wreck.”

The Academy Award-winning documentary “Inside Job” vividly attributes the crisis to Wall Street insiders taking advantage of uninformed borrowers and investors. Consistent with this “bad incentives” view, there is evidence that employees in securitized finance profited from lucrative fees and bonuses by selling securities backed by dubious-quality subprime mortgage loans to uninformed investors and taking massive housing price risks for their firms (e.g., Keys, et al. (2010), Berndt and Gupta (2009), and Bebchuk, Cohen and Spamann (2010)).

Building on the premise that Wall Street employees anticipated earlier than others, the bad incentives view holds that the crisis was avoidable if appropriately designed incentives and necessary government oversight were in place, and has thus stimulated intensive calls for more stringent regulation of the financial system. However, there are open disagreements among policy makers and academic researchers about this view, and, in particular, regarding whether Wall Street employees were truly aware of the housing bubble. Interestingly, one of the two minority reports contained in the Financial Crisis Inquiry Report (2011) challenges the premise that warning signs were clear to people in finance, and instead attributes them to hindsight:

“There always are [warning signs] if one searches for them; they are most visible in hindsight, in which the Commission majority, and many of the opinions it cites for this proposition, happily engaged.”

Two salient competing views argue Wall Street employees might not have anticipated the housing bubble (e.g., Gerardi, et al. (2008) and Barberis (2012)). One of the competing views

emphasizes that Wall Street employees were too optimistic and their over-optimism induced them to sell securities backed by dubious-quality mortgage loans to investors and to take massive housing market risks for their firms. This occurred either because they used bad models to over-extrapolate past growth of home prices (e.g., Coval, Jurek and Stafford (2009)), or because psychological biases and cognitive dissonance caused them to ignore risk and warning signs (e.g., Gennaioli, Shleifer, and Vishny (2011) and Benabou (2011)), or because optimistic shareholders used short-term stock price based compensation to select and motivate optimistic managers (e.g., Bolton, Scheinkman and Xiong (2006)). According to this “bad models” view, distorted beliefs and over-optimism on Wall Street resulted in individuals, even those properly incentivized, failing to anticipate the housing market crash.

The other competing view attributes the crisis to an enormous negative tail shock that led to the collapse of housing markets across the U.S. Instead of blaming distorted beliefs, this “bad luck” view maintains that rational individuals, even ones with the right incentives, would not have assigned a high probability, ex-ante, to the presence of the housing bubble and the subsequent crash. In effect, this view posits that no one could have seen the crash coming.

Motivated by these views, we examine the following question: *What did Wall Street employees know about the housing bubble and when did they know about it?* The challenge in addressing this question lies with how to isolate their beliefs about the housing markets from their job incentives.

This paper confronts this challenge by exploiting the special nature of housing markets. Different from typical financial assets, residential homes are an indispensable part of everyone’s life. A home typically exposes its owner to housing price risk in hundreds of thousand dollars. As a result, even employees in the financial industry, despite their relatively high incomes, should have maximum incentives to make informed decisions in their home transactions regardless of any potential biased incentive from their jobs. Building on this insight, we use their personal home transactions during the housing bubble to extract information about their beliefs regarding the housing markets at the time.

We focus on a sample of mid-level managers who worked directly in the securitization business, a central part of the housing bubble. We deliberately focus on mid-level managers

rather than top C-suite executives because mid-level managers made many important business decisions in financial firms and because they were closer to housing markets and thus might be more informed of the bubble than C-suite executives. We randomly sample a group of mid-level securitization managers from a publicly available list of conference attendees of the 2006 American Securitization Forum, the largest industry conference. Using the Lexis-Nexis Public Records database, which aggregates information available from public records, such as deed transfers, property tax assessment records, public address records, and utility connection records, we are able to collect the home transaction history of these securitization managers.

We organize our analysis in two steps. In the first step, we address the question of whether the securitization managers knew about the bubble by analyzing whether they were more aware of the housing bubble than *uninformed* control groups, which had no private information about the housing and securitization markets. We distinguish between two forms of awareness, a strong form and a weak form. Under the strong form, the securitization managers knew about the bubble so well that they were able to time the housing markets better than others. That is, securitization managers who were homeowners anticipated the housing price crash and divested homes before the crash in 2007-2009. The awareness might also appear in a weaker form: Securitization managers who were non-homeowners knew enough to be cautious and thus avoided entering the housing markets during the bubble period of 2004-2006. In the second step, we address the question of whether the crisis was predictable by analyzing whether there was any outside group, potentially less influenced by distorted beliefs, who was more aware of the housing bubble than securitization managers.

In the first step, we compare the behavior of securitization managers to that of two uninformed control groups. The first control group consists of a random sample of lawyers who did not practice in real estate law, who were part of the general public with a relatively high income and who were not directly involved in housing markets. We construct this sample to be age and location-matched to the securitization manager sample. Our analysis shows little evidence of securitization managers' awareness of the bubble in their own home transactions. When compared to the non-real estate lawyers, the securitization managers who were non-homeowners were significantly *more* likely to purchase a first home during 2004-2006, and those

who were homeowners were also *more* likely to purchase second homes, rather than divesting homes, during this time.

One might argue that while lawyers in general had high incomes, they did not experience the same enormous wealth shocks to finance employees during the bubble years. To address this concern, we choose the second control group to be a sample of financial analysts covering non-homebuilding companies in the S&P 500. Due to their work outside the securitization and housing markets, they were less likely to be informed about the housing bubble than securitization managers but experienced wealth shocks similar to those experienced by securitization managers during the bubble period. There is no evident difference between the securitization managers and non-housing analysts in their home acquisition and divestiture propensities in 2004-2006. This lack of difference indicates that securitization employees were not more alerted by the housing bubble than analysts working outside the securitization and housing markets. Both of these groups bought more homes than the non-real estate lawyer group during the bubble period.

We also construct a performance index for each individual in our samples to quantitatively measure the returns of the individual's home transactions across the housing boom/bust cycle in 2004-2010. The performance index is defined by the difference between a person's home portfolio return in 2004-2010 and the buy-and-hold return of their initial 2004 home position during the same period. We find no significant difference between the performance of securitization managers and the two control groups. This again indicates that securitization managers were not more aware of the housing bubble than the two less informed control samples.

In the second step, we analyze whether there were other outside groups, potentially less influenced by distorted beliefs, who were more aware of the housing bubble than securitization managers. We focus on a random sample of lawyers who practiced in real estate. Real estate lawyers were well-educated and sophisticated, and possessed direct knowledge of housing markets, as they provided legal services in real-estate related businesses. As they were not direct parties to real estate transactions, they were arguably less susceptible to distorted beliefs caused by biases such as "groupthink" (Benabou (2011)) and the "inside view" bias of Kahneman and

Lovullo (1993), whereby active participants of a market are more likely to treat the decision problem they currently face as unique and believe that “this time is different.” In other words, their lack of direct involvement in transactions might have made them more conscientious observers of the markets than securitization managers. Real estate lawyers thus provide a test group by which we can examine whether distorted beliefs played a role in securitization managers’ behavior. Our analysis shows that real estate lawyers performed significantly better in their home transactions than securitization managers in 2004-2010. They maintained significantly less direct exposure to housing by purchasing less aggressively, and, in some instances, selling more aggressively. We also compare the two samples of real estate lawyers and non-real estate lawyers, who were otherwise similar except their differential knowledge about housing markets. Interestingly, real estate lawyers also performed significantly better than non-real estate lawyers during this period, indicating that real estate lawyers’ beliefs might have made them more aware of the bubble rather than other confounding factors.

Taken together, our analysis gives little support to the bad incentives view that securitization managers knowingly ignored warning signs of the bubble as they on average failed to either time the housing markets or exercise caution in their personal home purchases relative to other less informed groups. Our analysis also casts doubt on the bad luck view, as real estate lawyers, a knowledgeable although less involved group in housing markets, were able to exercise caution in their home transactions. Our findings thus highlight the relevance of distorted beliefs in the recent crisis.

Our results echo the view of Gerardi, et al. (2008) and Foote, Gerardi and Willen (2012), who argue that during the housing bubble, borrowers and investors under-estimated the possibility of large housing price depreciation. By comparing personal home transactions of finance industry employees and lawyers, our micro-level evidence isolates finance industry employees’ beliefs from effects related to their job incentives.

Our analysis complements the literature on the link between bank performance during the financial crisis and executive incentives before the crisis. On one hand, Bebchuk, Cohen, and Spamann (2010) show that the top-five executives of Bear Stearns and Lehman Brothers cashed out large amounts of short-term performance based compensation during 2000-2008 even though

their companies eventually failed in 2008. They interpret this finding as evidence for governance failure leading to short-termist managerial behavior. On the other hand, Fahlenbrach and Stulz (2011) find no evidence of better performance during the crisis by banks with CEOs whose incentives were better aligned with the shareholders. Their finding casts doubts on important roles played by incentives and governance in understanding bank performance during the crisis. Similarly, Cheng, Hong and Scheinkman (2011) find evidence that banks' risk-taking behavior was consistent with shareholders' demands. Our analysis does not aim to test the effects of incentives in isolation of Wall Street employees' beliefs about the housing bubble. Instead, our findings highlight widespread over-optimism among them during the housing bubble, which in turn suggest that ignoring distorted beliefs of Wall Street employees will confound any effects attributed to failures in governance.

Over-optimism among Wall Street employees during the housing bubble helps explain the pro-cyclical leverages of financial firms (e.g., Adrian and Shin (2009)). While it is easy to explain the contraction of leverage during downturns via binding capital constraints, it is puzzling why they choose to expand leverage during booms, when it is easy to raise equity. Our findings also lend support to the shadow banking theory of Gennaioli, Shleifer and Vishny (2011, 2012), which argues that because investors tend to ignore certain unlikely risks, intermediaries have incentives to engineer securities that are perceived to be safe but exposed to neglected risks.

The paper proceeds as follows. Section 1 introduces our empirical hypotheses. Section 2 describes the data, and Section 3 summarizes descriptive statistics. Section 4 reports the empirical analysis, while Section 5 concludes.

1. Empirical Hypotheses

1.1. Competing Views of the Crisis

There are three competing views of the roles played by Wall Street employees during the housing bubble. The popular bad incentives view emphasizes that the recent crisis was avoidable as there were numerous warning signs of the housing bubble and the imminent risk of bubble bursting. As vividly advocated by the Academy Award-winning documentary "Inside

Job,” this view attributes the root of the crisis to moral hazard that caused the employees of financial firms and other well informed insiders to ignore these warning signs.

The recent academic literature has identified several sources of bad incentives, although not necessarily in conjunction with the warning signs of the housing bubble. See Acharya, et al (2010) for an overview of these bad incentives. One of the commonly mentioned bad incentives is the lack of skin in the game in the originate-and-distribute lending model. During the period preceding the crisis, the securitization boom allowed mortgage lenders to pass on the mortgage loans they originated to investors down the securitization chain, which in turn loosened their incentives to scrutinize borrowers. Several recent papers provide evidence consistent with the lax screening of subprime mortgage lenders: Keys, et al. (2010) find that loans made in 2001-2006 to borrowers with FICO scores slightly above 620, an ad hoc threshold widely used in the lending market, were 10%-25% more likely to default than loans made to borrowers with FICO scores slightly below 620; Berndt and Gupta (2009) find that borrowers whose loans were sold in the secondary market under performed other bank borrowers by between 8% and 14% per year on a risk-adjusted basis over the three-year period following the sales of their loans.

Another widely discussed source of bad incentives is short-term performance based compensation schemes for Wall Street executives and traders. As they are compensated by short-term profits booked on their positions at the year end and do not get penalized by the future losses, they have incentives to pursue short-term gains even at the expense of greater future losses. Consistent with such short-term incentives, Bebchuk, Cohen, and Spamann (2010) show that the top-five executives of Bear Stearns and Lehman Brothers cashed out large amounts of compensation in 2000-2008 although their companies failed in 2008.¹

A key element of the bad incentives view is that Wall Street employees knowingly ignored warning signs of the housing bubble. In contrast, two competing views argue that Wall Street employees might not have anticipated the bubble even if they had the right incentives. The bad

¹ Note that the presence of short-term incentives is not necessarily a reflection of governance failure. To the extent that shareholders of these firms might have short-term speculative objectives (e.g., Bolton, Scheinkman and Xiong (2006)), the executives’ short-termist behavior could be aligned with the objectives of the shareholders. Consistent with this notion, Cheng, Hong and Scheinkman (2011) and Fahlenbrach and Stulz (2011) find evidence that the risk-taking behavior of financial firms was consistent with shareholders’ demands.

models view emphasizes that they were too optimistic to fully comprehend the substantial risk presented by the housing bubble, while the bad luck view posits that the crisis was caused by an unpredictable negative tail shock. See Barberis (2012) for extensive discussions of these two views.

According to the bad models view, several reasons might have made Wall Street employees too optimistic about the housing markets during the bubble period. First, they might have used bad models to over-extrapolate the past growth of home prices. The rapid growth of securitization in early 2000s allowed a large number of subprime households to obtain credit that was previously unavailable to them. This credit expansion precipitated the housing market boom (e.g., Mian and Sufi (2009)) and made the previously largely unrelated housing markets in different regions dependent on a common factor---the strength of the credit market. However, the models used by financial firms during the bubble period to value mortgage backed securities were commonly calibrated to historical housing price data and thus ignored the newly emerging correlations between different housing markets. As a result, these models under-estimated the default correlations of different mortgage loans and thus systematic risk in a mortgage pool. See Coval, Jurek and Stafford (2009) for extensive discussions of this issue.

Second, behavioral biases and cognitive dissonance might have also caused Wall Street employees to ignore tail risk and warning signs about the housing bubble. Gennaioli, Shleifer and Vishny (2011, 2012) build a theory of shadow banking in which both investors and financial intermediaries exhibit the so-called local thinking bias. This bias causes them to make inferences based on a selected subset of events, not the entire state space. As a result, during normal times, they ignore unlikely tail risk and only realize the risk after a bad shock, which in turn exacerbates the downturn. Benabou (2011) builds a model of groupthink, in which anticipatory preferences cause agents to distort their beliefs about market or firm-level fundamentals and, in particular, the interaction structure in groups and organizations can make wishful thinking (denial of bad news and warning signs) contagious across agents. Kahneman and Lovallo (1993) argue that active participants of a market are more exposed to the so-called inside view bias, and tend to treat the decision problem they currently face as unique and subsequently ignore past experiences and statistics in evaluating the current project. In effect, active market participants are more likely to think that “this time really is different.”

Third, in a speculative environment with investors holding heterogeneous beliefs about economic fundamentals, firms' shareholders tend to be optimists and, as a result, would prefer to hire optimistic executives and incentivize them to take aggressive investment positions. Bolton, Scheinkman and Xiong (2006) derive such a model, in which current shareholders adopt short-term stock price based compensation contracts to motivate firm executives to seek risk that boosts the shares' resale values to future optimists.

The bad luck view posits that even if managers had proper incentives, they would have missed the housing bubble, not due to their use of bad models, but because the crisis was caused by a perfect storm that was ex ante unpredictable by anyone. Consistent with this view, according to the recently released transcript of a closed-door Federal Reserve Board meeting in May 2006 (e.g., Hilsenrath, Leo, and Derby (2012)), Chairman Bernanke described the cooling of the housing boom as healthy and most other Fed officials were also expecting a manageable slowdown in the housing sector, with little damage to the financial system or broader economy. To the extent that these Fed officials failed to anticipate the severe crisis caused by the housing bubble in 2006, it is reasonable to hypothesize that this crisis was a perfect storm and no one could have systematically anticipated it.

1.2. Empirical Design

The emphasis of our analysis is to examine the extent to which Wall Street employees anticipated the housing bubble. Figure 1 depicts the housing price indices of U.S. and three metropolitan areas: New York, Chicago, and Los Angeles, in 2000-2011. Los Angeles had the most dramatic boom and bust cycle with housing prices increasing by over 150% from 2000 to the peak in 2006 and then crashing down by over 30% in 2006-2009. New York also had a severe cycle with prices increasing by over 100% in 2000-2006 and then dropping by over 20% in 2006-2009. Chicago and the overall U.S. market had less dramatic but nevertheless pronounced cycles with prices increasing by over 60% in 2000-2006 and then falling by over 15% in 2006-2009. Despite the differences in magnitudes, the cycles across different regions were highly synchronized with rapid price expansions in 2004-2006, which we define as the bubble period in our analysis, gradual declines in 2007, followed by steeper falls in 2008-2009.

We choose mid-level managers in the securitization business as our “treatment” group. As securitization was an indispensable part of the housing bubble, understanding the beliefs of securitization managers about the housing markets is important. There are several reasons to analyze the beliefs of mid-level managers rather than C-level executives. First, they made many important business decisions for their firms. It is well known that the positions taken by a few mid-level managers of AIG Financial Products and UBS during the housing bubble led to losses in tens of billions of dollars, which eventually caused financial distress in these firms. Second, mid-level managers were closest to the housing markets. There is a growing notion that perhaps mid-level managers knew about the problems in the housing markets even if C-level executives did not – for example, Joseph Cassano of AIG FP or Fabrice Tourre of Goldman Sachs. Third, we aim to directly address the question of whether selling dubious-quality mortgage backed securities and taking massive risk despite anticipating a crash was a systematic problem at the middle levels of management.

We use a revealed belief approach based on their personal home transactions. As a home is typically a significant portion of a household’s balance sheet, people (including those Wall Street employees who tend to have high incomes) should pay close attention to the values of their homes. To the extent that homeowners have thick skin (typically in the magnitude of hundreds of thousand dollars) in their homes, they have maximum incentives to acquire information and make informed buying and selling decisions. In particular, for the Wall Street employees, we do not expect the aforementioned biased incentives from their jobs to affect their personal home transactions. This is a key feature that allows us to isolate their beliefs from their job incentives. Home transactions are also more informative of individuals’ beliefs than buying and selling of their companies’ stocks, which is contaminated by potential signaling effects of dis-loyalty and lack of confidence to their bosses and colleagues.

We take two steps to separate the three aforementioned views. In the first step, we examine whether securitization managers were aware of the bubble by comparing their behavior in personal home transactions with that of two uninformed control groups. This analysis allows us to test the bad incentives view, which motivates a hypothesis that securitization managers were more aware of the housing bubble than the control groups. Their awareness may reflect in two possible forms, one strong form and another weak form. Under the strong form, the

securitization managers knew about the bubble so well that they were able to time the housing markets better than others. This means that securitization managers who were homeowners anticipated the housing price crash in 2007-2009 and reduced their exposures to the housing prices by either divesting homes or downsizing homes in the pre-crash period of 2004-2006.

There are two caveats in testing this market-timing form of awareness: First, the cost of moving out of one's home, especially the primary residence, is high, and may prevent the securitization managers from actively timing the housing price crash. Second, even if the securitization managers knew about the presence of a housing bubble, they might not be able to precisely time the crash of the housing prices. While these caveats reduce the power of using the securitization managers' home divestiture behavior to detect their awareness of the bubble, it is useful to note that the cost of moving out of second homes is relatively low and should not prevent the securitization managers from divesting their second homes. More importantly, the cost of moving and inability to time the crash should not prevent alerted non-homeowners from avoiding buying homes. This consideration motivates a weaker form of awareness that securitization managers knew enough to be cautious and thus those who were non-homeowners avoided acquiring homes during the bubble period of 2004-2006.

We use two uninformed control groups, one group from the general population outside the finance industry and the other group from inside the finance industry but outside securitization and housing business. We choose lawyers as the control group from outside finance because lawyers are well educated and sophisticated professionals, and because they also have relatively high incomes among the general public.² We separate lawyers specialized in real estate from non-real estate lawyers and use only non-real estate lawyers as the uninformed control group. In selecting these lawyers, we also make sure that are matched with similar ages and geographic locations as the securitization managers in our sample.

We recognize that securitization managers experienced large wealth shocks during the financial market boom that accompanied the housing bubble and lawyers did not experience such

² According to the survey of the U.S. Census Bureau, in 2006 the average annual compensation of individuals in legal services was \$92,430, which was comparable with that of individuals in finance and insurance (\$97,991) although less than that of individuals in securities, commodity contracts and investments (\$225,821).

wealth shocks. Thus, it is useful to have another control group which experienced similar wealth shocks as those by securitization managers. We choose financial analysts who covered non-housing companies in S&P 500 index as such a control group. These analysts also had large bonuses during the boom years. Since their work is not directly related to housing and securitization business, we expect them to have less informed about the housing bubble than securitization managers.

Taken together, we have the following hypothesis for testing whether securitization managers were aware of the housing bubble:

Hypothesis 1: Securitization managers exhibited more awareness of the housing bubble relative to non-real estate lawyers and non-housing analysts in two possible forms:

- A. (market timing form) Securitization managers who were homeowners were more likely to divest homes and down-size homes in 2004-2006.*
- B. (cautious form) Securitization managers who were non-homeowners were less likely to acquire homes in 2004-2006.*

Overall, securitization managers had better performance after controlling for their initial holdings of homes at the beginning of 2004.

In the second step, we test whether there were other groups less susceptible to distorted beliefs who exhibited more awareness of the housing bubble than securitization managers. This analysis allows us to differentiate the bad luck view from the other two views. The existence of a sophisticated group being more aware of the bubble disputes the bad luck view that the crisis was unpredictable by anyone (even with right incentives and models.)

We choose real estate lawyers as our test group. Real estate lawyers are knowledgeable of the housing markets through providing legal services in real estate related businesses. However, as they are not active participants in the housing markets and not in the “nexus” of the financial industry, we expect them to be less exposed to any potential psychological biases such as “groupthink” and the “inside view” bias which may have affected securitization managers. For example, the groupthink theory of Benabou (2011) emphasizes that agents who are already

vested in an asset (or others correlated with it) are more susceptible to wishful thinking about its return and therefore more likely to accumulate more of it. The “inside view” bias emphasizes that active market participants tend to believe the current decision problem is unique and different than past experiences. Taken together, we hypothesize that real estate lawyers might have had more objective beliefs about the housing markets in 2004-2006 than securitization managers. We will also compare the behavior of real estate lawyers and non-real estate lawyers. As these groups have similar backgrounds excepting real estate lawyers’ greater knowledge of housing markets, any difference between them was likely to be driven by the difference in their beliefs about the housing markets.

Taken together, we have the following hypothesis for testing whether real estate lawyers were more aware of the housing bubble:

Hypothesis 2: Real estate lawyers exhibited more awareness of the housing bubble relative to securitization managers and non-real estate lawyers in two possible forms:

- A. (market timing form) Real estate lawyers who were homeowners were more likely to divest homes and down-size homes in 2004-2006.*
- B. (cautious form) Real estate lawyers who were non-homeowners were less likely to acquire homes in 2004-2006.*

Overall, real estate lawyers had better performance after controlling for their initial holdings of homes at the beginning of 2004.

2. Data

2.1. Data Collection

We begin by collecting names of people working in the securitization business as of 2006. To do so, we obtain the list of registrants at the 2006 American Securitization Forum’s (ASF) securitization industry conference, hosted that year in Las Vegas, Nevada, from January 29, 2006 through February 1, 2006. This list is publicly available via the ASF website. The ASF is the major industry trade group focused on securitization, publishing an industry journal as well as

hosting the “ASF 20XX” conference every year since 2004, which attracts a broad range of participants from around the world who work in the securitization business. The conference in 2006 featured 1760 registered attendees, with 1015 representing the investor (buy) side and 715 representing the issuer (sell) side, and over 30 lead sponsors, ranging from every major US investment bank (e.g., Goldman Sachs, Lehman Brothers, and so forth) to large commercial banks such as Bank of America and Wells Fargo, to international investment banks such as Societe Generale, UBS and Credit Suisse, to monoline insurance companies such as MBIA and XL Capital.

We randomly sample a list of 240 names, with 120 names from the buy side and 120 from the sell side. The registration list includes the name, position and firm for which the person worked. The conference attendees are upper management and mid-level managers rather than CEOs and CFOs. In our sample, the most common positions are Vice President, Senior Vice President, and Director-type positions. We then oversample 42 names from a list of ten prominent banks such as Lehman Brothers and Citigroup.³ We call this sample of 282 people the securitization manager sample.

We use the Lexis-Nexis Public Records database to research the background information of our sample. The database aggregates information available from public records, such as deed transfers, property tax assessment records, public address records, and utility connection records. We provide a detailed description of the system and available information in the Appendix. We summarize a few key features of the data here. First, the system aggregates information from public records into a report about a person and typically contains the month and year of a person’s date of birth. Second, the system not only displays information on every property a person has ever owned, but allows us to look up all historical deed transfer records and tax assessment records associated with each property. These records often have the transaction date, transaction type, and transaction price. This allows us to scan the history of each property to see

³ We oversample names from the following banks with the goal of having at least four bankers from each bank with home transaction information in our final analysis: Bank of America (5), Bear Stearns (7), Citigroup (4), Countrywide (4), Goldman Sachs (3), JP Morgan Chase (4), Lehman Brothers (4), Merrill Lynch (4), Morgan Stanley (3), and Wells Fargo (4). Goldman Sachs sent three people to the conference; Morgan Stanley sent four and one was in our initial random sample. For all other banks, we sampled names until we had at least four people in our sample from each bank after eliminating top executives, those not found in public records, those we cannot isolate confidently, and internationals.

if a house was transacted under a spouse's name or trust instead. Finally, even if a person does not ever own property, a person is often still in the Lexis/Nexis database, as it tracks other types of records such as utility connection records. This allows us to identify people even if they never own property.

We collect data for all properties a person has ever owned, including the location, when the property was bought and sold, and the transaction price, when available.⁴ Our data collection began in May 2011 and we thus have all transactions for all people we collect through this date. Our analysis focuses on the period 2000-2010, the last full year we have data. We do, however, collect data for any transactions we observe, even if they are after 2010. This mitigates any bias associated with misclassifying transactions, as we discuss below. It also helps us ensure that we do not miss any transaction if Lexis/Nexis is not fully updated for whatever reason. To ease data collection requirements, we skip properties sold well before 2000, as they are immaterial for our analysis.

Our sample of S&P 500 analysts consists of analysts who covered companies during 2006-2009 that were members of the S&P 500 anytime during that same period, excluding homebuilding companies. These people worked in the finance industry but were less directly exposed to housing, where the securitization market was most active. We download the names of analysts covering any company in the S&P 500 during 2006-2009 outside of SIC codes 152, 153 and 154 from I/B/E/S. These SIC codes correspond to homebuilding companies such as Toll Brothers, DR Horton, and Pulte Homes.⁵ There are 2,978 analysts, from which 201 names are randomly selected to collect information about their home transaction history.

To construct our sample of lawyers, we select a set of matching lawyers for each person in our securitization sample from the *Martindale-Hubbell Law Directory*, an annual national directory of lawyers which has been published since 1868. Each entry in the directory typically includes information such as the lawyer's name, employer, position, address of the employer, date of birth, legal fields of specialization, and the law school from which the lawyer graduated.

⁴ If we do not find a record of a person selling a given property, we verify that the person still owns the property through the property tax assessment records. In cases where the property tax assessment indicates the house has been sold to a new owner, or if the deed record does not contain a transaction price, we use the sale date and sale price from the property tax assessment, when available.

⁵ Our references for SIC codes is CRSP, so a company needs to have a valid CRSP-I/B/E/S link.

For each person in the securitization manager sample, we randomly choose matching lawyers at most five years older or younger and working at firms located in counties in the same MSA as the matched person. Our matching procedure is described in more detail in the appendix. Our final sample of lawyers consists of 527 names. We split our sample of lawyers into 85 real estate lawyers—those who explicitly mention real estate as a specialization—and the remaining 442 non-real estate lawyers.⁶

2.2. Classifying Home Purchases and Sales

Our starting point for understanding home purchase behavior is a broad framework which allows us to categorize what the purpose of a transaction is for a given person. We think of person i at any time t as either being a current homeowner, or not. If he is not a current homeowner, he may purchase a house and become a homeowner (which we refer to generically as “buying a first home”). Note that one may have been a homeowner at some point in history and still “buy a first home” if one is currently not a homeowner. If a person is currently a homeowner, he may do one of the following:

- A) Purchase an additional house (“buy a second home”),
- B) Sell a house and buy a more expensive house (“swap up”),
- C) Sell a house and buy a less expensive house (“swap down”),
- D) Divest a home but remain a homeowner (“divest a second home”),
- E) Divest a home and not remain a homeowner (“divest last home”).

To operationalize this classification of transactions, we define a pair of purchase and sale transactions by the same person within a six month period as a swap, either a swap up or a swap down based on the purchase and sale prices of the properties.⁷ If either the purchase or sale price is missing, we classify the swap generically as a “swap with no information.”

⁶ Due to constraints on data gathering, we constructed a composite sample of matched lawyers before splitting them into non-real estate lawyers and real estate lawyers. We test whether this significantly affects the comparability of the distribution of ages between the securitization sample and each of these two distributions.

⁷ Specifically, we sort home transactions of one person in order of purchase date. We then examine the purchase date of each home transaction and look to see if there is any transaction whose sale date was within a six month

We allow for a person in a swap to buy first and sell later as well as to sell first and buy later. In the latter case, the person was not in possession of any property after he sold his current home but before he bought the next one. However, for our later analysis, we still think of this person as a “homeowner” in the sense that we think of this person as having planned to buy a replacement house when he sold his current home. That is, we think of the set of homeowners at any time t as the set of people who either currently own homes plus those people who do not own any home but are in the middle of swap transactions. The set of non-homeowners are people who do not own any home and are not in the middle of a swap transaction.

The purchases that are not swaps are either non-homeowners buying first homes, or homeowners buying second homes.⁸ We use the term “second” to mean any home in addition to the person’s existing home(s). Divestitures are classified similarly: among sales that are not involved in swaps, if a person sells a home and still owns at least one home, we say he is divesting a second home; if he has no home remaining, we say the person divests his last home. When classifying transactions in 2010, we use information collected on purchases and sales in 2011 to avoid over-classifying divestitures and first-home/second-home purchases in the final year of data.

2.3. Transaction Intensities

Our analysis centers on the annual intensity of each transaction type and the relative differences in these intensities across samples.⁹ We focus on an annual frequency to avoid time periods with overly sparse transaction frequencies. Formally, the intensity of one type of

period of the purchase date, on either side. If there was, we have a pair of swap transactions. We classify the purchase transaction in the pair as a “swap buy” leg of the swap, and the sale transaction in the pair as a “swap sell” leg of the swap. We also take care to ensure that one buy or sell transaction is not counted in two swaps. We also require the transaction date of the “swap sell” house to be before the transaction date of the “swap buy” leg. This is to rule out the following case. Suppose a person buys home A in January, buys home B in February, and sells home B in March. Homes A and B would be linked as a swap in our algorithm, which it is clearly not. One person in our sample did this once. If multiple homes were sold within a six month window, the house with the closest sale date to the date of a purchase is paired with the purchase. If multiple homes were sold on the same day in a six month window, we pair the house bought earlier with the purchase (“first in, first out”); this is extremely rare.

⁸ If a home is on record for an individual, but the home does not have a purchase date, we assume the owner had the home at the beginning of our sample.

⁹ We focus on the intensity of transactions rather than the probability of an eligible person making a given transaction because one person may make multiple transactions of one type in one year. However, focusing instead on probabilities yields nearly identical results as it is rare for one person to make multiple transactions of one type in a year.

transaction in year t in a sample group is defined as number of transactions of the type divided by the number of people eligible to make the type of transactions:

$$Intensity_t = \frac{\# Transactions_t}{\# people\ eligible\ for\ the\ transactions_t}.$$

For example, the intensity of buying a first home is determined by the number of first home purchases during the year divided by the number of non-homeowners (people eligible for this type of transactions.) A complication in this calculation is that, in a given year, a person may make multiple transactions. As a result, the number of non-homeowners at the beginning of the year does not fully represent the number of people eligible for buying a first home, because, for instance, a homeowner may sell his home in February and then buy another home in September. To account for such possibilities, we define “adjusted non-homeowners”, who are eligible for buying a first home during a year, to be the group of non-homeowners at the beginning of the year plus individuals who divest their last homes in the first half of the year. We similarly adjust the number of homeowners and multiple homeowners, and provide detailed description of the adjustments in the Appendix.

3. Descriptive Statistics

We first examine the distribution of people across groups. Table 1, Panel A presents the number of people in each sample. After eliminating names who are CEOs, CFOs, or COOs, and those who we cannot isolate confidently in Lexis/Nexis, we have information in Lexis/Nexis for 207 people in the securitization manager sample. After similarly eliminating people for the other sample groups, we have 161 S&P 500 analysts, 426 non-real estate lawyers, and 81 real estate lawyers in our sample.

Table 1, Panel B presents the age distribution for all samples. The median ages in 2011 for the securitization manager, S&P500 analyst, non-real estate lawyer, and real estate lawyer samples are 45, 41, 46, and 46, respectively. The S&P 500 analysts tend to be slightly younger than people in the securitization manager sample. Lawyers are more similar in age; a chi-square test of homogeneity of the age distribution has a p-value of 0.25 for non-real estate lawyers and 0.35 for real estate lawyers, respectively.

Turning our attention to properties, Table 2, Panel A breaks down the number of properties owned over 2000-2010. Over this period, 82% of people in the securitization manager sample owned at least one home. Among these homeowners, 58% were associated with more than one property during this period, either because they moved or owned more than one home at a time. This percentage is higher than that of any other sample group. The table also reports the number of properties for which we have no purchase or sale date. A missing sale date reflects that the owner still owns the property. A missing purchase price reflects missing data, which we deal with below.

Panels B and C present the regional distribution of these properties. The most represented areas for all groups are the Middle Atlantic (NJ-NY-PA) and Pacific areas (dominated by California). The New York combined statistical area (roughly the NJ-NY-CT tri-state metro area plus Pike County, PA) is the most prominent metro area, followed by Southern California (Los Angeles plus San Diego). S&P 500 analysts tend to be concentrated more in New York.

Table 3 summarizes purchase and sale activities each year. Analyzing the home purchase prices, particularly in the early years, gives us a guide as to whether there were initial wealth differences between these groups. Evidently, the S&P 500 analysts began with more initial wealth than the other groups. Their average home purchase price in 2000 was \$835,000, over twice the average price of any other group. Interestingly, real estate lawyers and securitization managers were very comparable, and both were higher than non-real estate lawyers. Through 2004 and 2005, the average purchase price paid by securitization managers nearly tripled to \$1.2M; the median that year was \$950K. This likely reflects substantial wealth shocks to the securitization manager group. The purchase prices paid by other groups also had large increases through time, although they were not as substantial. For real estate lawyers, the price pattern before 2006 was nearly flat, but rose sharply in 2007-2009 and, especially, in 2009.

Figure 2 plots the housing stock of each group through time as a ratio relative to the housing stock for each group at the end of 1999. Both the securitization manager and S&P 500 analyst groups doubled their stock of houses by 2006, with slight declines thereafter. This plot already suggests that, as a group, securitization managers did not time the bubble, as there is no dip in the housing stock for the group before 2007. The growth in their housing stock is very similar to

S&P 500 analysts, who, although were likely initially wealthier, were also likely to receive large increases in wealth during the boom period of 2004-2006. Relative to this group, the securitization manager sample also shows little evidence of being cautious, as their housing stock doubled by 2006. Within lawyers, the real estate lawyers had very low growth in their housing stock compared to non-real estate lawyers.

Examining only the stock of housing for each group is reduced form and masks the underlying choices that individuals are making. Table 4 breaks down the number of transactions by transaction type over the entire period 2000-2010. As expected, the number of purchase transactions exceeds the number of sale transactions, since a number of people may be still living in homes they purchased. The most common purchase type observed is buying a first home. Swapping a home (up, down, or missing price) is the next common purchase. Among sales, a sale involved in any type of swap is the most common transaction.¹⁰

Table 5 presents the number of homeowners and non-homeowners each year in our sample for the four groups. As expected, the number of homeowners rose through time in all of our samples, likely reflecting decisions to purchase houses for life-cycle reasons. This is true even when looking at adjusted homeowners, which reflects the number of people in our sample each year who were eligible to buy a second home, swap a home, or divest a home. The number of adjusted non-homeowners actually rose from 2007-2010 for our securitization sample, distinct from the other groups. This, coupled with the dip in housing stock observed in Figure 2, likely reflects job losses on the part of our securitization manager sample.

4. Empirical Results

4.1. Were Securitization Managers Aware of the Bubble?

We first examine Hypothesis 1, which posits that securitization managers were more aware of the bubble than other less informed groups: non-real estate lawyers and S&P 500 analysts. As discussed in Section 1.2, we examine two forms of this hypothesis. The first form posits that

¹⁰ The number of swap sales and swap purchases over 2000-2010 may not exactly match. In this case, there was one swap where the sale leg was executed in 2000 while the purchase leg was executed in 1999 for securitization managers, and vice versa for one swap pair of non-real estate lawyers.

securitization managers were able to better time the housing markets on their own accounts, i.e., had higher intensities of divestitures and swap downs in 2004-2006, relative to the control groups. Table 6 presents the divestitures per person for each group through time. These intensities are also plotted in Figure 3. The raw divestiture intensities for the securitization manager sample are, if anything, lower than the divestiture rates of S&P 500 analysts and non-real estate lawyers during the bubble period. For example, there were almost no divestitures in 2005 for the securitization manager sample. On an unadjusted basis, the rate of divestiture is qualitatively lower for the securitization manager sample compared to both of the S&P 500 analysts and non-real estate lawyers in every year from 2004-2006.

To account for heterogeneity in the age profiles of each group, we compute regression-adjusted differences by estimating the following equation for each possible pairing of the securitization sample with other samples using OLS in a person-year panel:

$$E[\#Divestitures_{it}|X] = \alpha + \gamma_t + \beta_t \times Securitization_i + \sum_{j=1}^7 \delta_j Age_j(i, t) + \lambda MultiHO_{it}. \quad (1)$$

The variable $\#Divestitures_{it}$ is the number of divestitures for individual i in year t ; $Securitization_i$ represents an indicator for whether individual i is part of our securitization manager sample; $Age_j(i, t)$ represents an indicator for whether individual i is part of age group j in year t [where eight age brackets are defined according to Table 1, Panel B, and one age group is excluded], and $MultiHO_{it}$ represents whether individual i was also a multi-homeowner in year t . We use indicators for age brackets instead of a polynomial specification for age as it makes the regression easily interpretable as a difference in means. In each year t , only the eligible homeowners for year t (i.e., those who started year t as homeowners or became a homeowner during year t) are included in the estimation. The coefficients β_t are thus the annual difference in average divestitures per person within the homeowner category across samples, adjusted for these age and multi-homeownership factors. We cluster standard errors by person.

Table 6 also presents these differences in means. As expected, being a multiple homeowner is associated with a significantly higher rate of divestiture than being a single homeowner.

Qualitatively, the securitization manager sample has a lower rate of divestiture again for every year from 2004-2006, and a significantly lower rate of divestiture (0.007 compared to 0.044 homes per person) compared to non-real estate lawyers in 2005. Because intensities are very similar to fractions of people selling, one can also interpret these results as saying that, although 4.4% of non-real estate lawyers divested homes in 2005, only 0.07% of people in our securitization manager sample did the same. Overall, there is little evidence that suggests people in our securitization manager sample sold homes more aggressively prior to the peak of the housing bubble relative to either S&P 500 analysts or non-real estate lawyers.

We next examine whether securitization managers were cautious in purchasing homes in 2004-2006, the “cautious form” of Hypothesis 1. One alternative story is that they knew about the bubble, but that the optimal response was to avoid purchasing homes given the difficulty in timing the crash precisely. Table 7 examines the rate of intensity of first home purchases among eligible non-homeowners. We compute regression-adjusted differences following the same specification as in equation (1), replacing the number of first home purchases as the left-hand side variable and omitting the $MultiHO_{it}$ as it does not apply to non-homeowners. Figure 4, Panel A plots the unadjusted intensities through time.

The securitization manager sample had a very similar rate of first home purchases compared to the S&P 500 analysts. Both of these samples had higher rates of first home purchases than the lawyer groups. Compared to non-real estate lawyers, the intensity of first home purchase for the securitization manager sample was significantly higher in 2005, when the rate of first home purchases was 17% per non-homeowner for the securitization manager sample, compared to 7.4% per non-real estate lawyer non-homeowners, a difference that persists on a regression-adjusted basis. This suggests that, although securitization managers were likely getting wealth shocks during this time, they were not particularly cautious from an investment perspective. There is almost no difference between the rate of first home purchase between non-homeowners in the securitization manager sample and the S&P 500 analyst sample; both groups purchased first homes aggressively during this period when they were likely receiving large bonuses.

Homeowners in the securitization manager sample also showed a similar lack of caution when swapping up or purchasing second homes. Table 8 tabulates the raw intensities and also

regression-adjusted differences in intensities of buying a second home or swapping up to a more expensive home. Figure 4, Panel B plots the raw intensities through time. The regression-adjusted differences are computed using a specification analogous to equation (1) where we replace the left-hand side variable with the number of second home purchases plus swap-up transactions for individual i during year t . The raw difference implies that the rate of transactions per person per year was 0.07 higher in 2005 for the securitization group relative to the S&P 500 group. On a regression-adjusted basis, the difference in intensities is nearly 0.1 between the securitization sample and the S&P 500 sample and 0.07 for the non-real estate lawyer sample, both of which are statistically at the 5% level or better.

As a robustness check, we estimate a full Poisson regression model for our transaction types. This approach explicitly models the discrete nature of the number of occurrences and estimates the intensity of the transaction via a Poisson model via maximum likelihood; the approach essentially estimates equation (1) in logs. We further pool together intensities every other year (2000-2001, 2002-2003, and so forth) to mitigate the concern that our results are driven by spurious differences between a small number of transactions we may observe during a single year. Our estimated intensities for each of these year groupings reflect the average intensity over the two years in each grouping. Formally, the estimated model for divestitures is:

$$\begin{aligned} \log E[\#Divestitures_{it}|X] \\ = \alpha + \gamma_{s(t)} + \beta_{s(t)} \times Securitization_i + \sum_{j=1}^7 \delta_j Age_j(i, t) + \lambda MultiHO_{it}, \quad (2) \end{aligned}$$

where $s(t) = 0$ if $t=2000$ or 2001 , $s(t) = 1$ if $t=2002$ or 2003 , and so forth. Other transaction types are defined analogously. We report the exponentiated coefficients, $\exp(\beta_{s(t)})$, which correspond to the ratio of the intensity for the securitization sample with the comparison sample for each year grouping, and test the null hypothesis that this ratio is 1.

Results from this exercise, which are reported in Table 9, follow our results from before closely. The Poisson regression facilitates economic interpretation easily. Panel A shows that the rate at which securitization managers divest property is only 50% of the rate of S&P 500 analysts, and 39% of the rate for non-real estate lawyers in 2004-2005, a difference that is

significant at the 10% level. Panel B shows that the rate of first home purchases is qualitatively higher for every year grouping until 2008-2009 when compared to the S&P 500 analysts and non-real estate lawyers.¹¹ Panel C shows that during the 2004-2005 boom years, the annual rate at which people in the securitization manager sample acquired second homes or swapped up was over 75% higher than the S&P 500 analyst sample, and 37% higher than the non-real estate lawyer sample.

4.2. Were Real Estate Lawyers More Aware?

In order to further distinguish whether the lack of behavior consistent with knowledge of the bubble is more symptomatic of distorted beliefs or an un-anticipatable shock, we examine Hypothesis 2 by comparing the behavior of our securitization manager sample with that of a group with real estate lawyers, an outside yet arguably sophisticated group of agents with direct knowledge of the real estate markets. A difference in the behaviors of these groups would suggest a role for distorted beliefs rather than bad luck.

Returning to Figure 2, we see that the evolution of the aggregate housing stock of real estate lawyers is less aggressive than that of both of our finance groups, the securitization manager sample and S&P 500 analysts. Tables 6 through 8 document the disaggregated individual behavior. From Table 6, the intensity of divestitures in the real estate lawyer sample was substantially higher than the securitization manager sample in 2004 and 2006, as borne out in Figure 3. Around the same period, the intensity of first home purchases was higher in 2004 and 2005 for the securitization manager sample than for the real estate sample, as evidenced in Table 7. Finally, Table 8 shows that the intensity of second home purchases and swap-ups early on during 2002 and 2003 was slightly higher for the securitization manager sample than for real estate lawyers. These results suggest that people in the securitization manager sample moved in more aggressively, swapped up and purchased second homes more aggressively early on, yet did not aggressively divest homes during the 2004-2006 period.

One concern may be that differences in the behaviors of real estate lawyers and people in the securitization manager sample do not reflect beliefs, and rather reflects other confounding

¹¹ Since the number of first home purchases for the real estate lawyers is zero from 2008 onwards, the ratio of expected outcomes cannot be estimated and this column is omitted from Table 9.

factors such as risk aversion. For example, people who are more risk averse may self-select into becoming lawyers while more risk-seeking people tend to select into finance careers. To address this concern, we compare the behavior of the real estate lawyers with that of our non-real estate lawyers. If differences in behavior between our securitization manager sample and real estate lawyers purely reflected differences in risk aversion between lawyers and people in our securitization manager sample, then we should see no difference between the behavior of real estate lawyers and non-real estate lawyers.

From Figure 2, we see that the housing stock of non-real estate lawyers grows more aggressively than that of real estate lawyers. Tables 6 and 8 reveal little significant difference between the divestiture behavior and second home purchase behavior of the two groups through time. On the other hand, Table 7 reveals that real estate lawyers were significantly less aggressive than even non-real estate lawyers in moving into first homes in 2004 and 2005. Furthermore, they were also averse to buying homes during a period when prices were falling in 2008, which prevented further losses.

One concern is that this is driven by age heterogeneity and that the age controls built into our regression-adjusted differences do not sufficiently neutralize the effects of this difference. This may be concerning given that a relatively larger fraction of the real estate population is older. To further check these results, we re-run our analyses by dropping anyone who is 40 or older in 2000. The results are identical and available from the authors. Overall, the results suggest that real estate lawyers were more cautious during the housing boom and bust by maintaining smaller exposures to housing throughout.

4.3. Performance

We systematically analyze which groups fared better during this episode by comparing the average trading performance during the housing boom and bust. Our strategy is to compare their performances based on the relative differences in the location and timing of their sales and purchases alone from the beginning of 2004 onwards. This strategy focuses attention on the largest part of the price run-up and crash, and puts all groups on equal footing in terms of leverage, alternative investment opportunities, and performance gains from home improvements,

most of which we do not observe. Our test is only focused on the performance of their purchase and sale behavior along the timing and location dimensions.

Our thought experiment is the following: if we assume agents follow a self-financing strategy where the available investments are houses in different metro areas and a risk-free asset, what would have been their performance from 2004 onwards? We proceed with the following assumptions. First, we assume that agents each purchase an initial supply of houses at the beginning of 2004 equal to whichever houses they own in each metro area. Second, we assume that time flows quarterly. We mark the value of each house in each metro area each quarter in accordance with the quarterly Federal Housing Finance Agency metro area home price index with 2009 OMB CBSA definitions. Agents trade at the end of each quarter by purchasing or selling homes in each metro area in accordance with their observed purchase or sale transactions. Agents may borrow and lend at the risk-free rate through a cash account. Specifically, cash is invested at the end of each quarter in a 3-month Treasury bill with yield equal to the observed 3-month T-bill yield observed at the end of the quarter, which we obtain from the Federal Reserve Board H.15 series. Third, we endow each agent with enough cash to finance the entirety of their future purchases and thus abstract away from differences in leverage.¹²

We proceed with two versions of our exercise. The first strategy assigns the initial value of each house to be one dollar and thus equal-weights the prices of homes across metro areas in the initial quarter. Note that the evolution of prices is still heterogeneous across metro areas as we mark the value of each house each quarter using the observed price indices. The second “value-weighted” strategy assigns the value of a house in the initial quarter by marking the value of that house up or down from the actual observed purchase price in the data.

We compute both the return from the self-financed strategy and the return from a counterfactual buy-and-hold strategy, where agents purchase their initial set of houses and then

¹² We endow each agent with enough initial cash to cover all future transactions in the following way. To do so, we first compute the maximum amount of debt that each agent would incur over the 2004-2010 period to finance their positions if each agent began with no cash. We then endow the agent with this amount of cash in a “second pass” from which we compute their trading performance. We endow agents who do not ever trade in the 2004-2010 period (and thus would issue zero debt) with the mean cash level of agents in their sample who do trade houses over this period. This approach essentially fully collateralizes all future trades and assumes that agents who do not trade earn the risk-free rate. We can easily assume that agents follow a given leverage policy into our framework although it only magnifies the losses of losers when prices fall; we view our assumption as conservative.

subsequently never trade. We denote the difference between the returns of these two strategies as the performance index for each individual. Differences in the average performance index across groups are a “difference-in-difference” where the first difference is over the buy-and-hold performance and the second difference compares the other group’s performance. We focus on differences in the performance index instead of gross returns because gross returns may be heavily influenced by the size of the initial housing stock, and thus differences in the gross return across the groups may be dominated by differences in the initial housing stock.

Table 10 presents the results from our equal-weighted exercise. Panel A presents summary statistics for the per-person average number of properties, value of properties, cash account, and total portfolio value at the end of 2003q4, the initial period, and 2010q4, the final period. Panel B tabulates their raw performance and performance indices computed over the entire period 2004-2010, while Panel C tests for differences in returns and the performance index. There were little differences in the composite buy-and-hold return between real estate lawyers and people in the securitization manager sample. However, Panel C shows that the securitization manager sample underperformed the real estate lawyers during this period; their performance index is lower by 261 basis points, a difference that is statistically different at the 5% level. Although this magnitude is difficult to interpret as we fully collateralized all our agents, we view it as an underestimate of the true performance differential. Notably, Panel C also shows that real estate lawyers performed better than non-real estate lawyers by 228 basis points on average, which is statistically significant at the 5% level. Panel D shows that these results are robust to regression-adjusted differences where we regress the performance index on a group indicator plus indicators for age brackets.

Table 11 shows that the results are very similar under our value-weighted exercise. The average performance index in the securitization manager sample is 276 basis points lower than the average performance of real estate lawyers. The difference between real estate lawyers and non-real estate lawyers is qualitatively similar to the equal-weighted exercise in that real estate lawyers outperform by 210 basis points. Both economic magnitudes are very similar to the magnitudes under the equal-weighted exercise.¹³

¹³ One worry is that differences are spurious to our initial quarter, 2003q4. In order to test this, check whether these differences persist if we choose 2002q4 and 1999q4 as our initial points. Although statistical significance is more

Figure 5, Panels A (equal-weighted) and B (value-weighted) illustrate the comparative evolution of the performance indices through time. We take the cumulative return of the trading strategy less the cumulative return of the buy-and-hold return each quarter. The figures show that real estate lawyers did better by being more cautious during the boom and by not increasing their initial exposure to housing.

5. Conclusion

Although there was certainly unsavory behavior on Wall Street during the housing boom – Fabric Tourre and Bernie Madoff, for example – we find little systematic evidence that the average securitization manager was aware of the severity of problems in the housing markets. They neither managed to time the market nor exercised caution, relative to non-real estate lawyers and S&P 500 analysts. Our evidence thus lends little support to the view that the average securitization manager anticipated the crash earlier than most. While the housing bubble was largely unanticipated, there is some evidence of cautiousness among real estate lawyers, as they fared better than securitization managers. The fact that these real estate lawyers were outsiders and arguably sophisticated suggests that the dominant narrative of bad incentives ignores the potentially important role of distorted beliefs in the housing boom and bust.

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limited, the magnitudes of differences in the performance index are strikingly similar. In other robustness checks, we again test whether throwing out people with ages greater than 40 in 2000 affect our results, and they do not.

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Figure 1: Home Price Indices

This figure plots the home price Federal Housing Finance Agency (FHFA) home price indices from 1999q4 through 2011q4, where we normalize 1999q4 to be 100.

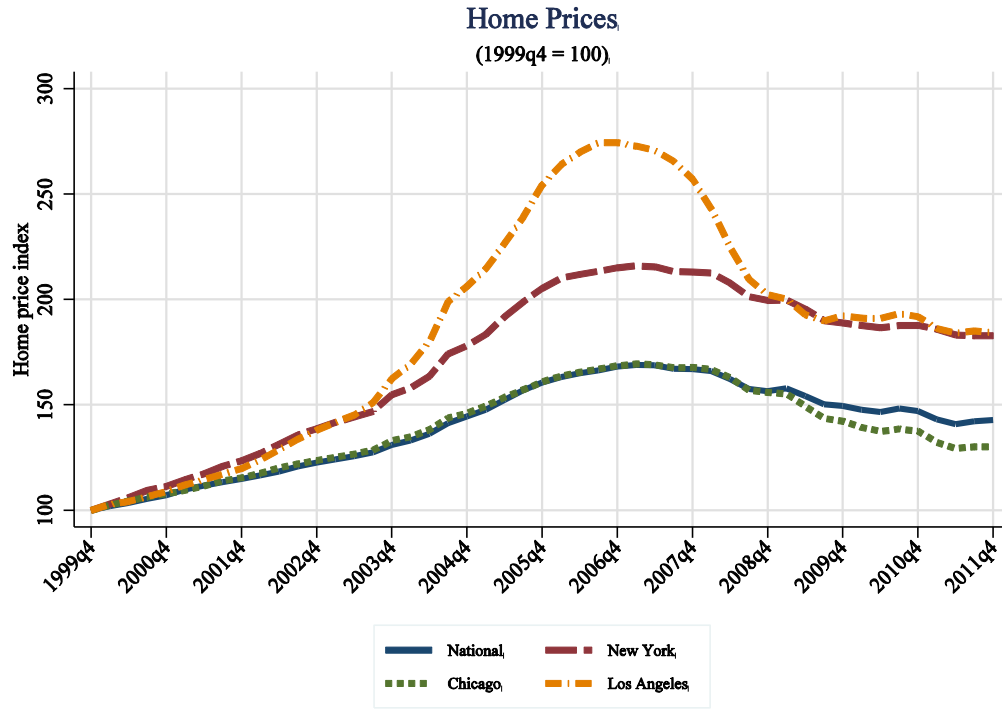


Figure 2: Housing Stock

This figure plots the ratio of total houses owned at the end of each year to total houses owned at the end of 1999.

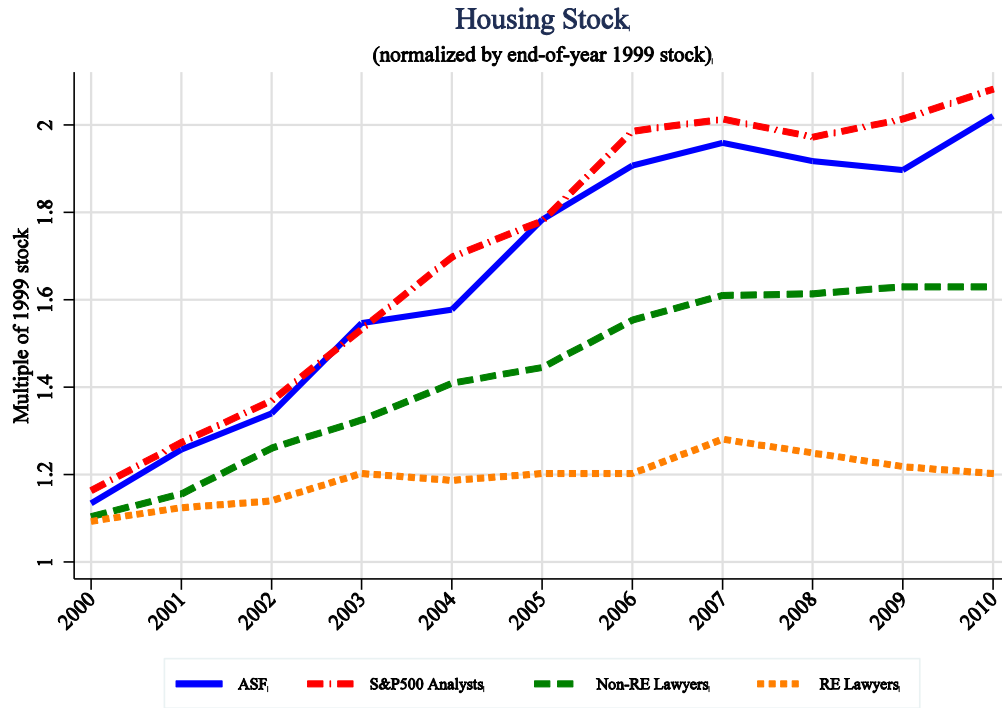


Figure 3: Divestitures

This figure plots the intensity of divestitures through time, defined as the number of divestitures per adjusted homeowner each year, for each sample.

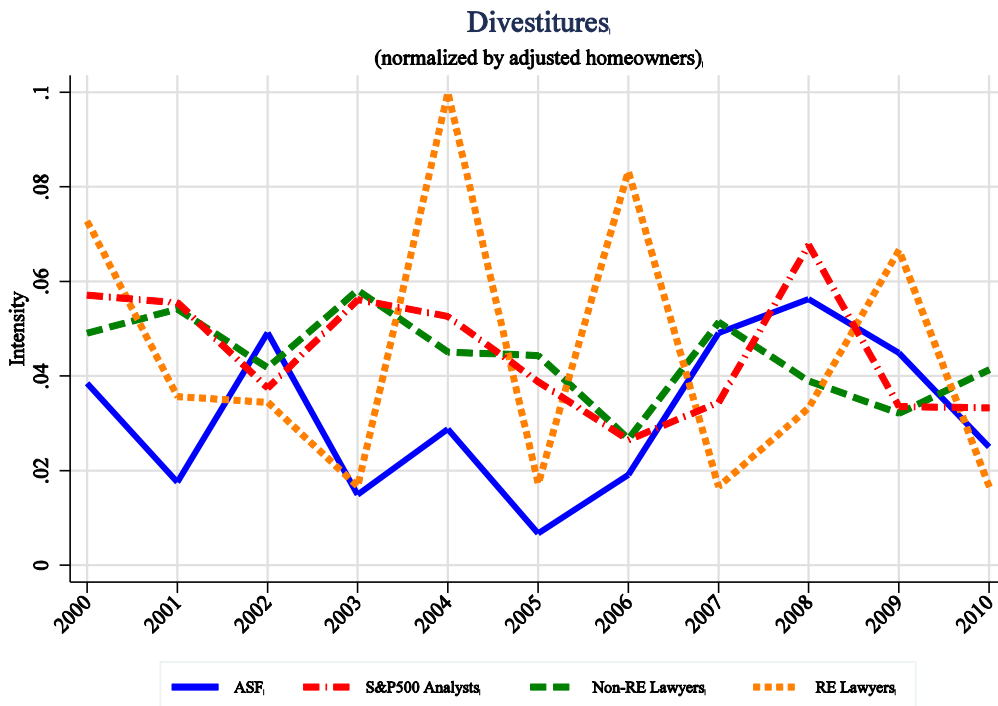
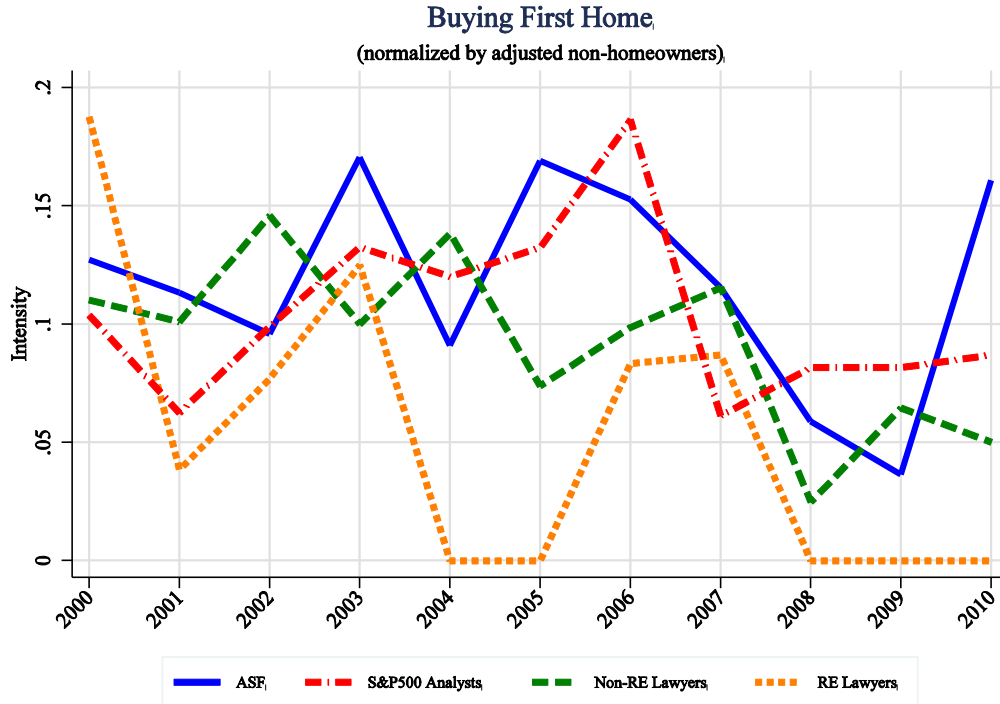


Figure 4: Purchases

Panel A plots the intensity of first home purchases, or the number of first home purchases per adjusted non-homeowner, through time. Panel B plots the intensity of buying a second home or swapping up, the number of second home purchases plus swap-up transactions per adjusted homeowner, through time.

Panel A: Buying First Home



Panel B: Buying Second Home or Swapping Up

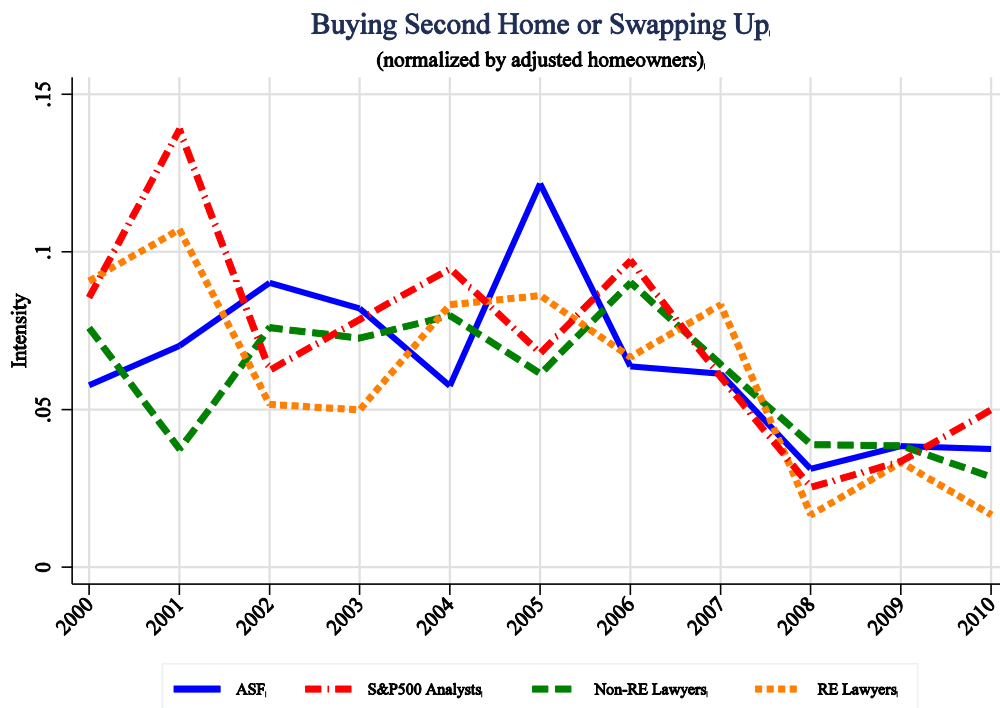
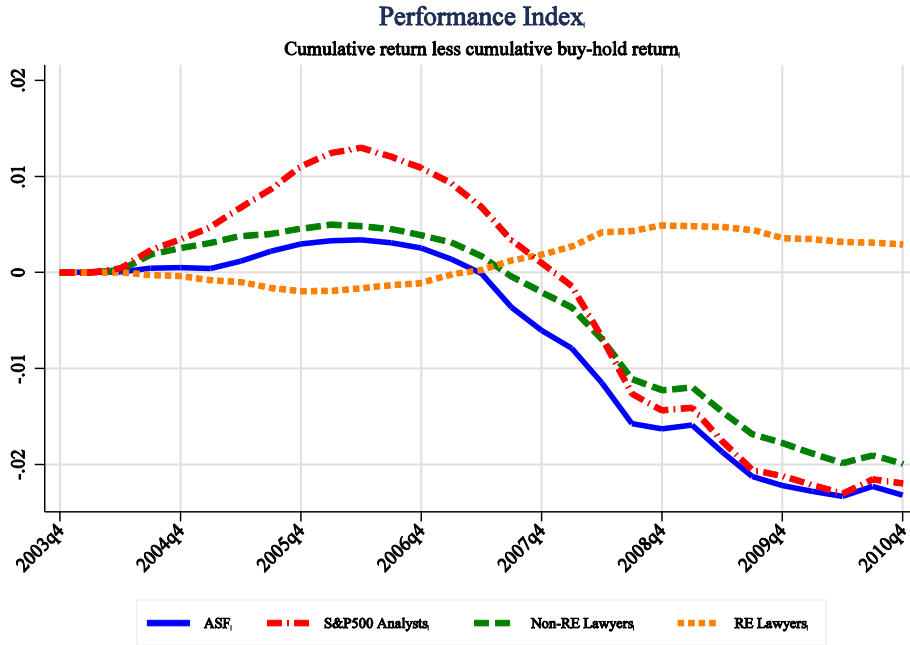


Figure 5: Performance Indices

This figure plots the average performance index, defined as the cumulative return on the self-financed strategy less the buy-and-hold return of the initial stock of houses, where 2003q4 is taken as the initial quarter. Panel A plots the performance index under the equal-weight strategy where each house is initially valued at \$1, while Panel B plots the performance index under the value-weight strategy.

Panel A: Equal-weighted



Panel B: Value-weighted

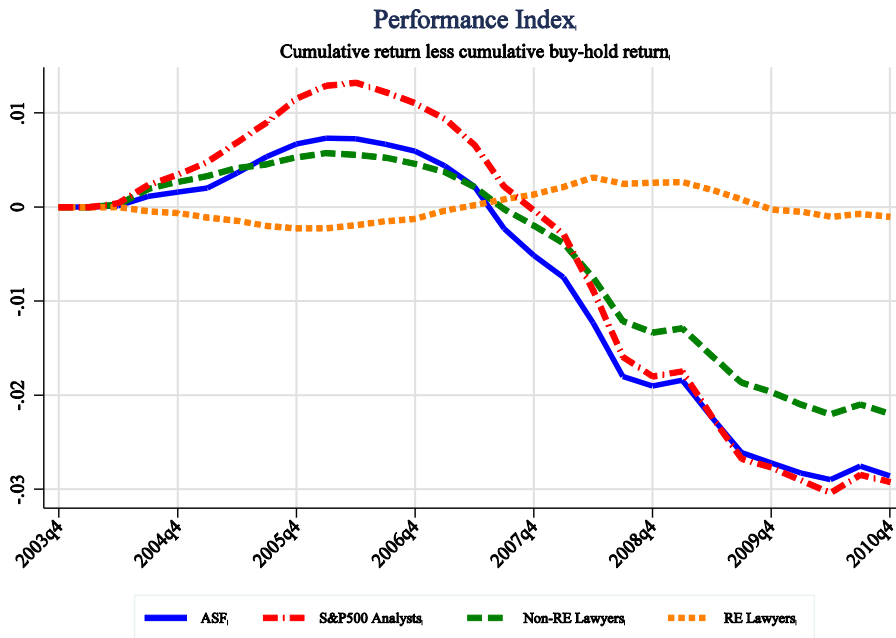


Table 1: Number of People in Sample

This table lists the number of people for which we gathered information in each of four samples: Securitization Analysts, Non-Real Estate Lawyers, Real Estate Lawyers, and S&P 500 Analysts. Panel A shows the number of names we searched for, found, and had properties in our sample. Panel B shows the age distribution of people in our sample.

Panel A: Number of People

Sample	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
Number of names	282	201	438	86
Not mid-level manager	11	N/A	N/A	N/A
Not found in public records	20	9	9	5
Multiple found in public records	20	12	2	0
International	24	18	0	0
Deceased	0	1	2	0
People in sample	207	161	425	81
Person found, but no homes owned	37	29	73	15
People who sold all properties before 2000	0	0	4	2
People who only own homes beginning after 2010	0	2	1	0
People in sample owning at least one home, 2000-2010	170	130	347	64

Panel B: 2011 Age Distribution

Age	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
30 and under	0.53%	2.56%	1.43%	0.00%
31 to 35	6.95%	12.18%	3.81%	2.50%
36 to 40	16.04%	28.21%	17.14%	18.75%
41 to 45	29.41%	23.08%	25.71%	22.50%
46 to 50	24.06%	20.51%	20.00%	22.50%
51 to 55	12.83%	3.85%	16.43%	15.00%
56 to 60	5.35%	4.49%	8.57%	7.50%
Over 60	4.81%	5.13%	6.90%	11.25%
Total with age data	187	156	420	80
Missing age data	20	5	5	1
Chi-Square Test of Homogeneity with Sctzn Sample	N/A	20.49	9.10	7.76
Homogeneity Test, p-value	N/A	0.00	0.25	0.35
Median age	45	41	46	46

Table 2: Summary Statistics for Properties

This table provides summary statistics for properties owned anytime over 2000-2010. Panel A presents the fraction of people owning more than one address over 2000-2010. Panel B presents the distribution of addresses associated with people in our sample. Panel C presents the distribution of properties within select metropolitan areas. New York is the New York-Newark Bridgeport, NY-NJ-CT-PA combined statistical area (CSA). Southern California is a combination of Los Angeles-Long Beach-Riverside, CA CSA and San Diego-Carlsbad-San Marcos, CA Metropolitan Statistical Area. Chicago is the Chicago-Naperville-Michigan City, IL-IN-WI CSA. Boston is the Boston-Worcester-Manchester, MA-RI-NH CSA. Philadelphia is the Philadelphia-Camden-Vineland, PA-NJ-DE-MD CSA. CSA definitions follow the 2009 definitions issued by the Office of Management and Budget (OMB).

Panel A: Fraction of people owning more than one address over 2000-2010

	Number of houses owned	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
	One house	41.76%	46.15%	49.57%	43.75%
	Two houses	39.41%	30.00%	31.70%	28.13%
	Three houses	12.94%	16.92%	10.95%	17.19%
	4 or more	5.88%	6.92%	7.78%	10.94%
<hr/>					
	% people in sample owning at least one home	82.13%	80.75%	81.65%	79.01%
	# homes with no purchase date	12	22	68	23
	# homes with no sale date	20	50	73	24
	Total properties ever owned	339	252	680	147

Panel B: Regional distribution

	Region	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
	Pacific	21.07%	12.81%	23.15%	13.95%
	Mountain	6.29%	2.89%	3.78%	0.78%
	West North Central	7.55%	2.89%	2.05%	0.00%
	East North Central	11.95%	8.26%	11.97%	10.85%
	West South Central	5.66%	3.72%	8.66%	1.55%
	East South Central	2.52%	2.07%	2.05%	5.43%
	South Atlantic	12.58%	11.98%	14.65%	23.26%
	Middle Atlantic	22.33%	38.43%	21.26%	26.36%
	New England	10.06%	16.94%	12.44%	17.83%
<hr/>					
	Total properties	318	242	635	129

Table 2, continued

Panel C: Geographical distribution over select metro areas

Region	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
New York	21.40%	43.40%	16.50%	25.60%
Southern California	13.20%	3.70%	13.90%	9.30%
Chicago	7.90%	5.80%	4.90%	9.30%
Boston	4.70%	5.80%	7.20%	13.20%
Philadelphia	4.10%	0.40%	6.10%	3.10%
Other most common metro area	Minneapolis (4.1%) Wash., DC (4.1%)	San Fran. (5.4%)	Dallas (4.6%)	Wash., DC (9.6%)

Table 3: Purchases and Sales

Panel A tabulates the mean purchase price for each group, by year. Panel B tabulates sale prices. The price is reported in thousands and the number of transactions is reported. For non-securitization groups, t-statistics associated with a t-test of the null hypothesis that the securitization minus other group purchase price equals zero are reported in brackets. The N is the number of transactions that year for which price data are recorded. ***/*** represent significant at the 10%, 5% and 1% level, respectively.

Panel A: Average Purchase Price, 2000-2010

	Securitization		S&P 500 Analysts		Non-RE Lawyers		RE Lawyers	
	Price	N	Price	N	Price	N	Price	N
2000	391.587	21	835.474	13	275.467	25	354.194	9
			[-2.34]**		[1.75]*		[0.28]	
2001	439.355	20	777.531	16	340.862	26	386.000	8
			[-1.87]*		[1.25]		[0.50]	
2002	388.438	19	559.960	14	464.609	39	445.699	5
			[-1.77]*		[-0.82]		[-0.56]	
2003	592.756	27	729.545	18	415.447	29	444.150	6
			[-0.75]		[1.59]		[0.70]	
2004	1195.660	15	1025.055	16	478.388	41	328.200	5
			[0.54]		[4.36]***		[2.18]**	
2005	878.859	29	768.486	17	567.667	25	413.635	4
			[0.69]		[2.17]**		[1.54]	
2006	769.653	17	758.214	22	541.466	32	408.250	6
			[0.07]		[2.06]**		[1.96]*	
2007	578.069	16	1281.279	9	534.289	34	879.714	7
			[-2.72]**		[0.28]		[-1.45]	
2008	871.222	9	1812.929	7	608.262	13	607.000	3
			[-1.83]*		[1.05]		[0.60]	
2009	614.651	9	1186.562	8	599.323	18	1105.000	2
			[-2.06]*		[0.07]		[-1.04]	
2010	614.143	14	1169.500	9	525.579	15	370.000	1
			[-2.34]**		[0.72]		[.]	

Table 3, continued

Panel B: Average Sale Price, 2000-2010

	Securitization		S&P 500 Analysts		Non-RE Lawyers		RE Lawyers	
	Price	N	Price	N	Price	N	Price	N
2000	340.367	8	450.417	6	271.060	15	362.625	4
			[-0.83]		[0.79]		[-0.14]	
2001	375.175	8	574.140	9	295.771	17	384.880	5
			[-1.11]		[0.69]		[-0.04]	
2002	347.325	12	621.375	8	381.056	18	426.933	3
			[-2.44]**		[-0.49]		[-0.52]	
2003	359.562	8	923.778	7	349.535	19	453.406	4
			[-2.68]**		[0.13]		[-0.78]	
2004	718.192	13	1028.500	5	501.514	23	1057.500	3
			[-1.37]		[1.56]		[-1.02]	
2005	588.850	10	967.147	10	472.579	16	348.944	5
			[-0.86]		[1.00]		[2.04]*	
2006	676.281	8	335.833	6	500.265	10	513.630	3
			[1.16]		[0.65]		[0.38]	
2007	621.664	11	937.286	7	535.384	16	839.500	2
			[-1.06]		[0.48]		[-0.68]	
2008	567.831	13	728.438	8	260.725	4	478.833	3
			[-1.12]		[2.06]*		[0.45]	
2009	784.000	9	757.500	4	513.750	12	582.000	2
			[0.08]		[1.18]		[0.41]	
2010	905.092	6	1073.060	5	489.039	10	702.888	2
			[-0.36]		[1.85]*		[0.38]	

Table 4: Transaction Types

We tabulate the number of purchases (Panel A) and sale transactions (Panel B) across all samples over the period 2000-2010, with transaction types defined in the text.

Panel A: Purchase Transactions, 2000-2010

	Securitization		S&P 500 Analysts		Non-RE Lawyers		RE Lawyers	
	Count	Fraction	Count	Fraction	Count	Fraction	Count	Fraction
Buy a First Home	99	44.80%	81	47.93%	170	44.04%	16	24.62%
Buy a Second Home	51	23.08%	46	27.22%	122	31.61%	26	40.00%
Swap Up Purchases	48	21.72%	29	17.16%	65	16.84%	14	21.54%
Swap Down Purchases	9	4.07%	4	2.37%	8	2.07%	3	4.62%
Swap Purchase- Missing Price	14	6.33%	9	5.33%	21	5.44%	6	9.23%
Total Purchases	221		169		386		65	

Panel B: Sale Transactions, 2000-2010

	Securitization		S&P 500 Analysts		Non-RE Lawyers		RE Lawyers	
	Count	Fraction	Count	Fraction	Count	Fraction	Count	Fraction
Divest Last Home	29	13.12%	23	13.61%	60	15.54%	5	7.69%
Divest Second Home	21	9.50%	25	14.79%	76	19.69%	24	36.92%
Swap Up Sale	48	21.72%	29	17.16%	64	16.58%	14	21.54%
Swap Down Sale	9	4.07%	4	2.37%	8	2.07%	3	4.62%
Swap Sell- Missing Price	15	6.79%	9	5.33%	21	5.44%	6	9.23%
Total Sales	122		90		229		52	

Table 5: Number of Homeowners and Non-Homeowners

We tabulate the number of homeowners (HO), adjusted homeowners, adjusted non-homeowners and adjusted multiple-homeowners for the different samples. Adjusted homeowners are people eligible to buy a second home or swap a home during the year. Adjusted non-homeowners are people eligible to purchase a first home during the year. Adjusted multiple homeowners are people eligible to divest a second home during the year. Note that the number of adjusted homeowners plus adjusted non-homeowners may be greater than the number of people in the sample.

Year	Securitization				S&P 500 Analysts			
	Adjusted HO	Adjusted Non-HO	Adjusted Multi-HO	Fraction Adj. HO	Adjusted HO	Adjusted Non-HO	Adjusted Multi-HO	Fraction Adj. HO
2000	104	118	15	0.502	70	106	13	0.435
2001	114	106	15	0.551	72	96	21	0.447
2002	122	94	18	0.589	80	91	20	0.497
2003	134	88	22	0.647	89	83	23	0.553
2004	139	77	28	0.671	95	75	27	0.590
2005	148	71	29	0.715	103	68	31	0.640
2006	157	59	30	0.758	113	59	32	0.702
2007	163	52	35	0.787	116	49	33	0.720
2008	160	51	35	0.773	118	49	32	0.733
2009	156	55	33	0.754	119	49	29	0.739
2010	160	56	35	0.773	120	46	31	0.745
Total people ever:	170	131	86	0.821	130	114	63	0.807
With age info.:	161	113	82	0.778	127	108	62	0.789

Table 5, continued

Year	Non-RE Lawyers				RE Lawyers			
	Adjusted HO	Adjusted Non-HO	Adjusted Multi-HO	Fraction Adj. HO	Adjusted HO	Adjusted Non-HO	Adjusted Multi-HO	Fraction Adj. HO
2000	224	227	53	0.527	55	32	12	0.679
2001	240	208	54	0.565	56	26	14	0.691
2002	263	192	62	0.619	58	26	14	0.716
2003	275	170	61	0.647	60	24	17	0.741
2004	288	159	68	0.678	60	23	16	0.741
2005	293	149	69	0.689	58	23	18	0.716
2006	299	142	77	0.704	60	24	18	0.741
2007	311	130	85	0.732	60	23	20	0.741
2008	308	121	83	0.725	60	21	20	0.741
2009	311	124	90	0.732	60	21	18	0.741
2010	314	120	84	0.739	60	21	15	0.741
Total people ever:	347	267	167	0.816	64	37	35	0.790
With age info.:	344	265	165	0.809	63	37	34	0.778

Table 6: Divesting Houses

The first four columns tabulate the number of divestitures per person for each group, by year. Z-statistics from a two-sample test of differences in proportions with the securitization sample are reported each group-year other than the securitization group. The next four columns report regression-adjusted differences in the number of divestitures per person each year, where we control for the eight age groups defined in Table 1 as well as an indicator for whether someone is a multi-homeowner at the start of the year, and the sample period is 2000-2010. The number of people in-sample each year is the number of homeowners at the beginning of each year for the two groups that are compared. T-statistics computed from person-clustered standard errors are reported in brackets below each difference. */**/** represents statistically significant at the 10%, 5%, and 1% levels, respectively.

Year	Divestitures per person				Regression-Adjusted Difference			
	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	Securitization minus:			RE Lawyers
					S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	minus non-RE Lawyers
2000	0.038	0.057	0.049	0.073	-0.0146	-0.00234	-0.0315	0.0290
		[-0.57]	[-0.40]	[-0.94]	[-0.42]	[-0.095]	[-0.82]	[0.78]
2001	0.018	0.056	0.054	0.036	-0.0292	-0.0309	-0.0108	-0.0198
		[-1.43]	[-1.50]	[-0.73]	[-0.97]	[-1.55]	[-0.40]	[-0.70]
2002	0.049	0.037	0.042	0.034	0.0180	0.0157	0.0218	-0.00474
		[0.39]	[0.33]	[0.44]	[0.59]	[0.67]	[0.70]	[-0.17]
2003	0.015	0.056	0.058	0.017	-0.0374	-0.0385	0.00606	-0.0442
		[-1.73]*	[-1.49]	[-0.09]	[-1.35]	[-1.74]*	[0.30]	[-1.73]*
2004	0.029	0.053	0.045	0.100	-0.0190	-0.0125	-0.0664	0.0538
		[-0.93]	[-0.72]	[-2.12]**	[-0.68]	[-0.60]	[-1.60]	[1.33]
2005	0.007	0.039	0.044	0.017	-0.0175	-0.0352	-0.00373	-0.0342
		[-1.79]*	[-2.13]**	[-0.69]	[-0.95]	[-2.48]**	[-0.20]	[-1.61]
2006	0.019	0.027	0.027	0.083	-0.00309	-0.00216	-0.0622	0.0582
		[-0.41]	[-0.50]	[-2.26]**	[-0.16]	[-0.15]	[-1.60]	[1.53]
2007	0.049	0.034	0.051	0.017	0.0125	-0.00354	0.0346	-0.0396
		[0.59]	[-0.10]	[1.09]	[0.50]	[-0.16]	[1.42]	[-1.74]*
2008	0.056	0.068	0.039	0.033	-0.0161	0.0150	0.0424	-0.0290
		[-0.40]	[0.61]	[0.69]	[-0.53]	[0.56]	[1.66]*	[-1.09]
2009	0.045	0.034	0.032	0.067	0.0219	0.0186	-0.0177	0.0378
		[0.47]	[0.69]	[-0.60]	[0.92]	[0.89]	[-0.39]	[0.87]
2010	0.025	0.033	0.041	0.017	-0.00597	-0.0154	0.0122	-0.0240
		[-0.41]	[-0.91]	[0.37]	[-0.26]	[-0.84]	[0.54]	[-1.10]
			Multi-homeowner?		0.0521	0.0742	0.0776	0.0910
					[4.23]***	[6.21]***	[5.60]***	[6.89]***
			Age Indicators?		Y	Y	Y	Y
			N		2519	4484	2087	3637
			R-Squared		0.021	0.025	0.044	0.035
			People		288	505	224	407

Table 7: Buying a First Home

The first four columns tabulate the number of first home purchases per person for each group, by year. Z-statistics from a two-sample test of differences in proportions with the securitization sample are reported each group-year other than the securitization group. The next four columns report regression-adjusted differences in the number of first home purchases per person each year, where we control for the eight age groups defined in Table 1. The number of people in-sample each year is the number of non-homeowners at the beginning of each year for the two groups that are compared, and the sample period is 2000-2010. T-statistics computed from person-clustered standard errors are reported in brackets below each difference. */**/** represents statistically significant at the 10%, 5%, and 1% levels, respectively.

Year	First home purchases per person				Regression-Adjusted Difference			
	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	Securitization minus:			RE Lawyers
					S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	minus non-RE Lawyers
2000	0.127	0.104 [0.54]	0.110 [0.44]	0.188 [-0.87]	0.0249 [0.52]	0.0309 [0.73]	-0.0526 [-0.67]	0.0830 [1.13]
2001	0.113	0.062 [1.26]	0.101 [0.33]	0.038 [1.14]	0.0672 [1.52]	0.0280 [0.66]	0.0842 [1.53]	-0.0527 [-1.21]
2002	0.096	0.099 [-0.07]	0.146 [-1.18]	0.077 [0.29]	-0.00948 [-0.19]	-0.0480 [-1.11]	0.0148 [0.22]	-0.0604 [-1.04]
2003	0.170	0.133 [0.69]	0.100 [1.57]	0.125 [0.53]	0.0506 [0.83]	0.0907 [1.73]*	0.0691 [0.83]	0.0321 [0.45]
2004	0.091	0.120 [-0.58]	0.138 [-1.00]	0.000 [1.50]	-0.0151 [-0.28]	-0.0453 [-0.95]	0.106 [2.63]***	-0.139 [-4.55]***
2005	0.169	0.132 [0.60]	0.074 [2.17]**	0.000 [2.14]**	0.0771 [1.09]	0.136 [2.28]**	0.237 [4.03]***	-0.0865 [-3.55]***
2006	0.153	0.186 [-0.49]	0.099 [1.09]	0.083 [0.84]	-0.0129 [-0.16]	0.0781 [1.25]	0.108 [1.31]	-0.0198 [-0.31]
2007	0.115	0.061 [0.95]	0.115 [0.00]	0.087 [0.36]	0.0483 [0.74]	-0.00667 [-0.11]	0.0373 [0.47]	-0.0262 [-0.39]
2008	0.059	0.082 [-0.44]	0.025 [1.11]	0.000 [1.13]	0.00699 [0.11]	0.0453 [0.99]	0.0971 [2.10]**	-0.0297 [-1.67]*
2009	0.036	0.082 [-0.98]	0.065 [-0.68]	0.000 [0.88]	-0.0244 [-0.44]	-0.0254 [-0.57]	0.0607 [1.61]	-0.0675 [-2.51]**
2010	0.161	0.087 [1.11]	0.050 [2.48]**	0.000 [1.98]*	0.134 [1.69]*	0.152 [2.30]**	0.216 [3.37]***	-0.0490 [-2.05]**
			Age Indicators?	Y	Y	Y	Y	
			N	1376	2363	916	1959	
			R-Squared	0.030	0.030	0.052	0.032	
			People	221	378	150	302	

Table 8: Buying a Second Home or Swapping Up

The first four columns tabulate the number of second home/swap up purchases per person for each group, by year. Z-statistics from a two-sample test of differences in proportions with the securitization sample are reported each group-year other than the securitization group. The next four columns report regression-adjusted differences in the number of second home/swap up purchases per person each year, where we control for the eight age groups defined in Table 1 as well as an indicator for whether someone is a multi-homeowner at the start of the year. The number of people in-sample each year is the number of homeowners at the beginning of each year for the two groups that are compared, and the sample period is 2000-2010. T-statistics computed from person-clustered standard errors are reported in brackets below each difference. */**/** represents statistically significant at the 10%, 5%, and 1% levels, respectively.

Year	Second home/swap up purchases per person				Regression-Adjusted Difference			
	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	Securitization minus:			RE Lawyers
					S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	minus non-RE Lawyers
2000	0.058	0.086	0.076	0.091	-0.00991	0.00655	-0.0161	0.0229
		[-0.66]	[-0.57]	[-0.71]	[-0.25]	[0.26]	[-0.35]	[0.51]
2001	0.070	0.139	0.037	0.107	-0.0167	0.0583	0.0106	0.0527
		[-1.38]	[1.27]	[-0.82]	[-0.35]	[2.51]**	[0.28]	[1.49]
2002	0.090	0.062	0.076	0.052	0.0663	0.0382	0.0649	-0.0235
		[0.71]	[0.47]	[0.90]	[2.00]**	[1.50]	[1.88]*	[-0.77]
2003	0.082	0.079	0.073	0.050	0.0358	0.0217	0.0639	-0.0364
		[0.09]	[0.32]	[0.80]	[1.07]	[0.89]	[1.89]*	[-1.18]
2004	0.058	0.095	0.080	0.083	-0.00470	-0.0131	-0.00756	-0.000364
		[-1.01]	[-0.78]	[-0.67]	[-0.13]	[-0.55]	[-0.21]	[-0.010]
2005	0.122	0.068	0.061	0.086	0.0982	0.0742	0.0691	0.0110
		[1.29]	[2.01]**	[0.66]	[2.80]***	[2.51]**	[1.64]	[0.31]
2006	0.064	0.097	0.090	0.067	0.00774	-0.0109	0.0207	-0.0274
		[-1.02]	[-0.96]	[-0.08]	[0.26]	[-0.47]	[0.59]	[-0.80]
2007	0.061	0.060	0.064	0.083	0.0339	0.0124	0.00359	0.0143
		[0.03]	[-0.09]	[-0.54]	[1.19]	[0.44]	[0.087]	[0.35]
2008	0.031	0.025	0.039	0.017	0.0246	0.00198	0.0422	-0.0342
		[0.29]	[-0.40]	[0.59]	[1.08]	[0.10]	[1.51]	[-1.39]
2009	0.038	0.034	0.039	0.033	0.0211	0.0234	0.0327	-0.00454
		[0.21]	[-0.01]	[0.18]	[0.89]	[1.18]	[1.03]	[-0.16]
2010	0.037	0.050	0.029	0.017	0.00347	0.0150	0.0278	-0.00909
		[-0.51]	[0.52]	[0.78]	[0.14]	[0.83]	[1.02]	[-0.40]
			Multi-homeowner?		0.305	0.274	0.304	0.242
					[14.9]***	[16.4]***	[13.1]***	[14.4]***
			Age Indicators?		Y	Y	Y	Y
			N		2519	4484	2087	3637
			R-Squared		0.248	0.204	0.255	0.171
			People		288	505	224	407

Table 9: Robustness

We report annual incidence ratios of a maximum likelihood estimation of the transaction intensity in a Poisson model. Panel A compares the intensity of divestitures. Panel B compares the intensity of first home purchases, while Panel C compares the intensity of second home purchases plus swap-up purchases. T-statistics for the null hypothesis that the ratio equals one are reported below each coefficient in brackets. */**/** represents statistically significant at the 10%, 5%, and 1% levels, respectively. In Panel B, since the number of first home purchases for the real estate lawyers is zero from 2008 onwards, the ratio of expected outcomes involving the real estate lawyers is omitted from the panel.

Panel A: Divestitures					Panel B: First home purchases		
Annual incidence ratio (exp(β))					Annual incidence ratio (exp(β))		
Securitization divided by:					Securitization divided by:		
Year	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	RE Lawyers div. by non-RE Lawyers	Year	S&P 500 Analysts	Non-RE Lawyers
2000-2001	0.595 [-1.00]	0.639 [-1.01]	0.609 [-1.01]	1.074 [0.18]	2000-2001	1.515 [1.36]	1.270 [1.03]
2002-2003	0.781 [-0.51]	0.736 [-0.75]	1.513 [0.64]	0.497 [-1.18]	2002-2003	1.151 [0.50]	1.139 [0.54]
2004-2005	0.505 [-1.25]	0.424 [-1.77]*	0.351 [-1.83]*	1.161 [0.37]	2004-2005	1.222 [0.64]	1.365 [1.13]
2006-2007	1.127 [0.24]	0.918 [-0.22]	0.716 [-0.65]	1.197 [0.40]	2006-2007	1.097 [0.24]	1.352 [0.90]
2008-2009	1.057 [0.14]	1.499 [1.01]	1.294 [0.48]	1.175 [0.28]	2008-2009	0.797 [-0.41]	1.251 [0.41]
2010	0.834 [-0.26]	0.647 [-0.77]	1.613 [0.45]	0.439 [-0.82]	2010	2.497 [1.64]	3.671 [2.65]***
Multi-homeowner indicator?	Y	Y	Y	Y	Age Indicators?	Y	Y
N	2519	4484	2087	3637	N	1376	2363
Pseudo R-Squared People	0.047	0.070	0.093	0.091	Pseudo R-Squared People	0.022	0.035
	288	505	224	407		221	378

Table 9, continued

Panel C: Second home purchases and swap-up purchases

Year	Annual incidence ratio (exp(β))			
	Securitization divided by:			RE Lawyers
	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	div. by non-RE Lawyers
2000-2001	1.043 [0.16]	1.856 [2.37]**	1.185 [0.56]	1.533 [1.34]
2002-2003	2.134 [2.62]***	1.511 [2.08]**	2.697 [2.61]***	0.541 [-1.60]
2004-2005	1.758 [2.04]**	1.374 [1.47]	1.452 [1.10]	0.975 [-0.079]
2006-2007	1.173 [0.56]	0.986 [-0.059]	1.169 [0.45]	0.806 [-0.67]
2008-2009	1.348 [0.62]	1.106 [0.27]	1.916 [1.01]	0.560 [-0.95]
2010	0.925 [-0.15]	1.374 [0.63]	2.292 [0.81]	0.612 [-0.50]
Multi-homeowner?	Y	Y	Y	Y
Age Indicators?	Y	Y	Y	Y
N	2519	4484	2087	3637
R-Squared	0.422	0.406	0.429	0.374
People	288	505	224	407

Table 10: Equal-Weighted Performance Index

Panel A presents summary statistics for the performance index exercise on an equal-weighted basis. Averages per person are reported while standard deviations are reported below in parentheses. Dollar amounts are in thousands. Panel B reports average performance. Standard errors for two-sample t-tests with unequal variances of the null hypothesis that the difference in performance between the securitization group and other groups is zero are reported in brackets. */**/** denotes statistically significant at the 10%, 5% and 1% levels, respectively.

Panel A: Summary Statistics

	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
Number of properties: 2003q4	0.700 (0.605)	0.671 (0.748)	0.755 (0.728)	0.951 (0.757)
Value of properties: 2003q4	0.700 (0.605)	0.671 (0.748)	0.755 (0.728)	0.951 (0.757)
Cash account: 2003q4	0.901 (0.745)	1.073 (0.791)	0.961 (0.908)	0.857 (0.753)
Portfolio value: 2003q4	1.602 (0.640)	1.744 (0.658)	1.717 (0.891)	1.807 (0.752)
Number of properties: 2010q4	0.913 (0.655)	0.901 (0.726)	0.915 (0.785)	0.926 (0.755)
Value of properties: 2010q4	1.025 (0.762)	1.038 (0.845)	1.022 (0.871)	1.024 (0.818)
Cash account: 2010q4	0.766 (0.751)	0.932 (0.804)	0.889 (0.894)	1.026 (0.894)
Portfolio value: 2010q4	1.791 (0.709)	1.971 (0.725)	1.912 (0.868)	2.050 (0.837)
Number of people	207	161	425	81

Panel B: Performance, 2003q4-2010q4

	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
Return	0.122 (0.104)	0.135 (0.0935)	0.128 (0.111)	0.139 (0.0948)
Buy-and-hold return	0.145 (0.0626)	0.157 (0.0580)	0.147 (0.0661)	0.136 (0.0694)
Performance index	-0.0232 (0.0990)	-0.0219 (0.0885)	-0.0199 (0.0957)	0.00294 (0.0931)
Risk-free return	0.167 (0)	0.167 (0)	0.167 (0)	0.167 (0)

Table 10, continued

Panel C: Performance Differences

	Securitization minus:			RE Lawyers minus non- RE Lawyers
	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	
Return	-0.0134 [-1.30]	-0.00556 [-0.62]	-0.0168 [-1.32]	0.0113 [0.95]
Buy-and-hold return	-0.0122* [-1.93]	-0.00226 [-0.42]	0.00929 [1.05]	-0.0115 [-1.38]
Performance index	-0.00125 [-0.13]	-0.00330 [-0.40]	-0.0261** [-2.10]	0.0228** [2.01]
N	368	632	288	506

Panel D: Regression-Adjusted Performance Differences

	Securitization minus:			RE Lawyers minus non- RE Lawyers
	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	
Performance Index	-0.00259 [-0.25]	-0.00234 [-0.28]	-0.0249* [-1.89]	0.0234** [1.99]
Age controls?	Y	Y	Y	Y
N	343	607	267	500
R-Squared	0.011	0.013	0.027	0.032

Table 11: Value Weighted Performance Index

Panel A presents summary statistics for the performance index exercise on a value-weighted basis. Averages per person are reported while standard deviations are reported below in parentheses. Dollar amounts are in thousands. Panel B reports average performance. Standard errors for two-sample t-tests with unequal variances of the null hypothesis that the difference in performance between the securitization group and other groups is zero are reported in brackets. */**/** denotes statistically significant at the 10%, 5% and 1% levels, respectively.

Panel A: Summary Statistics

	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
Number of properties: 2003q4	0.700 (0.605)	0.671 (0.748)	0.755 (0.728)	0.951 (0.757)
Value of properties: 2003q4	402.4 (486.3)	518.4 (914.5)	331.3 (387.1)	500.0 (622.6)
Cash account: 2003q4	625.5 (579.1)	942.0 (888.2)	428.3 (422.2)	455.3 (462.8)
Portfolio value: 2003q4	1027.9 (650.2)	1460.4 (1107.4)	759.6 (498.9)	955.2 (715.4)
Number of properties: 2010q4	0.913 (0.655)	0.901 (0.726)	0.915 (0.785)	0.926 (0.755)
Value of properties: 2010q4	657.2 (624.3)	879.7 (1003.9)	469.9 (504.3)	565.8 (693.5)
Cash account: 2010q4	483.9 (508.6)	765.2 (848.6)	377.5 (375.5)	519.5 (502.9)
Portfolio value: 2010q4	1141.1 (699.5)	1644.9 (1209.7)	847.4 (536.1)	1085.3 (839.7)
Number of people	207	161	425	81

Panel B: Performance, 2003q4-2010q4

	Securitization	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers
Return	0.119 (0.105)	0.130 (0.0872)	0.125 (0.108)	0.135 (0.101)
Buy-and-hold return	0.148 (0.0576)	0.160 (0.0525)	0.147 (0.0651)	0.136 (0.0679)
Performance index	-0.0286 (0.0984)	-0.0292 (0.0809)	-0.0220 (0.0938)	-0.00102 (0.0998)
Risk-free return	0.167 (0)	0.167 (0)	0.167 (0)	0.167 (0)

Table 11, continued

Panel C: Performance Differences

	Securitization minus:			RE Lawyers minus non- RE Lawyers
	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	
Return	-0.0114 [-1.13]	-0.00628 [-0.70]	-0.0163 [-1.22]	0.0100 [0.81]
Buy-and-hold return	-0.0120** [-2.08]	0.000326 [0.064]	0.0113 [1.32]	-0.0109 [-1.34]
Performance index	0.000620 [0.066]	-0.00661 [-0.80]	-0.0276** [-2.12]	0.0210* [1.75]
N	368	632	288	506

Panel D: Regression-Adjusted Performance Differences

	Securitization minus:			RE Lawyers minus non- RE Lawyers
	S&P 500 Analysts	Non-RE Lawyers	RE Lawyers	
Performance Index	-0.00144 [-0.14]	-0.00589 [-0.70]	-0.0258* [-1.83]	0.0212* [1.71]
Age controls?	Y	Y	Y	Y
N	343	607	267	500
R-Squared	0.039	0.020	0.045	0.035

Appendix

A. Lexis/Nexis data

The Lexis-Nexis Public Records database aggregates information available from public records, such as deed transfers, property tax assessment records, public address records, and utility connection records. The interface allows us to search for people based on a variety of factors, as illustrated in Figure A1 of the Appendix. For each name, we begin by looking for any public information on the web that may provide additional clues on how to find them. For example, many people publish public profiles on LinkedIn, which include current location information (e.g., “Greater Chicago Area”) as well as educational history. Both of these allow us to narrow our search down to specific areas. Educational background is useful as Lexis/Nexis allows us to search for a person who has been in multiple locations. For example, although “John Doe” may be difficult to find, “John Doe” who lives in Illinois and also lived in Minnesota narrows it down. Campaign contribution records are also typically helpful. The Federal Election Commission’s website allows a name-based lookup of campaign contributions and displays the city and state of the person as well as a workplace. We use this and any additional information to attempt to find a person in Lexis/Nexis.

The system aggregates information from public records into a report about the person, as shown in Figure A2. The amount of information available varies by person. Typically, the data show the month and year of a person’s date of birth. We use this to compute the age of each person as of a common reference point, December 2011. The data often reports an address history for each person, any real properties ever owned, as well as other information (such as who the person’s neighbors are), based on the public records it has analyzed. The address history contains properties a person has owned and also often includes properties a person has rented based on utility connection records.

For people we are able to isolate, we check the address history of each person to determine which properties they have owned. Lexis/Nexis can compile a Property Report for any location where a person has resided, as shown in Figure A3. This report compiles information about the property itself and who the current and previous owners of the property are based primarily on deed transfers and property tax assessments. To obtain whether a person bought and sold a particular home, we examine deed and tax assessment records, as shown in Figures A4 and A5. The deed records often have the transaction date, transaction price, and some information about the mortgage terms, although mortgage information is

often incomplete. The difference in information availability arises due to varying recording requirements across counties.

To ease replication, we provide our full data collection manual online at the corresponding author’s website.

B. Matching lawyers

For each person in the securitization manager sample, we randomly choose matching lawyers at most five years older or younger and working at firms located in counties in the same MSA as the matched person. The location of the person in the securitization manager sample is determined by the first property they owned after 2000. We select five names per person except in Massachusetts, where age is not reported and we select ten names. We choose our lawyers with replacement, although we have almost no overlap of lawyers across securitization managers due to the large population of lawyers in Martindale-Hubbell. Having narrowed the list of names in a first round, we then further randomly select two names out of these five for data collection. We also subsequently attempt to collect data for the full matched set (five or ten lawyers) of lawyers for just under half of our securitization managers.

C. Transaction intensities

In the simplest conceptual setup where a person may only engage in one transaction per year, a basic estimate of the intensity of transaction type k occurring in year t is the number of people who conduct transaction k in year t divided by the number of people who could have conducted that transaction in that year. In this setup, the number of people eligible for each type of transactions at the beginning of the year is given in the following table.

Table A1: Eligible People for Different Transactions

Transaction Type	Eligible People That Year
Buy a first home during the year	Non-homeowners at beginning of year t
Buy a second home during the year	Homeowners at beginning of year t
Swap a home (up, down or missing) during the year	Homeowners at beginning of year t
Divest any home during the year	Homeowners at beginning of year t
Divest a second home during the year	Homeowners with multiple homes at beginning of year t

However, one person may engage in more than one type of transactions per year. For example, a non-homeowner at the start of year t may buy a first and second home during the year. In this case, the

person was a non-homeowner at the beginning of year t and bought a second home in year t . On the one hand, this may suggest that everyone in each sample is eligible to make every type of transaction each year. However, measuring the number of people eligible each year as the whole sample implicitly assumes that each person i in the sample has an equal probability of conducting transaction k irrespective of his homeowner status at the beginning of the year, which is clearly not true. For example, a non-homeowner at the beginning of the year has a much lower probability of buying a second home during the year than a homeowner, since the non-homeowner must buy two houses. Taking the whole sample as the number of eligible people ignores valuable conditioning information about whether he is a homeowner and will mix together two distinct sets of outcomes.

A full treatment of this problem requires creating multiple new transaction types – for example, buying a second home when beginning the year as a non-homeowner, buying a first home during the year when beginning the year as a homeowner, and so on. Since these types of multiple-transaction outcomes are infrequently observed, we instead modify our framework by counting the number of “adjusted homeowners” by taking the number of homeowners at the beginning of year t and adding the number of non-homeowners who bought a first home during year t . The number of people eligible to buy a second home or swap a home during year t is this adjusted homeowners group. Although this still mixes the two channels, it mitigates the issue by only including the non-homeowners who in fact buy a first home during the year.

Similarly, we create an “adjusted non-homeowners” group, which adds together people who are not homeowners at the beginning of year t with the number of people who divest their last property during the first six months of the year, and use this as the number of eligible people for buying a first home. Note that the number of adjusted homeowners plus the number of adjusted non-homeowners may exceed the total number of people in each sample.

To handle divestitures, we split the number of adjusted homeowners into those with two houses or more at any point during year t (or on the last day of year $t-1$) and label them adjusted multiple homeowners. Adjusted multiple homeowners are eligible to divest a second home. Because one may sell off houses in rapid succession, we take all adjusted homeowners as eligible to divest their last home. We summarize the adjustments homeowners, non-homeowners, and multiple homeowners in Table A2

and the people eligible for each type of transactions after accounting for the possibilities of multiple transactions per year in Table A3.

Table A2: Adjustments to Different Groups

Group	Definition
Adjusted homeowners at beginning of year t	Homeowners at beginning of year t plus non-homeowners who buy a first home during year t
Adjusted non-homeowners at beginning of year t	Non-homeowners at beginning of year t plus those who divest their last property in the first six months of year t
Adjusted multiple homeowners at beginning of year t	Adjusted homeowners at beginning of year t who have more than two houses at any point during the year t

Table A3: Eligible People for Each Type of Transactions after Adjustments

Transaction Type	Eligible People That Year
Buy a first home during the year	Adjusted non-homeowners at beginning of year t
Buy a second home during the year	Adjusted homeowners at beginning of year t
Swap a home (up, down or missing) during the year	Adjusted homeowners at beginning of year t
Divest any home during the year, including the last	Adjusted homeowners at beginning of year t
Divest a second home during the year	Adjusted homeowners with multiple homes at beginning of year t

Figure A1: Lexis-Nexis Development Professional Person Search Interface

LexisNexis® Custom Solution

https://w3.nexis.com/hottopics/Indp/

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Individual Company **Public Records** Dossier Advanced Sources History & Alerts Sign Out What's New

Public Records

Person Report
Business Report
Location Report

Comprehensive Person Report **People** **Businesses** **Location** **All Searches** Coverage Tips

Enter information in at least one field below.

Strict Search (Find only exact names and addresses)

Find similar-sounding last names

Find nicknames of first name

Last Name Street Address

First Name City

Middle Name/Initial State

Link ID Zip Code

SSN Previous City

Date of Birth Previous State

Age Range To Other Previous State

Alias Last Name Radius

Relative's First Name Telephone

Other Relative's First Name

[Save Search](#) [Clear Form](#)

Your DPPA Permissible Use: [No Permissible Use](#)
Your GLBA Permissible Use: [No Permissible Use](#)

Important: The Public Records and commercially available data sources used in Smartlinx® Reports have errors. The data collected by government agencies or private data vendors may sometimes be entered poorly, processed incorrectly and could contain factual defects. This system should not be relied upon as definitively accurate. Before relying on any data this system supplies, it should be independently verified. For Secretary of State documents, the information contained in the Smartlinx® Reports is for information purposes only and is not an official record. Certified copies may be obtained from that individual state's Department of State.

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Figure A2: Person Report

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<https://w3.nexis.com/hottopics/Indp/>

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Individual Company **Public Records** Dossier Advanced Sources History & Alerts Sign Out What's New

Public Records View: [Results List](#) | **Full** | [Visualize Report](#) 1 of 54 [NEXT](#)

Search: [Comprehensive Person Report](#) (1) > [Search Results](#) > Source Documents
 Terms: first-name [REDACTED] last-name [REDACTED] state(ALL) radius(30) ([Edit Search](#) | [New Search](#))

[Create Alert](#)
 Select for Delivery

Report Contents
[Top of Report](#)
[Subject Summary](#)
 Others Using SSN
[Address Summary \(5\)](#)
[Voter Registrations \(1\)](#)
 Professional Licenses
 Health Care Providers
 Health Care Sanctions
 Pilot Licenses
 Sport Licenses
[Real Property Assets \(4\)](#)
 Aircraft
 Bankruptcy Information
 Judgments/Liens
 UCC Liens
 Fictitious Businesses
[Potential Relatives \(5\)](#)
 Business Associates
 Person Associates
[Neighbors \(2\)](#)
 Employment Locator
 Criminal Filings
[Sources \(39\)](#)

Further Searches
[Health Care Provider](#)
[Health Care Provider](#)
[Sanctions](#)
[Civil Criminal Filings & Regulatory Actions](#)
[Jury Verdicts](#)
[Marriage & Divorce Records](#)

Full Name	Address	County	Phone
[REDACTED]	[REDACTED]	[REDACTED]	None Listed

ADDITIONAL PERSONAL INFORMATION

SSN	DOB	Gender	Link ID
[REDACTED] XXXX	[REDACTED]	[REDACTED]	[REDACTED]

Subject Summary

Name Variations [View All Name Variations Sources](#)

1:	[REDACTED]
2:	[REDACTED]
3:	[REDACTED]
4:	[REDACTED]
5:	[REDACTED]
6:	[REDACTED]
7:	[REDACTED]
8:	[REDACTED]

SSNs Summary [View All SSN Sources](#)

No.	SSN	State Iss.	Date Iss.	Warnings
Most frequent SSN attributed to subject:				
1:	[REDACTED] XXXX	[REDACTED]	[REDACTED]	

DOBs [View All DOB Sources](#)

Reported DOBs:
 [REDACTED]

Address Summary - 5 records found [View All Address Variations Sources](#)

No.	Address	Actions
1:	[REDACTED]	Get Report
2:	[REDACTED]	View Details Get Report
3:	[REDACTED]	View Details Get Report
4:	[REDACTED]	View Details Get Report
5:	[REDACTED]	View Details Get Report

Figure A3: Property Report

The screenshot displays the LexisNexis web interface for a property report. The browser address bar shows the URL <https://w3.nexis.com/hottopics/Indp/>. The page header includes navigation tabs: Individual, Company, Public Records (selected), Dossier, Advanced, Sources, History & Alerts, Sign Out, and What's New. The LexisNexis logo and tagline "for Development Professionals" are in the top right.

The main content area is titled "Public Records" and includes a search bar with the following details:

- Search: Comprehensive Person Report (1) > Search Results > Source Documents
- Terms: first-name [redacted] last-name [redacted] state(ALL) radius(30) (Edit Search | New Search)

On the left, a sidebar lists report types: Person Report, Business Report, and Location Report. Below this is a "Report Content" section with links for:

- Top of Report
- Location Information (1)
- Address Variations (2)
- APN(s) (4)
- Property Information (2)
- Neighbors (5)
- Associated Entities (10)
- Sources (100)

The main report content is divided into two sections:

- No. 1**:
 - Legal Description: [redacted]
 - Land Usage: CONDOMINIUM
 - Tax Year: [redacted]
 - Data Source: A
 - Property Sale Information:
 - Sale Date: [redacted]
 - Sale Price: [redacted]
 - Assessment Information:
 - Assessed Land Value: [redacted]
 - Assessed Improvement Value: [redacted]
 - Assessed Total Value: [redacted]
- No. 2**:
 - Legal Description: [redacted]
 - Tax Year: [redacted]
 - Data Source: B
 - Property Sale Information:
 - Sale Date: [redacted]
 - Sale Price: [redacted]
 - Assessment Information:
 - Assessed Land Value: [redacted]
 - Assessed Improvement Value: [redacted]
 - Assessed Total Value: [redacted]

Below the property details are two summary sections:

- Neighbors - 5 records found**: A table with columns for Neighbor Name, Neighbor Address, Neighbor Phone, and Actions. All data is redacted, and each row has a "Get Report" link.
- Associated Entities - 10 records found**: A table with columns for No., Current Owner(s)/Resident(s), and Actions.

No.	Current Owner(s)/Resident(s)	Actions
1.	[redacted]	Get Report
2.	[redacted] Current Owner(s):	Get Report
3.	[redacted] Current Resident(s):	Get Report
4.	[redacted] Previous Owner(s):	Get Report
5.	[redacted]	Get Report
6.	[redacted]	Get Report
7.	[redacted]	Get Report

Figure A4: Deed Record

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Individual Company **Public Records** Dossier Advanced Sources History & Alerts Sign Out What's New

Public Records View: [Return to Results](#)

Person Report
Business Report
Location Report

Search: [Comprehensive Person Report](#) > [Search Results](#) > Source Documents

Terms: first-name [redacted] last-name [redacted] state(ALL) radius(30) ([Edit Search](#) | [New Search](#))

Report Content
[Top of Report](#)
[Deed Records \(9\)](#)

Deed Record
This data is for informational purposes only.

Buyer Information
Standardized Name: [redacted]
Original Address: [redacted]
Standardized Address: [redacted]

Seller Information
Standardized Name: [redacted]
Original Address: [redacted]
Standardized Address: [redacted]

Property Information
Original Property Address: [redacted]
Standardized Property Address: [redacted]
Land Use: [redacted]
Data Source: [redacted]

Legal Information
Assessor's Parcel Number: [redacted]
Recording Date: [redacted]
Contract Date: [redacted]
Document Number: [redacted]
Document Type: [redacted]
Book/Page: [redacted]
Legal Description: [redacted]

Sales Information
Sales Price: [redacted]
Sales Price Description: [redacted]
Total Transfer Tax: [redacted]

Mortgage Information
Mortgage Type: [redacted]
Title Company: [redacted]

Deed Record
This data is for informational purposes only.

Buyer Information

Figure A5: Tax Assessment Record

The screenshot shows a web browser window with the LexisNexis logo and navigation tabs. The main content area displays a 'Tax Assessment Record' with the following sections:

- Owner Information**
 - Standardized Name: [REDACTED]
 - Original Address: [REDACTED]
 - Standardized Address: [REDACTED]
- Property Information**
 - Original Property Address: [REDACTED]
 - Standardized Property Address: [REDACTED]
 - Land Use: [REDACTED]
 - Data Source: [REDACTED]
- Legal Information**
 - Assessor's Parcel Number: [REDACTED]
 - Recording Date: [REDACTED]
 - Book/Page: [REDACTED]
 - Brief Description: [REDACTED]
 - Legal Description: [REDACTED]
- Sale Information**
 - Recording Date: [REDACTED]
 - Sale Price: [REDACTED]
- Assessment Information**
 - Assessment Year: [REDACTED]
 - Assessed Land Value: [REDACTED]
 - Assessed Improvement Value: [REDACTED]
 - Total Assessed Value: [REDACTED]
 - Zoning: [REDACTED]
- Tax Information**
 - Tax Amount: [REDACTED]
 - Tax Year: [REDACTED]
- Property Characteristics**
 - Year Built: [REDACTED]
 - Building Area: [REDACTED]
 - Style: [REDACTED]

At the bottom of the page, there is a red heading 'Assessment Record' followed by the text 'This data is for informational purposes only.'