

Wall Street and the Housing Bubble

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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 cautious behavior in purchasing homes on average, relative to two uninformed control groups: one composed of non-real estate lawyers and the other of non-housing equity analysts. Our findings cast doubt on the popular “inside job” view of the recent financial crisis that Wall Street employees knowingly ignored warning signs of the housing bubble.

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Following financial crises, concerns often arise about insiders having taken advantage of outsiders by first pumping up asset prices and then selling before prices crashed. In the aftermath of the collapse of the Internet bubble in early 2000s, public outrage about the perception of conflicts of interest among sell-side analysts within investment firms led to lawsuits and settlements in the billions of dollars. After the collapse of Enron and WorldCom, outrage regarding executives and directors enriching themselves by selling their shares shortly before their companies' failures led to the Sarbanes-Oxley Act of 2002. These concerns have resurfaced after the recent crisis on Wall Street. 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, and, in particular, blames employees in securitized finance for selling securities backed by dubious-quality subprime mortgage loans to uninformed investors. Building on the premise that Wall Street employees anticipated the housing bubble earlier than others, this inside job view holds that the crisis was avoidable if appropriately designed incentives and necessary government oversight were in place.

However, there are open disagreements among policy makers and academic researchers about this view, and, in particular, 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.”

Motivated by this disagreement and the importance of this issue, 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 market from their job incentives.

This paper confronts this challenge by exploiting the special nature of personal transactions in housing markets. Different from typical financial assets, residential homes are an indispensable part of everyone’s life. A home typically exposes its owner to house price risk in the hundreds of thousands of dollars. As a result, even employees in the financial industry, despite their relatively high incomes, should have maximum incentives to make informed home-transaction decisions regardless of any potentially 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 for two reasons. First, mid-level managers make many important business decisions in financial firms. In contrast, as revealed by the recent risk management failure of JP Morgan, the top executives may be detached from operations of individual groups inside their firms. Second, the relatively large sample of mid-level managers makes it possible to statistically compare their behavior against other control groups.

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 personal home transaction history of these securitization managers.

We address the question of whether securitization managers were more aware of the housing bubble by comparing their home transactions to those of uninformed control groups, which arguably had no private information about housing and securitization markets. We distinguish between two forms of awareness, a strong form and a weak form. Under the strong form, 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 bust in 2007-2009. Given the difficulties of timing the market, however, any awareness of a bubble might appear in a weaker form, where 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.

We construct two uninformed control groups. The first control group consists of a random sample of lawyers who did not specialize in real estate law. The people in this group are part of the general public with a relatively high income, and who were not directly involved in housing markets as part of their job. A nuanced issue for our analysis is that securitization managers received large bonuses during the bubble years. The large income shocks can motivate them to buy houses despite their potential awareness of the housing bubble. To address this concern, we choose the second control group to be a sample of equity 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 arguably experienced income shocks similar to those experienced by securitization managers during the bubble period.

Our analysis shows little evidence of securitization managers' awareness of the bubble in their own home transactions. When compared to lawyers, those 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, and less likely to divest homes, during this time. There is also no evident difference between securitization managers and equity analysts in their home acquisition and divestiture propensities in 2004-2006. This indicates that securitization managers 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 lawyer group during the bubble period.

One might argue that, even if securitization managers were well informed of the housing bubble, they might have chosen to ride the bubble rather than immediately selling out. This argument implies that while they might not have sold their homes exactly at the peak of the bubble, they should have done better in timing the bubble than the control groups. We evaluate the net performance of housing transactions during the bubble by constructing 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. Relative to the buy-and-hold strategy, all groups perform worse over this period as a result of their home transactions. Cross-sectionally, 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.

We are also able to obtain income information for a subset of people in our samples in the year they purchased a home by matching the year of their purchase, their mortgage amount, and property location with the information provided in the 2000-2010 Home Mortgage Disclosure Act (HMDA) mortgage application data. Indeed, during the bubble period, home purchasers in the securitization manager group experienced income shocks larger than those in the lawyer group but comparable to those in the equity analyst group. To the extent that awareness of the housing bubble should have led securitization managers to realize that their income shocks were unlikely to persist, the inside job view implies that securitization managers should have acquired homes with more conservative value-to-income ratios than the control groups. We find little evidence of securitization managers being less aggressive in this quality-dimension. However, we confirm their income shocks being transitory as the income of home purchasers dropped dramatically in the bust period, and that the securitization managers who purchased homes during the boom divested homes at a higher rate than the control groups during the bust. This suggests that purchasers in 2004-2006 did not live happily ever after in the homes they recently purchased.

Taken together, our analysis gives little support to the inside job view that securitization managers knowingly ignored warning signs of the bubble, as they on average failed to either time the housing markets or exercise cautious behavior in the timing and size of their personal home purchases relative to other less informed groups.

We emphasize that our results do not contradict the existing evidence that bad incentives caused loan officers and securitization managers to relax lending standards and seek unwarranted risk without the knowledge of the housing bubble (e.g., Keys, et al. (2010), Berndt and Gupta (2009), and Agarwal and Ben-David (2012)). Instead, our analysis highlights that the role played by over-optimism and distorted beliefs, as emphasized by the theoretical work of Gennaioli, Shleifer, and Vishny (2011) and Benabou (2011), should not be dismissed, but rather, taken seriously. In this sense, our results reinforce the analysis of Gerardi, et al. (2008) and Foote, Gerardi and Willen (2012), who document that issuing firms of mortgage-backed securities kept a large amount of risk on their own books, which eventually resulted in large losses. By comparing personal home transactions of securitization managers to lawyers and equity analysts, our micro-level evidence isolates securitization managers' beliefs from 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. 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 that overstating Wall Street employees' knowledge of the housing bubble is likely to lead to exaggeration of any effects attributed to failures in governance.

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

Loosely speaking, Wall Street employees might have contributed to the recent financial crisis through two orthogonal forces, one due to their incentives and the other through their beliefs. The recent academic literature has put great emphasis on the poorly designed incentives of financial firms. Acharya, et al (2010) provide an overview of bad incentives originating from different sources. One of the commonly mentioned bad incentives is the lack of skin in the game in the originate-and-distribute lending model. 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 (e.g., Keys, et al. (2010), Berndt and Gupta (2009), and Agarwal and Ben-David (2012)). Another potential source is short-term performance-based compensation schemes for Wall Street executives and traders. As they are compensated by short-term profits based 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 (e.g., Bebhuk, Cohen, and Spamann (2010)). Finally, to prevent systemic failures of the financial system, the government gives both explicit and implicit guarantees to bail out banks and financial firms, which, in turn, encourages them to seek systemic risk (e.g., Rajan (2010)).

A small strand of the literature emphasizes that behavioral biases and cognitive dissonance might have caused Wall Street employees to be too optimistic to fully comprehend the risk presented by the housing bubble. Barberis (2012) discusses this view and emphasizes that Wall Street employees might have over-extrapolated the past growth of home prices during the housing bubble. Gennaioli, Shleifer and Vishny (2011, 2012) build a theory of shadow banking in which local thinking bias causes investors and financial intermediaries to ignore unlikely tail risk during normal times and only realize the risk after a bad shock, which in turn exacerbates the

downturn. Benabou (2011) develops a model of groupthink, in which the interaction structure in groups and organizations makes wishful thinking (denial of bad news and warning signs) contagious across agents. Finally, Bolton, Scheinkman and Xiong (2006) emphasize that shareholders tend to be optimistic about firms' fundamentals and, as a result, prefer hiring optimistic executives and incentivize them to pursue aggressive investment strategies.

The inside job view is a strong form of the bad incentives view as it posits not only poorly designed incentives but also Wall Street employees' recognition of the housing bubble. Our analysis aims to test this latter necessary condition of the inside job view. Any evidence against this view would not necessarily reject the more general bad incentives view, but would suggest that distorted beliefs might have played a more important role than assumed under the inside job view.

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 fairly 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 focus on the behavior of mid-level managers in the securitization business as our object of study. As securitization was an indispensable part of the housing bubble, understanding the beliefs of securitization managers about the housing market 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, even if the top executives and group heads were more informed about the structural problems in the housing markets, we expect their concerns to affect their subordinates. This motivates us to directly test whether selling dubious-quality mortgage backed securities and taking massive risks despite anticipating a crash was a systematic problem at the middle levels of management. Finally, the large sample of mid-level managers makes it possible to statistically compare their behavior against other control groups.

We use a revealed belief approach based on people's personal home transactions. A home is typically a significant portion of a household's balance sheet. As our data will confirm later, this is true even for the mid-level securitization managers in our sample. To the extent that homeowners have thick skin in their homes, they have maximum incentives to acquire information and make informed buying and selling decisions. In particular, for financial sector employees, we do not expect any aforementioned bad 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.¹

Our analysis focuses on testing whether securitization managers were more aware of the housing bubble than uninformed control groups. Their awareness may present itself 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 bubble period of 2004-2006.

¹ 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.

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 securitization managers from actively timing the housing price crash. Second, even if securitization managers knew about the presence of a housing bubble, they might not be able to precisely time the crash of 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 housing and finance industries, and the other group from inside the finance industry but outside the 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. We separate lawyers specialized in real estate from non-real estate lawyers and use only non-real estate lawyers as the first of our uninformed control groups. In selecting these lawyers, we also make sure that they are matched with similar ages and geographic locations as the securitization managers in our sample.

We recognize that securitization managers experienced large income shocks during the financial market boom that accompanied the housing bubble and lawyers did not experience such income shocks. Thus, it is useful to have another control group which experienced similar income shocks as those by securitization managers. We choose financial equity analysts who covered non-housing companies in S&P 500 index as such a control group. These equity 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 be less informed about the housing bubble than securitization managers.

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

Hypothesis 1 (Inside Job View): Securitization managers exhibited more awareness of the housing bubble relative to lawyers and non-housing equity 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.

A nuanced issue in our analysis is that the large income shocks experienced by securitization managers during the bubble period might induce them to acquire homes despite their awareness of the bubble. The housing finance literature (e.g., Yao and Zhang (2005), Cocco (2005), and Ortalo-Magne and Rady (2006)) provides models to analyze individuals' home purchase decisions in the presence of income shocks, credit constraints, and life-cycle and investment portfolio considerations. To the extent that large bonuses received by securitization managers during the bubble period relaxed their credit constraints by allowing them to afford the down payments of home purchases, one might interpret their home purchases during the period as a reflection of the relaxed credit constraints rather than their expectations of the future housing prices. We partially control for this concern by using equity analysts, who experienced similar income shocks, as a control sample.

The models also imply the size of houses purchased by securitization managers as an indicator of their expectations of the persistence of their incomes. To the extent that a home provides a utility stream over time and is costly to swap, a household should choose an optimal size based on its expected permanent income rather than the current income. Thus, the awareness of the housing bubble, posited by the inside job view, should have led securitization managers to realize that their current incomes were unlikely to persist and purchase homes with more conservative value-to-income ratios than the control groups, which we test directly. We

also test whether securitization managers who purchased homes in the 2004-2006 period “lived happily ever after” by testing whether they divested more heavily than other groups during the housing bust.

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, and 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 and position of each person, and the name of the firm for which the person worked. The conference attendees are typically 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 Managing Director-type positions. We then oversample 42 names from a list of ten prominent banks such as Lehman Brothers and Citigroup.² We call this sample of 282 people the securitization manager sample. Of these, 11

² 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

were C-level executives, and 12 worked for companies clearly not involved in housing, so we eliminate them from our sample.

We use Lexis-Nexis Public Records data to research the background information of our sample. This data 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 also allows us to look up all historical deed transfer records and tax assessment records associated with each property. This allows us to scan the history of each property to see if a house was transacted under a spouse's name or trust instead. These records often have the transaction date, transaction price, names of parties involved, and, in many cases, the mortgage amount associated with the purchase. 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.³ 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 the purpose of transactions, as we discuss below. To ease data

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.

³ 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.

collection requirements, we skip properties sold well before 2000, as they are never owned during the 2000-2010 period and are thus immaterial for our analysis.

Our sample of equity 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.⁴ 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. We exclude lawyers who operate in real-estate-related fields as real estate lawyers may have potentially been informed about conditions in housing markets. 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 425 names.

2.2. Classifying home purchases and sales

Our starting point for understanding home purchase behavior is a broad framework which allows us to categorize the purpose of a transaction 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

⁴ Our references for SIC codes is CRSP, so a company needs to have a valid CRSP-I/B/E/S link.

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.⁵ If either the purchase or sale price is missing, we classify the swap generically as a “swap with no price information.”

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

⁵ Specifically, we sort home transactions for each 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 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 take care to ensure that one buy or sell transaction is not counted in two swaps. We also require the purchase date of the “swap sell” house to be before the purchase 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, with the purchase of home A in January as the swap buy leg and the sale of home B in March as the swap sell leg, but this is clearly not a swap. One person in our sample did this once. If multiple homes were sold within a six month window of a purchase, the house with the closest sale date to the date of a purchase in absolute terms 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.

own any homes but are in the middle of swap transactions. The set of non-homeowners are people who do not own any homes 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.⁶ 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 is divesting 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 and underclassifying swaps in the final year of data.

2.3. Transaction intensities

Our main analysis centers on the annual intensity of each transaction type – that is, the number of transactions per person per time period – and the relative differences in these intensities across groups.⁷ We focus on an annual frequency to avoid time periods with no transactions.

Formally, the intensity of one type of transaction in year t in a sample group is defined as the number of transactions of that type in year t divided by the number of people eligible to make that type of transaction at the beginning of year t :

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

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 at the beginning of the year (people eligible for this type of transaction). An important feature of our data is that we

⁶ 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, January 2000.

⁷ 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.

observe not only transaction activity but also transaction *inactivity*, due to the comprehensiveness of the public records tracked by Lexis/Nexis. This allows us to test the hypothesis that one group was more cautious [i.e., bought less] than other groups, as we can compute the number of transactions of a certain type normalized by the total number of people who could have made that transaction, rather than only the number of people who actually did make that transaction.

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 during the year, 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. We use these adjusted groups as the basis for evaluating our intensities.

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, those who work for companies clearly not involved in housing, and those we cannot identify uniquely in Lexis/Nexis, we have information for 197 people in the securitization manager sample. After similarly eliminating people for the other sample groups, we have 160 equity analysts and 425 lawyers in our sample.

Table 1, Panel B presents the age distribution for all samples. The median ages in 2011 for the securitization manager, equity analyst, and lawyer samples are 45, 41, and 46, respectively. The equity 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 0.32.

Turning our attention to properties, Table 2, Panel A breaks down the number of properties owned over 2000-2010. Our data spans 302 properties owned for securitization managers during the 2000-2010 period, 240 for equity analysts, and 633 for lawyers. Of these, the majority were bought during the same period; for example, securitization managers bought 211 properties during this period. Roughly 35-40% of these were sold during this period; for example, securitization managers sold 117 properties during this period. There are a small number of properties for which we have no purchase date. A missing purchase price reflects missing data, which we deal with below. There are a substantial number of properties with either no sale date or a sale date after December 31, 2010; these are homes that were still owned as of that date.

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). Equity analysts tend to be concentrated more in New York.

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 equity analyst groups nearly doubled their stock of houses through 2007, with a decline through the end of 2009 for the securitization group before rising again. This already suggests that there is little evidence of that the securitization manager sample was cautious as a group, as their stock grew aggressively through time and displays little cross-sectional difference with that of the equity analyst group. The plot also 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.

Table 3 summarizes transaction prices each year. Through 2004, the average purchase price paid by securitization managers nearly tripled to \$1.2M; the median that year was \$950K. This likely reflects substantial income shocks to the securitization manager group, which we investigate in more detail in Section 4.3. The purchase prices paid by other groups also had large increases through time, although they were not as substantial. For lawyers, the price pattern before 2006 was nearly flat, but rose sharply in 2007-2009 and especially in 2010.

Examining annual purchase and sale activity is reduced form in that it masks the underlying choices that individuals made. Table 4 breaks down purchases and sales 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.⁸

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 more aware of the bubble?

We now turn our attention to examining the hypothesis that securitization managers were more aware of the bubble than other less informed groups. As discussed in Section 1.2, we examine this in two forms. The first form posits that securitization managers were able to better time the housing markets on their own accounts, i.e., that they had higher intensities of divestitures and swap downs in 2004-2006, relative to the control groups. Table 6 presents the divestitures per person per year 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 equity analysts and lawyers during the bubble period. For

⁸ The total number of swap sales and swap purchases over 2000-2010 may not exactly match as there may be corresponding swap legs six months before and after this period. 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 lawyers.

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 equity analysts and lawyers in every year from 2003-2006.⁹

To account for heterogeneity in the age profiles of each group, we compute regression-adjusted differences in intensities. We do this by constructing a strongly-balanced person-year panel that tracks the number of divestitures each year for each person, including zero if no divestiture was observed. We then estimate the following equation for each possible pairing of the securitization group with other groups using OLS:

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

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 , we condition the sample such that only the adjusted 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

⁹ The raw number of divestitures each year may be read off of Tables 5 and 6 by multiplying the intensity in a given year from Table 6 by the number of homeowners in that year given by Table 5. For example, in 2008, there were nine divestitures (0.059 times 152) in the securitization sample. The number of divestitures each year is thus small relative to the number of non-divestitures. We view our annual statistics as descriptive. We present more formal tests where we pool together intensities across different years in the robustness section. In contrast to our regression-adjusted differences, we do not condition on having age information when reporting these raw intensities.

homeowner category across samples, adjusted for these age and multi-homeownership factors. We cluster standard errors by person.¹⁰

Table 6 presents these regression-adjusted differences. 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 lawyers in 2005. 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 equity analysts or lawyers.

We next examine whether non-homeowners among securitization managers were cautious in purchasing homes in 2004-2006, the “cautious form” of Hypothesis 1. This 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 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, conditioning the panel each year to non-homeowners, and omitting the $MultiHO_{it}$ term as it does not apply to non-homeowners. Figure 4, Panel A plots the raw intensities through time.

The securitization manager sample had a very similar rate of first home purchases compared to the equity analysts except for 2001, 2005, and 2010, when the securitization sample qualitatively purchased more first homes. Both of these samples had higher rates of first home purchases than the lawyers, particularly during 2005 and 2006. Compared to 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 0.17 per non-homeowner for the securitization

¹⁰ The effective sample size (number of people contributing to the variation) of this estimation will be the total number of people who we ever observed as adjusted homeowners during the 2000-2010 period for whom we have age information across these two groups. This may be read off from the last row of Table 5. For example, when estimating equation (1) for the securitization sample and the equity analyst sample, the number of people will be 279 (153 plus 126). The number of homeowners contributing to the variation each year may be read off from Table A4 in the Appendix, which lists the number of homeowners and non-homeowners each year with age information. For example, when estimating (1) for the securitization manager and equity sample, the number of people observed in 2000 is 166 (97 plus 69).

manager sample, compared to 0.081 per lawyer, a difference that is even larger on a regression-adjusted basis. Compared to equity analysts, the securitization group purchased first homes slightly more aggressively in 2005. This suggests that, despite income differences, there is little evidence that non-homeowners in the securitization managers were avoiding entering the housing market during the bubble period.

Homeowners in the securitization manager sample also showed a similar lack of cautious behavior 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 equity analyst group. On a regression-adjusted basis, the difference in intensities is nearly 0.11 between the securitization sample and the equity analyst sample and 0.08 for the lawyer sample, both of which are statistically significant at the 1% level.

As a robustness check, we estimate a Poisson regression model for our transaction types while pooling together intensities across years. This approach explicitly models the discrete nature of the number of occurrences and estimates the Poisson intensity of the transaction via maximum likelihood. We 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 model for divestitures is:

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

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 54% of the rate of equity analysts during the 2004-2005 period, and 45% of the rate for lawyers in 2004-2005, a difference that is significant at the 10% level. Panel B shows that the rate of first home purchases is nearly the same or even qualitatively higher for every year grouping until 2008-2009 when compared to the equity analysts. Panel C shows that during the 2004-2005 boom years, the annual rate at which securitization managers acquired second homes or swapped up was much higher than that of equity analysts and lawyers.

4.2. Net trading performance

We next 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, the latter two of which we do not observe, and the first of which is similar across groups, which we discuss in the next section. 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 was 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

quarterly zip-code level home price indices from Case-Shiller. We assign 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.¹¹ 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 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 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 securitization managers’ average performance with the control group’s average 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, whereas the performance index isolates the effect of trading activity starting in 2004.

Table 10 presents the results. 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

¹¹ As a robustness check, we also evaluate performance on an equally-weighted basis, where we assign the value of each house in the initial quarter to be \$1.

¹² We endow each agent with enough initial cash to cover all future transactions in the following way. 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.

tests for differences in returns and the performance index. Panel C shows that the net trading performance of the securitization sample was very similar to that of equity analysts and lawyers. Relative to equity analysts, their average trading performance was only better by 12 basis points, and worse than lawyers by 87 basis points. We also compute regression-adjusted differences by additionally projecting observed performance indices onto age category indicators, and find that this changes very little [omitted for brevity but available from the authors]. Figure 5 illustrates the comparative evolution of the performance indices. What is apparent is that all groups, including securitization managers, were worse off at the end of 2010 relative to a buy-and-hold strategy that began in 2004.

4.3. Consumption and income shocks

One concern is that these results may be consistent with securitization managers being aware of the bubble if they knowingly bought houses that they anticipated would fall in value due to the consumption stream provided by housing as a durable good. For example, if agents are credit constrained, then large income shocks during the bubble period would enable them to purchase homes even if they anticipated a decline in value later as long as the consumption stream was large enough. In short, the concern is that all our previous results are driven by income shocks allowing agents to consume housing even if anticipated a crash.

On the one hand, comparing the securitization managers to the equity analyst sample partially controls for income shocks to the extent that they both work in the financial sector; even conditional on wanting to consume out of income shocks, one would expect a group that was aware of the housing bubble to either time the market better or be more cautious. However, there may be heterogeneity within the financial sector, so this issue is worth exploring in more detail. We first examine whether equity analysts did in fact receive similar income shocks to our securitization group using a sample of incomes that we observe at purchase.

However, a remaining concern is that the income shocks are large enough as to make cautiousness difficult to detect by analyzing only the timing of home transactions. To further investigate this issue, we explore three more tests. First, we examine whether the securitization sample was less aggressive than other groups in terms of the value-to-income ratio of their purchases. *Ceteris paribus*, if securitization managers expected their income shocks to be

transitory but uninformed equity analysts did not, we should observe securitization managers purchase homes at lower value-to-income ratios, where current income is in the denominator. Second, we examine whether these income shocks were in fact transitory by comparing purchaser income in the 2004-2006 period with that in the 2007-2009 period. Finally, we examine whether the subsample of securitization managers who purchased homes during the 2004-2006 boom divested homes at a higher rate than other groups during the bust.

4.3.1. Income data

We are able to observe income for a subset of people in the year they purchase a home by matching information we observe about the year of their purchase, their mortgage amount, and property location with the information provided in the Home Mortgage Disclosure Act (HMDA) 2000-2010 mortgage application data. The HMDA information contains data on the income relied on by the originating institution to underwrite the loan. Although most identifying information – such as the borrower’s name, exact date of origination and property address and zip code – are not provided, the data provides the mortgage amount (up to the thousands) as well as the census tract of the property. Census tracts (a six digit code) form a very fine within-county geographical classification that is specifically constructed by the Census Bureau every ten years to be relatively homogeneous with respect to population characteristics, economic status, and living conditions.¹³

We match purchases to the income reported at the time of purchase using the following procedure. We first use the ArcGIS geocoding software to obtain the census tract associated each property address. For each purchase, we then look for records in HMDA with the same mortgage amount in the year of purchase within the census tract of the property. In HMDA, we search within the set of mortgage applications where the loan was originated and where the

¹³ Further information on census tract definitions come from http://www.census.gov/geo/www/cen_tract.html. Relative to zip codes, which are designed to meet the operational needs of the US Postal Service, census tracts are designed around economic characteristics. Discussion about how zip codes compare to census tracts may be found online at <http://www.census.gov/geo/www/tiger/tigermapping.html>. We use 2000 census tract definitions, as the purchases we are trying to match come from the 2000-2010 period.

purpose of the loan was a purchase.¹⁴ If we successfully find a match, we take the stated income on the HMDA application as the income of our person at the time the purchase was made.

Table 11, Panel A summarizes the results of our matching procedure. For the securitization sample, we have mortgage information for 162 purchases out of 211 purchases we observe during the 2000-2010 period. (We have the census tract for every property.) Of these, 125 are matched, a success rate of 77%; for the equity analyst and lawyer groups, this rate is 65% and 66%, respectively. Over the entire 2000-2010 period, the average income level at purchase was \$289K for the securitization sample, \$320K for the equity analyst sample, and \$193K for the lawyers.¹⁵ All income figures are reported in 2006 dollars adjusted using the Consumer Price Index (CPI) All Items series as of the end of December 2006. Since multiple purchases may be observed for one person, we compute a person-level average by first averaging over all purchases within a person, and then averaging over people, which yields similar income levels.

One concern is that, even given an exact mortgage amount (e.g., \$300K), census tract, and purchase year, there may be multiple matches within HMDA. However, the average number of matches per purchase is roughly three, and the median match is unique.¹⁶ Given the economic construction of census tracts, we average income over all matches in HMDA as the income for that purchase. One can repeat the analysis using only unique matches, which reduces our sample by slightly less than half, and obtain qualitatively similar results that are more influenced by a small number of observations at the tail ends of the distribution.

4.3.2. Income shocks during the boom

¹⁴ Although HMDA reports the gender and ethnicity of the applicant, we chose not to match on this information as the public records information do not contain this and we did not wish to hazard guesses for a number of names.

¹⁵ One concern is that these numbers are a bit too “small” relative to what is commonly perceived as banker pay. First, it is notable that the most common titles in our sample are Vice President and Senior Vice President. Second, these incomes reflect average incomes at purchase; the median purchase ages (distinct from the 2011 ages for all people presented in Table 1) are 37 for the securitization group, 34 for the equity analyst group, and 36 for the lawyers. Finally, this may not represent all income from the borrower, only the income relied on by the banks to extend loans. This may understate the differences between the two finance sector groups and the lawyers as finance sector groups anecdotally receive large bonuses that were effectively like salary.

¹⁶ A similar concern theoretically arises about multiple purchases that we are trying to match having the same mortgage amount, census tract, and purchase year. This does not arise in our data.

We next compare the time-variation in purchase income across groups to assess whether the securitization and equity analyst groups received similar shocks across time. Because we only observe income at purchase, and not income in other years (and for non-purchasers), observed income levels are not unbiased representations of the true distribution of underlying income. Furthermore, our analysis does not represent income of the same people at repeat purchases, and the small sample size makes it difficult to draw definitive conclusions. However, with these caveats in mind, we view this as a useful descriptive exercise that allows us to assess whether there is consistent evidence across multiple tests for the hypothesis that securitization managers were aware of the bubble but nevertheless consumed out of income.

The top half of Table 11, Panel B reports mean and median income for purchasers in three three-year time periods: the 2001-2003 pre-boom period, 2004-2006 boom, and 2007-2009 bust. The table also reports the within-group increase in average income both in dollars in logs across the pre-boom and boom periods in Rows A-B.¹⁷ Between the pre-boom and boom periods, incomes rose for all groups. The increase in average purchaser income was \$142K for the securitization sample, well over half of the \$193K average income among people who purchased in 2004-2006 in the pre-boom period; taking the log of income first reveals that the increase in average log purchaser income was 0.47.¹⁸ Interestingly, these numbers are comparable for the equity analyst group, where the dollar increase was \$146K and the increase in logs was 0.38.

These numbers suggest that the income shocks received at the two groups were at least comparable in order of magnitude. Rows C-D of Table 11, Panel B presents coefficients from a formal “difference in difference” across these two groups, and reveals that the average purchasers in the securitization and equity analyst samples experienced relatively similar increases in income both in dollar terms and log terms during the boom. In particular, average purchaser income for securitization managers increased by \$4000 less than that of equity analysts. In log terms, average purchaser income increased by 0.09 more for the securitization

¹⁷ Throughout, we average within person over purchases to obtain a person-level average income for the period before averaging over people in each period.

¹⁸ We test whether the increase in average is significant by employing an OLS regression where the left-hand side is observed purchaser income and the right hand side is an indicator for whether the period is the boom period in a sample comprising the pre-boom and boom periods. We use standard errors clustered at the person level. We also analyze changes in median incomes using quantile regressions setup analogously, estimated using least absolute deviations, with bootstrapped standard errors. Results are very similar

manager group, as their pre-boom purchaser income was lower. As a benchmark, the average income for the securitization group was 0.353 higher in log terms than the lawyer group. These results are consistent with our initial hypothesis that the two finance industry groups received similar income shocks.

4.3.3 Value-to-income ratios

We now turn to the value-to-income (VTI) ratio of purchasers across these three time periods. We are interested in seeing whether securitization managers were less aggressive in this quality dimension than other groups during the boom. We compute the VTI ratio for the subsample of purchases where we both have income data from HMDA and an observed purchase price. Due to the nature of VTI as a ratio, we require a minimum nominal reported income of \$100K in the year of purchase to avoid drawing conclusions based on possible extreme tails overly influencing our analysis.

Table 12 tabulates the mean and median VTI for each group in each of the three periods. Rows A-B report the increase going from the pre-boom to the boom period within each group. The average VTI for purchasers in the securitization sample increased from 3.1 to 3.9; the median showed a slight decrease from 3.2 to 3.1, suggesting there are some purchasers who purchased homes at a very large VTI ratio, even after trimming out those with low incomes. The average VTI among equity analyst purchasers increased slightly from 3.3 to 3.4, while the median increased from 3.3 to 3.7. Rows C-D report the differences in differences with the securitization sample. The average VTI for purchasers among securitization managers rises slightly more relative to that of equity analysts, even when examining the log ratio. Overall, the evidence does not display any pattern consistent with the hypothesis that the securitization managers were less aggressive. If anything, there were a number of securitization managers who purchased homes at very aggressive VTI ratios in the 2004-2006 period.

We analyze the VTI ratio as it should be positively correlated with the size of the housing portfolio weight on a household's balance sheet relative to their income. One could alternatively analyze how aggressive households were in financing their purchases to understand whether they were exposing themselves to large default risk. This analysis is complicated by the possibility that leverage may be undone by larger savings elsewhere and also the positive incentives for

leverage provided by the mortgage interest rate tax shield. To be complete, we also examine loan-to-income (LTI) ratios of this sample and find consistent effects; in fact, LTI and VTI display an in-sample correlation of 0.89. This is not altogether surprising since many households put 20% down at purchase. Returning to Table 11, Panel A, we see that the median LTV is nearly 80% for all groups, with a mean around 72%.¹⁹

4.3.4 Incomes shocks during the bust

Table 13 returns to the income data to analyze what in fact happened to the purchaser incomes of our various groups. Of course, news reports and anecdotal evidence already suggest that incomes among these two groups fell substantially. What is less clear is whether these drops were similar across the securitization manager and the equity analyst groups.

Rows A-B report the within-group change in income across the boom and bust periods in average dollars and logs. Average purchaser income dropped by \$104K for the securitization manager sample and \$25K for the equity analyst sample; the resulting “difference in difference” reported in row C is drop of \$79K more for the securitization manager group. In logs, the amount drops by 0.27 more for the securitization manager group. The evidence suggests that the securitization manager group experienced if anything larger negative drops in income even relative to equity analysts. This not only underlines the transitory nature of this income, but also suggests that it is unlikely that previously credit constrained agents who were nonetheless aware of an impending crash in housing are driving the aggressive purchase behavior we saw before in 2005. If credit constrained agents knew that housing was going to crash substantially and that their and income would suffer dramatic drops as a result, it is unlikely that buying an expensive new house was the optimal response.

4.3.5 Did 2004-2006 purchasers sell during the bust?

We next examine whether there were differential patterns of selling during the bust across our groups within the subsample of purchasers during the 2004-2006 period. Table 14, Panel A

¹⁹ For four observations with extreme LTV values, we cap the LTV distribution at 1.2.

reports the number of properties purchased during this period and the number of purchasers for each group. For this analysis, we do not condition on having income data.

Table 14, Panel B reports the percentage of properties purchased in 2004-2006 remaining after each year starting in 2007 as well as the percentage of properties sold in each year. We find that securitization managers if anything sell off these properties more aggressively during this period relative to equity analysts, and at a similar rate with lawyers. At the end of 2007, 93% of the housing stock remains for the securitization manager group; by the end of 2010, only 76% remains. In contrast, for the equity analyst group, 95% of the housing stock remains at the end of 2007, dropping to 89% by 2010, as shown in Figure 6.²⁰ If anything, in the prime crisis years (2007 and 2008), securitization managers seem to sell off larger proportions of the initial stock of 2004-2006 purchases than either lawyers or equity analysts. Compared to equity analysts, we find that, during the bust, securitization managers sell more of the properties they purchased in 2004-2006 (see Table 14, Panel D, row 1).

Rather than look at what happens to the properties bought in 2004-2006, we also look at what happens to purchasers in 2004-2006. The idea is that purchasers in 2004-2006 have a portfolio of houses and may divest other houses than the one they just purchased. Table 14, Panel C reports the divestiture intensity among 2004-2006 purchasers, where we include a divestiture of any house even if it was purchased before this period. Again, if anything, in 2007 and 2008, securitization managers tend to divest at a higher intensity than both lawyers and equity analysts.

We next analyze net sales to see if these sales were replaced by purchases of other homes. Tests performed on the difference in sales during the bust period on all properties owned by people who purchased properties in 2004-2006 show that the intensity of net sales of properties by securitization managers is a bit higher than that of the equity analysts (see Table 14, Panel D, row 2). There is little significant difference in net sales between securitization managers and lawyers. Taken together, these results suggest that purchasers in 2004-2006 did not live happily ever after in the homes they recently purchased.

²⁰ Note that we are counting all properties bought over the 2004-2006 period. A house bought in 2004 may have been sold in 2005, meaning that the initial percentage in 2006 is not 1 across all groups.

5. Conclusion

Although there was certainly unsavory behavior on Wall Street during the housing boom – Fabrice 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 non-housing S&P 500 equity analysts. Our evidence thus lends little support to the view that the average securitization manager anticipated the crash earlier than most. Rather than emphasizing awareness of the bubble, our research points towards the need to understand how beliefs were formed among people in finance during the bubble.

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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.

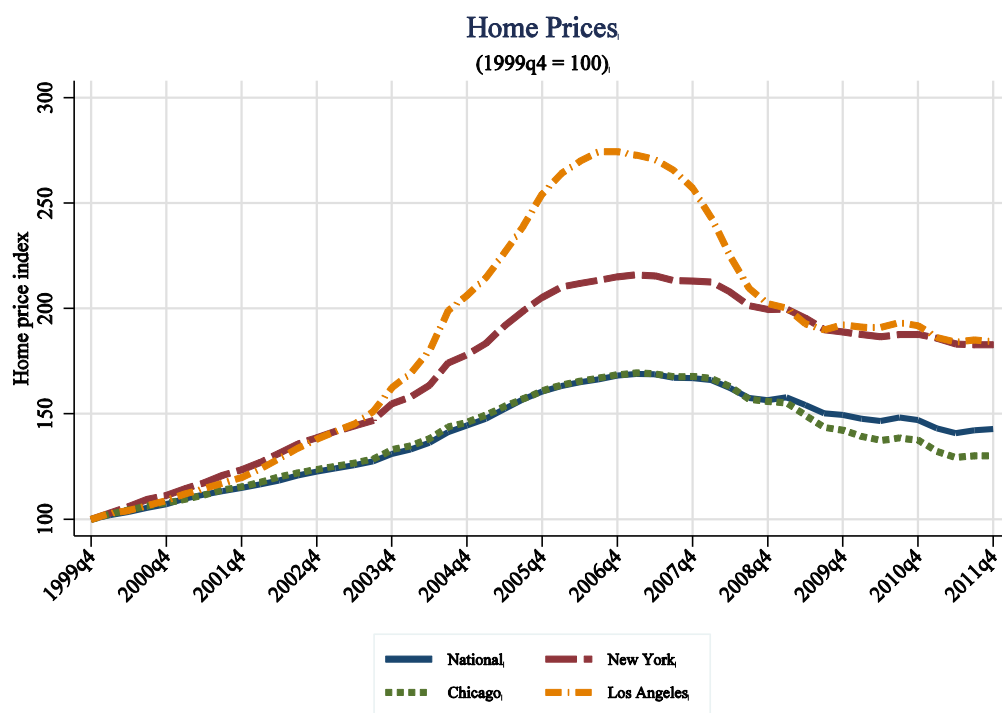


Figure 2: Housing Stock

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

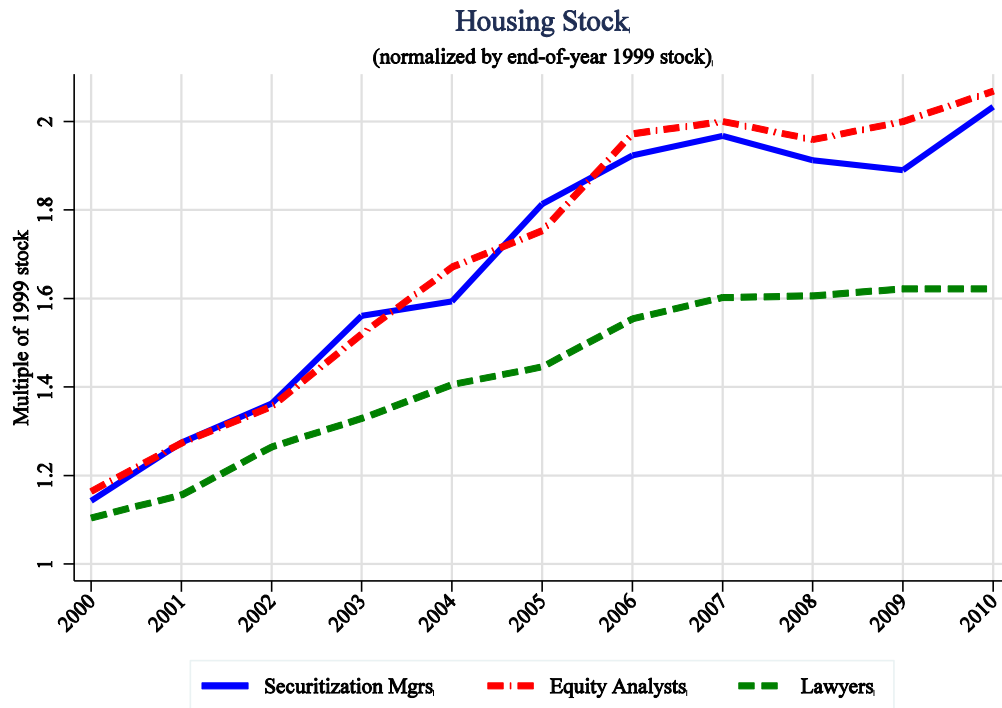


Figure 3: Divestitures

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

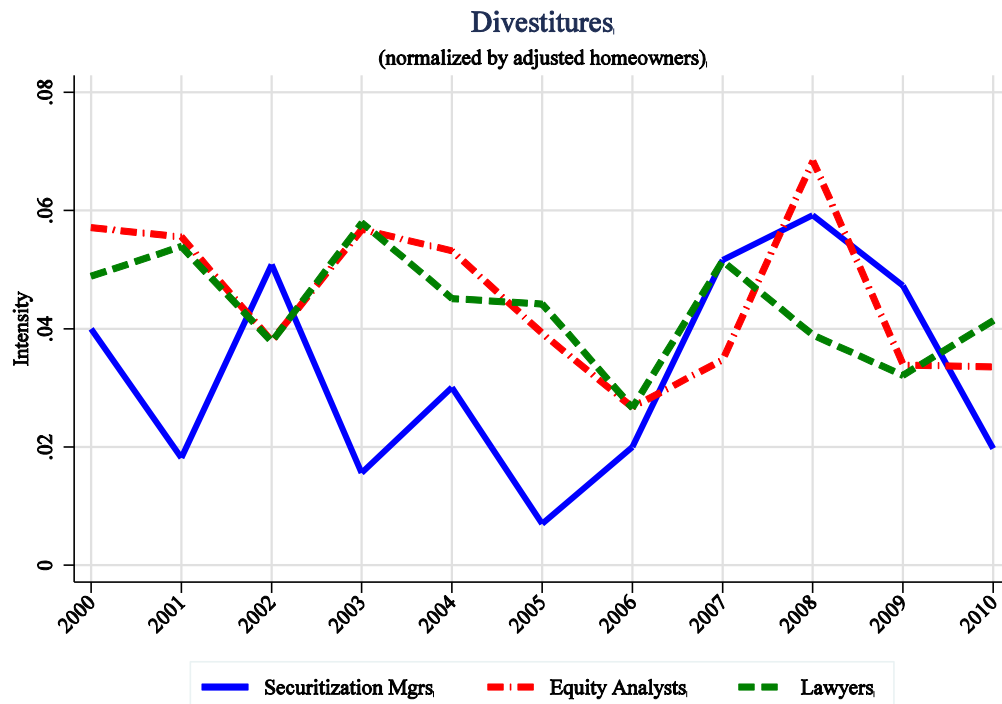
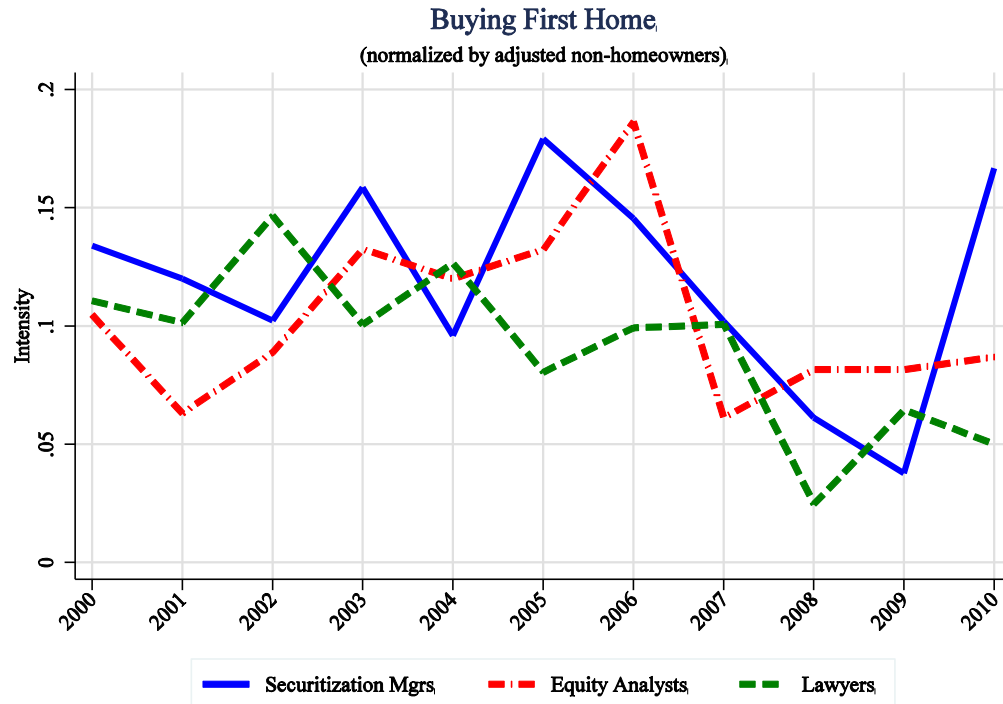


Figure 4: Purchases

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

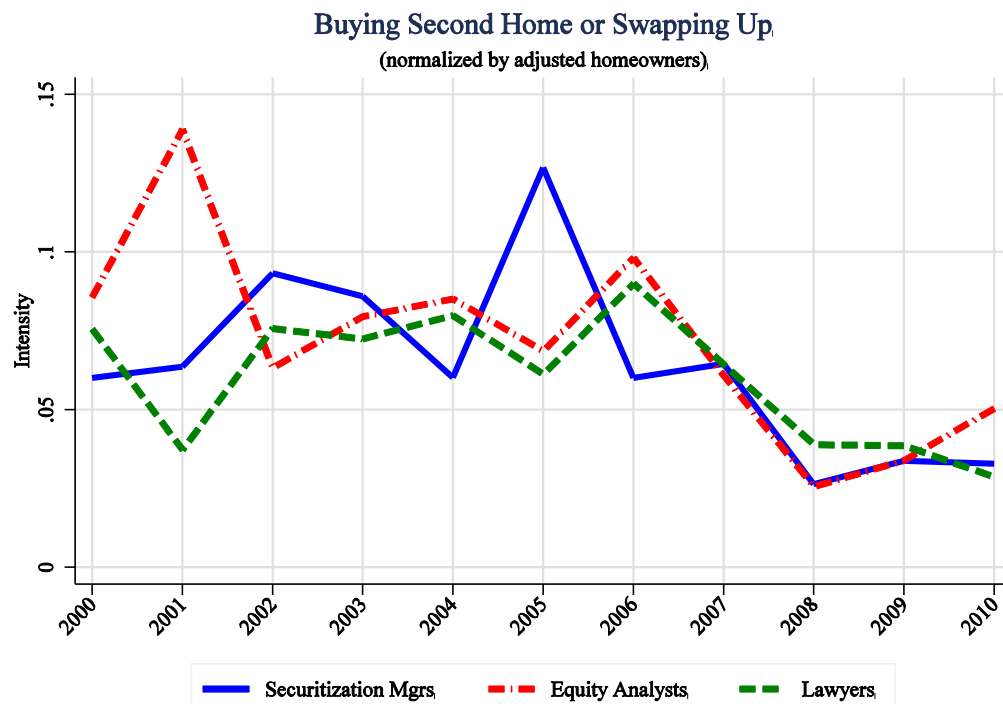


Figure 5: Trading Performance Indices

This figure plots the average performance index, defined as the cumulative return on the self-financed trading strategy less the buy-and-hold return of the initial stock of houses, where 2003q4 is taken as the initial quarter and houses are marked-to-market using quarterly zip-code level home price indices and the observed purchase price.

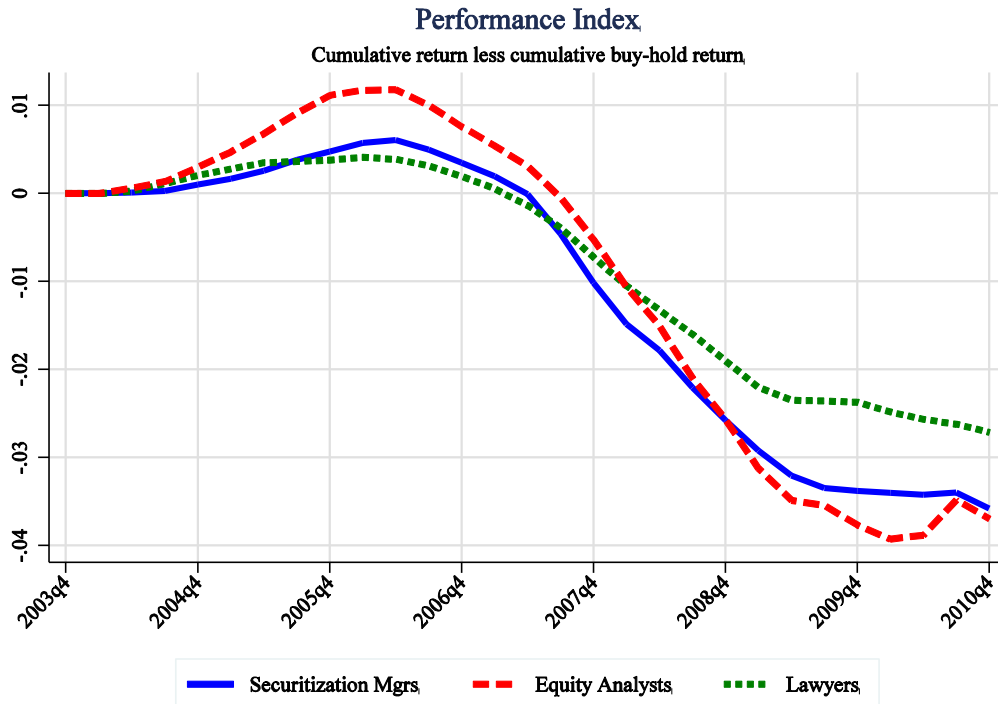


Figure 6: Properties Still Owned

This figure plots the percentage of properties purchased in 2004-2006 still owned at the end of each year. Note that a house bought in 2004 may be sold before 2006, hence the proportion is not 1 at the end of 2006.

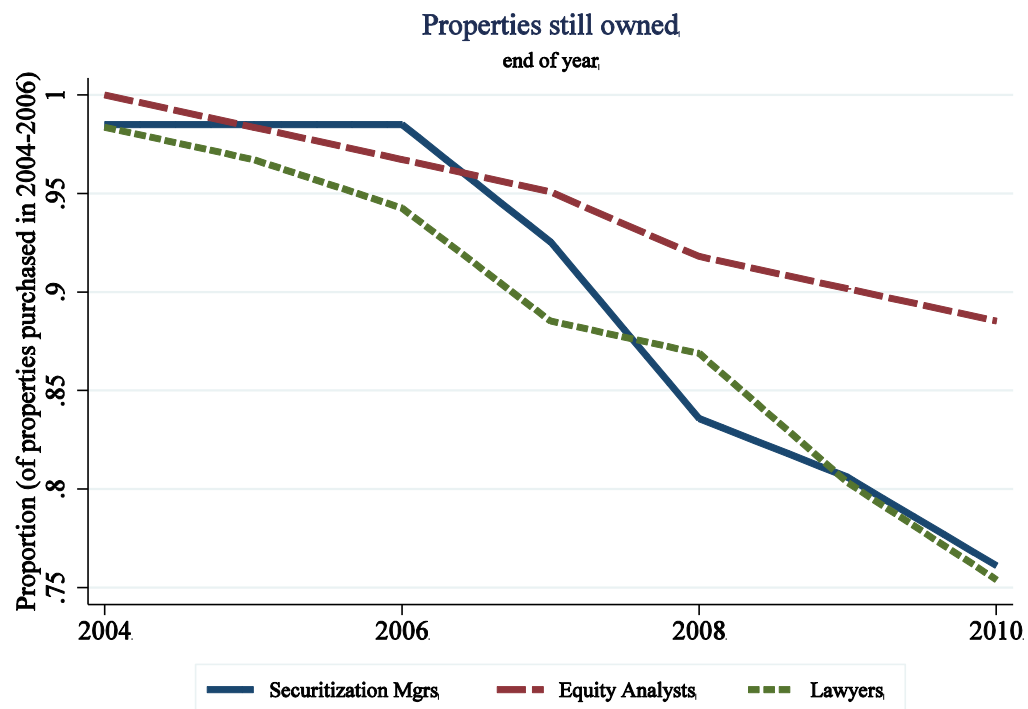


Table 1: People

This table lists the number of people for which we gathered information in each of three samples: Securitization Analysts, Equity Analysts, and Lawyers. 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	Equity Analysts	Lawyers
Number of names	282	200	438
Not mid-level manager	11	N/A	N/A
Not housing	12	N/A	N/A
Not found in public records	20	9	9
Multiple found in public records	20	12	2
International	22	18	0
Deceased	0	1	2
People in sample	197	160	425
Person found, but no homes owned	35	29	73
People who sold all properties before 2000	0	0	4
People who only own homes beginning after 2010	0	2	2
People in sample owning at least one home, 2000-2010	162	129	346

Panel B: 2011 Age Distribution

Age	Securitization	Equity Analysts	Lawyers
30 and under	0.56%	2.58%	1.43%
31 to 35	6.18%	12.26%	3.81%
36 to 40	16.29%	28.39%	17.14%
41 to 45	28.65%	22.58%	25.71%
46 to 50	25.28%	20.65%	20.00%
51 to 55	12.92%	3.87%	16.43%
56 to 60	5.06%	4.52%	8.57%
Over 60	5.06%	5.16%	6.90%
Total with age data	178	155	420
Missing age data	19	5	5
Chi-Square Test of Homogeneity with Sctzn Sample	N/A	20.97	8.16
Homogeneity Test, p-value	N/A	0.00	0.32
Median age	45	41	46

Table 2: 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: Total Properties, Purchases and Sales

	Securitization	Equity Analysts	Lawyers
Total properties ever owned, 2000-2010	302	240	633
Total purchases, 2000-2010	211	167	384
with purchase price	186	147	297
Number of homes with no purchase date	12	22	70
Total sales, 2000-2010	117	89	229
with sale price	102	81	160
Number of homes with no sale date or sold after Dec 31 2010	185	151	404

Panel B: Regional distribution

Region	Securitization	Equity Analysts	Lawyers
Pacific	18.21%	12.92%	23.22%
Mountain	5.96%	2.92%	3.79%
West North Central	7.95%	2.92%	2.05%
East North Central	12.58%	8.33%	11.85%
West South Central	5.96%	3.75%	8.69%
East South Central	2.65%	2.08%	2.05%
South Atlantic	12.91%	11.25%	14.69%
Middle Atlantic	23.18%	38.75%	21.17%
New England	10.60%	17.08%	12.48%

Table 2, continued

Panel C: Geographical distribution over select metro areas

Region	Securitization	Equity Analysts	Lawyers
New York	22.20%	43.80%	16.40%
Southern California	9.90%	3.80%	13.90%
Chicago	8.30%	5.80%	4.90%
Boston	5.00%	5.80%	7.30%
Philadelphia	4.30%	0.40%	6.20%
Other most common metro area	Minneapolis (4.1%) Wash., DC (4.1%)	San Fran. (5.4%)	Dallas (4.6%)

Table 3: Purchase and Sale Prices

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		Equity Analysts		Lawyers	
	Price	N	Price	N	Price	N
2000	454.151	21	887.979	13	454.895	29
			[-1.70]*		[1.71]*	
2001	516.879	18	658.760	16	509.773	40
			[-2.08]**		[4.29]***	
2002	433.315	19	798.818	13	570.219	26
			[-0.62]		[2.29]**	
2003	671.224	25	1155.622	18	541.466	32
			[0.33]		[2.35]**	
2004	1267.915	15	788.011	15	518.849	33
			[0.69]		[0.30]	
2005	901.187	29	739.447	17	583.876	13
			[0.42]		[1.12]	
2006	813.607	15	1231.038	22	560.055	18
			[-2.60]**		[-0.92]	
2007	565.206	15	1740.249	9	483.905	15
			[-1.66]		[0.70]	
2008	869.319	8	1108.819	7	319.478	25
			[-3.75]***		[1.07]	
2009	397.079	8	1076.769	8	389.281	26
			[-2.25]**		[0.73]	
2010	566.873	13	418.150	9	513.919	40
			[0.33]		[-0.99]	

Table 3, continued

Panel B: Average Sale Price, 2000-2010

	Securitization		Equity Analysts		Lawyers	
	Price	N	Price	N	Price	N
2000	394.747	8	655.696 [-0.91]	9	382.725 [0.13]	19
2001	458.843	7	693.165 [-2.44]**	8	531.820 [1.56]	23
2002	387.453	12	1011.494 [-2.68]**	7	484.586 [1.00]	16
2003	393.704	8	1090.653 [-1.37]	5	500.265 [0.65]	10
2004	761.593	13	991.719 [-0.86]	10	514.391 [0.48]	16
2005	603.811	10	358.900 [0.98]	5	250.273 [2.06]*	4
2006	676.281	8	900.533 [-1.06]	7	480.089 [1.37]	12
2007	597.287	11	699.235 [-1.12]	8	450.262 [3.17]***	10
2008	545.067	13	707.869 [0.23]	4	314.367 [1.03]	15
2009	786.248	8	987.976 [0.22]	5	337.784 [0.26]	17
2010	1093.238	4	409.934 [-0.11]	13	425.080 [0.41]	18

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		Equity Analysts		Lawyers	
	Count	Fraction	Count	Fraction	Count	Fraction
Buy a First Home	95	45.02%	80	47.90%	167	43.49%
Buy a Second Home	49	23.22%	46	27.54%	122	31.77%
Swap Up Purchases	45	21.33%	28	16.77%	65	16.93%
Swap Down Purchases	9	4.27%	4	2.40%	8	2.08%
Swap Purchase- Missing Price	13	6.16%	9	5.39%	22	5.73%

Panel B: Sale Transactions, 2000-2010

	Securitization		Equity Analysts		Lawyers	
	Count	Fraction	Count	Fraction	Count	Fraction
Divest Last Home	29	13.74%	23	13.77%	60	15.63%
Divest Second Home	20	9.48%	25	14.97%	75	19.53%
Swap Up Sale	45	21.33%	28	16.77%	64	16.67%
Swap Down Sale	9	4.27%	4	2.40%	8	2.08%
Swap Sell- Missing Price	14	6.64%	9	5.39%	22	5.73%

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.

	Securitization				Equity Analysts				Lawyers			
Year	Adjusted HO	Adjusted Non-HO	Adjusted Multi- HO	Fraction Adj. HO	Adjusted HO	Adjusted Non-HO	Adjusted Multi- HO	Fraction Adj. HO	Adjusted HO	Adjusted Non-HO	Adjusted Multi- HO	Fraction Adj. HO
2000	100	112	13	0.508	70	105	13	0.438	225	226	53	0.529
2001	110	100	12	0.558	72	95	21	0.450	241	207	54	0.567
2002	118	88	16	0.599	79	90	20	0.494	264	191	62	0.621
2003	128	82	20	0.650	88	83	23	0.550	276	169	61	0.649
2004	133	73	26	0.675	94	75	27	0.588	288	158	67	0.678
2005	142	67	27	0.721	102	68	31	0.638	294	149	68	0.692
2006	150	55	27	0.761	112	59	31	0.700	300	141	76	0.706
2007	155	49	32	0.787	115	49	33	0.719	311	129	83	0.732
2008	152	49	32	0.772	117	49	32	0.731	308	121	81	0.725
2009	148	53	29	0.751	118	49	29	0.738	311	124	88	0.732
2010	152	54	31	0.772	119	46	31	0.744	314	120	82	0.739
Distinct people	162	125	80	0.822	129	113	62	0.806	347	266	165	0.816
With age	153	108	76	0.777	126	107	61	0.788	344	264	163	0.809

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.

	Divestitures per person			Regression-Adjusted Difference	
				Securitization minus:	
		Equity Analysts	Lawyers	Equity Analysts	Lawyers
Year	Securitization				
2000	0.040	0.057 [-0.52]	0.049 [-0.33]	-0.0122 [-0.35]	0.000866 [0.034]
2001	0.018	0.056 [-1.38]	0.054 [-1.44]	-0.0268 [-0.89]	-0.0279 [-1.39]
2002	0.051	0.038 [0.42]	0.038 [0.58]	0.0208 [0.67]	0.0228 [0.96]
2003	0.016	0.057 [-1.68]*	0.058 [-1.43]	-0.0361 [-1.29]	-0.0367 [-1.64]
2004	0.030	0.053 [-0.88]	0.045 [-0.65]	-0.0170 [-0.60]	-0.0105 [-0.49]
2005	0.007	0.039 [-1.75]*	0.044 [-2.07]**	-0.0163 [-0.87]	-0.0342 [-2.40]**
2006	0.020	0.027 [-0.36]	0.027 [-0.43]	-0.00194 [-0.098]	-0.000409 [-0.027]
2007	0.052	0.035 [0.66]	0.051 [0.01]	0.0153 [0.60]	-0.000700 [-0.030]
2008	0.059	0.068 [-0.31]	0.039 [0.69]	-0.0134 [-0.42]	0.0180 [0.66]
2009	0.047	0.034 [0.54]	0.032 [0.80]	0.0251 [1.01]	0.0220 [1.01]
2010	0.020	0.034 [-0.71]	0.041 [-1.20]	-0.0113 [-0.51]	-0.0208 [-1.17]
Multi-homeowner?				0.0542 [4.22]***	0.0762 [6.17]***
Age Indicators?				Y	Y
N				2441	4421
R-Squared				0.022	0.026
People				279	497

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	Equity Analysts	Lawyers	Securitization minus:	
				Equity Analysts	Lawyers
2000	0.134	0.105 [0.66]	0.111 [0.59]	0.0306 [0.62]	0.0375 [0.86]
2001	0.120	0.063 [1.37]	0.101 [0.49]	0.0725 [1.57]	0.0350 [0.80]
2002	0.102	0.089 [0.30]	0.147 [-1.01]	0.00590 [0.12]	-0.0418 [-0.93]
2003	0.159	0.133 [0.47]	0.101 [1.28]	0.0350 [0.57]	0.0752 [1.42]
2004	0.096	0.120 [-0.47]	0.127 [-0.67]	-0.0117 [-0.21]	-0.0291 [-0.61]
2005	0.179	0.132 [0.75]	0.081 [2.15]**	0.0877 [1.21]	0.140 [2.25]**
2006	0.145	0.186 [-0.58]	0.099 [0.92]	-0.0252 [-0.31]	0.0652 [1.03]
2007	0.102	0.061 [0.73]	0.101 [0.02]	0.0257 [0.41]	-0.0143 [-0.26]
2008	0.061	0.082 [-0.39]	0.025 [1.16]	0.00857 [0.13]	0.0481 [1.02]
2009	0.038	0.082 [-0.94]	0.065 [-0.64]	-0.0244 [-0.43]	-0.0245 [-0.55]
2010	0.167	0.087 [1.18]	0.050 [2.57]**	0.138 [1.72]*	0.157 [2.33]**
Age Indicators?				Y	Y
N				1339	2322
R-Squared				0.030	0.031
People				215	372

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	Equity Analysts	Lawyers	Securitization minus:	
				Equity Analysts	Lawyers
2000	0.060	0.086 [-0.59]	0.076 [-0.48]	-0.00222 [-0.056]	0.0145 [0.58]
2001	0.064	0.139 [-1.52]	0.037 [1.03]	-0.0153 [-0.32]	0.0588 [2.62]***
2002	0.093	0.063 [0.75]	0.076 [0.58]	0.0737 [2.20]**	0.0456 [1.79]*
2003	0.086	0.080 [0.17]	0.072 [0.46]	0.0426 [1.25]	0.0287 [1.16]
2004	0.060	0.085 [-0.72]	0.080 [-0.67]	0.0114 [0.35]	-0.00880 [-0.36]
2005	0.127	0.069 [1.36]	0.061 [2.15]**	0.106 [2.97]***	0.0812 [2.68]***
2006	0.060	0.098 [-1.15]	0.090 [-1.07]	0.00527 [0.17]	-0.0115 [-0.49]
2007	0.065	0.061 [0.11]	0.064 [0.01]	0.0405 [1.39]	0.0172 [0.60]
2008	0.026	0.026 [0.03]	0.039 [-0.65]	0.0223 [0.97]	-0.00233 [-0.12]
2009	0.034	0.034 [-0.01]	0.039 [-0.24]	0.0214 [0.90]	0.0222 [1.13]
2010	0.033	0.050 [-0.72]	0.029 [0.25]	0.00268 [0.11]	0.0129 [0.73]
Multi-homeowner?				0.312 [15.4]***	0.281 [16.6]***
Age Indicators?				Y	Y
N				2441	4421
R-Squared				0.257	0.210
People				279	497

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))		
Year	Securitization divided by:		Year	Securitization divided by:	
	Equity Analysts	Lawyers		Equity Analysts	Lawyers
2000-2001	0.638 [-0.87]	0.703 [-0.80]	2000-2001	1.562 [1.45]	1.328 [1.22]
2002-2003	0.833 [-0.38]	0.824 [-0.47]	2002-2003	1.160 [0.50]	1.103 [0.39]
2004-2005	0.538 [-1.13]	0.449 [-1.66]*	2004-2005	1.272 [0.78]	1.465 [1.41]
2006-2007	1.176 [0.33]	0.970 [-0.077]	2006-2007	0.966 [-0.086]	1.267 [0.66]
2008-2009	1.112 [0.26]	1.588 [1.15]	2008-2009	0.812 [-0.38]	1.283 [0.46]
2010	0.663 [-0.53]	0.516 [-1.04]	2010	2.528 [1.66]*	3.736 [2.69]***
Multi-homeowner indicator?	Y	Y	Age Indicators?	Y	Y
Age Indicators?	Y	Y		N	
N	2441	4421	Pseudo R-Squared	0.022	0.037
Pseudo R-Squared	0.048	0.071	People	215	372
People	279	497			

Table 9, continued

Panel C: Second home purchases and swap-up purchases

Annual incidence ratio ($\exp(\beta)$)

Year	Securitization divided by:	
	Equity Analysts	Lawyers
2000-2001	1.164 [0.57]	2.085 [2.84]***
2002-2003	2.353 [3.03]***	1.670 [2.71]***
2004-2005	2.006 [2.61]***	1.437 [1.70]*
2006-2007	1.189 [0.59]	0.997 [-0.012]
2008-2009	1.214 [0.38]	0.977 [-0.058]
2010	0.807 [-0.38]	1.222 [0.36]
Multi-homeowner?	Y	Y
Age Indicators?	Y	Y
N	2441	4421
R-Squared	0.430	0.414
People	279	497

Table 10: 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, while Panel C reports differences in performance. In Panel C, 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	Equity Analysts	Lawyers
Number of properties: 2003q4	0.721 (0.637)	0.694 (0.793)	0.779 (0.729)
Value of properties: 2003q4	429.0 (521.6)	556.5 (997.8)	349.7 (398.1)
Cash account: 2003q4	638.1 (596.6)	971.0 (897.9)	420.5 (421.1)
Portfolio value: 2003q4	1067.1 (684.8)	1527.6 (1172.6)	770.2 (510.7)
Number of properties: 2010q4	0.939 (0.690)	0.944 (0.754)	0.951 (0.798)
Value of properties: 2010q4	656.9 (637.3)	894.0 (1029.1)	471.2 (505.7)
Cash account: 2010q4	494.4 (513.8)	797.3 (917.5)	368.1 (377.0)
Portfolio value: 2010q4	1151.3 (719.5)	1691.3 (1292.8)	839.3 (551.6)
Number of people	197	160	425

Panel B: Performance, 2003q4-2010q4

	Securitization	Equity Analysts	Lawyers
Return	0.0869 (0.123)	0.100 (0.108)	0.0939 (0.123)
Buy-and-hold return	0.123 (0.0759)	0.137 (0.0708)	0.121 (0.0796)
Performance index	-0.0358 (0.121)	-0.0370 (0.100)	-0.0272 (0.115)
Risk-free return	0.167 (0)	0.167 (0)	0.167 (0)

Table 10, continued

Panel C: Performance Differences

	Securitization minus:	
	Equity Analysts	Lawyers
Return	-0.0132 [-1.08]	-0.00693 [-0.66]
Buy-and-hold return	-0.0144* [-1.85]	0.00172 [0.26]
Performance index	0.00115 [0.099]	-0.00866 [-0.84]
N	357	622

Table 11: Income Shocks

Panel A presents summary statistics about our income matching process. Panel B presents average income in three three-year periods for each group. We first average income across all purchases observed each person-period to obtain an average person-level income for the period before averaging across people to obtain an average income for the period. All income numbers are reported in December 2006 dollars adjusted using the CPI All Items series. Rows A-B test whether average the boom minus pre-boom difference in average purchaser income was positive by projecting person-level income onto an indicator for the boom period in a two-period panel of person-level income. Rows C-D test whether the difference in difference is significant across groups. Standard errors are clustered at the person level. */**/** denotes significant at the 10%, 5%, and 1% levels, respectively.

Panel A: Summary Statistics

	Securitization	Equity Analysts	Lawyers
Purchases, 2000-2010	211	167	384
with mortgage info	162	109	253
mean, median LTV	0.72 / 0.80	0.72 / 0.78	0.77 / 0.80
with income from HMDA match	125	89	185
income at purchase, property average (\$K)	289.2	319.6	193.3
median purchase age	37	34	36
People purchasing, 2000-2010	138	116	283
with income from HMDA match	100	68	146
income at purchase, person average (\$K)	271.6	341.9	184.3
Average # HMDA mortgage applications per match	2.85	1.93	2.95
Median # HMDA mortgage applications per match	1	1	1

Table 11, continued

Panel B: Pre-Boom and Boom Income

		Securitization	Equity Analysts	Lawyers
Pre-Boom period (2001-2003)	Mean	198.3	243.1	162.3
	Median	147.0	183.4	139.6
	SD	177.4	173.1	88.2
	People	34	28	47
Boom period (2004-2006)	Mean	340.5	389.3	196.3
	Median	223.6	246.8	147.3
	SD	471.0	476.0	150.1
	People	46	32	62
Bust period (2007-2009)	Mean	236.1	364.4	213.9
	Median	168.2	253.2	157.8
	SD	154.2	287.3	164.7
	People	18	15	42
A) Boom-PreBoom	Point Est.	142.1	146.1	33.93
	t-stat	[1.90]*	[1.67]	[1.63]
	N	80	60	109
	R2	0.035	0.039	0.017
B) Boom-PreBoom, Logs	Point Est.	0.468	0.378	0.115
	t-stat	[3.25]***	[1.75]*	[1.12]
	N	80	60	109
	R2	0.107	0.047	0.009
C) DID Sctzn. minus Control	Point Est.		-4.004	108.2
	t-stat		[-0.035]	[1.40]
	N		140	189
	R2		0.039	0.064
D) DID, Logs Sctzn. minus Control	Point Est.		0.0904	0.353
	t-stat		[0.35]	[2.00]**
	N		140	189
	R2		0.078	0.105

Table 12: Value-to-Income

This table presents average VTI in three three-year periods for each group. We first average VTI across all purchases observed each person-period to obtain an average person-level VTI for the period before averaging across people to obtain an average VTI for the period. Rows A-B test whether average the boom minus pre-boom difference in average purchaser income was positive by projecting person-level income onto an indicator for the boom period in a two-period panel of person-level income. Rows C-D test whether the difference in difference is significant across groups. Standard errors are clustered at the person level. */**/** denotes significant at the 10%, 5%, and 1% levels, respectively.

		Securitization	Equity Analysts	Lawyers
Pre-Boom period (2001-2003)	Mean	3.1	3.3	2.8
	Median	3.2	3.3	2.6
	SD	1.2	1.6	1.3
	People	22	25	30
Boom period (2004-2006)	Mean	3.9	3.4	2.8
	Median	3.1	3.7	2.6
	SD	2.6	1.6	1.2
	People	39	26	40
Bust period (2007-2009)	Mean	3.4	3.7	3.1
	Median	3.3	3.5	2.9
	SD	1.2	1.4	1.5
	People	16	14	34
A) Boom-PreBoom	Point Est.	0.868	0.135	-0.0442
	t-stat	[1.73]*	[0.31]	[-0.15]
	N	61	51	70
	R2	0.035	0.002	0.000
B) Boom-PreBoom, Logs	Point Est.	0.187	0.0323	-0.00880
	t-stat	[1.27]	[0.16]	[-0.080]
	N	61	51	70
	R2	0.026	0.001	0.000
C) DID Sctzn. minus Control	Point Est.		0.733	0.912
	t-stat		[1.10]	[1.57]
	N		112	131
	R2		0.030	0.073
D) DID, Logs Sctzn. minus Control	Point Est.		0.154	0.195
	t-stat		[0.63]	[1.07]
	N		112	131
	R2		0.017	0.050

Table 13: Boom and Bust Income

This table tests whether incomes reported in Table 11 are different across the boom and bust periods. Rows A-B test whether average the bust minus boom difference in average purchaser income was positive by projecting person-level income onto an indicator for the boom period in a two-period panel of person-level income. Rows C-D test whether the difference in difference is significant across groups. Standard errors are clustered at the person level. ***/*** denotes significant at the 10%, 5%, and 1% levels, respectively.

		Securitization	Equity Analysts	Lawyers
A) Bust-Boom Diff.	Point Est.	-104.3	-24.82	17.64
	t-stat	[-1.33]	[-0.23]	[0.62]
	N	64	47	104
	R2	0.013	0.001	0.003
B) Bust-Boom Diff., Logs	Point Est.	-0.206	0.0639	0.0819
	t-stat	[-1.26]	[0.29]	[0.68]
	N	64	47	104
	R2	0.021	0.002	0.004
C) DID Sctzn. minus Control	Point Est.		-79.52	-122.0
	t-stat		[-0.59]	[-1.47]
	N		111	168
	R2		0.015	0.045
D) DID, Logs Sctzn. minus Control	Point Est.		-0.270	-0.288
	t-stat		[-0.98]	[-1.42]
	N		111	168
	R2		0.023	0.077

Table 14: 2004-2006 Purchasers

This table provides details on the differences between groups in the sales of properties purchased in the years 2004 through 2006. Panel A provides the number of properties purchased by each group in 2004-2006 and the number of people that purchased properties in 2004-2006. Panel B provides a year-by-year breakdown of what percentage of the properties purchased in 2004-2006 by each group were sold and what percentage are remaining. Panel C gives a yearly mean divestiture intensity per person in each group. This divestiture intensity is calculated over all properties owned by people who purchased properties in 2004-2006. Panel D shows the results of various t-tests performed on the sales transactions done by each group during the crisis period. For non-securitization groups, t-statistics associated with a t-test of the null hypothesis that the Sectn. minus other group purchase price equals zero are reported in brackets. */**/** represent significant at the 10%, 5% and 1% level, respectively.

Panel A: Sample sizes

Sample	Securitization	Equity Analysts	Lawyers
Number of properties purchased in 2004-2006	67	61	122
Number of people who purchased in 2004-2006	60	58	104

Panel B: Percent of properties purchased in 2004-2006 sold, by year

Year	Securitization		Equity Analysts		Lawyers	
	% sold	% remaining	% sold	% remaining	% sold	% remaining
2007	5.97%	92.54%	1.64%	95.08%	5.74%	88.52%
2008	8.96%	83.58%	3.28%	91.80%	1.64%	86.89%
2009	2.99%	80.60%	1.64%	90.16%	6.56%	80.33%
2010	4.48%	76.12%	1.64%	88.52%	4.92%	75.41%

Panel C: Divestiture intensities per 2004-2006 purchaser, by year

Year	Securitization	Equity Analysts	Lawyers
2007	0.0833	0.0172	0.0769
2008	0.0833	0.0517	0.0577
2009	0.0333	0.0172	0.0769
2010	0.0167	0.0172	0.0673

Panel D: Sales during the bust

Statistic	Securitization	Equity Analysts	Lawyers
Sales of 2004-2006 properties per purchaser	0.2000	0.0690 [2.10]**	0.1635 [0.56]
Net sales of all properties per purchaser	0.1500	0.0000 [1.89]*	0.0865 [0.79]

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 report 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 for homeowners, non-homeowners, and multiple homeowners in Table

A2 and the people eligible for each type of transaction 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

Table A4: Number of Homeowners and Non-Homeowners with Age

We tabulate the number of homeowners (HO), adjusted homeowners, adjusted non-homeowners and adjusted multiple-homeowners with age information 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				Equity Analysts				Lawyers			
	Adjusted HO	Adjusted Non- HO	Adjusted Multi- HO	Fraction Adj. HO	Adjusted HO	Adjusted Non- HO	Adjusted Multi- HO	Fraction Adj. HO	Adjusted HO	Adjusted Non- HO	Adjusted Multi- HO	Fraction Adj. HO
2000	97	96	12	0.492	69	99	13	0.431	222	224	53	0.522
2001	107	84	12	0.543	70	89	21	0.438	238	205	53	0.560
2002	114	72	15	0.579	76	85	19	0.475	261	188	62	0.614
2003	123	67	20	0.624	85	78	22	0.531	272	166	61	0.640
2004	125	59	24	0.635	90	70	26	0.563	284	155	67	0.668
2005	134	53	26	0.680	98	64	30	0.613	287	146	68	0.675
2006	140	41	26	0.711	107	55	30	0.669	291	137	75	0.685
2007	143	36	31	0.726	110	44	31	0.688	299	124	82	0.704
2008	141	37	31	0.716	112	44	30	0.700	289	117	76	0.680
2009	137	40	27	0.695	113	44	27	0.706	290	119	82	0.682
2010	137	41	28	0.695	113	41	30	0.706	290	115	75	0.682
Distinct people	153	108	76	0.777	126	107	61	0.788	344	264	163	0.809

Figure A1: Lexis-Nexis Development Professional Person Search Interface

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First Name

Middle Name/Initial

Link ID

SSN

Date of Birth

Age Range To

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Figure A2: Person Report

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

Name Variations

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

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No.	SSN	State Iss.	Date Iss.	Warnings
1:	[redacted] XXXX	[redacted]	[redacted]	

Most frequent SSN attributed to subject:

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Reported DOBs:

[redacted]

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

Figure A3: Property Report

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[Neighbors \(5\)](#)
[Associated Entities \(10\)](#)
[Sources \(100\)](#)

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]

☐ **Neighbors - 5 records found**

Neighbor Name	Neighbor Address	Neighbor Phone	Actions
[REDACTED]	[REDACTED]	[REDACTED]	Get Report
[REDACTED]	[REDACTED]	[REDACTED]	Get Report
[REDACTED]	[REDACTED]	[REDACTED]	Get Report
[REDACTED]	[REDACTED]	[REDACTED]	Get Report
[REDACTED]	[REDACTED]	[REDACTED]	Get Report

☐ **Associated Entities - 10 records found**

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

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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](#))

☐ Select for Delivery

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

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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](#)
[Tax Assessor Records \(24\)](#)

Assessment Record
This data is for informational purposes only.

Estimated Roll Certification Date: 01/01/2011

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]

Assessment Record
This data is for informational purposes only.