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ESSAYS ON FINANCIAL INCENTIVES

DISSERTATION

A dissertation submitted in partial
fulfillment of the requirements for
the degree of Doctor of Philosophy
in the Gatton College of Business
and Economics at the
University of Kentucky

By
Tyson D. Van Alfen
Lexington, KY

Director: Dr. William C. Gerken, Professor of Finance

2019

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ABSTRACT OF DISSERTATION

ESSAYS ON FINANCIAL INCENTIVES

In my first chapter, I use a novel dataset of customer reviews from Amazon.com to study the impact of managerial myopia on product market reputation. Using exogenous variation due to the timing of CEO equity vesting events, I show that short-term incentive shocks predict declines in reputation. A changing product market lineup and a deterioration of existing products are two mechanisms through which reputation is affected. The effect is larger when the CEO has other short-term concerns and when the firm has a low reputation in the product market. However, higher advertising expenses mitigate the negative reputational effect among consumers. Using an alternative empirical methodology, I find that higher short-term ownership in the firm is also associated with declining product market reputation, while higher long-term ownership is associated with increasing reputation. My second chapter uses a different setting to examine the consequences of personal wealth incentives. We test whether household wealth shocks affect professional misconduct by financial advisors. We use a panel of advisors' home addresses and examine within-advisor variation relative to other advisors who work at the same firm and live in the same ZIP code. We show that advisors increase misconduct following declines in their homes' values. The increased misconduct is due, in part, to willful actions, such as churning. We show that advisors' housing returns explain misconduct targeting out-of-state customers, breaking the link between customer and advisor housing shocks. Further, the results are stronger for advisors with lower career risk from committing misconduct.

KEYWORDS: Financial incentives, Product markets, Misconduct, Reputation, Brokers and advisors, Real estate

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Date: April 29, 2019

ESSAYS ON FINANCIAL INCENTIVES

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Chapter 1 Managerial Myopia and Product Market Reputation: Evidence from Amazon.com Reviews

1.1 Introduction

The relevance of intangible assets has been steadily increasing in recent decades. In 2016, U.S. companies had over \$8 trillion of intangible assets, about half of the combined market capitalization of the S&P 500 at the time.¹ While the use of intangible assets has been increasing, much of the current research on firm investment still focuses on investment in physical assets.

Executives have long understood the value of intangible assets such as intellectual property, training, client relationships, and reputation. John Stuart, the CEO of Quaker Oats in the early 20th century, once said “If this business were split up, I would be glad to take the brands, trademarks and goodwill and you could have all the bricks and mortar—and I would fare better than you.” Empirical research on intangible assets, however, has historically been constrained due to the difficulty in valuing many of these assets and the relative lack of data, issues which have become easier to address with modern data. Using a novel dataset of crowd-sourced customer reviews from Amazon.com, I explore an important intangible asset, product market reputation, and show how it is impacted by managerial incentives.

Product market reputation is an amalgam of consumer opinions. It encompasses factors such as quality, customer service, and corporate social responsibility, and it plays an important role in the decision making process of consumers. For example, customers are willing to pay higher prices and are more loyal to the firm when the firm has a high reputation.² Technology consumers who have a preference for the Apple ecosystem are typically aware they pay an “Apple premium,” but perceived intangible factors such as reputation are usually cited as the justification for paying this premium. On the other hand, a low reputation harms the firm. After admitting to using illegal software to cheat on emissions testing, Volkswagen experienced a consumer exodus, and sales have still not fully recovered. *Ceteris paribus*, a positive reputation is a valuable asset for firms.

Given the growth in the importance of intangible assets, an understanding of their determinants leads to a better understanding of firm behavior. In much the same way executives decide to invest in physical capital, they may also decide to invest in intangible capital such as reputation. However, the characteristics of product market reputation are notably different than physical capital, so it may be the case that investment in reputation is handled differently by executives. For instance, reputation is harder to value and firms cannot resell it in the same way they can resell physical capital. It is also more difficult for firms to use intangible capital as collateral in lending arrangements.

¹<https://www.wsj.com/articles/accountings-21st-century-challenge-how-to-value-intangible-assets-1458605126>

²See, e.g., Larkin (2013) and Melnik and Alm (2002)

In this paper I study the impact of CEO short-term incentives on product market reputation. Specifically, using multiple measures of short-termism, or myopia, I find that the product market reputation of a firm is negatively impacted in the years following CEO short-term incentive shocks. In my primary tests I use the plausibly exogenous vesting of stock and options grants to the CEO as shocks to the incentive structure. These vesting schedules are typically set up years in advance and are therefore unlikely to be related to contemporaneous product market conditions. Yet these vesting events create distortions in the incentive structure faced by management since they are usually accompanied by large amounts of insider selling (Edmans, Fang, and Lewellen, 2017). Executives even admit to a surprising degree that they are willing to sacrifice positive NPV projects when they have concerns about the near-term stock price (Graham, Harvey, and Rajgopal, 2005). I find that when stock vests to the CEO, the product market reputation, as measured by the average number of stars on Amazon.com, declines by approximately 10% in the subsequent year.

A variety of mechanisms exist through which management can affect reputation in the product market. They may change product quality directly by adjusting the inputs used in the manufacturing process (e.g., substituting a metal component for a plastic one). Relatedly, they might also change the customer experience by increasing or decreasing the amount of support they provide for their products following purchases (e.g., outsourcing a call center). In additional tests, I incorporate product fixed effects to show that the myopic incentive shocks cause within-product declines in reputation, consistent with a cost cutting mechanism.

Another potential mechanism through which reputation might be affected relates to the product market composition. Firms choose when to release new products and when to retire old products, and the release of a new product to the market is a potentially risky event. If the product is well-received it obviously bodes well for the company, but a poorly received product can result in lower sales and lower reputation. A CEO that faces short-term stock price incentives could become more risk averse and delay the release of new products, reducing the rate of product releases in the event year. Indeed I find that in years in which the CEO has a vesting event the firm is more likely to reduce the rate at which they introduce new products, causing a reduction in the number of total products offered by the firm.

While firm reputation in the product market has been explored theoretically for decades, only recently has the data allowed empirical investigation. Fortunately, the rise in online shopping, and more specifically the availability of crowd-sourced customer reviews, now makes empirical measurement of widespread customer perceptions more feasible. I create the measure of product market reputation using a large sample of user-submitted product reviews on Amazon.com, a large online retailer. In addition to information on product identifiers, the review submission date, the customer rating of the product (out of five stars), and the text of the review, I also collect the brand and firm information from each product's Amazon page. This allows me to match products to companies. This process yields information on approximately 400,000 products,³ for which approximately 4.3 million reviews were written.

³A product is defined as a unique Amazon Standard Identification Number (ASIN).

The Amazon.com data is particularly well-suited for this setting since products remain on the site even after they are discontinued, thus avoiding survivorship bias. I am also able to observe reviews that were designated as *Helpful* by the other customers. I limit my analysis to these *Helpful* reviews in robustness tests, and my results hold: a short-term incentive shock causes product market reputation to decline among the most helpful consumers.

As an alternative methodology, I use the firm's ownership composition as a proxy for the degree of short-termism. Previous work has shown that firms with *Transient* (i.e., short-term) owners are on average more preoccupied with near-term results, while firms with *Dedicated* (long-term) owners are not (Bushee, 2001). These supplementary results are consistent with the primary findings: firms with CEOs who have immediate concerns about the stock price experience a decline in product market reputation.

I then investigate whether there is a differential effect across existing incentive structures. CEOs that are closer to retirement may have less of an incentive to care about long-run firm value, which in turn could mean that they are more likely to be affected by these incentive shocks. A similar effect may exist for highly levered firms. Matsa (2011) shows that CEOs of highly levered supermarket firms, with near-term cash flow concerns for debt service, reduce quality to conserve cash. Consistent with these hypotheses, I find that the negative reputational shock in the product market is largest for firms with CEOs nearing retirement and firms with relatively higher leverage.

The reputational effect may also vary across other reputational factors. If investment in reputation faces diminishing marginal returns, then the shock to short-term incentives should have more severe consequences for firms who already have a low reputation. This effect would be consistent with the findings of Larkin (2013) if reputation begets loyalty. She shows that higher brand loyalty is associated with a more inelastic demand curve on the part of the consumers, which in turn allows the firm to support more leverage and maintain less cash since the cost of financial distress is lower due to lower cash flow volatility. However, if reputation, unlike physical capital, is a fragile asset, then short-term decisions could result in more severe reputational consequences for firms with high reputation.

My results are consistent with the first hypothesis. Firms with low reputation are more likely to experience further reductions in product market reputation following vesting events. This hypothesis is also supported by subsequent results that show the effect is mitigated when firms spend more on advertising. Higher advertising expenses could support the inelastic demand discussed by Larkin (2013) and safeguard the firm against these reputational consequences. The results also lend support to the notion of diminishing marginal returns to investment in intangible capital.

I contribute to the literature in a number of ways. First, I build and use a novel dataset that allows me to measure product market reputation, an important intangible asset. Prior work has examined consequences of reputation. Using a sample of corporate donations, Lev, Petrovits, and Radhakrishnan (2010) find that when firms are seen as philanthropic it positively predicts sales growth, particularly for firms whose primary customers are individuals and not other firms. Their results

are consistent with the findings of [Melnik and Alm \(2002\)](#) who show that vendors with a higher reputation are able to sell an identical product for a higher price. [Gerken, Starks, and Yates \(2018\)](#) show that mutual fund customers are more likely to purchase a fund from the same family when they have had a positive experience with the family in the past, consistent with the benefits to positive reputation.⁴ While this strand of research focuses on the consequences of reputation, I instead contribute to the literature by helping to explain how reputation is affected and how it can be managed.

I also contribute to the literature on executive compensation. It is important to understand how compensation contracts interact with reputation in the product market if we are to have a better understanding of the compensation arrangements. When the incentives of the manager are not aligned with the shareholders, the manager may make decisions for short-term personal gain at the expense of long-term shareholder value (see [Jensen and Meckling, 1976](#); [Narayanan, 1985](#)). While research has documented the impact of myopia on physical investment ([Edmans, Fang, and Lewellen, 2017](#); [Moore, 2018](#)), mergers and acquisitions ([Gaspar, Massa, and Matos, 2005](#)), and earnings forecasts ([Ajinkya, Bhojraj, and Sengupta, 2005](#)), I am the first to my knowledge to document how CEO incentive structures relate to reputation among consumers.

Reviews from customers offer unique and informative data about the firm that the market cannot find in financial statements, news articles, or management guidance. Though each individual review may not convey anything material, in aggregate, research has demonstrated the value of these crowdsourced measures. [Giles \(2005\)](#) examines the error rate in Wikipedia entries, a crowd-sourced knowledge base, and compares it to the error rate in the Encyclopedia Britannica. Despite the potential for contribution by uneducated sources, he finds the Wikipedia error rate to be almost identical to the error rate of Encyclopedia Britannica. A similar relation has been shown with earnings forecasts. [Jame, Johnston, Markov, and Wolfe \(2016\)](#) find that crowdsourced earnings estimates provide incremental value and can be used in tandem with other more traditional information to better forecast a company's earnings. This provides additional support to the hypothesis that customer reviews provide a unique and important insight into the operation of the company.

In one of the first papers to use Amazon.com reviews, [McAuley and Leskovec \(2013\)](#) use the reviews data to improve recommender system algorithms. [Huang \(2018\)](#) instead connects the data to stock returns and finds that abnormal changes to reviews predict stock returns, earnings surprises, and revenue surprises. While [Huang \(2018\)](#) highlights another outcome of this measure, I contribute by studying factors that affect reputation among consumers.

I also contribute to the growing discussion in the literature on product markets. [Sheen \(2014\)](#) uses data on product quality from Consumer Reports to show that, following a merger, the quality of products between the two companies converges

⁴Related work examines other types of intangible assets. For example, [Clifford and Gerken \(2018\)](#) find that the advisor-client relationship in the market for financial advice is an important intangible asset that significantly impacts advisor behavior when ownership of the asset is transmitted from employer to employee.

and the price falls relative to competitors. Cavallo (2018) examines the growth of the online product market and its macroeconomic implications. I instead study firm-level changes to better understand the microeconomic implications.

Lastly, I contribute to the literature that explores the consequences of varying ownership structures. Bushee (1998) finds that firms with high *Dedicated* institutional ownership (owners with a long time horizon and low portfolio diversity) are more focused on the long-term and are less likely to cut R&D in order to reverse an earnings decline. The opposite is true for *Transient* ownership (owners with a short time horizon and high portfolio diversity). Similarly, long-term institutional ownership has been shown to predict firm profitability and stock market performance (Cella, 2009; Clifford, 2008). Consistent with this literature, I find the investment horizon of institutional owners predicts future changes in product market reputation.

1.2 Data

1.2.1 Amazon Data

My measure of product market reputation comes from customer reviews posted on Amazon.com, one of the largest retailers in the world.⁵ Individual shoppers who use the Amazon site are able to leave feedback about their experiences with products. This feedback usually consists of a small amount of text and a rating, or number of stars out of five they assign the product. Specifically, each observation in my data contains the Amazon Standard Identification Number (ASIN) which uniquely identifies the product, a user identifier, the text of the written review, the number of stars (out of five) that the user assigned to the product, the date and time the review was published, and *Helpful* votes. When a person publishes a review on the platform other users are then able to upvote the review or downvote the review.⁶

I use reviews from 20 of the Amazon product categories: Arts, Automotive, Baby, Beauty, Cell Phones and Accessories, Clothing and Accessories, Electronics, Gourmet Foods, Health, Home and Kitchen, Industrial and Scientific, Musical Instruments, Office Products, Patio, Pet Supplies, Shoes, Sports and Outdoors, Tools and Home Improvement, Toys and Games, and Watches. I specifically avoid using reviews from the Music, Movies, and Video categories because of the difficult nature of disentangling the reputation of the artist from the reputation of the company selling the product.

Since not all reviews convey new or especially valuable information, many of my tests limit the measure of reputation to only *Helpful* reviews. I use *Helpful* reviews in much of my analysis because of the noise that can arise from other reviews. Shoppers at Amazon.com will occasionally notice reviews that are either unhelpful, uninformed, fake, or simply posted in the wrong place.⁷ Illegitimate reviews are a minor concern

⁵The product reviews are similar to those used in McAuley and Leskovec (2013) and Huang (2018).

⁶Since the time of data collection Amazon has changed this process to only allow upvotes.

⁷There have been companies that have emerged as posters of fake reviews for sellers (e.g., www.buyamazonreviews.com). The sellers that purchase these reviews though are smaller and

considering Amazon’s aggressive legal pursuit of those who write fake reviews.⁸ Even though this noise affects only a small subsample of the reviews, I use *Helpful* ratings as a robustness.

Chen, Dhanasobhon, and Smith (2008) study reviews on Amazon.com and find that helpful reviews influence the purchasing decisions of consumers more than other reviews. Using helpful reviews then provides me with a more precise measure, though the cost of using the measure is a reduction of power in the tests.

For each ASIN, I scrape the Amazon site using a Python script for the name of either the firm that sells the product or the brand that is associated with the product. The initial dataset consists of approximately 4.3 million reviews written on about 400,000 products from 1999 to 2012.⁹ Though the data spans more than a decade, most of the observations are concentrated in the second half of the time series, representing the rise in popularity of the site.

I am unable to collect company information for about 3.5% of the products because they do not have a functioning web page, and only 1.8% of the products do not have company or brand information listed on the page. Though a small portion of the products do not have a functioning web page, survivorship bias is a minor concern since the vast majority of discontinued products remain on the Amazon site and can be referenced at any time.

An example observation is given in Appendix A. At the top of the product description, just below the product name, there is a byline with the name of the producer/brand. In this example observation the product is the iPod Classic, and Apple is listed as the maker of the product. At the time, over 3,000 reviews had been written for this iPod, with an average review of about 4.5 stars over the life of the product. The average rating displayed on the Amazon Website is a proprietary weighted average of all reviews of the product, with weight given to more recent and more helpful reviews. In order to test within-firm variation in product market reputation, I build an annual measure of reputation using only the reviews written in the year.

Below the product description is an example review of the product. Alex (the reviewer) gives the product five stars because of the impressive storage space and the reliability relative to other products. Above Alex’s star rating there is also a line that says 1,651 people out of 1,696 voted the review as helpful.

For many products the name of the firm is not listed on the byline. Instead it contains either a subsidiary or a brand (e.g., Gillette razors are manufactured and sold by the company Proctor & Gamble). For these observations I use a combination of LexisNexis and Google searches to identify the parent company. I do this for a majority of the reviews with brand and subsidiary information instead of firm information.

usually private companies. Since my analysis focuses on larger public companies this does not pose an issue.

⁸<http://www.wsj.com/articles/amazon-sues-sellers-for-offering-fake-goods-on-its-site-1479243852>

⁹I drop Amazon.com, the company, from my sample since it has been shown that Amazon manipulates its product market positions in relation to products from competitors on the site (Zhu and Liu, 2018).

Figure 1.1 plots the number of reviews posted in the Amazon.com markets through time. The number of reviews posted on Amazon exploded throughout the decade ending in 2007, before plateauing at about 50,000 reviews each year. The dark blue bars represent the total number of reviews posted each year. The lighter beige bars represent the number of reviews that are designated as *Helpful* each year. In order for a review to be designated as *Helpful* at least 50 percent of the voters must classify it as such. It is important to note here that the decline in helpful reviews after 2007 is mechanical and not related to changing review quality through time. A review written in 2007 had years to collect votes, whereas a review written in 2012 only had months to collect user votes.

I then match the product reviews data with data from the Center for Research on Security Prices (CRSP) and Compustat using company names. Prior to the name match I clean the names by dropping punctuation (e.g., “.”, “-”, “&”) and certain words (e.g., “corporation”, “incorporated”). I only keep observations that match perfectly on these cleaned names.

Table 1.1 presents summary statistics on the Amazon data. Panel A presents information on the products in the sample, and Panel B presents information on the firms in the sample. The initial sample covers approximately 400,000 products. However, many of the products are sold by private companies, and many observations do not survive the perfect name match. I also require that the firm be included in the ISS Incentive Lab data which contains information on executive compensation. The ISS Incentive Lab data covers the S&P 500 as well as a “significant portion of the S&P 400”. After imposing these filters the dataset covers approximately 12,000 products from 90 companies from 1999 to 2012. The mean product has an average rating of 3.95 stars out of five and has about 26 reviews (half of which are *Helpful*). The typical product has a lifetime on the site of five years.¹⁰

In Panel B, the median firm has 94 products, introduces 3.7 products each year, and drops 2.4 products each year. The typical firm also has over 200 reviews written about its products and has an average rating of just under 4 stars. These variables are highly skewed.

The Amazon dataset of reviews was originally scraped in 2013¹¹, so any products that were introduced on Amazon.com and then subsequently removed prior to 2013 would not be in my sample and could potentially bias my results. There are two reasons this concern is significantly lessened. First, I incorporate tests which include product fixed effects, which limit my analysis to within-product reputation responses to myopia.

Second, Amazon tends to leave products on its site even when they are no longer sold. In the data there are many instances of unsuccessful products that were only sold on Amazon for a short period of time. Though they would not be include in my tests, examples can be found of products that received a single review but remain on the Amazon site for years. I do, however, find few instances of product removal. But

¹⁰I use the year of the first review as the year the product was introduced, and I consider a product discontinued when it no longer receives ratings.

¹¹For more information about the scraping process and original use of the data in a recommender systems setting, see McAuley and Leskovec (2013)

even if these cases were widespread, it would bias my estimates downward. If myopia affects reputation in a significant enough way to cause the product to drop out of my sample then my estimates on the treatment effect would be too low.

1.2.2 Equity Vesting Data

My primary measure of changes to the CEO incentive structure is their vesting of stock and options. As part of the compensation plan a CEO will frequently be awarded equity and option grants that vest at regular intervals. Many times the vesting payment will only take place if certain accounting goals have been met. These *performance-based* grants are frequently intricate and elaborate. For the sake of simplicity I omit these types of payments from my analysis. I instead focus on *time-based* grants since these contracts are typically awarded years in advance and are not based on any performance criteria.

Within time-based plans, grants can vest with either a *cliff* schedule or a *ratable* schedule. Ratable schedules pay a portion of the grant on a recurring basis (typically annually) while cliff schedules pay a lump sum on the vesting date. I limit my analysis to the most plausibly exogenous subset of vesting payments: time-based grants with a cliff schedule. I also require that these grants be awarded at least a year in advance of the vesting date. Many of these grants vest 3-5 years after the award date.

Previous work has already used similar measures as a proxy for the CEO's short-term concern for the stock price. CEOs typically sell a large amount of equity in years in which they receive vesting payments. It is this large degree of selling that creates the incentive to focus on the short-term stock price. [Moore \(2018\)](#) finds that firms are more likely to repurchase stock if the CEO has vesting equity in the period, and [Edmans et al. \(2017\)](#) use similar vesting data to show that the rate of investment in R&D and CAPEX is reduced when CEO equity vests.

I create three vesting event variables to measure shocks to short-term incentives. *Any Vesting Event (CEO)* is set to 1 in any year in which the firm's CEO has either a stock vesting event or an option vesting event that is time-based with a cliff vesting schedule. I then decompose this measure into its two components: *Any Stock Vesting (CEO)* and *Any Option Vesting (CEO)*. I make similar variables for vesting events for any of the executives at the firm.¹² Panel A of [Table 1.2](#) reports the percent of firm-years affected by each type of vesting event. In my sample approximately one fourth of firm-years have a CEO vesting event, 17% have a stock vesting event, and 11% have an option vesting event. While some form of vesting is likely occurring every year for large companies, the frequency of these vesting events is much lower since I focus on the events that are the most plausibly exogenous.

1.2.3 Institutional Ownership Classification and Controls

As an alternative measure of short-termism I use institutional ownership as categorized by [Bushee \(2001\)](#). He examines the portfolio diversity as well as the portfolio

¹²In untabulated results I use a measure that includes awards for all executives. The inferences remain the same, though the magnitude of the effect is approximately two thirds the size.

turnover of institutions to identify transient owners (high diversity, high turnover), dedicated owners (low diversity, low turnover), and quasi indexers (high diversity, low turnover). I obtain these classifications from his website and merge them into the Thompson Reuters 13F holdings data to build a firm-year measure of each type of owner. Panel B of Table 1.2 reports summary statistics on the ownership variables. In the final sample, the average firm-year has just over 50% of its shares owned by *Quasi Indexers*, approximately 15% owned by *Transient* institutions, and under 4% owned by *Dedicated* institutions.

From CRSP, I use data on returns, prices, volume, and shares outstanding. I also merge with Compustat to get data on assets, liabilities, advertising expense, cash, sales, R&D, and book value per share. Summary statistics for control variables are also reported in Table 1.2.

Following Sheen (2014), I include $LN(\text{Market Cap})$, *Leverage*, *R&D*, and *Operating Margin* as controls in my analysis. I also use *Book-to-Market* and *12-month Return*. $LN(\text{Market Cap})$ is the natural log of price times shares outstanding. *Leverage* is total liabilities divided by total assets. *R&D* is research and development expenses scaled by the previous year's sales. *Operating Margin* is operating income before depreciation scaled by the previous year's sales. *Book-to-Market* is the book value per share divided by the price per share. *12-month Return* is the return on the company's stock over the prior 12 months.

For the interacted tests I also include *Bad Reputation* which is an indicator equal to 1 if the firm is in the lowest quartile of reputation for the year as measured by Amazon.com reviews, *High Leverage* which is an indicator variable equal to 1 if the firm is above the median leverage ratio for the year, *Old CEO* which is an indicator variable equal to 1 if the CEO is over 50 years of age, and *High Advertising* which is an indicator variable equal to 1 if the firm is in the top quartile of advertising expenses.

1.3 Identification Strategy

In order to test the causal effect of myopia on product market reputation, I use stock and option vesting events. The timing of these vesting events has been shown to be plausibly exogenous to contemporaneous factors since they are usually set up years in advance. I use the subset of events that is most exogenous as a parsimonious indicator variable. Many of the grants awarded to CEOs are based on performance measures, but some are strictly time-based grants that vest according to a predetermined schedule. I restrict my sample to time-based grants.

Within time-based grants, awards can vest with either a cliff schedule or a ratable schedule. Since cliff schedules have the largest portion of the award further into the future, the future payments are arguably more exogenous to the firm conditions at the time of vesting. In order for an endogeneity problem to exist, the compensation committee would have to be able to accurately and systematically forecast firm conditions and the reputational environment years in the future. This is unlikely.

1.3.1 Panel Specifications

My primary dependent variable is one-year ahead product market reputation. As robustness checks I also use two-years ahead reputation and a reputation measure built using only *Helpful* reviews. I use a forward looking dependent variable since Sheen (2014) finds that following a merger the quality of products between the two companies converges, but this convergence takes about two to three years to materialize. Changes made to the product lineup cannot be immediately reflected in the product market since changes take time to make their way through the manufacturing process. However, this change would occur more rapidly when a firm is not also concerned about combining the resources of another company.

The model I formally test is:

$$\text{Reputation}_{i,j,t+1} = \beta_0 + \beta_1 \text{Vesting Event}_{i,t} + \gamma \mathbf{X}_{i,t} + \theta_i + \phi_t + \epsilon_{i,t} \quad (1.1)$$

where i indexes the firm, j indexes the product, t indexes the year, $\mathbf{X}_{i,t}$ is a vector of control variables, θ_i represents the time-invariant firm fixed effect, and ϕ_t is the year fixed effect that accounts for annual factors that could impact all observations in a year. *Reputation* is measured as the average number of stars (out of 5) assigned to that product in the subsequent year. In other specifications I decompose *Vesting Event* into its stock and option components, and in later tests I replace the *Vesting Event* indicator variable with varying measures of institutional ownership types. Standard errors are clustered by product in order to address the potentially correlated error terms within the groups.¹³

In my second set of tests I attempt to understand the mechanism through which reputation is affected. A firm could respond by changing the existing products or changing the product lineup by increasing/decreasing the number of products they introduce/drop each year. In order to test the first hypothesis I rerun Equation (1.1) including product fixed effects (which subsume the firm fixed effect).

In order to determine whether the product lineup is changing, I employ a firm-year panel and estimate the following model:

$$\Delta \text{Characteristic}_{i,t} = \beta_0 + \beta_1 \text{Vesting Event}_{i,t} + \gamma \mathbf{X}_{i,t} + \theta_i + \epsilon_{i,t} \quad (1.2)$$

where i indexes the firm, t indexes the year, $\mathbf{X}_{i,t}$ is a vector of control variables, and θ_i represents the time-invariant firm fixed effect. *Characteristic* _{i,t} is either *Number of Products*, *Number of New Products*, or *Number of Dropped Products* depending on the specification. In some specifications I again decompose *Vesting Event* into its stock and option components. Standard errors are clustered by firm in order to address the potentially correlated error terms within the groups. Given that the dependent variable measures the change in the characteristic and that the model uses firm fixed effects, the β_1 estimate measures how the event impacts the rate of change of the characteristic.

My final set of tests explore the differential effect that myopia has across several measures: *High Leverage*, *Old CEO*, *High Advertising*, and *Bad Reputation*. I use

¹³See Bertrand, Dufflo, and Mullainathan (2004) and Petersen (2009).

the exogenous specification of Equation (1.1), but include estimates on the interacted coefficients as well.

1.4 Empirical Analysis

1.4.1 Main Results

My first set of tests, using the vesting events as exogenous shocks to myopic incentives, are presented in Table 1.3. In the first two specifications the outcome variable of interest is product market reputation in the subsequent year. The next two specifications examine the effect two years ahead, and the final two limit the measure of reputation to only include *Helpful* reviews. Odd numbered specifications use *Any Vesting Event* as the shock to myopia, while the even numbered specifications split the indicator into its stock and option components. As stated in the discussion of Equation (1.1), firm and year fixed effects are included, and standard errors, clustered by product, are reported in parentheses.

Results are consistent across all specifications.¹⁴ The estimate on *Any Vesting Event* is negative and statistically significant at the one percent level in all three specifications. The magnitude of the *Stock Vesting* estimate is slightly larger (though the difference is not significantly different from zero) and also statistically significant. The estimates for the coefficients on *Option Vesting* are indistinguishable from zero. These results are consistent with myopia negatively impacting reputation, and they are also consistent with Edmans et al. (2017) who find that the reduction in physical investment is concentrated primarily in the stock vesting.¹⁵

The lack of significance on the *Option Vesting* is not surprising. Many of these option vesting events grant the CEO an option that is out of the money. In these instances, the CEO does not experience a myopic shock.

The magnitudes on the estimates are also economically meaningful. A stock vesting event to the CEO is associated with an average decline of 0.10 stars in the next year’s average rating, or 10% of the unconditional average interquartile range.

1.4.2 Mechanism

Table 1.4 repeats the analysis using product fixed effects instead of firm fixed effects to test the product deterioration hypothesis. The quantitative results are similar, but the interpretation is slightly different. The tests provide evidence that following short-term incentive shocks, individual products deteriorate. Multiple explanations exist for this deterioration. Two potential explanations are that the manufacturing process may be altered, or perhaps the firm reduced customer service for the product following sales. Either way, the reputation of the product among consumers declines.

Table 1.5 estimates Equation (1.2). Again, even specifications decompose the vesting event variable into its stock and option components. Specifications (3) and (4)

¹⁴See Internet Appendix Table IA1.1 for control variable details.

¹⁵Internet Appendix Table IA1.2 repeats this analysis using all executives instead of just the CEO.

use two-year ahead reputation as the dependent variable, and specifications (5) and (6) limit the reputation measure to only include *Helpful* reviews. In all specifications standard errors are clustered at the firm level and reported in parentheses.

The results are consistent with the risk aversion story. Firms with CEOs who are shocked with a short-term incentive change reduce the number of products they offer. Specifications (3) and (4) show that this reduction in the number of products is due to a reduction in the rate at which new products are released. Since releasing new products is a risky activity, CEOs appear to be reducing their risk-taking to protect the stock price in vesting years.

1.4.3 Cross-Sectional Variation in Myopia and Reputation

The next set of tests repeats specifications (1) and (2) from Table 1.3 with interacted indicator variables. Table 1.6 reports results from testing whether or not there is a differential effect across firms with existing short-term pressures. Table 1.7 reports results from testing whether or not there is a differential effect across other reputational factors.

CEOs that are older and closer to retirement have less of an incentive to care about the long-run firm prospects, which in turn could mean that they are more likely to be affected by these incentive shocks. Similarly, Matsa (2011) shows that CEOs of highly levered supermarket firms, with a near-term concern about cash flow for debt service, reduce quality to conserve cash. Consistent with these hypotheses, I find that the negative reputational shock in the product market is largest for firms with an older CEO and firms with high leverage.

Consistent with the notion of diminishing marginal returns to investment in reputation, I find that the shock to short-term incentives has more severe consequences for firms who already have a bad reputation. This is consistent with the findings of Larkin (2013) that higher brand loyalty is associated with a more inelastic demand curve on the part of the consumers. Firms with a high reputation are able to “weather a storm” better than poorly-perceived firms. This hypothesis is also supported by the results that show the effect is mitigated when firms spend more on advertising. Higher advertising expenses appear to safeguard the firm against these reputational consequences.

1.4.4 Institutional Ownership

In the last set of tests I use an alternative empirical strategy, looking instead at institutional ownership and its effect on product market reputation. Several papers have used institutional ownership as a measure of firm myopia.¹⁶ I decompose institutional ownership into separate components since the time horizon of the owner has been shown to affect firm behavior. Bushee (2001) classifies institutional owners into *Transient*, *Dedicated*, and *Quasi Indexers*. These classification are available on his Website. Table 1.8 presents the results.

¹⁶See, e.g., Cella (2009), Bushee (1998), Derrien, Kecskés, and Thesmar (2013).

The two measures of interest are *Dedicated Ownership* and *Transient Ownership*. *Dedicated Ownership* measures the fraction of shares outstanding that are owned by institutional owners with long investing horizons and low levels of diversification, while *Transient Ownership* measures the fraction of shares outstanding that are owned by institutional owners with short investing horizons and high levels of diversification. Investors classified as *Quasi Indexers* have long investing horizons and high levels of diversification.

Increases to *Dedicated Ownership* predict higher product market reputation. The opposite result holds for *Transient Ownership*. Limiting the measure of reputation to *Helpful* reviews, the interpretation and inferences hold. When a firm has an increase in owners with a short-term focus its product market reputation declines.

The institutional ownership specifications do face an endogeneity problem, however. It is possible that dedicated institutions are particularly good at targeting firms with better future prospects and that institutions are not actually affecting myopia at all. However, Edmans (2009) argues that simply the threat of exit by these large institutions causes management to focus on the long-term. If management decides to act myopically then these institutions will “vote with their feet” and add selling pressure to the stock, thus reducing the incentive to act myopically. At a minimum these tests provide additional evidence in support of the conclusions reached in the primary analysis.

1.5 Conclusion

In this paper I examine a firm’s product market consequences when its management has myopic, or short-term, incentives. Specifically, I find that the firm’s product market reputation is negatively impacted when the firm’s management has short-term vesting incentives in the current year. In my primary tests I use the exogenous vesting of options and stock grants to the firm’s CEO. These vesting events are set up years in advance and are therefore unlikely to be related to current product market outcomes. I find evidence that this decline is driven by a deteriorating of existing products (cost cutting) as well as changes to the firm’s product lineup (risk aversion).

I find that the negative reputational shock in the product market is largest for firms with an older CEO and firms with high leverage. Consistent with the notion of diminishing marginal returns to investment in reputation, I find that the shock to short-term incentives has more severe consequences for firms who already have a bad reputation, though higher advertising expenses appear to safeguard the firm against these reputational consequences.

I use the composition of the firm’s ownership as an alternative empirical methodology to estimate the degree of short-term focus. Previous work has shown that firms with transient owners are on average more preoccupied with near-term results, while firms with more dedicated ownership are not. These results confirm the initial findings that a firm’s myopic focus negatively impacts its reputation in the product market.

Table 1.1: Amazon Summary Statistics

This table presents summary statistics on Amazon variables. Product variables are in Panel A while firm variables are in Panel B. *Average Rating* measures the average number of stars (out of five) assigned by users to either the product or the firm. *Average Rating from Helpful* uses only reviews that were classified as *Helpful* by Amazon voters. Also included are the number of reviews, number of helpful reviews, number of products, average product lifespan, and the average number of products introduced and dropped each year by the typical firm.

<i>Panel A: Product variables</i>					
	Mean	Q1	Median	Q3	N
Average Rating	3.952	3.500	4.091	4.500	12,309
Average Rating from Helpful	3.951	3.500	4.111	4.714	11,340
Reviews	26.07	3	7	20	12,309
Helpful Reviews	13.97	2	4	11	12,309
Product Life (years)	5.540	3	5	7	12,309

<i>Panel B: Firm variables</i>					
	Mean	Q1	Median	Q3	N
Products	869.2	15	94	748	90
Yearly New Products	23.21	0.895	3.734	21.96	90
Yearly Dropped Products	16.94	0.429	2.417	16.52	90
Average Rating	3.904	3.667	4.008	4.199	90
Average Rating from Helpful	3.856	3.580	3.948	4.200	90
Reviews	3,745	24	220.5	4,203	90
Helpful Reviews	2,007	9	124.5	2,137	90

Table 1.2: Firm-Year Summary Statistics

This table presents summary statistics on the measures of short-term incentives as well as other variables used in the analysis. Panel A presents the percent of firm-years affected by vesting events as defined in the paper. *Any Vesting Event (CEO)* is set to 1 in a year where a time-based grant with a cliff schedule vests to the CEO. The grant must be set up at least one year in advance. *Any Vesting Event (All Execs)* is defined similarly, but includes vesting to all executives. The measures are also split into their stock and option components. The mean, median, 25th percentile, and 75th percentile, along with the number of associated firm-years, are reported in Panel B for other variables used in the analysis. See the data section for detailed variable definitions.

<i>Panel A: Vesting events</i>	
	%
Any Vesting Event (CEO)	24.8
Any Vesting Event (All Execs)	36.4
Any Stock Vesting (CEO)	17.0
Any Stock Vesting (All Execs)	26.4
Any Option Vesting (CEO)	11.3
Any Option Vesting (All Execs)	15.8

<i>Panel B: Firm controls</i>					
	Mean	Q1	Median	Q3	N
Market Cap (millions)	30,672	2,152	5,128	19,903	698
Book-to-Market	0.374	0.218	0.348	0.517	698
Leverage	0.598	0.437	0.593	0.730	698
12-month Return	0.122	-0.172	0.0683	0.308	698
Advertising Expense	0.0410	0.00997	0.0247	0.0515	482
Operating Margin	0.164	0.112	0.153	0.222	698
R&D	0.0616	0.0155	0.0320	0.0868	698
Short Interest Ratio	0.0384	0.0139	0.0266	0.0475	671
Dedicated Ownership	0.0376	0.00001	0.00160	0.0591	620
Transient Ownership	0.163	0.0886	0.139	0.213	620
Quasi Index Ownership	0.518	0.413	0.532	0.639	620

Table 1.3: Do Vesting Events Predict Product Market Reputation?

This table presents the results from estimating Equation (1.1) and its variants. The dependent variable in columns (1) and (2) is measured as the average number of stars (out of 5) assigned to the product in the subsequent year. The second and third sets of specifications use two-year ahead product market reputation and the reputation as measured by the *Helpful* reviews, respectively. *Any Vesting Event* is an indicator variable set to 1 in any year in which the firm's CEO has either a stock vesting event or an option vesting event. *Any Stock Vesting* and *Any Option Vesting* decompose *Any Vesting Event* accordingly. See the data section for detailed variable definitions. Control variables are included in the specifications and defined in the text. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered at the product level, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}		Reputation _{t+2}		Helpful Reputation _{t+1}	
	(1)	(2)	(3)	(4)	(5)	(6)
Any Vesting Event	-0.067*** (0.018)		-0.071*** (0.021)		-0.070*** (0.017)	
Stock Vesting		-0.101*** (0.020)		-0.114*** (0.024)		-0.098*** (0.020)
Option Vesting		0.022 (0.029)		0.035 (0.037)		-0.008 (0.031)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.063	0.063	0.064	0.064	0.066	0.067
Observations	52,924	52,924	40,646	40,646	50,168	50,168

Table 1.4: Within-Product Decline

This table repeats the analysis from Table 1.3 using product fixed effects instead of firm fixed effects. Control variables are included in the specifications and defined in the text. Indicators for fixed effects are reported beneath the panels. Standard errors (reported in parentheses) are clustered at the product level. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}		Reputation _{t+2}		Helpful Reputation _{t+1}	
	(1)	(2)	(3)	(4)	(5)	(6)
Any Vesting Event	-0.054*** (0.016)		-0.017 (0.018)		-0.045*** (0.014)	
Stock Vesting		-0.079*** (0.019)		-0.038* (0.022)		-0.059*** (0.016)
Option Vesting		0.031 (0.027)		0.030 (0.031)		0.014 (0.026)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Product FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.528	0.529	0.542	0.542	0.687	0.687
Observations	50,400	50,400	38,961	38,961	48,032	48,032

Table 1.5: Changing Product Lineup

This table presents results from estimating variants of Equation (1.2). $\Delta Products$ is the one-year change in the number of products listed by the firm. $\Delta New Products$ is the change in the number of new products listed on Amazon by the company in the year. $\Delta Dropped Products$ is the change in the number of products that were reviewed in the previous year but not the current year. Control variables are included in the specifications and defined in the text. Indicators for fixed effects are reported below. Standard errors (reported in parentheses) are clustered at the firm level. Asterisks represent the conventional levels of statistical significance.

	$\Delta Products$		$\Delta New Products$		$\Delta Dropped Products$	
	(1)	(2)	(3)	(4)	(5)	(6)
Any Vesting Event	-4.12*		-1.70		0.52	
	(2.23)		(1.25)		(0.72)	
Stock Vesting		-8.41***		-2.90**		0.77
		(2.53)		(1.23)		(0.68)
Option Vesting		2.14		0.97		0.54
		(3.68)		(2.18)		(1.77)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.13	0.14	0.05	0.05	0.27	0.27
Observations	732	732	647	647	647	647

Table 1.6: Additional Short-Term Pressures

This table presents estimates from versions of specifications (1) and (2) from Table 1.3. The dependent variable is one-year-ahead product market reputation and is measured as the average number of stars (out of 5) assigned to the product. *Near Retirement* is an indicator variable equal to 1 if the CEO is over 50 years of age. *High Leverage* is an indicator variable equal to 1 if the firm is above the annual median leverage ratio. *Any Vesting Event* is an indicator variable set to 1 in any year in which the firm's CEO has either a stock vesting event or an option vesting event. *Any Stock Vesting* and *Any Option Vesting* decompose *Any Vesting Event* accordingly. Control variables are included in the specifications and defined in the text, though they are omitted for brevity. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered by product, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}			
	(1)	(2)	(3)	(4)
Any Vesting Event	-0.0296 (0.0225)		-0.0196 (0.0302)	
Stock Vesting Event		-0.0358 (0.0283)		-0.0385 (0.0318)
Any × Near Retirement	-0.1226*** (0.0383)			
Stock × Near Retirement		-0.1225*** (0.0418)		
Any × High Leverage			-0.0663* (0.0340)	
Stock × High Leverage				-0.0952*** (0.0362)
Near Retirement	-0.0460* (0.0269)	-0.0464* (0.0272)		
High Leverage			-0.0769*** (0.0183)	-0.0781*** (0.0182)
Controls	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
R ²	0.066	0.066	0.063	0.063
Observations	37,107	37,107	52,924	52,924

Table 1.7: Existing Reputational Factors

This table presents estimates from versions of specifications (1) and (2) from Table 1.3. The dependent variable is one-year-ahead product market reputation and is measured as the average number of stars (out of 5) assigned to the product. *Low Reputation* is an indicator variable equal to 1 if the firm is in the lowest annual quartile of reputation as measured by Amazon.com reviews. *High Advertising* is an indicator variable equal to 1 if the firm is in the top annual quartile of advertising expenses. *Any Vesting Event* is an indicator variable set to 1 in any year in which the firm's CEO has either a stock vesting event or an option vesting event. *Any Stock Vesting* and *Any Option Vesting* decompose *Any Vesting Event* accordingly. Control variables are included in the specifications and defined in the text, though they are omitted for brevity. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered by product, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}			
	(1)	(2)	(3)	(4)
Any Vesting Event	-0.0638*** (0.0176)		-0.0572*** (0.0212)	
Stock Vesting Event		-0.0903*** (0.0203)		-0.1003*** (0.0244)
Any × Low Reputation	-0.2379** (0.1008)			
Stock × Low Reputation		-0.2065** (0.1031)		
Any × High Advertising			0.0822** (0.0405)	
Stock × High Advertising				0.1206** (0.0498)
Low Reputation	-0.2413*** (0.0314)	-0.2420*** (0.0312)		
High Advertising			0.0575 (0.0411)	0.0666 (0.0407)
Controls	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
R ²	0.065	0.065	0.053	0.053
Observations	52,924	52,924	46,382	46,382

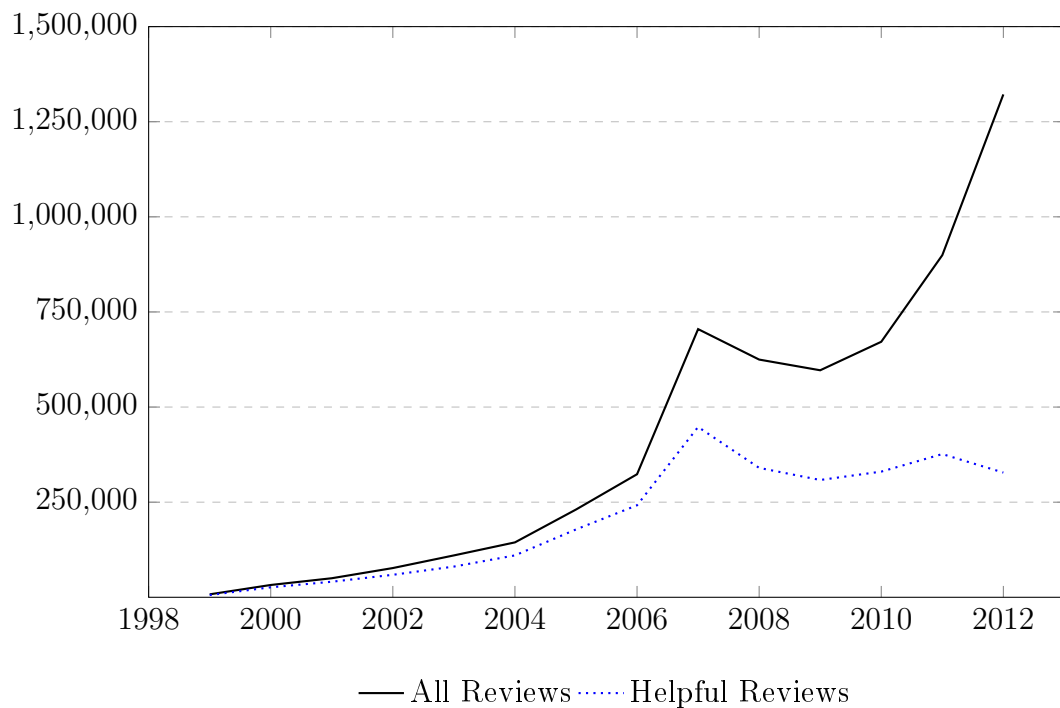
Table 1.8: Does Institutional Ownership Predict Product Market Reputation?

This table presents estimates of the effect of institutional ownership on product market reputation. The dependent variable in columns (1) and (2) is measured as the average number of stars (out of 5) assigned to the product in the subsequent year. The second and third sets of specifications use two-year ahead product market reputation and the reputation as measured by the *Helpful* reviews, respectively. *Transient*, *Dedicated*, and *Quasi Indexer* measure the fraction of shares outstanding that are owned by institutional owners who have been classified as such as in [Bushee \(2001\)](#). Control variables are included in the specifications and defined in the text, though they are omitted for brevity. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered at the product level, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}			Reputation _{t+2}			Helpful Reputation _{t+1}		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Transient	-0.174*** (0.064)			-0.017 (0.079)			-0.367*** (0.069)		
Dedicated		0.953*** (0.123)			0.738*** (0.137)			1.118*** (0.126)	
Quasi Indexer			-0.262*** (0.052)			-0.495*** (0.067)			-0.099* (0.054)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.067	0.067	0.067	0.060	0.060	0.061	0.080	0.081	0.080
Observations	79,319	79,319	79,319	61,951	61,951	61,951	75,342	75,342	75,342

Figure 1.1: Number of Reviews Posted on Amazon.com in Selected Categories

This chart plots the time series of reviews posted on Amazon.com in any of the following categories: Arts, Automotive, Baby, Beauty, Cell Phones and Accessories, Clothing and Accessories, Electronics, Gourmet Foods, Health, Home and Kitchen, Industrial and Scientific, Musical Instruments, Office Products, Patio, Pet Supplies, Shoes, Sports and Outdoors, Tools and Home Improvement, Toys and Games, and Watches. The solid black line represents the total number of reviews posted each year. The dashed blue line represents the number of reviews each year which were classified as *Helpful* by other users. In order for a review to be classified as *Helpful* at least 50 percent of the voters must have designated it as such.



Chapter 2 Real Estate Shocks and Financial Advisor Misconduct

2.1 Introduction

Do household level financial shocks cause employees to commit financial misconduct? Anecdotal evidence has long suggested a relation between financial well-being and deviant behavior. Indeed, over two thousand years ago Aristotle called poverty the “parent” of crime. More recently, in a series of interviews with professionals convicted of white collar crimes, [Cressey \(1971\)](#) found that financial pressure nearly always preceded misconduct. Interpreting the observed relation between financial pressure and misconduct is challenging, however, because financial pressure is often the result of the individual’s own choices. For example, [Cressey \(1971\)](#) found that financial pressure was primarily due to gambling, alcoholism, drug use, and extravagant spending. Thus, it is unclear if negative financial shocks cause misconduct or if they are both symptoms of the same underlying personality traits or preferences.

Theory also does not provide definitive guidance, because the effect of wealth shocks on misconduct is ambiguous without strong assumptions about utility. On the one hand, financial misconduct is a risky activity; it could create illicit gains, but could result in penalties and negative career consequences.¹ Under decreasing absolute risk aversion, negative wealth shocks increase sensitivity to risk, implying less willingness to engage in misconduct. On the other hand, [Block and Heineke \(1975\)](#) show that, if individuals have ethical preferences, the relation between wealth and misconduct is considerably more complicated, and the effect of a wealth shock depends upon whether ethical behavior is a normal or an inferior good. Their model further shows that understanding the relation between wealth and misconduct is critical for evaluating policy responses.

Ultimately, whether financial pressure causes misconduct is an empirical question that can only be tested with exogenous wealth shocks. In this paper, we use plausibly exogenous shocks to financial advisors’ wealth based on housing price shocks. We use a series of fixed effect strategies to exploit within advisor, within ZIP code, and within firm-year variation to identify whether household wealth shocks affect the propensity of financial advisors to engage in misconduct.

We examine the financial advisory industry for several reasons. First, advisors have large effects on household financial well-being. [Hung, Clancy, Dominitz, Talley, Berribi, and Suvankulov \(2008\)](#) show that the majority of individual investors consult an advisor for financial decisions, and [Foerster, Linnainmaa, Melzer, and Previtero \(2017\)](#) show advisors strongly influence household portfolio choice. Given this key role in facilitating household access to financial markets, it is critical that households are able to trust their advisor with their money ([Gennaioli, Shleifer, and Vishny, 2015](#)). Second, advisors are primarily compensated through commissions, which cre-

¹Prior studies showing negative career consequences for financial misconduct include [Egan, Matvos, and Seru \(2018a\)](#) for financial advisors, [Karpoff, Lee, and Martin \(2008\)](#) for corporate executives, and [Fich and Shivdasani \(2007\)](#) for corporate directors.

ates conflicts of interest and incentives for misconduct.² Empirical studies such as Dimmock, Gerken, and Graham (2018) and Egan, Matvos, and Seru (2018a) show that misconduct in this industry is common. A surprisingly high fraction of advisors have a history of exploiting their clients through activities such as churning, unauthorized trading, misrepresentation, and selling unsuitable investments. Third, the data for this industry allow us to link misconduct to specific individuals within a firm.

Our data come from detailed mandatory disclosure filings made by financial advisors, which identify financial misconduct committed by specific advisors as well as their employment histories (see Dimmock, Gerken, and Graham, 2018; Egan, Matvos, and Seru, 2018a,b). In addition, these mandated filings also include the advisors' home addresses and the dates of residency. We combine the advisor addresses with ZIP code level house price indexes created by Zillow and impute a purchase price and time-series of house price returns for each advisor in the sample.³

We use house price returns as exogenous shocks to financial advisors' personal wealth. For our purposes, we require a shock that is both unanticipated and economically meaningful. Cheng, Raina, and Xiong (2014) analyze whether midlevel managers working in securitized finance believed there was a housing bubble in 2004–2006 by examining the managers' personal home transactions; they find no evidence that these individuals anticipated the housing crisis. A large literature shows that housing price fluctuations have large, economically meaningful effects on consumption (see Campbell and Cocco, 2007; Carroll, Otsuka, and Slacalek, 2011; Mian, Rao, and Sufi, 2013; DeFusco, 2018). Gan (2010) shows that housing shocks affect consumption even for households that do not refinance, and argues this is consistent with changes in precautionary savings due to real estate wealth effects. Thus, real estate shocks appear to be both unexpected and economically meaningful.

Following other studies on risk taking by professionals around the housing crisis (Bernstein, McQuade, and Townsend, 2018; Pool, Stoffman, Yonker, and Zhang, 2018), we estimate differences-in-differences models, in which we regress changes in an advisor's misconduct on his housing price shock during the financial crisis. These specifications include firm fixed effects, which control for potential confounding variation among employees within a firm (e.g., if within-firm incentives to commit misconduct change during the financial crisis). The results show that advisors who suffer larger housing price declines subsequently increase their commission of misconduct. For example, advisors who suffer a price decline of 10% or more increase misconduct by 41% relative to advisors with smaller price declines.

We then extend the differences-in-differences results to fixed effect panel regressions using cumulative housing returns since purchase. In these tests, the unit of observation is advisor-year, which allows us to use housing price declines that oc-

²See Bergstresser, Chalmers, and Tufano (2009), Hackethal, Haliassos, and Jappelli (2012), Inderst and Ottaviani (2012), Mullainathan, Noeth, and Schoar (2012), Chalmers and Reuter (2015), and Hoechle, Ruenzi, Schaub, and Schmid (2018).

³In robustness tests, we show that the imputed house price returns are a highly significant predictor of actual bankruptcies filed by financial advisors. We also have a subsample of advisors for whom we have residence-specific Zillow valuation estimates. We find that the average time series correlation between the residence-specific price changes and the ZIP code price changes is 0.803.

curred at any point during the 1999–2013 period. These specifications include advisor, firm-year, and ZIP code fixed effects. The advisor fixed effects remove the advisor’s overall propensity to commit misconduct, as well as individual characteristics such as gender, education, and religious background. The firm-year fixed effects remove variation from the employing firms’ tolerance of misconduct or its business model (even if these effects are time-varying). The firm-year fixed effects also remove any time-series changes that affect all advisors, such as the overall economy. The ZIP code fixed effects remove the characteristics of the area, such as demographics, local culture, state-level regulation, etc. The panel regression results are consistent with the differences-in-differences test: advisors who suffer negative house price shocks are significantly more likely to commit misconduct. Additional results show that the relation between cumulative housing returns and misconduct is non-linear and that misconduct is significantly more sensitive to large losses on housing.

In the next set of tests, we exploit our ability to observe each advisor’s *cumulative* housing return since purchase. Even advisors living in the same ZIP code at the same time can have very different cumulative housing returns. For example, consider two advisors living in the same ZIP code in 2008 but who purchased their homes at different times. Although prices declined in 2008, an advisor who purchased a home in 1986 would likely have a positive cumulative return, while an advisor who purchased a home in 2006 would likely have a negative cumulative return. This variation in cumulative returns across advisors in the same ZIP code during the same year allows us to include ZIP-year fixed effects to remove any local time-varying confounding variation, such as shocks to the home prices of the local customer base. In an additional test, we include branch-ZIP-year fixed effects. In this specification, the fixed effects limit the comparison to advisors who live in the same ZIP code during the same year, and who also work at the same branch of the same firm. Even with these more stringent fixed effects, we continue to find that large negative cumulative returns are associated with higher misconduct.

In our next tests, we use two alternative dependent variables based on misconduct reported by non-local parties. This alleviates concerns about the commonality of the home price shock suffered by the advisor and the shock suffered by local customers. First, we limit the dependent variable to include only instances of misconduct for which the advisor and the customer live in different states. Second, we define misconduct as either a finalized regulatory sanction or a termination of the advisor by his employer (and exclude all advisor-year observations that include a customer-driven complaint to ensure this alternative dependent variable is distinct from the primary dependent variable). For both of the alternative dependent variables, we continue to find a significant negative relation between cumulative returns and misconduct.

We next examine cross-sectional variation in the risk that an advisor is terminated for misconduct. Egan, Matvos, and Seru (2018a,b) show there is large variation in the likelihood an advisor is terminated after committing misconduct — some firms are more tolerant of misconduct and women are punished more severely than men. All else equal, higher career risk implies a lower expected return to misconduct, reducing the incentive to commit misconduct. Consistent with this intuition, we find that the relation between cumulative housing returns and misconduct is stronger for advisors

who are less likely to be terminated if misconduct is detected.

We next explore the mechanism through which housing losses affect professional misconduct. The relation could be caused by *active* misconduct, in which advisors deliberately exploit clients for financial gain. Alternatively, the relation could be caused by *passive* misconduct, in which advisors harm clients through inattention when they are distracted due to financial distress. To test these competing mechanisms, we categorize misconduct as active (misrepresentation, churning, unauthorized trading, etc.) or passive (negligence or omission of key facts). We find evidence of passive misconduct, but more importantly we find highly significant evidence of *active* misconduct; following housing losses, advisors deliberately exploit their clients.

In additional tests, we show that the results are robust to alternative definitions of misconduct. We also validate our key independent variable by showing that our measure of housing price shocks accurately predicts actual bankruptcy filings and underwater home sales by financial advisors.

Our paper is related to the recent literature on misconduct by financial advisors. [Dimmock, Gerken, and Graham \(2018\)](#) show evidence of peer effects in misconduct; financial advisors are more likely to commit misconduct if they are exposed to co-workers with a history of misconduct. [Egan, Matvos, and Seru \(2018a\)](#) study how misconduct affects the labor market for financial advisors and find that certain firms specialize in misconduct while others strive to maintain clean reputations. [Egan, Matvos, and Seru \(2018b\)](#) study gender differences in the punishment for misconduct and find that following misconduct female advisors are more likely to be terminated and less likely to find new positions. [Charoenwong, Kwan, and Umar \(2018\)](#) show that variation in regulatory oversight affects the propensity of advisors to engage in misconduct. [Clifford and Gerken \(2018\)](#) show that the assignment of property rights to client relationships reduces misconduct by advisors. In our paper, we show that willingness to engage in misconduct is a pliable characteristic of the individual advisor; advisors are more likely to commit misconduct when they are under personal financial pressure. Understanding such causes of financial misconduct is important for designing and implementing monitoring and regulatory systems.

Our paper is also related to several recent studies that show how personal financial issues affect professional behavior. [Pool, Stoffman, Yonker, and Zhang \(2018\)](#) show that mutual fund managers who suffer negative shocks to their home's value subsequently reduce portfolio risk and tracking error. [Bernstein, McQuade, and Townsend \(2018\)](#) show that workers who suffered larger losses on their house values during the financial crisis subsequently undertook less risky and less innovative projects. [Maturana and Nickerson \(2018\)](#) show that when teachers declare bankruptcy, their students' scores on standardized tests fall. Our paper also studies the effect of household financial losses on professional behavior, but we study misconduct rather than portfolio risk or productivity.

Our results show there are wealth effects in ethical behavior – a finding that has implications for interpreting economic models of crime and misconduct (see [Block and Heineke, 1975](#)). Extant empirical studies of crime and economic shocks largely focus on labor markets, where the key issue is how labor market shocks affect the substitution between time spent on labor versus criminal activity (see [Chalfin and](#)

McCrary, 2017). Relative to these studies, our setting has two unique features. First, the wealth shock is not directly related to the return on labor or on misconduct. Second, we study misconduct at work, and so the allocation of time between labor and misconduct is not a relevant issue. These features allow us to clearly identify wealth effects in ethical behavior, independent from the issue of time allocation.

We also document another important externality of housing price shocks. Aside from the direct wealth effects of housing price declines, several papers document less obvious adverse consequences. Campbell, Giglio, and Pathak (2011) show that foreclosures have spillover effects that reduce the value of neighboring houses. Mian, Rao, and Sufi (2013) highlight the decline in consumption following the housing crisis. We show another externality — investors suffer increased active misconduct and passive mismanagement as a result of their financial advisors’ real estate price shocks.

2.2 Data and Sample Construction

Our financial advisor data come from a panel of mandatory disclosures made in Form U4 filings. All registered representatives⁴ in the U.S. are required to file and update these forms following any material changes. FINRA assigns each advisor a unique individual permanent identifier that allows us to track advisors even if they switch employers. We obtain this panel through a combination of Freedom of Information Act (FOIA) requests filed with state regulators by the authors, third-party data obtained from a vendor (Meridian IQ), and the FINRA BrokerCheck Website.⁵ See Dimmock, Gerken, and Graham (2018) and Egan, Matvos, and Seru (2018a,b) for detailed explanations of the data and industry.

The financial advisor data include residential addresses for the 1999–2013 period, as well as all misconduct disclosures made during the 1999–2017 period. The sample includes 428,108 advisors with complete residential histories. We combine the advisor data with ZIP code level housing price data from Zillow. However, because Zillow does not provide the necessary data for all ZIP codes,⁶ our final sample contains 329,418 advisors.

2.2.1 Financial Advisor Data

The Form U4 data includes each advisor’s employment history, licenses, qualifications, and any mandated disclosures. Advisors are required to disclose certain information about customer complaints, regulatory actions, civil and criminal legal cases, terminations, and bankruptcies. Table 2.1 summarizes this disclosure information.

⁴“Registered representative” is the term FINRA uses for these individuals. Following the recent academic literature, in this paper we use the term financial advisor. These individuals are also commonly referred to as brokers or financial planners.

⁵The BrokerCheck website allows investors to access a subset of the data reported in the Form U4 filings. BrokerCheck does not report the advisors’ home addresses. See <https://brokercheck.finra.org/>.

⁶Since the Zillow data is also limited to the United States we are unable to include foreign addresses.

Columns (1) and (2) report the number of advisor-years with the disclosure and the percentage, respectively. Column (3) reports the percent of advisors who make a disclosure at any time during the sample period.

Following [Dimmock, Gerken, and Graham \(2018\)](#), our primary measure of misconduct is based on validated customer complaints. Customer complaints are formal complaints, in which a customer demands compensation for damages caused by an advisor’s misconduct. Although some customer complaints allege negligence, most complaints allege improper behavior taken to increase the advisor’s compensation. Financial advisors are compensated primarily based on the amount of revenue they generate for their firm (see [Hung, Clancy, Dominitz, Talley, Berribi, and Suvankulov, 2008](#)), which creates strong incentives for advisors to generate trading commissions or to sell products with high distribution fees. Theoretical models show that these incentives encourage misconduct⁷ and empirical tests find support for the models’ predictions.⁸

After a customer files a complaint, it can be resolved through arbitration, settlement, or withdrawal. Following [Dimmock, Gerken, and Graham \(2018\)](#), we create the variable *Misconduct* based on customer disputes for which either the arbitration panel rules in the customer’s favor or the dispute is settled for at least a certain minimum cash payment to the customer.⁹ Thus, this variable includes only customer complaints that are validated either through an arbitration decision or a sizable monetary payment (i.e., we do not include customer complaints that are withdrawn or unresolved as of the end of our sample). Table 2.1 shows that 0.63% of advisor-years and 4.66% of advisors in our final sample report at least one *Misconduct* event.

Table 2.1 also summarizes additional disclosures that are used in several robustness tests. *Out-of-State Misconduct* is a subcategory of the main *Misconduct* variable, which includes only cases in which the customer and advisor live in different states. *Regulatory* reports finalized regulatory sanctions from entities such as the SEC, FINRA, or state regulators. *Employment Separation After Allegations* reports whether the advisor has ever been terminated or permitted to resign following allegations of misconduct.¹⁰

In robustness tests, we follow [Egan, Matvos, and Seru \(2018a\)](#) and combine *Misconduct* with *Regulatory*, *Employment Separation After Allegations*, and certain civil

⁷See [Inderst and Ottaviani \(2009, 2012\)](#) and [Stoughton, Wu, and Zechner \(2011\)](#).

⁸See [Bergstresser, Chalmers, and Tufano \(2009\)](#), [Hackethal, Inderst, and Meyer \(2011\)](#), [Hackethal, Haliassos, and Jappelli \(2012\)](#), [Mullainathan, Noeth, and Schoar \(2012\)](#), [Chalmers and Reuter \(2015\)](#), and [Hoechle, Ruenzi, Schaub, and Schmid \(2018\)](#).

⁹Financial advisors must report settlements of \$10,000 or more before May 19, 2009 and settlements of \$15,000 or more afterwards. Settlements smaller than these thresholds need not be disclosed, as they are potentially “nuisance” settlements and do not represent valid complaints.

¹⁰*Employment Separation After Allegations* occurs if the advisor is discharged, voluntarily resigns, or is permitted to resign after allegations accusing the advisor of: “(1) violating investment-related statutes, regulations, rules, or industry standards of conduct; (2) fraud or wrongful taking of property; or (3) failure to supervise in connection with investment-related statutes, regulations, rules, or industry standards of conduct.” For more details see <https://www.finra.org/sites/default/files/AppSupportDoc/p015111.pdf>.

law and criminal disclosures,¹¹ to create the variable *EMS Misconduct*. The summary statistics show that 6.11% of advisors in our final sample report at least one event under the broader Egan, Matvos, and Seru (2018a) definition.

Table 2.1 shows that 1.55% of advisors report a bankruptcy and 0.53% of advisors report a compromise with creditors in relation to selling a home for less than the outstanding mortgage amount (i.e., underwater sale). We use these variables as checks of the validity of our primary measure of financial distress.

Panel A of Table 2.2 reports additional summary statistics related to advisor misconduct. For advisors with a settled or awarded customer complaint, the average (median) alleged damages is \$659,666 (\$19,000) and the average (median) settlement amount is \$239,485 (\$15,754). For advisors with regulatory sanctions, the average (median) penalty is \$13,125 (\$5,000). Note that for approximately 40% of the cases we are unable to observe settlement, award, or penalty amounts. In addition to the misconduct information, Table 2.2 summarizes additional information from the Form U4 filings. The median advisor has 10 years of industry experience, and in all regressions we control for the logarithm of this variable.

2.2.2 Financial Advisor’s Homes and Real Estate Price Shocks

The Form U4 disclosures include residence histories of the advisors’ home addresses and ZIP codes.¹² Each advisor must report the address and dates of occupation for each residency throughout the sample period. E.g., an advisor who has resided in the same house since July of 1960 would report a move-in date of July 1960 even though the sample period does not begin until 1999. We do not observe whether the advisor rents or owns the home. However, robustness tests reported in Internet Appendix 1 that use proxies for the likelihood of ownership suggest it is unlikely that renting materially affects our results.¹³

We filed FOIA requests with all state regulators, however, numerous states did not supply home addresses. As a result, we have home addresses only for advisors who register in the District of Columbia, Florida, Georgia, Hawaii, Indiana, Iowa, New Jersey, Oregon, Rhode Island, Tennessee, Texas, Washington, or West Virginia.

¹¹The civil law disclosures report court issued injunctions regarding investment-related activity, findings of a violation of any investment-related statute or regulation, and actions brought by a state or foreign financial regulatory authority that are dismissed by a court pursuant to a settlement agreement. The criminal disclosures report any felony convictions or charges, as well as certain misdemeanors such as bribery, perjury, fraud, or wrongful taking of property even if these occur outside of their financial advisory role.

¹²Section 11 of the Uniform Application for Securities Industry Registration (Form U4) asks advisors to “provide their residential addresses for the past five (5) years. Leave no gaps greater than three (3) months between addresses. Begin by entering your current residential address. Enter ‘Present’ as the end date for your current address. Post Office boxes are not acceptable. Report changes as they occur.”

¹³If measurement error from incorrectly assigning housing returns to renters is pure measurement error, it will result in attenuation bias our results will understate the true effect. If, however, renting is disproportionately associated with areas that had the largest price declines and is also positively correlated with increased misconduct following price declines, this could bias upwards our results (although it is not obvious why this correlation pattern would occur).

Because advisors must register with each state in which they plan to do business, and many advisors register in all states, we have home addresses of advisors in all 50 states (e.g., we have home addresses for 69,167 advisors who live in New York and are registered in at least one state that provided data). Thus, although our sample of home addresses is not comprehensive, it covers all major real estate markets. Importantly, the selection mechanism is the state regulator’s interpretation of The Privacy Act of 1974 as it relates to our FOIA request. It is not obvious that this selection mechanism would be systematically related to the correlation between local housing shocks and misconduct by financial advisors, and thus we do not see reasonable concerns related to selection bias.

We match the advisor residences to the Zillow ZIP code house price indexes. The combined advisor-residence-Zillow price dataset spans 13,679 ZIP codes, which collectively contain 74.7% of the U.S. population. Using the Zillow ZIP code indexes, we impute a purchase price and annual price changes for each advisor-residence combination.¹⁴ This approach is similar to that in [Bernstein, McQuade, and Townsend \(2018\)](#), who also impute individual house price shocks using ZIP code level price indexes.¹⁵ In Internet Appendix 2, we also use the House Price Index produced by the Federal Housing Finance Agency¹⁶ as an alternative measure of real estate prices and find similar results. For advisors who report multiple residences at the same time (e.g., vacation homes), we use the purchase-price-weighted average return for all residences. In Internet Appendix 3 we show our results are robust to using only the highest value residence or excluding all advisor-years with multiple residences.

Panel B of Table 2.2 shows that the average house in the sample has an imputed purchase price of \$320,539 and a current price of \$373,679. On average, the advisors have lived in their current house for 5.8 years. Panel C reports the distribution of annual house price changes. The median annual price change is \$3,700, which is 1.72% of the beginning of year value. As the percentiles show, there is considerable variation and many advisor-year observations experience negative returns.

We use the imputed purchase price and imputed annual returns to calculate the cumulative return¹⁷ for each advisor-year. Importantly, this varies across advisors even within a fixed ZIP code depending upon when the advisors purchased their house. For example, suppose Advisor A purchased a home in 2000 at an imputed price of \$200,000. House prices then doubled by 2007, when Advisor B purchased a house at an imputed price of \$400,000. The next year, house prices declined by 25% to \$300,000. The cumulative return for Advisor A is +50%, but the cumulative return for Advisor B is -25%. Panel B of Table 2.2 shows that the median cumulative

¹⁴[Pool, Stoffman, Yonker, and Zhang \(2018\)](#) use house level imputed values from Zillow, instead of ZIP code level imputation. This is feasible in their study as they have fewer than 1,000 individuals. This approach is not feasible for us given our large sample size.

¹⁵[Bernstein, McQuade, and Townsend \(2018\)](#) use the indexes developed by [Bogin, Doerner, and Larson \(2018\)](#), but report that robustness results using the Zillow indexes are similar.

¹⁶See <https://www.fhfa.gov/DataTools/Downloads/pages/house-price-index.aspx>.

¹⁷Our use of cumulative returns is similar to that of [Gerardi, Herkenhoff, Ohanian, and Willen \(2018\)](#) who use cumulative returns (based on state-level housing indexes) as an instrument for housing equity, who argue that cumulative returns avoid the endogeneity problems of loan-to-value ratios (driven by homeowner’s borrowing choices).

return is 9.72%, but the variation is large and many advisor-year observations have negative cumulative returns. In our baseline specification, we focus on cumulative returns for several reasons. First, it is natural to evaluate the current price relative to the purchase price, due to the salience of the purchase price. Second, mortgages are a function of purchase prices and so cumulative returns are important determinants of whether an advisor is underwater. Third, because of variation in the timing of purchases, cumulative returns vary across advisors within a ZIP code in any given year, which allows for the inclusion of more stringent fixed effects in some tests.

To summarize the time-series and regional variation in real estate prices during our sample period, Figure 2.1 shows the percentage of financial advisors who experience an annual price decline of at least 10% during our sample period. The figure plots results for the entire U.S. as well as for a few select states. There is a clear time-series pattern. Negative real estate price shocks are concentrated around the housing market crash of 2007–2009, although there is some variation across states in the timing of the crash. For example, the peak of the housing market crash occurred earlier in Nevada than in Illinois. There is also considerable cross-sectional variation: in 2008 real estate price shocks of 10% or worse affected more than 96% of advisors in Nevada but fewer than 6% of advisors in Texas. The figure also shows that price declines are not limited to the financial crisis period; many advisors experience substantial price declines in 2010 and 2011. Indeed, 70,537 advisors experience a 10% annual housing price decline outside of the 2007–2009 period.

There is also large cross-sectional variation within states and even within metropolitan statistical areas (MSA), as documented by [Bogin, Doerner, and Larson \(2018\)](#) and [Edlund, Machado, and Sviatschi \(2019\)](#). For example, Figure 2.2 displays ZIP-code level declines in housing prices for the Atlanta metropolitan area in 2008: the hardest hit ZIP codes lost as much as 27%, while other (often nearby) ZIP codes had negligible losses.

2.2.3 Real Estate Shocks and Misconduct by Financial Advisors

Figure 2.3 and Figure 2.4 present simple visual summaries of the unconditional relation between real estate shocks and misconduct by financial advisors. In Figure 2.3, we categorize the advisor-year observations into ventiles based on the advisor’s annual house price change, with the average return within each ventile shown along the x-axis. We then plot the average misconduct rate for each ventile. The dashed line shows the unconditional average for the entire sample and the gray shaded area shows the 95% confidence interval (with standard errors clustered by individual and by ZIP code). The annual return switches from negative to positive in the seventh ventile; the misconduct rate increases as returns decline in the negative return region and is largely flat in the positive return region except for a small but insignificant increase for the highest return ventiles.

In Figure 2.4, we plot the average misconduct rate in event time for the sample of advisors who experience an annual house price shock of -10% or worse.¹⁸ The average

¹⁸If an advisor experiences multiple shocks of -10% or worse we include only the first shock in the figure.

misconduct rate is flat in the years before the shock, rises sharply in the year of the shock, remains high for the next two years, and then reverts in the subsequent year. Thus, the simple visual evidence in both figures is consistent with a relation between real estate shocks and misconduct. Of course, this simple visual evidence should be interpreted cautiously, as the figures do not control for potential confounding factors, such as year effects, advisor characteristics, or regional differences — issues we discuss in the next section.

2.3 Identification Strategy

Identifying whether wealth shocks cause financial advisors to commit misconduct is complicated because of various unobservable factors that affect both housing prices and misconduct. For example, Las Vegas has a high rate of financial misconduct and also suffered large price declines during the financial crisis. This does not necessarily indicate a causal relation; possibly the type of person who chooses to live in Las Vegas is also the type of person who commits misconduct. To remove the potential effect of such unobservable variation, we use two identification strategies. First, we use ZIP-code level housing return shocks and estimate a differences-in-differences model. Second, in our primary specification we use individual-specific housing return shocks based on cumulative housing returns since purchase and multiple fixed effects to remove confounding variation.

2.3.1 Differences-in-Differences

Following the identification strategy used by [Bernstein, McQuade, and Townsend \(2018\)](#) and [Pool, Stoffman, Yonker, and Zhang \(2018\)](#), we examine cross-sectional variation around the 2008 financial crisis. Although the crisis affected all advisors in our sample, as [Figure 2.1](#) and [Figure 2.2](#) show, there was significant cross-sectional variation in its effect on local real estate prices. To exploit this variation, we estimate the following differences-in-differences specification:

$$\Delta Misconduct_i = \beta Crisis\ Price\ Change_z + \gamma \log(Experience)_i + \delta_f + \delta_{lr} + \epsilon_i \quad (2.1)$$

where i indicates a financial advisor, z indicates a ZIP code, f indicates a financial advisory firm, and lr indicates the decile of the advisor’s length of residency in their current home. The dependent variable, $\Delta Misconduct_i$, measures the change in the number of misconduct events committed by advisor i over the three year post-crisis period (2008–2010)¹⁹ compared to the three year pre-period, (2005–2007).²⁰

¹⁹To avoid survival bias, we include advisors who exit the industry during this three year period. The results are robust if we instead exclude these advisors from the sample.

²⁰Internet Appendix Table 4 shows the results are robust to using alternative time periods. In column (1), we follow [Pool, Stoffman, Yonker, and Zhang \(2018\)](#), who defined the pre-period as 2005–2006 and the post-period as 2009–2010. In column (2), we follow [Bernstein, McQuade, and Townsend \(2018\)](#), who defined the pre-period as 2005–2007 and the post-period as 2008–2012. In column (3), we treat 2008 as a gap year and define the post-period as 2009–2011.

$\log(\text{Experience})_i$ is the natural logarithm of the number of years that the advisor has worked in the industry.

In this specification, time-invariant effects are differenced away at the advisor level. Advisors in ZIP codes unaffected by the housing price collapse provide a control for any time-varying change in the level of misconduct (e.g., a general increase in misconduct or the detection of misconduct around the financial crisis). The firm-level fixed effects control for any firm-specific changes in misconduct (e.g., perhaps firms in areas more severely affected by the crisis changed in response to the crisis). The length-of-residency decile fixed effects control for any effects related to how long an advisor has lived in his house.

2.3.2 Cumulative House Price Changes

Our second identification strategy is a panel approach using cumulative housing returns.²¹ As described earlier, we construct the *Cumulative Return* for each advisor-year, using the change in the advisor’s house price since purchase, and estimate the following specification:

$$\text{Misconduct}_{i,t} = \beta \text{Cumulative Return}_{i,t} + \gamma \log(\text{Exp.})_{i,t} + \delta_i + \delta_z + \delta_{f,t} + \delta_{lr} + \epsilon_{i,t} \quad (2.2)$$

where $\text{Misconduct}_{i,t}$ is an indicator variable equal to one if the advisor reports a validated customer complaint during the current year.²² $\text{Cumulative Return}_{i,t}$ is the cumulative price change since purchase for advisor i , which varies across advisors in the same ZIP code at a given point in time. In addition to $\log(\text{Exp.})_i$, we rely on advisor, ZIP code, firm-year, and length of residence decile fixed effects to remove potential sources of confounding variation.

First, the financial advisor fixed effect, δ_i , removes all time-invariant characteristics of the advisor, including his overall propensity to commit misconduct, and also reduces the effect of advisor characteristics that are largely fixed throughout the sample, such as education and religious background. This fixed effect also removes the time-invariant part of the advisor’s business activities, such as customer characteristics, the types of products sold, etc. The advisor fixed effect also removes any time-invariant real estate preferences.

Second, the ZIP code fixed effect, δ_z , removes the time-invariant characteristics of the area in which the advisor lives. This is important as [Parsons, Sulaeman, and Titman \(2018\)](#) show there are large differences in misconduct rates across cities. This fixed effect also removes stable demographic and economic characteristics of the neighborhood.

Third, the firm-year fixed effect, $\delta_{f,t}$, removes the time-invariant characteristics of the firm that employs the advisor, as well as time-varying firm characteristics such as changes in the firm’s product offerings or monitoring procedures. Removing

²¹In Internet Appendix 5, we employ panel regressions with *annual* housing price returns. The results are similar, in that we find a significant negative relation between housing price returns and misconduct.

²²Robustness tests reported in Internet Appendix 6 show the results are robust to using an indicator variable for misconduct occurring in the subsequent year or during a three-year window.

firm effects is important as Egan, Matvos, and Seru (2018a) show that certain firms specialize in committing misconduct. The firm-year fixed effects also subsume year-effects, removing the common time-series variation in housing returns and misconduct (e.g., it would remove any common time-series relation between asset prices and complaint rates).

Fourth, the length-of-residency fixed effects, δ_{lr} , remove variation across advisors based on how long they have lived in their current residence. The length-of-residency is potentially important for several reasons. The loan-to-value ratio typically decreases with length of residency, and differences in leverage ratios may affect financial advisors' actions. Further, longer residency may be associated with deeper community ties, increasing the reputational cost of engaging in misconduct.

2.4 Main Results

2.4.1 Changes in Misconduct and House Price Shocks during the Financial Crisis

Table 2.3 reports results from differences-in-differences specifications, in which there is one observation per advisor. The dependent variable is the change in misconduct, defined as the number of instances of misconduct during the three-year period²³ 2008–2010 less the number during 2005–2007. The key independent variables are based on the advisor's housing price shock during the financial crisis (the house return in 2008). In column (1), the independent variable is *Crisis Price Change* which is the percentage price change of the advisor's house. In columns (2), (3), and (4) the independent variables are indicators equal to one if the return on the advisor's house was equal to or less than -5%, -10%, or -15%, respectively. All columns include a control for the logarithm of years of industry experience. All specifications include firm fixed effects to absorb any variation common to an advisory firm such as its product offerings, internal monitoring procedures, etc., as well as length of residence fixed effects to absorb any variation related to how long an advisor has lived in his house. The standard errors are clustered by ZIP code.

The results in all four columns show a significant relation between house price shocks and changes in misconduct; misconduct increases for advisors who suffer the largest house price declines during the financial crisis. The economic magnitudes implied by the results are large relative to the baseline frequency of misconduct. For example, the coefficient estimate in column (3) implies that an advisor whose house price dropped by 10% or more increases misconduct by 24.6% relative to the baseline frequency of misconduct.

²³Figure 2.4 shows that misconduct rates are higher for a three-year period following a real estate price shock. Accordingly, and following the existing literature on financial advisor misconduct around shocks (e.g., Dimmock, Gerken, and Graham, 2018; Charoenwong, Kwan, and Umar, 2018), we use a three-year window for misconduct.

2.4.2 Misconduct and Cumulative House Price Changes

Table 2.4 reports results from panel regressions in which the unit of observation is advisor-year. The dependent variable is an indicator variable equal to one if the advisor commits misconduct in the current year. The key independent variable is *Cumulative Return*, which is the cumulative percentage return on the advisor’s house since purchase. In column (1), we do not include advisor fixed effects, and instead control for the advisor characteristics: age, gender, previous misconduct, and licensing (Series 6, 7, 24, 65, and 66).²⁴ In columns (2) and (3), we include advisor fixed-effects. All specifications include controls for the logarithm of years of industry experience and firm-year, length of residence, and ZIP fixed effects. In column (3), we test whether the effect of returns on misconduct is non-linear. In this specification, we add an interaction term, $Cumulative\ Return \times I_{Extreme}$, where $I_{Extreme}$ indicates a cumulative return worse than -20%. The standard errors are clustered by advisor and ZIP code.

Column (1) of Table 2.4 reports the simplest of the three specifications. We report this specification, which does not include individual fixed effects, because it is easier to interpret and to show the results are not dependent upon the inclusion of these fixed effects. The negative coefficient shows that advisors with worse cumulative returns are significantly more likely to commit misconduct. The inclusion of firm-year fixed effects means that the specification limits the comparison to advisors who work for the same firm during the same year — even within this limited comparison group, advisors with worse returns commit more misconduct. The specification also includes ZIP and length of residency fixed effects, meaning that the findings for *Cumulative Return* are relative to other advisors living in the same area and advisors with similar lengths of residency.

The specification reported in column (2), which we use as our benchmark specification for the remainder of the paper, includes individual fixed effects. In this specification, an advisor is effectively benchmarked against his own behavior throughout the sample. The results show that, even relative to his average misconduct behavior throughout the sample, an advisor is more likely to commit misconduct when his *Cumulative Return* is low. The results imply that a one standard deviation decrease in cumulative returns (35.8 percentage points change) results in an 7.5 basis point increase in the likelihood of misconduct, which is a 12% increase relative to the baseline misconduct rate.

The specification reported in column (3) includes an interaction term, $(Cumulative\ Return - (-20\%)) \times I_{Extreme}$, where $I_{Extreme}$ indicates a cumulative return worse than -20%. We include this specification with a cutoff of -20% for two reasons. First, as [Kau, Keenan, and Kim \(1994\)](#) show, the probability of financial distress increases non-linearly with the size of the cumulative loss, with the probability increasing sharply well below a cumulative return of 0% (i.e., for small losses down payments provide some protection). Second, many homeowners likely have only an imprecise estimate of their home’s value based on their neighbors’ home sales, local

²⁴For brevity, we report the coefficients on these control variables in [Internet Appendix Table IA2.7](#) instead of in Table 2.4.

news stories, observing new construction, etc. But it is unlikely the homeowner knows precisely when their return switches from slightly positive to slightly negative. By using a large negative cumulative return, we focus on individuals who almost surely know they have suffered large losses. The results in column (3) show the relation between cumulative housing returns and misconduct is non-linear. The increase in misconduct is much greater for large negative returns, indicating that misconduct is significantly more sensitive to large losses on housing.

2.4.3 A Placebo Test

As a robustness test, we employ a bootstrap-placebo procedure. For each repetition of this procedure, we randomly match each ZIP code with another ZIP code from a different state. We then assign each advisor-year the cumulative housing return from the matched ZIP code-year based on the advisor's year of purchase. The match between ZIP codes is fixed for all years within a single repetition of the bootstrap procedure. For example, suppose that in 2006 an advisor named Bob lived in a house in ZIP code 48823 that he purchased in 2000. Further suppose that, for this repetition of the bootstrap, ZIP code 48823 was randomly assigned returns from ZIP code 78722. Then, in 2006 Bob would be assigned the six-year cumulative housing return from ZIP code 78722. If Bob continued living in the same house, then in 2007 he would be assigned the seven-year cumulative housing return from ZIP code 78722. Other advisors in ZIP code 48823 would also be assigned returns from ZIP code 78722, but their cumulative housing returns would vary depending on their year of purchase. Using these pseudo-cumulative housing returns, we estimate the specification reported in column (2) of Table 2.4. We repeat this procedure 10,000 times.

This procedure assigns random cumulative returns for each advisor-year observation, but crucially, preserves all other time-series and cross-sectional relations in the panel. The time-series relations are preserved because the advisor's pseudo cumulative return is drawn from the same randomly assigned ZIP code for each year. The cross-sectional relations between advisors in a single ZIP code are preserved because their pseudo cumulative returns are all drawn from the same randomly assigned ZIP code. Further, any effects caused purely by the length of residency are preserved. For example, suppose that advisors with greater length of residency generally: (1) have higher cumulative returns and (2) are less likely to increase misconduct following house price shocks because of deeper community ties. This relation would be preserved in the placebo test because the pseudo cumulative return is a function of length of residency.

Figure 2.5 plots a histogram of the coefficient estimates on the *Pseudo-Cumulative Return* variable. The actual coefficient estimate of -0.2082 lies over six standard deviations below the mean of the bootstrapped coefficients (-0.010), and none of the 10,000 placebo estimates are below the actual coefficient estimate. Thus, the placebo results suggest that it is the *actual* loss suffered by the advisor that drives the relation between housing returns and misconduct.

2.5 Addressing Concerns about Commonality in Customer and Advisor Shocks

As is most studies of fraud and misconduct, we observe *detected* misconduct not *actual* misconduct. This creates the possibility of bias if variation in the detection rate is correlated with the independent variable of interest. In our study, a possible concern is that a customer's propensity to file a complaint against her advisor varies with the customer's real estate returns. That is, if a customer is under financial pressure due to losses on her home, she may become more likely to file a complaint against her advisor. In this section, we present a number of tests that address this concern.

2.5.1 ZIP-Year Fixed Effects

Column (1) of Table 2.5 is similar to the baseline specification, but includes ZIP-year fixed effects instead of ZIP fixed effects. These fixed effects exploit the fact that cumulative housing returns vary across advisors in the same ZIP code during the same year, based on when the advisors purchased their homes combined with the price path of housing in that ZIP code. The ZIP-year fixed effects remove variation that is common to all advisors in the same ZIP code during the year, such as local housing price shocks during the year, the economic and demographic characteristics of the local customer base, and any other local commonalities including the propensity of local customers to file complaints. In this specification, the variation in the dependent variable is limited to the time-series increase in misconduct by an advisor *relative* to the time-series increase of other advisors who live in the same ZIP code in that year. The variation in the key independent variable is limited to the cross-sectional variation across advisors living in the same ZIP code at that point in time.

This specification is quite conservative and likely removes much of the variation of interest, but it eliminates potential confounding effects. For example, suppose that dishonest advisors prefer to live in exciting metropolitan areas that have volatile real estate prices. These advisors commit misconduct regardless of local housing returns. Further suppose that customers are more likely to detect misconduct following a market downturn because of price declines, greater vigilance, or other reasons. Such a combination of events could create a spurious relation between housing returns and misconduct, however, it would be removed by the inclusion of the ZIP-year fixed effects.

The results in column (1) show that the relation between misconduct and cumulative housing returns remains negative and significant. Even after including the ZIP-year fixed effects, we find that advisors with worse cumulative returns are significantly more likely to commit misconduct. Further, the magnitude of the coefficient is similar to that in the baseline specification.

2.5.2 Branch-Year and Branch-ZIP-Year Fixed Effects

Column (2) of Table 2.5 includes branch-year fixed effects, which subsume the firm-year fixed effects in the baseline specification. This specification removes any variation

common to advisors who work at the same branch²⁵ of the same firm during the year, including local housing price shocks, the economic circumstances of the firm’s local customer base, local monitoring and oversight, and the firm’s product offerings. Even with these more stringent fixed effects, the results show a significant relation between housing returns and misconduct.

Column (3) of Table 2.5 includes branch-year-ZIP fixed effects, which subsume both the firm-year and zip-year fixed effects. This specification effectively limits the comparison to be between advisors who work for the same branch of the same firm and live in the same ZIP code during the year. These are very restrictive fixed effects that remove many sources of potentially confounding variation. The cost of including these fixed effects, however, is that we likely remove variation of interest and that we must drop more than half the observations due to insufficient variation within the fixed effect unit. The results show that, once again, there is a significant negative relation between *Cumulative Return* and misconduct. Advisors with worse cumulative returns on their home are significantly more likely to commit misconduct even relative to their local co-workers.

2.5.3 Out-of-State Customers

The previous fixed effect specifications include ZIP-year and branch-ZIP-year fixed effects. These fixed effects remove all variation common to advisors living in the same ZIP code during the same year — such as shared variation in the advisors’ customer base. It is possible, however, that even after removing ZIP-year variation, there remains a positive correlation between the real estate shocks of advisors and their customers. For example, suppose that because of demographic similarity advisors who recently purchased a house are disproportionately likely to match with customers who also recently purchased a house, resulting in similar cumulative real estate returns *even within ZIP-year*. In this section, we address the issue of advisor-customer commonality by using an alternative dependent variable.

Although many customer-advisor matches are between geographically proximate individuals, this is not always the case. In the data, we can observe the state of residence for customers who file complaints²⁶ and find that 15.4% of customer complaints are out-of-state customer complaints.²⁷ In the results reported in column (1) of Table 2.6, we limit the dependent variable to include only customer complaints filed by out-of-state customers. In column (2), we further limit the dependent variable to exclude complaints filed by customers living in bordering states (e.g., for advisors living in Florida we would exclude out-of-state complaints filed by customers living in Alabama and Georgia).

²⁵Following Egan, Matvos, and Seru (2018b), we classify “same branch” advisors as individuals who are employed by the same firm and live in the same county. This definition differs from that of Dimmock, Gerken, and Graham (2018) who use business address information that is not provided by all states.

²⁶Unfortunately, we do not observe the state of residence for customers who do not file complaints.

²⁷As multiple customers can file a complaint in a year, 27.3% of advisor-years with *Misconduct* include at least one out-of-state complaint.

In both columns, the results show that the relation between the advisor’s *Cumulative Return* and misconduct remains significant even with this restricted dependent variable. This test breaks the link between the local real estate price shock suffered by the advisor and the shock suffered by the (geographically distant) customer, suggesting that it is not the customers’ real estate losses that drive our results.

2.5.4 Regulatory Actions and Employment Separation After Allegations

In this section, we present another test to address the potential concern that, even after the inclusion of the fixed effects, an advisor’s *Cumulative Return* is correlated with his customer’s housing returns, or more generally, correlated with time-variation in his customer’s propensity to file complaints. Specifically, in column (3) of Table 2.6 the dependent variable is set equal to one if the advisor discloses a regulatory action or an employment separation after allegations. For this test, we exclude all advisor-year observations that include a customer complaint to ensure the regulatory actions and employment separations are not responses to customer complaints (e.g., if a firm terminated an advisor because of a customer complaint).²⁸

Government regulation of financial advisors is conducted by state governments. We include state-year fixed effects to remove the state regulator’s overall propensity to take actions in a given year.²⁹ We also continue to include firm-year fixed effects to remove each firm’s propensity to terminate its employees during a given year. These fixed effects remove the most obvious sources of confounding variation — as there is no obvious reason to expect that a state regulator or firm would disproportionately target advisors with worse cumulative housing returns.

The results in column (3) show that, even when misconduct is limited to exclude customer complaints, advisors with worse *Cumulative Return* are significantly more likely to commit misconduct. Overall, the results in Table 2.6 support the argument that wealth shocks affect the propensity of financial advisors to commit misconduct.

2.6 Cross-Sectional Variation in Termination Risk

In this section, we test how cross-sectional variation in termination risk affects the relation between real estate shocks and misconduct. Prior studies show that there is large variation in the likelihood that an advisor is terminated after committing misconduct (e.g., Egan, Matvos, and Seru, 2018a,b). Higher termination risk implies a lower expected return to committing misconduct. Thus, all else held equal, we expect advisors with higher termination risk will be less likely to increase misconduct following a real estate shock.

²⁸We hand checked a subsample of regulatory actions and employment separations that occur without customer complaints. Examples of the stated reasons include “incorrect reporting of information into company system,” “holding non-approved seminars,” “sale of non-approved products,” and “use of non-approved marketing materials.”

²⁹FINRA is responsible for some regulation at the national level, but national level effects will be subsumed by the state-year fixed effects.

Egan, Matvos, and Seru (2018a) show large across-firm variation in tolerance for misconduct. We measure each advisor’s termination risk based on the fraction of the other advisors working at the firm who have a history of prior misconduct. If this fraction is above the sample average, we set the indicator variable *High Firm-Year* equal to one.³⁰ The results, reported in column (1) of Table 2.7, show that the coefficient on the interaction term *Cumulative Return* \times *High Firm-Year* is significant and negative. Even after controlling for firm-year fixed effects, the effect of a real estate shock on misconduct is greater when the career risk associated with committing misconduct is smaller.

The specification in column (2) of Table 2.7 is conceptually similar to that in column (1), but here we consider tolerance for misconduct at a finer geographic level. For this test, we create an indicator variable *High Branch-Year*, which is set to one if an above average proportion of the advisor’s fellow employees *who work at the same branch* have a history of past misconduct. This variable allows for the possibility that a firm’s tolerance for misconduct in a given year could vary across branches due to the local manager, state regulators, or other reasons. The coefficient on the interaction term is significant and negative; advisors are more likely to commit misconduct following a real estate shock when they work for a branch with a high tolerance for misconduct.

Column (3) of Table 2.7 considers individual-level career risk from committing misconduct, rather than firm or branch level career risk. Egan, Matvos, and Seru (2018b) show that, relative to men, women are more likely to be terminated for misconduct and less likely to find new employment following termination. Accordingly, women face relatively more severe career risk for committing misconduct. As in Egan, Matvos, and Seru (2018b), 25% of advisors in our sample are female. The coefficient on the interaction term *Cumulative Return* \times *Female* is significant and positive. Indeed, for females the net effect of *Cumulative Return* is not significant. Overall, the results in this section show that the relation between real estate shocks and misconduct is stronger when there is less career risk from committing misconduct.

2.7 Active versus Passive Misconduct

Throughout the paper, we have interpreted misconduct as an *active* choice. That is, that advisors deliberately exploit their clients for financial gain. If advisors derive utility from ethical behavior,³¹ and ethical behavior is a normal good, then advisors’

³⁰We do not include the advisor’s own history of misconduct when constructing this variable because of biases that occur in fixed effect regressions in which an independent variable is a function of lagged values of the dependent variable. See Nickell (1981) for further discussion. Because the advisor’s own history of misconduct is not included in this variable, its direct effect is not fully subsumed by the firm-year fixed effect, and so it is included in the regression. However, due to its high correlation with the firm-year fixed effects, the coefficient on the direct effect is unreliable due to multicollinearity. However, the issue of multicollinearity does not affect the coefficient on the interaction term, which is the coefficient of interest.

³¹Deriving utility from ethical behavior implies an advisor may forgo at least some opportunities to commit misconduct even when the misconduct has a positive *financial* net present value. For formal modeling of ethical preferences, see Block and Heineke (1975) and Morrison and Thanassoulis

“consumption” of ethical behavior will decrease when wealth decreases. Alternatively, as advisors approach the bankruptcy boundary the financial penalties associated with detected misconduct may no longer provide a deterrent (for further discussion see [Block and Lind, 1975](#)).

In this section, we consider an alternative possibility — that real estate shocks result in “passive” misconduct through inattention or negligence. Passive misconduct could increase following real estate shocks if the advisor becomes distracted and less effective at work due to financial pressure (e.g., [Maturana and Nickerson, 2018](#), show that students’ standardized test scores suffer when their teacher undergoes financial distress). Note that active and passive misconduct are not mutually exclusive, and both could occur even for the same individual.

We separate misconduct into active and passive categories by parsing the text fields in the advisors’ disclosure statements and classifying misconduct based on key words. *Active Misconduct* includes misrepresentation, unauthorized trading, fee or commission related misconduct, churning, and fraud. These are acts of commission (intentional actions) that will enrich the advisor if undetected. *Passive Misconduct* includes negligence and omission of key facts. These include acts of omission, and are more consistent with carelessness or inattention instead of enrichment. There are some categories of misconduct, such as unsuitability and violations of fiduciary duty, that we do not classify. Of advisor-years with misconduct events, 53.6% are classified as active, 17.0% as passive, and the remainder cannot be unambiguously classified.

In column (1) of [Table 2.8](#) the dependent variable includes only *Active Misconduct*. The coefficient on *Cumulative Return* is significant and negative. The result shows that, following a decline in the value of their home, advisors are more likely to commit active misconduct — taking deliberate actions to exploit customers for financial gain. The results in column (2) show there is also evidence of passive misconduct.

2.8 Robustness Tests

2.8.1 Alternative Types of Misconduct

In this subsection we present several robustness tests that are similar to the baseline specification, but use alternative definitions of misconduct. In column (3) of [Table 2.8](#) the dependent variable is an indicator variable equal to one if the advisor committed misconduct involving mutual funds. Mutual funds are a relatively straightforward financial product, and are typically regulated and distributed at the national level. In contrast, other financial products may vary across states due to regulator differences (e.g., annuities and some insurance products) or may be distributed primarily in a limited geographic area (e.g., local micro-cap stocks). Limiting the dependent variable to include only mutual funds reduces the possibility that some type of assortative matching of local products to local customers could confound our analysis. Consistent with the baseline specification, column (3) finds a significant negative coefficient on *Cumulative Return*.

(2017).

In column (4) of Table 2.8 the dependent variable is set equal to one if there is an incident of misconduct in which the damages exceed \$100,000. A potential concern with the results is that the advisors' actions are unrelated to their own real estate returns but that customers become more likely to file complaints following negative real estate shocks (given the fixed effects, this concern would also require within-ZIP-year commonality in real estate returns between advisors and customers). This possibility is most plausible for borderline cases with relatively small dollar damages, as it is unlikely that customers would tolerate severe misconduct regardless of real estate wealth. The results show that, even with this restrictive definition of misconduct, there is a significant negative relation between the advisor's cumulative real estate returns and large cases of misconduct.

In column (5) of Table 2.8, we define misconduct following Egan, Matvos, and Seru (2018a) who use a broader measure of misconduct that, in addition to customer complaints, also includes regulatory actions, terminations by an employing firm, and criminal and civil disclosures. The results are similar to those in the baseline specification. Overall, the results in Table 2.8 show that advisors who suffer declines in the value of their home are more likely to commit misconduct of all types.

2.8.2 Imputed Housing Returns and Advisor Financial Distress

Throughout the paper we use imputed house price returns to measure wealth shocks to financial advisors. As a validation test of this measure, we test whether imputed house price returns predict actual financial distress. FINRA requires financial advisors to disclose any bankruptcy filing or other "compromise with creditors" in which "a creditor agrees to accept less than the full amount owed"³² for a 10 year period (because advisors no longer need to disclose these events after 10 years, for these tests the sample period begins in 2008). We use these disclosures to create two measures of financial distress.

In column (1) of Table 2.9, the dependent variable is an indicator equal to one if the advisor files for bankruptcy in the next year. Aside from this change to the dependent variable, the specification is identical to the baseline specification. The results show that advisors with worse cumulative housing returns are significantly more likely to declare bankruptcy. Further, the implied economic magnitudes of the estimates are large relative to the baseline rate of bankruptcy; the coefficient estimate implies that a one standard deviation decline in *Cumulative Return* is associated with an 0.11 percentage point increase in the probability of bankruptcy (a 36.4% increase relative to the baseline rate of bankruptcy).

In column (2) of Table 2.9, the dependent variable is an indicator equal to one if the advisor discloses a "compromise with creditors" related to an underwater sale (a house sale in which the proceeds are less than the debts secured by the property and the advisor does not pay the lender the difference) in the next year. This provides a very direct measure of financial distress related to housing returns. The results show that advisors with worse cumulative house returns are significantly more likely

³²See www.finracompliance.com/wp-content/uploads/2015/04/Finra-U4-U5-QnA.pdf for a more detailed definition of the reporting requirements.

to disclose underwater home sales. Further, the implied magnitudes are large; the coefficient estimate implies that a one standard deviation decline in *Cumulative Return* is associated with a 0.14 percentage point increase in underwater sales (a 130.4% increase relative to the baseline rate). Overall, the results in Table 2.9 provide evidence that our imputed measure of housing price returns captures meaningful wealth shocks for financial advisors.

2.9 Conclusion

We test whether household level financial shocks affect the propensity of employees to engage in financial misconduct. We measure financial shocks using financial advisors' housing price declines and measure misconduct using validated customer complaints disclosed in mandatory regulatory filings. We find that advisors who suffer large housing price declines become significantly more likely to commit misconduct — and that many of these misconduct events are willful actions such as churning and unauthorized trading. We also show that advisors' housing returns explain misconduct targeting out-of-state customers, breaking the link between customer and advisor housing shocks.

Our findings show that the willingness to commit misconduct is a pliable characteristic of the individual; advisors are more likely to commit misconduct when they are under financial pressure. The results provide useful information for firms designing compliance and monitoring systems and for regulators allocating limited auditing resources.

Our finding that misconduct increases following negative wealth shocks suggests important welfare implications. Although our empirical tests difference out aggregate time-series effects, to the extent wealth effects in misconduct are generalizable to aggregate wealth shocks, there are two implications. First, the willingness to commit misconduct will rise in states of the world in which marginal utility is highest, making the direct losses from misconduct particularly painful. Second, misconduct erodes trust and causes people to exit markets (see [Giannetti and Wang, 2016](#); [Gurun, Stoffman, and Yonker, 2018](#)) precisely when expected returns are the highest.

Table 2.1: Advisors and Disclosures

This table summarizes misconduct disclosures that financial advisors are required to make. The first column reports the number of advisors with the associated misconduct measure. The second and third columns report the percent of offending advisors and advisor-years, respectively. *Misconduct* is defined using customer disputes as in [Dimmock, Gerken, and Graham \(2018\)](#) and is comprised of customer disputes that either receive an award in arbitration or are settled for at least a certain dollar value (\$10,000 or \$15,000 depending upon the time period). *Out-of-State Misconduct* is defined using customer disputes only from out-of-state customers. *Regulatory* is a disclosure of finalized regulatory sanctions from entities such as the SEC, FINRA, or state regulators. *Employment Separation After Allegations* reports whether the advisor has ever been terminated or permitted to resign following allegations of misconduct. *EMS Misconduct* is defined using a broader set of disclosures as in [Egan, Matvos, and Seru \(2018a\)](#) and includes *Misconduct*, *Regulatory*, and *Employment Separation After Allegations*, as well as certain civil law and criminal disclosures. *Bankruptcy* is disclosures of bankruptcies by financial advisors. *Underwater Sale* is disclosures that pertain specifically to an underwater sale of a property. *Bankruptcy* and *Underwater Sale* are only available for the years 2008 through 2017. *Leave Firm* is an indicator variable set to one when an advisor leaves the firm for any reason.

	Advisors (#)	Advisors (%)	Advisor-Years (%)
Misconduct	14,691	4.66	0.63
Out-of-State Misconduct	4,016	1.28	0.16
Regulatory	2,535	0.80	0.10
Employment Separation After Allegations	3,420	1.09	0.12
EMS Misconduct	19,232	6.11	0.85
Bankruptcy	4,773	1.55	0.31
Underwater Sale	1,643	0.53	0.11
Leave Firm	204,679	64.98	13.97

Table 2.2: Summary Statistics

This table reports summary statistics. Panel A reports statistics regarding advisor misconduct. Panel B reports statistics regarding advisor residency and experience for the 2,860,572 advisor-year observations. Panel C summarizes the distribution of house price changes.

<i>Panel A: Advisor Misconduct</i>	1%	25%	Median	Mean	75%	99%	
Alleged Damages from Customer Disputes (\$)	0	5,000	19,000	659,666	100,000	3,500,000	
Settle Amount for Customer Disputes (\$)	0	2,870	15,754	239,485	59,347	3,100,000	
Regulatory Damages (\$)	100	1,000	5,000	13,125	10,000	200,000	
<i>Panel B: Advisor-Year Variables</i>	1%	25%	Median	Mean	75%	99%	
Industry Experience (years)	1	5	10	11.8	17	37	
Firm Tenure (years)	0	2	4	5.9	8	27	
House Purchase Price (\$)	65,800	153,100	229,700	320,539.0	376,300	1,502,300	
Zillow ZIP Price (\$)	73,800	176,000	268,900	373,678.6	445,700	1,714,400	
Years at Residence	1	3	5	5.8	8	22	
Number of Residences	1	1	1	1.7	2	5	
<i>Panel C: Distribution of House Price Changes</i>	1%	5%	10%	Median	90%	95%	99%
Annual Dollar Change	-91,300.0	-42,600.0	-26,000.0	3,700.0	49,000.0	76,400.0	164,100.0
Annual Percent Change	-18.1	-10.7	-7.6	1.7	13.7	17.8	26.7
Cumulative Dollar Change	-212,200.0	-97,900.0	-54,000.0	23,500.0	198,900.0	303,800.0	618,800.0
Cumulative Percent Change	-37.9	-20.9	-13.1	9.7	64.1	92.1	150.0

Table 2.3: Housing Price Shocks and Misconduct — Crisis Differences-in-Differences

This table reports OLS estimates of differences-in-differences regressions around the 2008 financial crisis. The dependent variable is the difference in the number of advisor misconduct incidences between the post-event period and the pre-event period. The pre-event period is a three-year window from 2005 to 2007. The post-event period is a three-year window from 2008 to 2010. The unit of observation is the advisor. *Crisis Price Change* is the 2008 house price return in the advisor's ZIP code. *Crisis Price Drop (X%)* is an indicator variable equal to 1 if the percentage change in house prices in the advisor's ZIP code decreases at least X%. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. All specifications include firm fixed effects and length at residency decile fixed effects. Standard errors are clustered by ZIP code. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Δ Misconduct			
	(1)	(2)	(3)	(4)
Crisis % Price Change	-0.0509*** (0.0121)			
Crisis Price Drop (5%)		0.0055*** (0.0013)		
Crisis Price Drop (10%)			0.0061*** (0.0015)	
Crisis Price Drop (15%)				0.0067*** (0.0020)
$\log(\text{Industry Experience})$	0.0050*** (0.0011)	0.0050*** (0.0011)	0.0050*** (0.0011)	0.0050*** (0.0011)
Firm FE	Yes	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes	Yes
R^2	0.025	0.025	0.025	0.025
Observations	248,432	248,432	248,432	248,432

Table 2.4: Cumulative House Price Return and Misconduct

This table reports estimates from regressions of misconduct on an advisor’s cumulative house price return since purchase. The unit of observation is the advisor-year. The dependent variable is an indicator variable for advisor misconduct (multiplied by 100). *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. In column (3), the specification also includes an interaction term, $(\text{Cumulative Return} - (-20\%)) \times I_{\text{Extreme}}$, where I_{Extreme} indicates a cumulative return worse than -20%. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. Specification (1) includes advisor control variables and firm×year, ZIP code, and length at residency decile fixed effects. The latter specifications replace the advisor controls with advisor fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Misconduct		
	(1)	(2)	(3)
Cumulative Return	-0.0847*** (0.0205)	-0.2082*** (0.0303)	-0.1951*** (0.0310)
Cumulative Return× I_{Extreme}			-0.4664* (0.2777)
$\log(\text{Industry Experience})$	0.2382*** (0.0079)	0.4914*** (0.0219)	0.4916*** (0.0219)
Advisor Controls	Yes	No	No
Advisor FE	No	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes
R^2	0.032	0.146	0.146
Observations	2,882,302	2,860,572	2,860,572

Table 2.5: Addressing Concerns about Commonality in Customer and Advisor Shocks

This table reports estimates from regressions of measures of misconduct on an advisor’s cumulative house price return since purchase. The unit of observation is the advisor-year. The dependent variable is an indicator variable for advisor misconduct in a year (multiplied by 100). *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. In all columns, the specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. In column (1), the specification also includes ZIP \times year fixed effects. In column (2), the specification also includes branch \times year fixed effects. In column (3), the specification also includes branch \times year \times ZIP fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Misconduct		
	(1)	(2)	(3)
Cumulative Return	-0.1574*** (0.0374)	-0.1268*** (0.0385)	-0.1707** (0.0796)
$\log(\text{Industry Experience})$	0.5016*** (0.0229)	0.5125*** (0.0283)	0.4651*** (0.0509)
Advisor FE	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
Firm \times Year FE	Yes	Subsumed	Subsumed
ZIP FE	Subsumed	Yes	Subsumed
ZIP \times Year FE	Yes	No	Subsumed
Branch \times Year FE	No	Yes	Subsumed
Branch \times Year \times ZIP FE	No	No	Yes
R^2	0.188	0.241	0.491
Observations	2,833,467	2,312,551	1,076,138

Table 2.6: Out-of-State Customers, Non-Customer Misconduct, and Advisor Shocks

This table reports estimates from regressions of measures of misconduct on an advisor's cumulative house price return since purchase. The unit of observation is the advisor-year. In column (1), the dependent variable equals one if advisor misconduct is reported by clients who live in a different state than the advisor. In column (2), the dependent variable equals one if advisor misconduct is reported by clients who live in a state that does not border the advisor's state. In column (3), the dependent variable equals one if a regulator or firm reported incidences of misconduct. In column (3), we exclude sample observations if a client also reports an incident of misconduct in the same year. In each column, we multiply the dependent variable by 100. *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. In all columns, the specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. In column (3), the specification also includes state \times year fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Out-of-State Customer (1)	Distant Customer (2)	Regulator or Firm Actions (3)
Cumulative Return	-0.0463*** (0.0149)	-0.0351*** (0.0126)	-0.0441** (0.0181)
$\log(\text{Industry Experience})$	0.1064*** (0.0100)	0.0736*** (0.0084)	0.0643*** (0.0134)
Advisor FE	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes
State \times Year FE	No	No	Yes
R^2	0.140	0.140	0.179
Observations	2,860,572	2,860,604	2,842,344

Table 2.7: Cross-Sectional Variation in Termination Risk

This table reports estimates from regressions of misconduct on an advisor's cumulative house price return since purchase, interacted with indicator variables. The unit of observation is the advisor-year. The dependent variable is an indicator variable for advisor misconduct (multiplied by 100). *High Firm-Year* is an indicator variable set to one if the firm-year has an above average misconduct rate (excluding the advisor's own misconduct). *High Branch-Year* is an indicator variable set to one if the branch-year has an above average misconduct rate (excluding the advisor's own misconduct). *Branch* is defined as a firm-county. *Female* is an indicator variable for gender. *Prior Misconduct* is set to one if the advisor has ever had an incidence of misconduct in the past. All specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Cumulative Return	-0.1641*** (0.0306)	-0.1347*** (0.0310)	-0.2839*** (0.0355)	-0.0669*** (0.0187)
High Firm-Year	5.1985*** (0.3923)			
C. Return \times High Firm-Year	-0.0849* (0.0467)			
High Branch-Year		0.0671*** (0.0239)		
C. Return \times High Branch-Year		-0.1559*** (0.0447)		
C. Return \times Female			0.3020*** (0.0423)	
Prior Misconduct				2.2214*** (0.0647)
C. Return \times Prior Misconduct				-0.3556*** (0.1265)
log(Industry Experience)	0.4892*** (0.0219)	0.4900*** (0.0219)	0.4922*** (0.0220)	0.2696*** (0.0066)
Advisor FE	Yes	Yes	Yes	No
Length at Residency FE	Yes	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
R^2	0.146	0.146	0.146	0.031
Observations	2,860,572	2,860,572	2,843,273	2,882,302

Table 2.8: Alternative Measures of Misconduct

This table reports results from regressions with alternative measures of misconduct as the dependent variables. *Mutual Fund* is an indicator variable if the advisor had misconduct related to mutual funds. In column (2), the dependent variable is a misconduct indicator in which the damages exceed \$100,000. *EMS* is the misconduct measure from Egan, Matvos, and Seru (2018a). In each column, we multiply the dependent variable by 100. The unit of observation is the advisor-year. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. All specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Mutual Fund (1)	$\geq 100k$ (2)	EMS (3)
Cumulative Return	-0.0767*** (0.0230)	-0.0795*** (0.0179)	-0.2584*** (0.0345)
$\log(\text{Industry Experience})$	0.4694*** (0.0169)	0.1724*** (0.0127)	0.5469*** (0.0261)
Advisor FE	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes
R^2	0.126	0.135	0.163
Observations	2,860,572	2,860,572	2,860,572

Table 2.9: Predicting Real Outcomes

This table tests whether our cumulative return measure reliably predicts negative financial outcomes for advisors. The unit of observation is the advisor-year. In column (1), the dependent variable, *Bankruptcy*, is an indicator equal to one if the advisor reports a bankruptcy or other compromise with creditors. In column (2), the dependent variable, *Underwater Sale*, is an indicator equal to one if the advisor reports an underwater home sales (for which the advisor does not make the lender whole). *Termination* is an indicator equal to one if the advisor was terminated or permitted to resign following allegations of misconduct. *Leave Firm* is an indicator equal to one if the advisor left the firm for any reason. In all columns, the dependent variable is multiplied by 100. For data availability reasons, columns (1) and (2) are only estimated starting in 2008. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. All specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Bankruptcy (1)	Underwater Sale (2)	Termination (3)	Leave Firm (4)
Cumulative Return	-0.3120*** (0.0703)	-0.3962*** (0.0536)	-0.0247** (0.0114)	-1.7784*** (0.1205)
$\log(\text{Industry Experience})$	-0.1137** (0.0515)	0.0939*** (0.0300)	0.0603*** (0.0100)	-1.0217*** (0.1099)
Advisor FE	Yes	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes	Yes
Firm \times Year FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
R^2	0.263	0.274	0.140	0.380
Observations	1,272,337	1,272,337	3,486,167	3,486,167

Figure 2.1: Real Estate Price Shocks by Year

This figure plots the fraction of financial advisors whose residence experiences a year-over-year decline in price of at least 10% during the sample period, as measured by the Zillow ZIP code level house price index. We plot this measure separately for the entire U.S. (black) and select states.

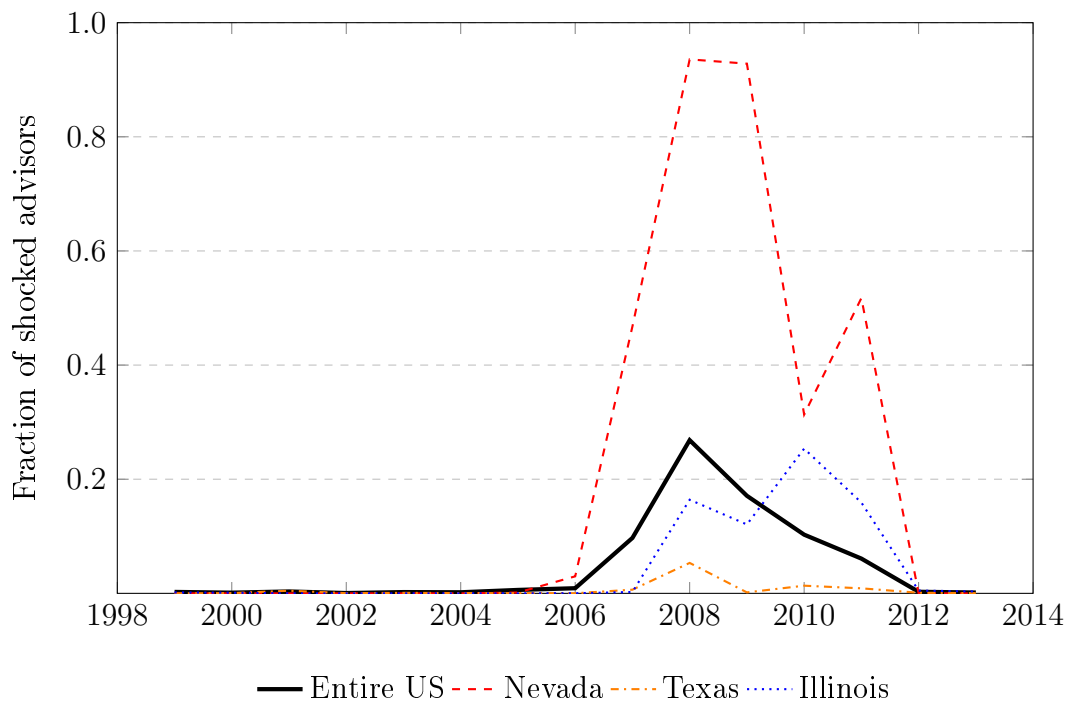


Figure 2.2: Crisis Price Changes in Metro Atlanta

This figure displays price declines by ZIP code in 2008 for the Atlanta, GA metropolitan statistical area. ZIP codes are color coded by level of 2008 loss where darker shades indicate more severe losses.

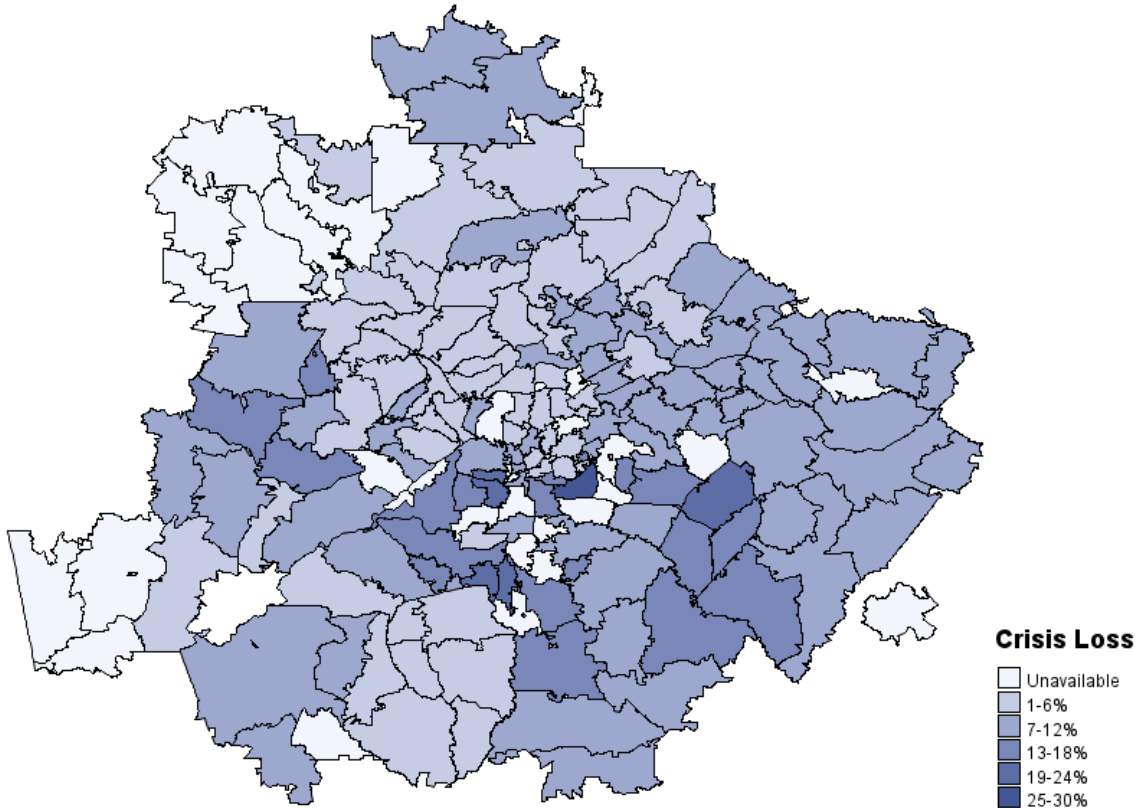


Figure 2.3: Misconduct by Ventile of Annual House Price Changes

This figure plots the misconduct rate of advisors by ventile of annual housing price changes, as measured by the Zillow ZIP code level house price index. The grey shaded area around the plot is the 95% confidence interval. The dashed line marks the unconditional average of misconduct (0.63%). Misconduct is measured as in [Dimmock, Gerken, and Graham \(2018\)](#) and includes customer disputes that are either settled for a non-trivial amount or awarded in favor of the customer.

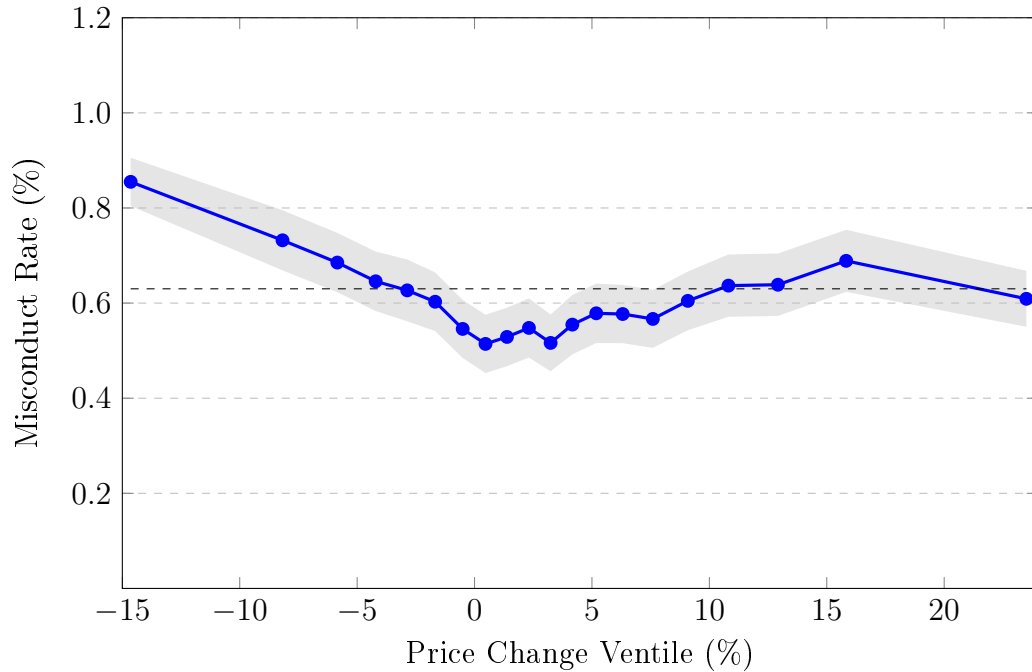


Figure 2.4: Misconduct Timing around Real Estate Shocks

This figure plots the average misconduct rate in event time, where the event is a 10% or worse decline in an advisor's house price, as measured by the Zillow ZIP code level house price index. The figure plots three years before and three years after the event. The grey shaded area around the plot is the 95% confidence interval. The dashed line marks the unconditional average of misconduct (0.63%). Misconduct is measured as in [Dimmock, Gerken, and Graham \(2018\)](#) and includes customer disputes that are either settled for a non-trivial amount or awarded in favor of the customer.

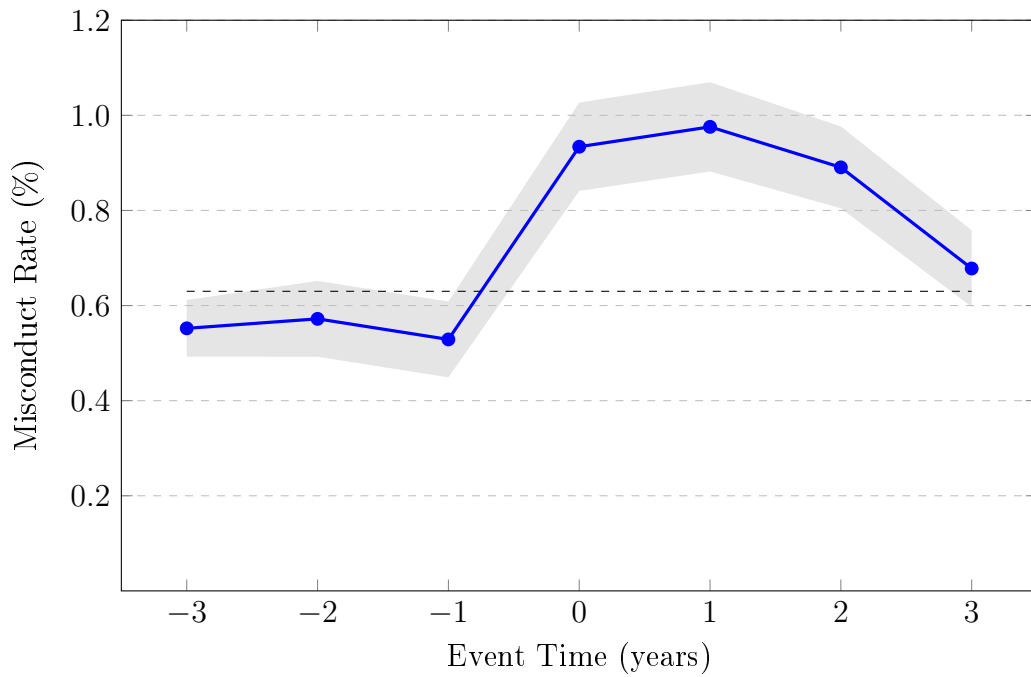
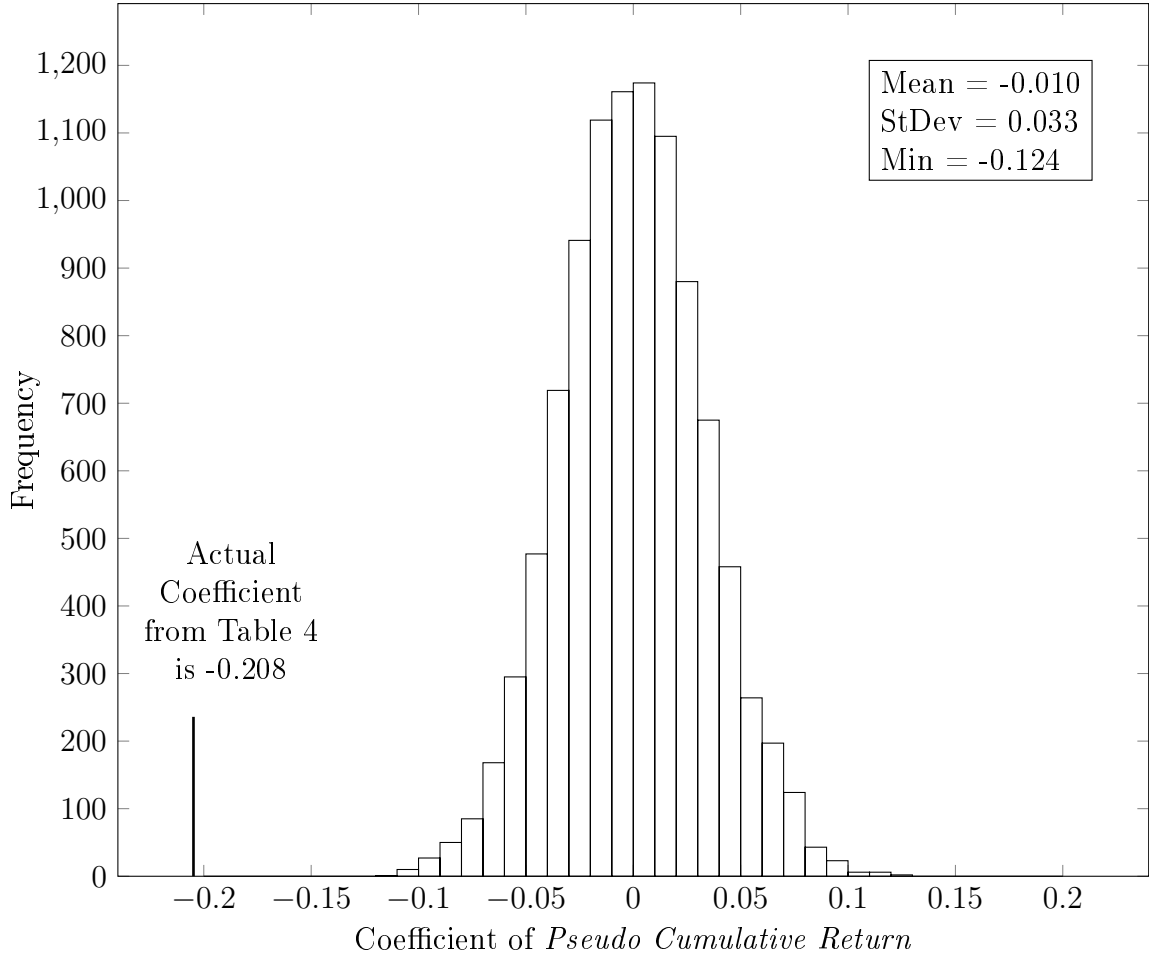
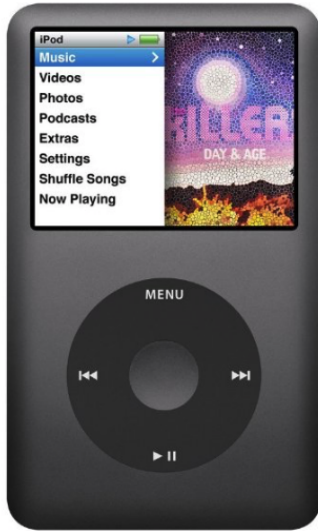


Figure 2.5: Coefficient of *Pseudo Cumulative Return* in Placebo Samples

The figure shows a histogram of *Pseudo Cumulative Return* coefficients from 10,000 iterations of the model in Table 2.4, column (2). For each iteration, each ZIP code is randomly assigned the returns of another out-of-state ZIP code (creating new counterfactual values for *Pseudo Cumulative Return*). The model is re-estimated using the counterfactual *Pseudo Cumulative Return* values. All other advisor characteristics remain the same.



Appendix A: Example Observation for Chapter 1



Apple MC297LL/A iPod Classic MP3/MP4 Player 160GB Black (Manufacturer)

by Apple

★★★★☆ 3,324 customer reviews | 412 answered questions

Price: ~~\$449.99~~ ✓Prime

Note: Available at a lower price from [other sellers](#), potentially without free Prime shipping.

In Stock.

Sold by [COOL-TEE](#) and Fulfilled by Amazon. Gift-wrap available.

Color: **Black**

Product Packaging: **Standard Packaging**

- 2.5-inch LED-backlit display
- 160 GB Storage Capacity (Estimated Free Space 142 GB) for 40,000 songs, 25,000 photos, or 200 hours of video playback when fully charged
- 320 x 240 pixel resolution
- Supports AAC, Protected AAC, MP3, MP3 VBR, Audible, Apple Lossless, AIFF, and WAV audio for
- Highly recyclable aluminum and stainless steel enclosure

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Used & new (118) from ~~\$173.77~~ ✓Prime

[Report incorrect product information.](#)



Don't Know What To Do With Your Old iPod?

Trade in your mp3 player for an Amazon.com Gift Card that can be redeemed for million

1,651 of 1,696 people found the following review helpful

★★★★★ **My favorite iPod to date. (A.K.A. The iPod Apple should've released in 2007.)**

By [Alex](#) on September 16, 2009

Color Name: Silver

The new 160GB iPod Classic is easily Apple Inc.'s best iPod to date, and out of all of the iPods that I own, this is my favorite.

First, the capacity of this iPod is simply unbeatable. I've yet to see another portable media player that can match the iPod Classic in capacity. I have a huge music library, and it's nice to be able to carry every song that I own on my person at all times. What's more, thanks to the iPod Classic's capacity, I also have room to carry a few videos with me, and some of my photos. If you don't like having to pick which songs to load onto your portable media player, the iPod Classic is the way to go.

The second thing that I love about this iPod can be summed up in two words: it works. The 160GB iPod Classic that was introduced in 2007 was extremely buggy, had a non-responsive Clickwheel on many units, crashed frequently, and required a hit-and-miss firmware update to stop the hard drive from spinning even when the device was "off," which often lead to dead batteries. All of these problems left the 2007 160GB iPod Classic warming shelves and earning it the infamous "honor" of being the "worst selling iPod ever," according to Apple. I'm pleased to say that the new 160GB iPod Classic released earlier this month has virtually none of these problems. There's no "spinning hard drive bug," the Clickwheel is incredibly responsive, and the device isn't crash-prone. While it's true that many of these issues were fixed with last year's iPod Classic, there hasn't been a truly functional 160GB model until now. To put it bluntly, this is the iPod that Apple should've released in 2007. [Read more >](#)

Chapter 1 Internet Appendix

Table IA1.1: Details on Estimates for All Control Variables

This table presents all results from estimating Equation (1.1) and its variants. See Table 1.3. The dependent variable in columns (1) and (2) is measured as the average number of stars (out of 5) assigned to the product in the subsequent year. Columns (3) and (4) use reputation as measured by the *Helpful* reviews. *Any Vesting Event* is an indicator variable set to 1 in any year in which the firm's CEO has either a stock vesting event or an option vesting event. *Any Stock Vesting* and *Any Option Vesting* decompose *Any Vesting Event* accordingly. See the data section for detailed variable definitions. Control variables are included in the specifications and defined in the text. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered at the product level, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}		Helpful Reputation _{t+1}	
	(1)	(2)	(3)	(4)
Any Vesting Event	-0.067*** (0.018)		-0.070*** (0.017)	
Stock Vesting		-0.101*** (0.020)		-0.098*** (0.020)
Option Vesting		0.022 (0.029)		-0.008 (0.031)
Book-to-Market	0.061 (0.041)	0.062 (0.041)	-0.011 (0.044)	-0.011 (0.044)
Operating Margin	0.061 (0.106)	0.067 (0.106)	0.115 (0.115)	0.119 (0.115)
Leverage	-0.150 (0.117)	-0.137 (0.117)	-0.163 (0.123)	-0.154 (0.123)
LN(Market Cap)	-0.069*** (0.019)	-0.068*** (0.019)	-0.052** (0.023)	-0.052** (0.023)
12-month Return	0.010 (0.014)	0.008 (0.014)	0.005 (0.014)	0.004 (0.014)
R&D	0.798** (0.311)	0.854*** (0.313)	1.167*** (0.380)	1.210*** (0.384)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R ²	0.063	0.063	0.066	0.067
Observations	52,924	52,924	50,168	50,168

Table IA1.2: Vesting Events for Any Executive

This table presents the results from estimating Equation (1.1). The dependent variable in columns (1) and (2) is measured as the average number of stars (out of 5) assigned to the product in the subsequent year. The second and third sets of specifications use two-year ahead product market reputation and the reputation as measured by the *Helpful* reviews, respectively. *Any Vesting Event* is an indicator variable set to 1 in any year in which any of the firm executives have either a stock vesting event or an option vesting event. *Any Stock Vesting* and *Any Option Vesting* decompose *Any Vesting Event* accordingly. See the data section for detailed variable definitions. Control variables are included in the specifications and defined in the text. Indicators for fixed effects are reported at the bottom of the table, and standard errors, clustered at the product level, are reported in parentheses. Asterisks represent the conventional levels of statistical significance.

	Reputation _{t+1}		Reputation _{t+2}		Helpful Reputation _{t+1}	
	(1)	(2)	(3)	(4)	(5)	(6)
Any Vesting Event (All Execs)	-0.046*** (0.014)		-0.044*** (0.016)		-0.032** (0.013)	
Any Stock Vesting (All Execs)		-0.058*** (0.015)		-0.070*** (0.017)		-0.033** (0.015)
Any Option Vesting (All Execs)		0.008 (0.025)		0.044 (0.030)		-0.026 (0.025)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.063	0.063	0.064	0.064	0.066	0.066
Observations	52,924	52,924	40,646	40,646	50,168	50,168

Chapter 2 Internet Appendix

Table IA2.1: Home Ownership

This table reports estimates from regressions of misconduct on an advisor's cumulative house price change since purchase for subsamples of our dataset. The unit of observation is the advisor-year. The dependent variable is an indicator variable for advisor misconduct in the current year (multiplied by 100). *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. In column (1), we exclude ZIP codes with low levels of home ownership (<50%) as reported in the American Community Survey. In column (2), we exclude multi-dwelling unit (MDU) addresses (e.g., apartment buildings). $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. All specifications include advisor, firm \times year, length at residency decile, and ZIP code fixed effects. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Misconduct	
	(1)	(2)
Cumulative Return	-0.2054*** (0.0364)	-0.2076*** (0.0332)
log(Industry Experience)	0.5174*** (0.0249)	0.5007*** (0.0240)
Advisor FE	Yes	Yes
Length at Residency FE	Yes	Yes
Firm \times Year FE	Yes	Yes
ZIP FE	Yes	Yes
Exclude Low Ownership ZIPs	Yes	No
Exclude MDUs	No	Yes
R-squared	0.151	0.151
Observations	2,342,187	2,522,052

Table IA2.2: Federal Housing Price Index

This table reports estimates from regressions of misconduct on an advisor’s cumulative house price return since purchase. The unit of observation is the advisor-year. Column (1) reports OLS estimates of differences-in-differences regressions around the 2008 financial crisis. The dependent variable is the difference in the number of advisor misconduct incidences between the post-event period and the pre-event period. The pre-event period is a three-year window from 2005 to 2007. The post-event period is a three-year window from 2008 to 2010. The dependent variable for columns (2) and (3) is an indicator variable for advisor misconduct (multiplied by 100). *Crisis Price Change* is the 2008 house price return in the advisor’s ZIP code. *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. Both measures of house price returns use the House Price Index produced by the Federal Housing Finance Agency instead of Zillow estimates. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. The presence of control variables and fixed effects is indicated at the bottom of the table. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Δ Misconduct (1)	Misconduct (2)	Misconduct (3)
Crisis Price Change	-0.0491*** (0.0106)		
Cumulative Return		-0.0427*** (0.0118)	-0.0706*** (0.0210)
$\log(\text{Industry Experience})$	0.0046*** (0.0011)	0.2473*** (0.0067)	0.4952*** (0.0181)
Advisor Controls	No	Yes	No
Advisor FE	Yes	No	Yes
Firm FE	Yes	Subsumed	Subsumed
Firm \times Year FE	No	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
ZIP FE	No	Yes	Yes
R ²	0.025	0.029	0.131
Observations	252,183	3,737,534	3,716,283

Table IA2.3: Addressing Multiple Residences

This table reports estimates from regressions of misconduct on an advisor's cumulative house price return since purchase. The unit of observation is the advisor-year. In columns (1) and (2), we calculate the cumulative return using only the highest value residence when an advisor owns more than one property in a year. In columns (3) and (4), we exclude all advisor-year observations in which the advisor reports multiple residences. The dependent variable is an indicator variable for advisor misconduct (multiplied by 100). *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. The presence of control variables and fixed effects is indicated at the bottom of the table. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Misconduct			
	(1)	(2)	(3)	(4)
Cumulative Return	-0.0779*** (0.0219)	-0.1816*** (0.0304)	-0.0652** (0.0289)	-0.2038*** (0.0428)
$\log(\text{Industry Experience})$	0.2416*** (0.0080)	0.5061*** (0.0228)	0.2474*** (0.0103)	0.5798*** (0.0312)
Advisor Controls	Yes	No	Yes	No
Advisor FE	No	Yes	No	Yes
Length at Residency FE	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
Highest Value Residence	Yes	Yes	No	No
Omit Multiple Residences	No	No	Yes	Yes
R ²	0.033	0.149	0.038	0.169
Observations	2,730,735	2,709,002	1,689,573	1,667,363

Table IA2.4: Alternate Differences in Differences Specifications

This table reports OLS estimates of differences-in-differences regressions around the financial crisis. The dependent variable is the difference in the number of advisor misconduct incidences between the post-event period and the pre-event period. In column (1), we follow [Pool, Stoffman, Yonker, and Zhang \(2018\)](#), who define the pre-period as 2005–2006 and the post-period as 2009–2010. In column (2), we follow [Bernstein, McQuade, and Townsend \(2018\)](#), who define the pre-period as 2005–2007 and the post-period as 2008–2012. Column (3) is similar to our main specification in Table 3, except that we introduce a gap year: the pre-event period is a three-year window from 2005 to 2007, and the post-event period is a three-year window from 2009 to 2011. The unit of observation is the advisor. *Crisis Price Change* is the 2008 house price return in the advisor’s ZIP code. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. All specifications include firm fixed effects and length at residency decile fixed effects. Standard errors are clustered by ZIP code. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Δ Misconduct		
	PSYZ (1)	BMT (2)	Gap Year (3)
Crisis Price Change	-0.0289*** (0.0103)	-0.0678*** (0.0147)	-0.0366*** (0.0120)
$\log(\text{Industry Experience})$	0.0022** (0.0009)	0.0119*** (0.0014)	0.0035*** (0.0011)
Firm FE	Yes	Yes	Yes
Length at Residency FE	Yes	Yes	Yes
R ²	0.022	0.030	0.027
Observations	248,432	247,514	248,432

Table IA2.5: Annual Returns

This table reports estimates from regressions of misconduct on an advisor's annual house price return. The unit of observation is the advisor-year. The dependent variable is an indicator variable for advisor misconduct (multiplied by 100). *Annual Return* is the annual price change divided by the house price at the beginning of the year. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. The presence of control variables and fixed effects is indicated at the bottom of the table. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Misconduct	
	(1)	(2)
Annual Return	-0.1937** (0.0775)	-0.2364*** (0.0804)
log(Industry Experience)	0.2369*** (0.0067)	0.4809*** (0.0183)
Advisor Controls	Yes	No
Advisor FE	No	Yes
Length at Residency FE	Yes	Yes
Firm×Year FE	Yes	Yes
ZIP FE	Yes	Yes
R ²	0.029	0.132
Observations	3,910,975	3,890,805

Table IA2.6: One-Year Ahead & Three-Year Misconduct

This table reports estimates from regressions of misconduct on an advisor’s cumulative house price return since purchase. The unit of observation is the advisor-year. In columns (1) and (2), the dependent variable is an indicator variable for advisor misconduct (multiplied by 100) in the subsequent year. In columns (3) and (4), the dependent variable is an indicator variable for advisor misconduct (multiplied by 100) in the three year window from $t=0$ to $t=2$. *Cumulative Return* is the aggregated cumulative price change divided by the purchase price. $\log(\text{Industry Experience})$ is the logarithm of the number of years an advisor has worked in the industry. The presence of control variables and fixed effects is indicated at the bottom of the table. Standard errors, clustered by advisor and ZIP code, are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable:	Misconduct ₁		Misconduct ₀₋₂	
	(1)	(2)	(3)	(4)
Cumulative Return	-0.0516** (0.0207)	-0.1451*** (0.0306)	-0.1454*** (0.0508)	-0.3605*** (0.0691)
log(Industry Experience)	0.2006*** (0.0082)	0.4569*** (0.0224)	0.5668*** (0.0209)	1.2585*** (0.0519)
Advisor Controls	Yes	No	Yes	No
Advisor FE	No	Yes	No	Yes
Length at Residency FE	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes
ZIP FE	Yes	Yes	Yes	Yes
R ²	0.031	0.149	0.054	0.326
Observations	2,882,302	2,860,572	2,882,302	2,860,572

Table IA2.7: Estimates for All Control Variables

This table reports the full control variable estimates from the regression of misconduct on an advisor's cumulative house price return since purchase from Column (1) of Table 2.4. See Table 2.4 for details.

Dependent Variable:	Misconduct
Cumulative Return	-0.0847*** (0.0205)
log(Industry Experience)	0.2382*** (0.0079)
Female	-0.3237*** (0.0106)
Age	-0.0014** (0.0007)
Series 6	0.0141 (0.0157)
Series 7	0.0956*** (0.0168)
Series 24	-0.0891*** (0.0168)
Series 65	0.2749*** (0.0163)
Series 66	0.1010*** (0.0147)
Firm×Year, ZIP, Missing Demographic & Length at Residency FE	Yes
R^2	0.032
Observations	2,882,302

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- [SSRN link](#)
- Presented at the Midwest Finance Association, Eastern Finance Association (scheduled), Financial Management Association Doctoral Student Consortium, Financial Management Association Special PhD Paper Presentations, James Madison University, Southern Illinois University Carbondale, and the University of Kentucky.

Real Estate Shocks and Financial Advisor Misconduct

- with [Stephen G. Dimmock](#) and [William C. Gerken](#)
- [SSRN link](#)
- Presented at the Western Finance Association*, Washington Summer Finance Conference*, European Finance Association*, NTU Finance Conference*, CEAR-RSI Household Finance Workshop*, Financial Management Association, Financial Intermediation Research Society, West Virginia University*, Hong Kong University of Science and Technology*, University of Kentucky, University of New South Wales, Cambridge University*, and the Commodity Futures Trading Commission. (* indicates presentation by coauthor)
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PROFESSIONAL ACTIVITIES

Referee

- 2019: Journal of Pension Economics and Finance, Pacific-Basin Finance Journal
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- 2019: Midwest Finance Association, Financial Intermediation Research Society (scheduled), Eastern Finance Association (scheduled)
- 2018: Financial Management Association, FMA Special PhD Paper Presentations, FMA Doctoral Student Consortium, Appalachian State University, Commodity Futures Trading Commission, James Madison University, Southern Illinois University Carbondale, University of Kentucky

Discussions

- 2019: Eastern Finance Association (scheduled)

- 2016: Financial Management Association
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Conference Participation

- 2019: American Finance Association, Midwest Finance Association, Eastern Finance Association (scheduled), Financial Intermediation Research Society (scheduled), Financial Management Association (scheduled)
- 2018: Western Finance Association, Financial Management Association
- 2017: American Finance Association, Financial Management Association, Commonwealth Computational Summit
- 2016: American Finance Association, Financial Management Association
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TEACHING EXPERIENCE

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- FIN 410: Investment Analysis
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- Introduction to Web Scraping with Python (PhD) 2015
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- ECN 6330: Applied Econometrics (MBA) Summer 2013
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AWARDS

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Lockett Fellowship	2018
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