



2016

Brokers and Investment Advisors

Nathaniel Phillip Graham

University of Kentucky, npgraham1@uky.edu

Digital Object Identifier: <https://doi.org/10.13023/ETD.2016.417>

[Right click to open a feedback form in a new tab to let us know how this document benefits you.](#)

Recommended Citation

Graham, Nathaniel Phillip, "Brokers and Investment Advisors" (2016). *Theses and Dissertations--Finance and Quantitative Methods*. 6.

https://uknowledge.uky.edu/finance_etds/6

This Doctoral Dissertation is brought to you for free and open access by the Finance and Quantitative Methods at UKnowledge. It has been accepted for inclusion in Theses and Dissertations--Finance and Quantitative Methods by an authorized administrator of UKnowledge. For more information, please contact UKnowledge@lsv.uky.edu.

STUDENT AGREEMENT:

I represent that my thesis or dissertation and abstract are my original work. Proper attribution has been given to all outside sources. I understand that I am solely responsible for obtaining any needed copyright permissions. I have obtained needed written permission statement(s) from the owner(s) of each third-party copyrighted matter to be included in my work, allowing electronic distribution (if such use is not permitted by the fair use doctrine) which will be submitted to UKnowledge as Additional File.

I hereby grant to The University of Kentucky and its agents the irrevocable, non-exclusive, and royalty-free license to archive and make accessible my work in whole or in part in all forms of media, now or hereafter known. I agree that the document mentioned above may be made available immediately for worldwide access unless an embargo applies.

I retain all other ownership rights to the copyright of my work. I also retain the right to use in future works (such as articles or books) all or part of my work. I understand that I am free to register the copyright to my work.

REVIEW, APPROVAL AND ACCEPTANCE

The document mentioned above has been reviewed and accepted by the student's advisor, on behalf of the advisory committee, and by the Director of Graduate Studies (DGS), on behalf of the program; we verify that this is the final, approved version of the student's thesis including all changes required by the advisory committee. The undersigned agree to abide by the statements above.

Nathaniel Phillip Graham, Student

Dr. William Gerken, Major Professor

Dr. Kenneth Troske, Director of Graduate Studies

BROKERS AND INVESTMENT ADVISORS

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

Nathaniel Phillip Graham

Lexington, Kentucky

Co-Directors: Dr. William Gerken, Assistant Professor of Finance
and Dr. Paul Childs, Associate Professor of Finance

Lexington, Kentucky

Copyright ©Nathaniel Phillip Graham 2016

ABSTRACT OF DISSERTATION

BROKERS AND INVESTMENT ADVISORS

Brokers and investment advisors are a large and important part of the financial industry. Using a novel data set that includes the majority of the financial advisory industry, in this dissertation I document the size of the financial advisory industry and the prevalence of customer complaints in the industry, which I use as a proxy for misconduct. I then test for two potential drivers of advisors' misconduct: professional standards in the form of fiduciary responsibilities and influence from social networks. I find that variation in fiduciary duties—specifically fiduciary versus suitability standards—does not appear to affect advisors' propensity for misconduct. My results indicate that a more likely explanation for differences between brokers and investment advisors stems from their respective product lines and business models. I then show that advisors often influence each other's propensity to engage in misconduct, and that endogenous selection mechanisms, correlated environments, and location cannot fully explain variation in misconduct across groups of co-workers.

KEYWORDS: Broker, Investment Advisor, Social Network, Fiduciary Standard, Financial Intermediaries

Nathaniel P Graham

September 27th, 2016

Date

BROKERS AND INVESTMENT ADVISORS

By

Nathaniel Phillip Graham

Dr. William Gerken

Co-Director of Dissertation

Dr. Paul Childs

Co-Director of Dissertation

Dr. Kenneth Troske

Director of Graduate Studies

September 27th, 2016

Date

ACKNOWLEDGMENTS

I would like to thank my dissertation committee, in particular my co-chairs William Gerken and Paul Childs, for their invaluable advice, guidance, and assistance. Additionally, I'd like to thank Kristine Hankins, the Director of Graduate Studies for the department, for the great service that she has done for me during my time as a graduate student. I'd also like to thank Stephen Dimmock, with whom Chapter 4 is coauthored, Christopher Clifford, Bradford Jordan, and the rest of the Department of Finance & Quantitative Methods for their time and feedback during the development of this work, the many conference participants who provided comments, and my fellow doctoral students, particularly Timothy Riley, David Moore, and Tyson Van Alfen. A special thanks is also due to my statistics and econometrics instructors, particularly Christopher Bollinger, J.S. Butler, and Carlos Lamarche. Finally, I would be remiss if I failed to thank Adib Bagh and Bob Gillette for helping me get into graduate school in the first place.

Some of the data used in this dissertation was purchased with funds provided by the Institute for Fraud Prevention.

Contents

Acknowledgments	iii
List of Tables	v
1 Introduction	1
2 Data Description	3
3 Brokers vs Advisors: The Fiduciary Standard	6
3.1 Background on Brokers and Advisors	8
3.2 Descriptive Statistics	10
3.3 Testing the Fiduciary Standard and the Effects of Product Lines . . .	11
3.4 Large Payouts and the Cross-section of Complaints	13
3.5 Choosing to be an Advisor	14
3.6 Location and Proxies for Oversight	15
3.7 Tables	17
4 Selection vs Contagion: Influence by Peers	27
4.1 Design	28
4.1.1 Identifying Mergers and Dates	29
4.1.2 Measures of Misconduct	29
4.2 Descriptive Statistics	30
4.3 Estimating the Effect of Co-Workers on Misconduct	30
4.4 Similarity and Contagion	32
4.5 Geographic Variation and Branch Size	32
4.6 Across-Branch Variation in Monitoring and Supervision	33
4.7 Additional Robustness Tests	34
4.8 Tables	36
5 Conclusion	47
References	49
Vita	53

List of Tables

3.1	Individual and Complaint Summary Statistics	17
3.2	Panel Means	18
3.3	Representatives in Fiduciary and Non-Fiduciary States	19
3.4	Misconduct Rates in Fiduciary and Non-Fiduciary States	20
3.5	Panel Tests of the Fiduciary Standard	21
3.6	Determinants of a Large Payout	22
3.7	Determinants of Becoming an Advisor	23
3.8	Univariate Complaint Rates Between Matched Advisors and Brokers .	24
3.9	Misconduct Rates Between Matched Advisors and Brokers	25
3.10	Panel Tests of Oversight Proxies on Complaint Rates	26
4.1	Summary Statistics: Firms, Branches, and Advisors	36
4.2	Misconduct by Financial Advisors	37
4.3	Mergers, Introduced Co-Workers, and Financial Misconduct	38
4.4	Placebo Tests of Introduced Co-Workers and Financial Misconduct .	40
4.5	Similarity and Contagion	41
4.6	Geography and Branch Size	42
4.7	Across-Branch Variation in Monitoring and Supervision	43
4.8	Additional Robustness Tests	45
4.9	Alternate Windows	46

Chapter 1

Introduction

In this dissertation I examine misconduct by financial advisors, also referred to as investment advisors, brokers, and registered representatives.¹ While there is a long literature on financial misconduct in a variety of forms, less is known about financial advisors. Despite the relative lack of research on advisors compared to, for example, mutual fund managers, financial advisors are common: With over 630,000 advisors in the United States registered with FINRA², there are more advisors than bartenders or car mechanics³. Financial advisors handle trillions of dollars in assets and are widely used by retail investors (PriceMetrix, 2015). Thus, financial advisors are an important part of the financial system, acting as intermediaries for millions of retail investors.

The size of the industry is indicative of a great need for financial advice due to poor financial literacy. Studies such as Lusardi and Mitchell (2011) and van Rooij, Lusardi, and Alessie (2011) find that most consumers in the United States are not financially literate, and generally lack a basic understanding of financial markets, instruments, and investment techniques. This lack of sophistication has created a large market for financial advice aimed at retail investors—above and beyond execution-related services such as buying and selling securities—particularly for retirement, college expenses, and estate planning. However, a lack of sophistication on the part of investors also means that unscrupulous advisors have many opportunities to take advantage of their clients, in the same way that mechanics have the opportunity to manipulate car owners.

In order to study misconduct by financial advisors empirically, I use individual and firm-level regulatory filings obtained from Meridian IQ and state securities regulators that allow me to observe the employers, locations, and disclosures (such as customer complaints) of individual advisors, as well as the products and services offered by firms. The data additionally contains examination and licensing information allowing me to distinguish between advisors licensed to act as Registered Investment Advisors

¹While the legal term is registered representative, in keeping with current literature and popular discussion, I refer to registered representatives as advisors, except in Chapter 3 where the distinction between investment advisors or RIAs and brokers is important.

²The Financial Industry Regulatory Authority.

³As reported by FINRA and the Bureau of Labor Statistics.

and those that are not permitted to provide general investment advice. Certain forms also disclose the reason for an advisor’s end of employment with a firm, allowing me to identify when firms have merged and introduced an exogenous shock to advisors’ sets of co-workers.

I study two different potential sources of variation in advisors’ propensity to engage in misconduct. In Chapter 3, I examine professional standards—specifically the suitability and fiduciary standards to which brokers and investment advisors are respectively held. This distinction has been the source of much recent debate, as the Department of Labor has proposed and finalized new rules requiring any advisor handling retirement accounts to adhere to a fiduciary standard, even if that advisor is not a Registered Investment Advisor (RIA) licensed to provide general financial advice. I find no evidence that applying a fiduciary standard uniformly will reduce misconduct by advisors, because those with a fiduciary standard already receive a disproportionately large amount of customer complaints, and that in many cases, advisors held to suitability versus fiduciary standards are engaged in different businesses. This effect is robust to firm, time, geographic, and individual-level controls, as well as controlling for proxies for oversight. Consistent with the hypothesis that RIAs and brokers are frequently engaged in different businesses, selection at the beginning of an advisor’s career cannot explain the difference in misconduct rates between the two groups. However, I do not, it should be stressed, find any evidence that imposing fiduciary standards will increase misconduct or harm investors.

In Chapter 4 (co-authored with Stephen Dimmock and William Gerken), we examine the role of peer effects on advisors’ misconduct. While influence in social networks or within peer groups of a causal nature is notoriously difficult to identify empirically, the financial advisory industry has some helpful structural and reporting features that make contagion separable from endogenous selection. We find strong evidence that peers influence each other’s behavior, and that working with peers that have a history of misconduct leads advisors to engage in more misconduct themselves. These effects are stronger for advisors that are ethnically similar, which we use as a proxy for social ties. This intuitively indicates that stronger social connections lead to greater influence, and cannot be explained by appealing to regulation, oversight, or wealth-related effects. These results are robust to a variety of additional controls and alternative specifications, and cannot be explained by ‘nuisance’ complaints, survival bias, proxies for oversight and supervision, firm-level effects, geographic effects, the financial crisis, or adaptation to new social norms.

Because investors may not be able to identify misconduct that they are the victim of, even after the fact⁴, it is highly likely that studies—including this one—underestimate the prevalence and scale of misconduct by financial advisors. Misconduct must be discovered and reported in order to be observed in the data, and investors who seek out professional financial advice are generally ill-equipped to do so.

⁴Financial advice falls into the category of credence goods, which are goods whose utility is difficult for a consumer to determine, even after purchase and consumption.

Chapter 2

Data Description

I use data constructed from individual-level disclosures from advisors' Form U4 and U5 filings provided by Meridian IQ and firm-level disclosures from Form BD. While the Financial Industry Regulatory Authority (FINRA) maintains the Central Registration Depository (CRD) from which all individual and firm data is originally drawn, Meridian IQ collects its data from state securities regulators, as FINRA has chosen not to make the CRD available in aggregate.¹ I supplement the Meridian IQ data with data provided by state regulators². The data spans 1999–2014; though the data was collected in 2014, analyses in Chapter 4 do not use mergers from 2012–2014 because I cannot observe complaints over a sufficient period.

Not all states chose to make their portion of the CRD available to Meridian IQ, or to turn over all disclosure files. As a result, while I have at least some data from 45 states³, only 32 of those states provide customer complaint disclosures. I use only representatives registered in those 32 states for which I have complaints. Because a representative must register with each state in which he or she wishes to conduct business, many representatives are registered with all 50 states, and thus I have at least some representatives located in all 50 states in my data. For example, North Carolina did not provide data, so I do not observe an advisor located in Nevada that does not do any business outside of the state, but I would observe any North Carolina-based advisor that has registered with any of the 32 states I do observe. In total, the data includes 522,363 representatives, and an average year has 336,588 representatives. FINRA reports 630,692 registered representatives in the year 2010; my data includes slightly more than half of the combined brokerage & advising industries at any given time.

The customer complaint disclosures (part of the U4) contain detailed information on a complaint's resolution, the amount of damage the customer claimed, and the amount paid to the customer in order to settle the complaint. Complaints that

¹Anyone may lookup any individual representative or firm in the CRD using FINRA's BrokerCheck service at <http://brokercheck.finra.org/>, but FINRA does not permit bulk downloads of the CRD.

²Data was provided by Alabama, Arkansas, California, Connecticut, Florida, Kentucky, Maryland, Michigan, Minnesota, Nevada, New York, Pennsylvania, Texas, Vermont, and Washington.

³Meridian IQ chose to anonymize the states providing data.

were denied or decided in favor of the representative are typically removed from an advisor's record by FINRA, therefore all complaints in my data have been judged to have some merit, either by a firm's decision to settle monetarily before judgment from a third party, or by an arbitration panel or court of law. Further, FINRA Rule 2080 allows complaints to be expunged if the claim was clearly erroneous. Expunged complaints are not present in any of the data sources. Because customer complaints are initiated by clients using a standardized process, they are unlikely to be related to state regulators' enforcement strategies or policies. FINRA Rule 3070 requires firms to report all written customer complaints to regulators via the CRD system, though customers may choose to file complaints directly with FINRA or the relevant state regulator(s).

FINRA classifies⁴ complaints that go to arbitration into a number of categories which are not mutually exclusive. These categories include breach of fiduciary duty, negligence, failure to supervise, misrepresentation, suitability breach of contract, omission of facts, fraud, unauthorized trading, and churning. A complaint regarding churning, for example, means that the advisor (allegedly) deliberately traded for a customer's account unnecessarily, usually in order to generate trading fees. Misrepresentation and omission of facts are very common allegations in customer complaints, and are difficult for customers to identify; a customer may never realize that they were misled or that an advisor omitted important facts about a security or that an advisor concealed a conflict of interest such as kick-backs or bonuses for certain securities. It is also important to note that while customers may allege a variety of misconduct types, this does not mean that an advisor engaged in all of these behaviors, even if the complaint is upheld. Customers may deliberately add as many allegations to their complaint as possible in order to pressure advisors to settle or to increase the apparent seriousness of the complaint. However, a complaint that is upheld (or settled prior to arbitration) is very likely to represent significant damage to a customer and/or serious ethical violations, otherwise advisors will likely have the complaint expunged from their record. I use customer complaints as a measure of financial misconduct by advisors throughout this dissertation.

Some papers, such as Qureshi and Sokobin (2015); Egan, Matvos, and Seru (2016); McCann, Qin, and Yan (2016) also use regulatory actions either as a separate measure of misconduct or added to customer complaints. I deliberately exclude regulatory actions for the following reasons: First, regulatory actions are not necessarily related to financial misconduct against customers, e.g., sanctions for record-keeping violations. Second, some regulatory actions cite the same actions that brought on a customer complaint, effectively double-counting misconduct and, due to differences in filing dates, potentially creating a "pattern" of misconduct from a single event. Finally, regulators are aware of customer complaints from branches and may choose to investigate or audit members of a branch that received complaints recently, producing the illusion of peer effects (see Chapter 4) where none exist.

Using the Series 65 & 66 exams to identify advisors classifies 247,458 of 522,363

⁴As of September of 2016, summary statistics are available from FINRA at <https://www.finra.org/arbitration-and-mediation/dispute-resolution-statistics>.

representatives (47.4%) in my sample as advisors; by comparison, using the cross-section of all advisors registered with the SEC as of July 2014 and comparing against FINRA's official statistics indicates that 48.5% of representatives were advisors in 2014 (308,897 advisors from the SEC and 636,707 representatives from FINRA). From this I conclude that series exams are an effective method of identifying financial advisors.

Advisory firms operate from a number of geographically separate branches. The U4 filings provide the street address of a branch as well as a branch identifier that is unique within a firm. From this I can identify all the members of a specific branch and its exact location. Since the U4 also provides the dates at which an advisor joined and left a branch, at any given time I can observe the complete makeup of any branch in any firm in the data.

Meridian IQ provides assets under management (AUM) for a subset of advisors. While valuable as a control variable, the AUM provided is available only as a cross-section observed in 2014. As a result, detailed analysis of AUM and its determinants is not feasible.

Detailed summary and descriptive statistics are provided and discussed in the following two chapters.

Chapter 3

Brokers vs Advisors: The Fiduciary Standard

The legal distinction between broker-dealers and investment advisors has been a popular topic, particularly since February of 2015 when the Department of Labor suggested extending the fiduciary standard required of investment advisors to brokers handling retirement products.¹ At issue is whether brokers handling retirement products should be required to meet the fiduciary standard (to act in the best interests of the client) or whether the suitability standard (offer products that meet the goals and means of a client) is sufficient. Proponents of the rule change implicitly assume that investment advisors provide better service to their clients because of their professional standard of conduct, and if that standard were imposed on brokers, brokers' behavior would improve. However, investment advisors—who are slightly less than half of all registered representatives²—receive more than three-quarters of all customer complaints about misconduct or fraudulent sales practices, which suggests that if professional standards affect representatives' behavior, the effect is dominated by other factors. I refer to this difference in the rates of complaints as the “advisor effect”.

I test whether the fiduciary standard can explain the advisor effect, examine predictors of complaints by representatives, and model the decision to become an advisor versus a broker. While a variety of factors significantly predict customer complaints (see, for example, Dimmock and Gerken (2012), which examines fraud by investment managers and finds that past behavior, conflicts of interest, variation in monitoring, and managers' demographics are all correlated with fraud), I find no evidence that the advisor effect is driven by fiduciary duties.

Another assumption implicit to the argument for extending the fiduciary standard is that advisors and brokers are essentially offering the same products and services, instead of being in significantly different lines of business. If both groups are in the same business and directly compete with one another in most or all markets, it would make sense for the two groups to have the same standard of conduct. However,

¹These rules have since been finalized and are scheduled to take effect in 2017.

²Registered representatives are the broader group to which both brokers and advisors belong. All individuals in my data are registered representatives, and all are either an advisor or a broker.

if brokers are primarily engaged in arms-length transactions with customers while advisors provide services to clients on a basis of trust, then it makes little sense for brokers and advisors to be bound by the same professional standards.

In fact, brokers and advisors work at different types of firms, though there is significant overlap. When I compare brokers and advisors working at similar firm types (which I identify as firms offering the same products and services), I find little difference between the rates at which the two groups receive customer complaints. Additionally, when using the cross-sectional set of complaints to examine whether a complaint went to a broker or an advisor, I find that the direction and magnitude of the advisor effect is highly sensitive to the choice of products to control for: Controlling for whether the representative was at a mutual fund retailer reverses the advisor effect. The advisor effect can be explained entirely by firm-level product lines; as a result, the role of professional standards in preventing or limiting misconduct is unclear.

Further, some states (California, Missouri, South Dakota, and South Carolina) already impose a fiduciary standard on all registered representatives located in their states, providing a natural quasi-experiment. I find that the advisor effect in these “fiduciary states” is no different from the effect in non-fiduciary states. More general models using state or zip-code level fixed effects do not explain the advisor effect; location is not significantly correlated with the advisor effect. Alternatively, it is possible that brokers and advisors have different clientele, and that advisors’ clients are better informed (more able to identify misconduct) and have greater means (more able to afford legal representation). While I cannot directly observe clients, brokers and advisors have similar assets under management, and controlling for AUM does not explain the advisor effect. It is possible that an advisor may not have a fiduciary duty to all of their accounts, acting as a broker to some of their clients and an advisor to others. If these arrangements are common, it would be difficult to understand how fiduciary duties—or the lack thereof—could drive differences in misconduct rates, but it would make the identification of representatives subject to the fiduciary standard more difficult.

An alternative hypothesis proposed by Gough (2014) is that the higher standard imposed on advisors lowers the threshold for lodging a successful complaint relative to making a complaint against a broker. The lowered threshold hypothesis is difficult to motivate given the results in the fiduciary states. It is possible that a higher standard of conduct manifests itself in higher settlements paid out to customers in order to resolve complaints, as misconduct on the part of a fiduciary could be considered more egregious when compared to misconduct in the course of a normal business relationship, instead of resulting in a greater number of successful complaints. However, customer payouts for advisors are slightly smaller than those for brokers, despite advisors receiving a higher amount of damage claimed. A lower threshold for complaints due to the fiduciary standard does not explain the advisor effect and is not well-supported by the data.

Investment advisors are, understandably, concentrated in firms that focus on retail products offered to individual consumers, as opposed to brokers, who can be found in nearly all financial firms. When advisors are compared to brokers in similar firms, the

rates at which they receive complaints are very similar, suggesting that professional standards have little effect on misconduct among registered representatives. While I find no evidence that imposing the fiduciary standard on retail brokers would reduce misconduct, I also find no evidence that it would be harmful—brokers in fiduciary states receive complaints at a rate very similar to brokers in non-fiduciary states. My findings indicate that broadening the application of the fiduciary standard will have little effect on the behavior of brokers. Because my data does not contain information about clients or advisors’ compensation structures, I cannot address concerns about reduced access to advisors (Litan and Singer, 2015).

Regardless of the advisor effect, many clients may be better off with an advisor instead of a broker; Finke, Huston, and Waller (2009) show that the clients of advisors frequently possess better insurance coverage than the customers of brokers, which is consistent with my findings that show that advisors handling life insurance receive fewer customer complaints than brokers who handle life insurance. Mehran and Stulz (2007) consider scenarios where conflicts of interest may result in benefits to both parties, and Emons (1997) shows that market mechanisms may be sufficient to induce sellers of credence goods to avoid fraudulent behavior. The equilibria that Emons describes involve a separation between diagnosis and repair and generally require that experts do not have excess capacity, the first of which is analogous to a separation between financial advice on the one hand and execution and portfolio management on the other. While advisors that only offer counseling exist, they do not appear to dominate the market, and since customers will still require execution and advisors may have (undisclosed) relationships with brokers, it is not clear that limiting advisors to counseling necessarily reduce misconduct. It is also unlikely that financial advisors generally operate at maximum capacity at all times, so they may still have an incentive to provide execution and management services in order to increase revenues.

Because advisors and brokers likely have better information than many of their customers (Lusardi and Mitchell, 2011), and I find that the advisor effect varies according to products and services, it is likely that the optimal contract³ from an investor’s standpoint depends on both the investor’s goals and financial sophistication.

3.1 Background on Brokers and Advisors

I use the term *registered representative* or simply *representative* to refer to anyone authorized to transact for or provide investment or financial advice to a customer. All individuals in my sample are representatives. In the terminology used by FINRA, a broker (or usually a broker-dealer) refers to a firm that acts as a broker and/or dealer for financial securities. Likewise, FINRA uses the term advisor to refer to a firm

³See Spence and Zeckhauser (1971) or Harris and Raviv (1979) for more general discussions of optimal contracting between principals and agents under asymmetric information. Spence (1977) considers cases where consumer misperception affects product markets. La Porta, Lopez-de Silanes, and Shleifer (2006) considers regulations regarding securities transactions and examines their effectiveness under asymmetric information, however they do not discuss credence good markets, where the buyer lacks not just information but also the sophistication necessary to use it.

that provides financial advisory services to clients. However, the common usage of broker and advisor refers to individuals (registered representatives) that, respectively, provide brokerage services or financial advice to clients on behalf of their employer. For simplicity, I refer to representative as a *broker* if that individual is not authorized to provide financial advice, and as an *advisor* if that individual is authorized to provide financial advice.

To become an advisor, a representative must pass either the Series 65 or Series 66 exam⁴. I thereby classify any representative that has passed either of the Series 65 or 66 exams as an advisor. The remainder of the representatives in my sample are classified as brokers. Classifying representatives as brokers before their exam date does not alter the results of my analysis substantially.

When providing financial advice, an advisor must adhere to what is often referred to as the *fiduciary standard*, which requires that advisors put the interests of their clients ahead of their own, and that advice given be in the client's best interest.⁵ Further, any agent with a fiduciary duty is required to avoid conflicts of interest relevant to their principals and may not profit from the relationship without their principal's knowledge and consent. Other examples of principal-agent relationships with a fiduciary duty include corporate officers acting for shareholders, attorneys and their clients, executors of estates, and parents or legal guardians of children. Financial advice is frequently a credence good (meaning that the buyer is never—even *ex post*—certain of the value or utility of the product they purchase) because a sophisticated investor, by definition, does not require advice from a third party.⁶ Advisors' clients are generally not sophisticated investors. Credence goods increase the opportunity for misconduct on the part of seller and make monitoring inherently difficult (Emons, 1997).

By contrast, a broker is bound by a looser standard often referred to as the *suitability standard*, which requires only that the products offered to a client meet the means and goals of the client. For example, if a client wishes to save for retirement, a broker may offer stocks, bonds, annuities, or mutual funds⁷ in any mixture or quantity that the client can afford. An advisor, by contrast, would need to consider whether, e.g., that specific client is better off investing in one or more mutual funds, and exactly which mutual funds are that client's best option before dispensing advice. A discount brokerage, on the other hand, could be said to meet the suitability standard once a client has provided funds and given an order to buy or sell a security. Having the necessary funds to purchase (or cover the sale of) a security meets the means requirement, and issuing a buy or sell order meets the goals requirement as the client

⁴A representative that has already passed the Series 7 in order to become a general securities representative—what is generically considered a broker—has the option to take the Series 66 in lieu of taking both the Series 63 and Series 65 exams. A broker that does not wish to become an advisor need only take the Series 63 exam.

⁵See Easterbrook and Fischel (1993) for a more general discussion of fiduciary duty in the context of contract law.

⁶A sophisticated investor may not be in possession of all the relevant information, but this is different from requiring advice in order to evaluate information.

⁷This is not necessarily a comprehensive list of the securities that could meet the goal of saving for retirement, but it covers the vast majority of the securities a broker would offer such a client.

can be said to have the goal of taking a position with respect to that security. For a sophisticated investor, it is unclear whether the suitability standard provides any value, as the investor likely does not care about the broker’s assessment of suitability, and the standard does not necessarily increase information disclosure to the investor.

3.2 Descriptive Statistics

Table 3.1 shows summary statistics for individuals and complaints. While there are slightly fewer advisors than brokers in my data, the overwhelming majority of complaints went to advisors (69.7%). I refer to this difference in complaint rates as the “advisor effect”—the unconditional effect on the rate at which a registered representative receives customer complaints due to being a investment advisor.⁸ Brokers pay out somewhat more (\$241,000 versus \$177,000) to settle a complaint than advisors, though advisors’ clients claim larger damages (\$645,000 versus \$551,000). The payouts and claims averages do not include complaints resolved without disclosing a cash settlement⁹ (i.e., payout must be greater than 0 for a complaint to be included in the payout and damage claimed averages). There is considerable variation in both payouts and damage claims, and the differences between advisors and brokers are neither economically or statistically significant. Brokers and advisors manage similar amounts (\$95 versus \$119 million in AUM¹⁰), and have similar proportions of men versus women.

In order to form a year-representative panel, I observe what firm(s) a representative was employed at during each year; when a representative was employed at multiple firms during a calendar year, I assign that representative to the firm employing the most representatives during that year, i.e., to the largest firm. Table 3.2 shows the pooled means for variables in the panel. Advisors and brokers have similar average ages and years of experience (42.3 versus 43.8 years and 10.99 versus 11.43 years, respectively). I calculate experience at a given time as the number of days from a rep’s earliest starting date divided by 365.24 to get the number of years of experience as a registered representative. A greater proportion of advisors are employed at firms offering investment advice, selling mutual funds, and variable life insurance or variable annuities¹¹. A greater proportion of brokers than advisors are employed at firms offering private placements.

Table 3.3 shows counts of representatives in fiduciary and non-fiduciary states over time and the number of complaints received for each. The number of representatives

⁸Recent work by McCann, Qin, and Yan (2016) and Egan, Matvos, and Seru (2016) find similar differences between brokers and advisors.

⁹For many complaints showing a 0 settlement amount, the terms of the settlement—including the amount paid to the client—are confidential. Defendants (representatives and their attorneys) cannot propose keeping the settlement confidential in exchange for a larger amount, but plaintiffs or their attorneys can.

¹⁰AUM is provided by Meridian IQ for a limited subset of brokers and advisors, and is observed only in 2014. By comparison, PriceMetrix (2015) reports that the average advisor managed \$97 million in 2014.

¹¹Variable life insurance & annuities is a single category on Form BD.

in both fiduciary and non-fiduciary states are fairly stable over time, with only minor variation. Customer complaints, however, vary substantially; years with economic growth (e.g., 2005 and 2012) have relatively few complaints, while years during recessions (e.g., 2001, 2008, and 2009) receive a relatively large number of complaints. Customers are likely more motivated to examine their portfolio’s performance during an economic downturn, after their investments have lost value. Table 3.4 scales complaints by the number of representatives to more directly examine whether there are differences in misconduct rates between fiduciary and non-fiduciary states. Misconduct rates vary over time in the same way counts in Table 3.3 do, but the observed rates in fiduciary and non-fiduciary states are nearly identical. Testing the differences in rates rarely show statistical significance and the sign of the difference varies from year to year, indicating that there is no systematic difference between misconduct rates in fiduciary and non-fiduciary states. In the multivariate tests below, I address time variation in misconduct rates using year fixed effects.

3.3 Testing the Fiduciary Standard and the Effects of Product Lines

To test for whether the advisor effect persists in a multivariate setting, I use a panel of all representatives, and, similar to Dimmock and Gerken (2012), predict how many complaints a representative will receive in year $t + 1$ using variables observed in year t . Because age, sex, and AUM are not provided for many reps, I set missing values for those variables equal to 0 and include a missingness indicator for each variable. The missingness indicators are omitted from tables that use them in the interest of brevity. I use these missingness indicators in any model that includes demographic and AUM controls. Additionally, I also control for industry experience, measured as the number of years since a representative first entered the industry.¹²

Four states (California, Missouri, South Dakota, and South Carolina) impose a fiduciary duty on brokers in addition to advisors.¹³ Much of the advisory business is local, based on personal relationships, and thus moving to another state to avoid fiduciary requirements is likely to incur both direct moving costs and lose customers. This state-level variation in fiduciary duties for brokers provides an opportunity to directly test for whether the advisor effect is due to the fiduciary standard, and to address a current debate regarding the imposition of the fiduciary standard on brokers who handle retirement products. If fiduciary duties are driving differences in the behaviors of advisors and brokers, states with uniform standards should show no difference between the two groups.¹⁴

¹²Brown and Minor (2013) suggest that more experienced agents are more likely to take advantage of their clients.

¹³See Gough (2014) for further discussion of state-level variation in fiduciary duties. He does not, however, use this variation to test for the effect of the fiduciary standard, as he only has data for the state of Florida.

¹⁴Hankins, Flannery, and Nimalendran (2008) raises the possibility that the state the client is located in may determine the legal standard applied to a principle-agent relationship, rather than the

Table 3.5 shows linear fixed effects models in which attempt to predict the number of complaints a representative will receive in the following year. In Column 1 I begin with a model including only an indicator for whether the representative is an advisor, demographic controls, and year fixed effects, in order to establish the relative size of the advisor effect in the panel. The coefficient on *Advisor* is 0.006, and is highly significant.¹⁵

Column 2 adds an indicator for whether the representative was located in a fiduciary state in year t and an interaction with *Advisor*. If fiduciary duties cause the advisor effect, we would expect the interaction between *Advisor* and *Fiduciary State* to be negative (the opposite of the coefficient on *Advisor*) and of a magnitude similar to *Advisor*, so that the interaction cancels out the advisor effect in fiduciary states. However, both *Fiduciary State* and its interaction with *Advisor* are small and insignificant. Representatives in fiduciary states receive insignificantly more complaints than other reps (0.0003) and advisors in fiduciary states receive insignificantly fewer (-0.0001) complaints than advisors in other states. The coefficient on *Advisor* remains 0.006, unchanged from Column 1.

In Column 3 I add firm fixed effects and interaction terms¹⁶ for the employing firm's product lines to *Advisor* instead of the demographic and AUM controls. The product line interactions I use are investment advisory, private placements, variable life insurance & annuities, dealing or brokering debt securities (corporate, municipal, or federal), and mutual fund retailer. As a result, the coefficient on *Advisor* becomes negative and marginally significant (-0.001). The coefficients on *Fiduciary State* and its interaction with *Advisor* are unaffected. Advisors at firms handling debt, advisory firms, and mutual fund retailers receive complaints at a significantly higher rate (0.002, 0.004, and 0.003, respectively). By contrast, advisors at firms handling variable life insurance & annuities receive complaints at a lower rate (-0.002 , significant at the 10% level), and advisors at firms handling private placements receive complaints at an insignificantly lower rate (-0.0004).

The decomposition of the advisor effect in Column 3 suggests that there is no generic, industry-wide effect to advisors, but rather a range of positive and negative effects depending on the products and services offered. From this we can speculate that some products might allow a representative greater opportunity or incentives for misconduct, perhaps due to kick-backs for flows from mutual funds or bonuses for selling in-house products discretion or have a relatively less sophisticated clientele who have difficulty monitoring their representative. Clients needing an advisor to guide their investment in mutual funds are likely to be less sophisticated or informed than clients who only require a the services of a broker, meaning that those clients are

state the agent in which the agent is located. However, this was in the context of differing fiduciary standards, instead of suitability versus fiduciary standards. In the same context, Schanzenbach and Sitkoff (2007) concluded that the principle's location determined the relevant legal standard, and not the client's location.

¹⁵Standard errors are computed using multi-way clustering following Cameron, Gelbach, and Miller (2011) in all regressions; see the tables for the dimensions used for clustering in each regression.

¹⁶I use interaction terms for product lines because firm fixed effects will absorb firm product lines in general, as product lines do not vary over time in the data.

less able to oversee their advisor’s behavior or see through a misleading sales pitch.

From Table 3.5 we can conclude that state-level imposition of fiduciary duties on brokers has little or no effect on the rate at which they receive complaints, but firm lines of products and services offered do. As advisors in important and common areas such as mutual fund sales receive complaints at a higher rate than brokers, and because the fiduciary standard does not appear to affect the rate at which a representative receives customer complaints, there is little evidence that the imposition of fiduciary duties on brokers at the federal level will significantly affect misconduct among brokers.

3.4 Large Payouts and the Cross-section of Complaints

While fiduciary duties do not appear to affect misconduct rates, it is possible that fiduciary duties affect the severity of the complaint. Gough (2014) suggests that fiduciary duties may decrease the threshold for lodging a successful complaint against an advisor relative to a broker. If misconduct by a fiduciary is considered more egregious than misconduct by an agent in an arms-length transaction, then advisors might be punished more severely than brokers by arbitration boards, or clients might demand larger settlements from advisors than they demand from brokers. I explore this possibility in Table 3.6, where I use logit models estimated using quasi-maximum likelihood estimation¹⁷. I use only complaints to those representatives for which firm-level product line data is available, reducing the number of observations to 39,708 from 58,315.

The dependent variable for all columns in Table 3.6 is an indicator equal to 1 if the complaint was settled by a payout to the customer of \$25,000 or more, and 0 otherwise. If fiduciary standards lead to larger payouts to satisfy clients and arbitration boards, we would expect the coefficient on *Advisor* to be positive and significant. In Column 1 I estimate a model using only a constant term and an indicator for whether the representative receiving the complaint is an advisor. The constant term is small and negative (-0.052) but highly significant, as a slight majority of complaints are settled with a small payout or no payout at all. The coefficient on *Advisor* is negative and insignificant (-0.020); advisors are no more likely to settle with a large payout than brokers.

Because Table 3.5 demonstrates that product lines are important to understanding misconduct rates, in Column 2 I add indicators for several product lines (mutual fund dealer, variable life insurance & annuities (VLA), private placements, brokering or dealing any form of debt securities, investment advisory). As a state’s policy with respect to fiduciary duties for brokers may be relevant, I include an indicator for whether a representative is located in a fiduciary state at the time of the complaint. *Advisor* remains negative and insignificant (-0.005). *Fiduciary State* is also negative and insignificant (-0.031), indicating that reps in fiduciary states do not settle with

¹⁷See Wooldridge (1997) for a description of QMLE and a comparison to standard MLE techniques.

large payouts any more often than reps in other states. All of the product indicators are highly significant. Being employed at an investment advisory firm is correlated with small payouts (-0.200, $e^{-0.2} = 0.819$ or about 18% less likely to settle with a large payout), being employed at a mutual fund retailer is also negative (-0.481, $e^{-0.481} = 0.618$ or about 38% less likely to settle with a large payout), as is being employed at a VLA dealer (-0.209, $e^{-0.209} = 0.811$ or about 19% less likely to settle with a large payout). Private placements is positive (0.263, $e^{0.263} = 1.301$ or about 30% more likely to settle with a large payout), as is handling debt securities (0.420, $e^{0.420} = 1.522$ or about 52% more likely to settle with a large payout).

In Column 3 I add demographic controls (age, experience, sex, and $\log(\text{AUM})$), but the results are quantitatively similar to Column 2. One interesting aspect of the payout results is that categories (advisory firm, mutual fund retailer) where advisors receive more complaints than brokers are correlated with smaller settlements, whereas categories (private placements, debt securities) where advisors receive fewer complaints than brokers are correlated with larger settlements; the advisor effect appears to be negatively correlated with settlement size.

3.5 Choosing to be an Advisor

While the previous sections have considered the rate at which representatives receive complaints, in this section I consider a rep's decision to become an advisor in addition to or instead of being a broker. In Table 3.7 I model this decision using a logit model, where the dependent variable is whether a representative becomes an advisor or not. All variables are observed at the beginning of a representative's career. I include indicators for all the previous product and service categories (mutual fund dealer, variable life insurance & annuities (VLA), private placements, brokering or dealing any form of debt securities, and investment advisory), as well as whether the representative is male, and the age the representative entered the industry. I also include fixed effects for the year a representative started their career to control for variation over time in the broker-advisor decision, and state-level fixed effects to control for potential geographic effects. I include only representatives for whom I have non-missing firm-level product line information about their first firm.

Table 3.7 indicates that men are more likely to become advisors than women, representatives who begin their careers earlier in life are less likely to become an advisor, and starting a career at a mutual fund dealer, a VLA dealer, a firm handling debt securities, or an investment advisory is associated with a greater likelihood of becoming an advisor; all are highly statistically significant. Working at a firm handling private placements is insignificantly associated with a greater likelihood of becoming an advisor.

Using the model of the broker-advisor decision estimated in Table 3.7, I construct a propensity score matched set of brokers and advisors. Specifically, I use nearest-neighbor matching with replacement to find the broker that is most similar to each advisor. As shown in Table 3.8, starting with 346,389 representatives, I match 59,285 brokers to 160,572 advisors (no advisors go unmatched). I then compare the rates

at which advisors and brokers receive complaints over their full careers in both the matched and unmatched samples. An advisor receives an average of 0.129 complaints over the course of their career, whereas brokers in general receive an average of 0.043 complaints (for a difference of 0.086 complaints). The matched brokers, however, receive 0.084 complaints in their careers (for a difference of 0.043 complaints). While selection at the beginning of a career accounts for approximately half of the unconditional advisor effect, there is still a substantial difference in complaint rates between advisors and matched brokers.

A multivariate test of differences in career misconduct rates, shown in Table 3.9, finds little difference in the advisor effect between the matched and unmatched samples, demonstrating that even if brokers and advisors begin their careers under similar circumstances, their misconduct rates still differ.

3.6 Location and Proxies for Oversight

While the advisor effect varies considerably from one type of product to another, it is possible that the effect could be explained by other factors. In Table 3.10 I present models controlling for other firm and branch characteristics. In particular, I use measures of firm and branch size, along with indicators for where a representative is supervised from to proxy for the amount of oversight a firm imposes on a representative. In Column 1 I control for (log) firm size, (log) branch size, their interaction, indicators for whether the firm or branch is a “solo” operation (*Solo Firm* for firms with only one representative and *Solo Branch* for branches with only one representative), for whether the branch is a *Private Residence*, and for whether the rep’s supervisor is not located at the same branch as the representative (*Unsupervised*). I also include demographic and AUM controls, as in Table 3.5.

Other than firm size (which is positive at 0.001), none of the firm or branch controls is significantly different from 0, and the coefficient on *Advisor* is essentially unchanged from Table 3.5 Column 2 at 0.004. While it is possible that other, better proxies for oversight could correlate with complaints or even explain the advisor effect, size and the relative location of supervision do not.

In Table 3.10 Column 2 I add fixed effects for each 5-digit zip code a representative’s office is located in; while state-level variation in fiduciary duties do not explain the advisor effect, it is possible that the effect is correlated more generally with geographic location. The R^2 doubles from 0.004 to 0.008 when I account for zip code fixed effects, indicating that complaints in general are clustered in certain areas. This is consistent with Eaglesham and Barry (2014), who find that complaints are disproportionately more likely in areas with many elderly investors and, studies that find that misconduct is often concentrated in specific geographic areas (Egan, Matvos, and Seru, 2016; Parsons, Sulaeman, and Titman, 2015). Otherwise, the results in Column 2 are essentially unchanged from Column 1, suggesting that while complaints are correlated with location, the advisor effect is not. Column 3 shows the results of a simpler model, similar to Column 2 of Table 3.5 but with the addition of zip code fixed effects, which shows that brokers and advisors in fiduciary states are similar to

those in non-fiduciary states after controlling for fine-grained geographic effects.

3.7 Tables

Table 3.1: Individual and Complaint Summary Statistics

Comparisons of brokers and advisors. *Total Number* is the number of representatives of that type observed in the data. *AUM* is Assets Under Management. *Male* is equal to 1 if the representative is male. *Number of Complaints per Rep* is the *Total Number of Complaints* received divided by the *Total Number*. *Mean Payout*, measured in thousands of dollars, is the average amount paid to clients as either a settlement or as required by an arbitration panel. *Mean Damage Claimed*, measured in thousands of dollars, is the average amount of damages clients claimed to have experienced as a result of the representative's actions. *Total Number of Complaints* is a count of the number of customer complaints observed in the sample.

	Brokers	Advisors
Total Number	274,905	247,458
Mean AUM (millions)	95.10	118.88
Male	0.74	0.78
Number of Complaints per Rep	0.06	0.16
Mean Payout (thousands)	241	177
Mean Damage Claimed (thousands)	551	645
Total Number of Complaints	17,670	40,645

Table 3.2: Panel Means

Comparisons of brokers and advisors in a panel spanning 2001–2012. *AUM* is Assets Under Management in millions of dollars. *Age* and *Experience* are in years, measured as the number of days from birth or the first observed date of employment, divided by 365.24. *Male* is equal to 1 if the representative is male. *RIA Firm*, *Mutual Fund Retailer*, *Variable Life Insurance & Annuities*, and *Private Placements* are equal to 1 if the representative was employed at a firm during that year offering that product or service.

	Brokers	Advisors
AUM	31.19	65.33
Age	43.81	42.30
Experience	11.34	10.99
Male	0.58	0.74
RIA Firm	0.54	0.66
Mutual Fund Retailer	0.67	0.73
Variable Life Insurance & Annuities	0.58	0.69
Private Placements	0.33	0.24

Table 3.3: Representatives in Fiduciary and Non-Fiduciary States

Number of complaints in each year of the sample. *Fiduciary States* are California, Missouri, South Carolina, and South Dakota, which impose fiduciary duties on brokers. All other states are *Other States*. *Complaints* is the number of customer complaints filed during that year.

Year	Fiduciary States		Other States	
	Complaints	Representatives	Complaints	Representatives
2001	438	46,105	2,246	270,225
2002	294	48,297	1,742	280,393
2003	238	49,465	1,358	284,688
2004	186	49,968	1,102	287,699
2005	165	50,138	805	287,979
2006	132	49,187	743	282,053
2007	818	49,785	4,929	285,356
2008	1,154	49,868	6,409	285,297
2009	733	49,813	4,428	285,112
2010	623	51,106	3,443	291,580
2011	488	52,132	2,648	297,737
2012	304	52,744	1,541	302,331

Table 3.4: Misconduct Rates in Fiduciary and Non-Fiduciary States

Rate of complaints in each year of the sample. *Fiduciary States* are California, Missouri, South Carolina, and South Dakota, which impose fiduciary duties on brokers. All other states are *Other States*. The rates indicated are the number of complaints divided by the number of representatives in the sample during that year.

Year	Complaints per Representative		Difference
	Fiduciary States	Other States	
2001	0.010	0.008	0.001**
2002	0.006	0.006	-0.0001
2003	0.005	0.005	0.00004
2004	0.004	0.004	-0.0001
2005	0.003	0.003	0.0005*
2006	0.003	0.003	0.00005
2007	0.016	0.017	-0.001
2008	0.023	0.022	0.001
2009	0.015	0.016	-0.001
2010	0.012	0.012	0.0004
2011	0.009	0.009	0.0005
2012	0.006	0.005	0.001*

Table 3.5: Panel Tests of the Fiduciary Standard

Linear fixed effects models where the dependent variable, the number of complaints a representative received, is measured in year t , while all explanatory variables are measured in year $t - 1$. The sample spans 2001–2012. *Firm Investment Advisory*, *Mutual Fund Retailer*, *VLA Dealer*, *Private Placements*, and *Other Products* are indicators equal to 1 if the rep’s firm offered that product, where *VLA* means variable life insurance and annuities. *Debt* is equal to 1 if the firm brokers or deals corporate, municipal, or government debt securities. *Fiduciary State* is equal to 1 if the complaint went to a representative located in a state with uniform fiduciary duties. Standard errors clustered by year and individual in columns 1 and 2, and clustered by year, firm, and individual in column 3 are in parentheses.

	(1)	(2)	(3)
Advisor	0.006*** (0.001)	0.006*** (0.001)	−0.001* (0.001)
Fiduciary State		0.0003 (0.0003)	0.0003 (0.0003)
Advisor at Debt			0.002*** (0.001)
Advisor at Advisory Firm			0.004*** (0.001)
Advisor at Placements			−0.0004 (0.001)
Advisor at VLA Dealer			−0.002* (0.001)
Advisor at Mutual Retailer			0.003** (0.001)
Advisor:Fid State		−0.0001 (0.0005)	−0.0001 (0.0005)
Age, Exp, Sex, AUM	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes
Observations	3,447,948	3,447,948	3,447,948
R ²	0.005	0.005	0.009

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.6: Determinants of a Large Payout

Logit model where the dependent variable is equal to 1 if the complaint was settled by a payout to the customer of \$25,000 or more, and 0 if not. *Age* and *Experience* are standardized/z-scored and measured at date of the complaint. *Firm Investment Advisory*, *Mutual Fund Retailer*, *VLA Dealer*, *Private Placements*, and *Other Products* are indicators equal to 1 if the rep's firm offered that product, where *VLA* means variable life insurance and annuities. *Any Debt Securities* is equal to 1 if the firm brokers or deals corporate, municipal, or government debt securities. *Fiduciary State* is equal to 1 if the complaint went to a representative located in a state with uniform fiduciary duties. Standard errors in parentheses.

	(1)	(2)	(3)
Constant	-0.052*** (0.019)	0.167* (0.085)	-0.853*** (0.117)
Advisor	-0.020 (0.022)	-0.005 (0.023)	0.017 (0.024)
Firm Investment Advisory		-0.200*** (0.032)	-0.201*** (0.033)
Mutual Fund Retailer		-0.481*** (0.091)	-0.469*** (0.091)
VLA Dealer		-0.209*** (0.049)	-0.169*** (0.049)
Private Placements		0.263*** (0.027)	0.233*** (0.027)
Any Debt Securities		0.420*** (0.064)	0.451*** (0.064)
Fiduciary State		-0.031 (0.028)	-0.027 (0.028)
Age, Exp, Sex, AUM	No	No	Yes
Observations	39,708	39,708	39,708

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.7: Determinants of Becoming an Advisor

Logit model where the dependent variable is equal to 1 if the representative was an advisor, 0 if a broker. *Age at Start* is standardized/z-scored and measured at date of the start of the representative's career. *Firm Investment Advisory*, *Mutual Fund Retailer*, *VLA Dealer*, *Private Placements*, and *Any Debt Securities* are indicators equal to 1 if the rep's first firm offered that product, where *VLA* means variable life insurance and annuities. *Any Debt Securities* is equal to 1 if the firm brokers or deals corporate, municipal, or government debt securities. Standard errors in parentheses.

Male	0.062*** (0.002)
log(AUM)	0.091*** (0.002)
Age at Start	-0.022*** (0.001)
Firm Investment Advisory	0.158*** (0.002)
Mutual Fund Retailer	0.042*** (0.003)
VLA Dealer	0.063*** (0.003)
Private Placements	0.002 (0.002)
Any Debt Securities	0.023*** (0.003)
Start-Year Fixed Effects	Yes
Start-State Fixed Effects	Yes
Observations	346,389

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.8: Univariate Complaint Rates Between Matched Advisors and Brokers

Univariate tests of the difference in complaint rates between advisors and brokers, using matched and unmatched samples. The unmatched test include all representatives for which firm product information is available at the start of the rep’s career (all reps included in Table 3.7), and uses Welch’s t-test. The matched test includes all advisors and those brokers to which they’ve been matched (matching nearest neighbor with replacement using the model from Table 3.7), and uses a paired t-test.

Panel A: Number of Unique Individuals by Sample				
	Advisors	Brokers		
Unmatched	160,572	185,817		
Matched	160,572	59,285		

Panel B: Full Career Complaint Rates by Sample				
	Advisors	Brokers	Difference	t
Unmatched	0.129	0.043	0.086***	49.83
Matched	0.129	0.084	0.045***	23.80

Table 3.9: Misconduct Rates Between Matched Advisors and Brokers

Linear fixed effects model where the dependent variable is the number of complaints a representative received during his or her career. All variables are observed at the start of the rep's career except AUM, which is observed in 2014 if available. *Age at Start* is standardized/z-scored and measured at date of the start of the rep's career. *Firm Investment Advisory*, *Mutual Fund Retailer*, *VLA Dealer*, *Private Placements*, and *Any Debt Securities* are indicators equal to 1 if the representative's first firm offered that product, where *VLA* means variable life insurance and annuities. *Any Debt Securities* is equal to 1 if the firm brokers or deals corporate, municipal, or government debt securities. Standard errors clustered by starting state in the unmatched and by starting state, individual, and by match in parentheses.

	Unmatched	Matched
Advisor	0.049*** (0.005)	0.055*** (0.005)
Male	0.066*** (0.005)	0.076*** (0.005)
log(AUM)	0.063*** (0.004)	0.064*** (0.006)
Age at Start	0.007*** (0.001)	0.008*** (0.002)
Firm Investment Advisory	0.012 (0.008)	0.018** (0.008)
Mutual Fund Retailer	-0.0001 (0.003)	0.002 (0.006)
VLA	0.008* (0.004)	0.009 (0.006)
Private Placements	0.017*** (0.002)	0.019*** (0.004)
Any Debt Securities	0.010*** (0.003)	0.009* (0.005)
Start-Year Fixed Effects	Yes	Yes
Start-State Fixed Effects	Yes	Yes
Observations	346,389	321,144
R ²	0.043	0.042

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.10: Panel Tests of Oversight Proxies on Complaint Rates

Linear fixed effects models where the dependent variable, the number of complaints a representative received, is measured in year t , while all explanatory variables are measured in year $t - 1$. *Firm Size* and *Branch Size* are the number of reps registered at a firm or branch, respectively. *Solo Firm* and *Solo Branch* are indicators for whether the firm or branch has only one registered rep. *Unsupervised* is an indicator for whether the rep's supervisor is located at a different branch. Standard errors clustered by year and individual in column 1, and clustered by year, zip code, and individual in columns 2 and 3 are in parentheses.

	(1)	(2)	(3)
Advisor	0.005*** (0.001)	0.005*** (0.001)	0.006*** (0.001)
log(Firm Size)	0.001** (0.0003)	0.001** (0.0003)	
log(Branch Size)	0.00004 (0.0001)	0.0001 (0.0001)	
Solo Firm	0.001 (0.001)	0.00005 (0.001)	
Solo Branch	0.00003 (0.0002)	-0.0001 (0.0002)	
Private Residence	-0.0003 (0.0004)	-0.0003 (0.0005)	
Unsupervised	0.0002 (0.0002)	0.0001 (0.0003)	
Fiduciary State			-0.010 (0.012)
log(Firm Size):log(Branch Size)	-0.00001 (0.00002)	-0.00001 (0.00001)	
Advisor:Fid State			-0.0001 (0.0004)
Age, Exp, Sex, AUM	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Zip Code Fixed Effects	No	Yes	Yes
Observations	3,409,635	3,409,635	3,447,948
R ²	0.005	0.011	0.011

Note:

*p<0.1; **p<0.05; ***p<0.01

Chapter 4

Selection vs Contagion: Influence by Peers

Do co-workers spread their behavior to one another?¹ There is a significant literature examining financial misconduct and its harms, largely following Becker (1968) in framing misconduct as a cost-benefit tradeoff by individuals or organizations, without reference to social networks or peer interactions. While highly useful, this is not a complete picture of misconduct.

We test empirically for evidence of contagion² in financial misconduct. Some empirical studies have demonstrated peer effects in related financial decisions, such as stock market participation (Brown, Ivković, Smith, and Weisbenner, 2008; Hong, Kubik, and Stein, 2004; Hvide and Östberg, 2015), entrepreneurship (Lerner and Malmendier, 2013), and corporate policies (Shue, 2013). Papers such as Glaeser, Sacerdote, and Scheinkman (1996) and Posner (1997) show that optimal enforcement policies are very different when contagion is economically significant. In our case, following the method of Glaeser, Sacerdote, and Scheinkman (2003), we find a social multiplier of 1.59, meaning that a single case of misconduct produces an additional 0.59 cases through contagion.

While several other papers have explored misconduct at the firm level (Bizjak, Lemmon, and Whitby, 2009; Brown, 2011; Kedia, Koh, and Rajgopal, 2015; Parsons, Sulaeman, and Titman, 2015), we focus on individual misconduct. An advantage of individual misconduct is that it allows for cleaner identification of peer effects, which is not feasible without accurate identification of peer groups (Manski, 1993).

¹This chapter contains material developed in conjunction with Stephen Dimmock and William Gerken and some portions are included in Dimmock, Gerken, and Graham (2016). I use 'we' instead of 'I' to indicate that the design and estimation of the tests described was a joint effort.

²I use the terms contagion, influence, and peer effects interchangeably to refer to a causal effect on behavior due to association.

4.1 Design

The principle issue in estimating peer effects is in disentangling selection from influence. Manski (1993, 2000) describes the issue in broad terms, but it can simply stated that when individuals can choose which people they associate with, distinguishing between causal effects on an individual (often termed influence or contagion) and individuals choosing to associate with similar people³ (often termed homophily or the reflection problem). What appears at first glance to be a change in behavior due to association may simply be the revealing of one’s type or preferences. Shalizi and Thomas (2011) demonstrate how difficult separating homophily and contagion are in an empirical setting: Networks constructed purely through selection mechanisms reliably pass tests intended to identify contagion and vice versa. Manski (1993) makes it clear that accurately identifying peer groups is crucial, and that causal effects cannot be distinguished from endogenous selection if the formation of peer groups is correlated with the outcome we wish to study.

The structure of the financial advice industry gives us several valuable tools with which we address the problem of endogenous selection. First, advisors are grouped into geographically distinct branches, and we observe the street address of each branch. As most branches are not very large—Table 4.1 shows that the average branch in our sample has less than twenty advisors—we can be reasonably certain that advisors working at the same branch are familiar with each other. Second, advisory firms periodically merge, and when they merge geographically close branches are often merged into a single branch. This provides an exogenous shock to an advisor’s group of peers, on the assumption that individual advisors have little or no input into firm-level merger decisions.

It is possible that firm-level merger decisions and misconduct are endogenously related—firms may choose to merge based in part on each others’ misconduct rates. However, because merging is a firm-level decision and not a branch-level decision, we can control for firm-level characteristics (including those related to merger decisions) by including merger-firm fixed effects: The target and the acquirer in a merger each have a fixed effect. Since we identify a merger as happening at a specific point in time, merger-firm fixed effects subsume time fixed effects as well, eliminating any time trends or time-specific shocks to misconduct from our analysis. Because we use branches of firms to identify peer groups and control for firm-level effects, our tests rely on comparisons between branches within the same firm; our design does not allow us to analyze firms with only one branch.

We structure our main test in a predictive manner: The dependent variable of our regressions is post-merger misconduct, and all the predictors are measured pre-merger. This avoids mechanical biases that could arise in contemporaneous designs. This design also mirrors the problem from an investor’s perspective, asking how likely an advisor is to engage in misconduct in the future given information about the past and present.

³Egan, Matvos, and Seru (2016) find that misconduct by financial advisors is not uniformly distributed, but instead appears to be concentrated in certain firms and counties.

4.1.1 Identifying Mergers and Dates

FINRA offers firms a convenient method for dealing with many advisors transferring from one entity to another, known as the Mass Transfer Program. An advisor’s U5 (termination notice) form will list the reason for termination as a mass transfer, allowing us to identify mergers in the data. We then hand-check each event to ensure that a merger is the cause of the mass transfer; we find that this method reliably identifies mergers. Because the mass transfer program is only open to transfers of at least fifty advisors, this method may not identify mergers between very small firms or acquisitions of very small firms by large firms, but many of these very small firms had only one branch, meaning that they cannot be estimated in our design. Firms that advisors leave due to a mass transfer are considered target firms, and the firm that advisors begin working for immediately after the transfer are considered to be the acquiring firms.

Some of the mergers in our sample are between very large firms which, due to organizational complexity, conduct multiple transfers in the course of merging operations. These transfers often take place at different dates. In these cases, we take the earliest transfer date as the date of the merger. We observe post-merger employment one hundred days after the merger date to ensure that we capture advisors’ employment correctly. Because of variation in filing dates on U4 and U5 forms, we observe pre-merger employment thirty days before the merger date. The merger period, $(-30, 100)$ where 0 is the merger date, is not included in our measures of pre-merger or post-merger misconduct.

Except for some robustness tests, we use three-year periods (or windows) around the merger to construct measures of misconduct. The three year pre-merger window ends at day -30 and the three year post-merger window begins on day 100. Table 4.9 shows that using different window lengths does not affect our results significantly.

4.1.2 Measures of Misconduct

Observing employment both before and after the merger allows us to observe peer groups before and after the exogenous shock, and to construct measures of misconduct based on who an advisor worked with before the merger, and who an advisor was introduced to by the merger. We begin by totaling the number of complaints the advisors in each group received during the pre-merger period, and scaling that number by the size of the group (the number of advisors). This produces a misconduct rate. We then create an indicator variable equal to one if that rate is above the mean for all such groups, excluding branches with no misconduct at all. For an advisor’s pre-merger peer group, we refer to this indicator as Original Branch High Misconduct, and for the peers introduced by the merger—our key variable of interest—we refer to it as Introduced Branch High Misconduct. Results are very similar if we instead use rates instead of high-rate indicators, or if we use indicators equal to one if any member of the group received a complaint.

For each advisor, we create an indicator equal to one if the advisor received a complaint during the pre-merger period (Pre-Merger Individual Misconduct Dummy)

and an indicator equal to one if the advisor received a complaint in the post-merger period (the dependent variable in most tests). We also create a count of the number of complaints an advisor received in the post-merger period for use in a negative binomial model.

4.2 Descriptive Statistics

Table 4.1 describes target and acquirer firms, branches, and advisors. Acquirer firms are generally larger, with more advisors (192.0 vs 124.2 advisors on average) and more advisors per branch (16.0 vs 7.7). Target firms, however, have more distinct branches (16.1 vs 12.0). A majority of the advisors in our sample are employed at acquirer firms (91,569 vs 59,264). Target advisors are somewhat more experienced (11.0 year vs 9.8), manage more assets (\$88.1 million vs \$80.0 million), and are slightly older (41.6 years vs 39.3 years of age). The proportion of advisors that are male is essentially equal (71.0% for acquirers vs 71.1% for targets).

Table 4.2 describes misconduct at the individual and branch levels. Slightly more than 1% of advisors engaged in misconduct during the three year pre-merger period, whereas about 1.4% did so in the post-merger period. The increase is largely due to the financial crisis appearing in more post-merger periods than pre-merger periods, and is absorbed in multivariate tests by fixed effects. 12.5% of advisors' original branches had an above average rate of misconduct, while 14.7% of advisors' introduced branches had above average misconduct rates. The third panel of Table 4.2 describes the distribution of payouts made to settle a complaint or were awarded by an arbitration panel. While the mean is approximately \$430 thousand dollars, the median is \$50 thousand. Less than 10% of payouts are of \$10,000 or less, and about 10% are greater than \$425,000. In 7.5% of cases, settlement amounts were not disclosed.

4.3 Estimating the Effect of Co-Workers on Misconduct

Using mergers between firms to identify the introduction of advisors to one another, we estimate the effect of co-workers on an advisor's propensity to engage in misconduct. Table 4.3 contains results for three different estimators: a conditional logit, linear fixed effects, and a negative binomial model. Each model includes controls for age, experience in the industry, sex, assets under management, missing demographic information, whether the advisor was located at the firm's main office, whether the advisor was the only advisor registered at their branch prior to the merger (Solo Original Branch), and (log) branch size before the merger and of the introduced branch. We also control for whether the advisor engaged in misconduct prior to the merger, and whether the advisor's pre-merger peers had a history of misconduct.

Column (1) of Table 4.3 shows coefficients and z-scores for the conditional logit model, where the dependent variables is an indicator variable equal to one if the

advisor engaged in misconduct in the post-merger period, and merger-firm fixed effects are conditioned out of the model. This is our primary or main test of the peer influence hypothesis. The main coefficient of interest is Introduced Branch High Misconduct, which is 0.313 and highly significant. This indicates that advisors who are introduced to peers that have a history of misconduct are 37% ($e^{0.313} = 1.3675$) more likely to engage in misconduct after the merger. This is an economically large effect, indicating that peers significantly affect each others' propensity for misconduct and that working with unethical people can lead to unethical behavior. The coefficient for Original Branch High Misconduct is even larger at 0.488, but we cannot interpret this causally—because an advisor chose to work with those individuals (and they him), we can only say that pre-merger peer behavior is strongly correlated with post-merger individual behavior. Finally, Pre-Merger Individual Misconduct Dummy is very large: 1.392, which intuitively indicates that one's own behavior in the past is highly predictive of one's future behavior.

The second and third columns of Table 4.3 report the results of a linear fixed effects model and a negative binomial model, respectively. Like the conditional logit model, the dependent variable for the linear fixed effects model is an indicator equal to one if the advisor engaged in misconduct in the post-merger period. The results are very similar to those in the conditional logit, where Introduced Branch High Misconduct is positive and significant, as are Original Branch High Misconduct and Pre-Merger Individual Misconduct Dummy. None the signs on the control variables change relative to the conditional logit, and except for age (which becomes statistically significant in the linear fixed effects model), all have the same statistical significance. The negative binomial model in column (3) uses the number of separate misconduct events in the post-merger period as its dependent variable instead of an indicator for whether this is greater than zero. The results in column (3) are nearly indistinguishable from the those of the conditional logit. The final, fourth column is also a conditional logit with the same controls as the first, except that we add an interaction to our main variable of interest: We include an Above Median indicator, equal to one if the introduced branch had an above median rate of misconduct, relative to other introduced branches with high misconduct. The resulting coefficient is positive but small and statistically insignificant, suggesting that the effect of peer misconduct does not become significantly stronger as peer misconduct becomes more pervasive.

In order to ensure that our design works as intended and that the merger-firm fixed effects control for any firm-level endogeneity, we construct a placebo test, reported in Table 4.4. In the first column, we estimate a logit identical to our conditional logit model in column (1) Table 4.3, except that fixed effects are omitted and the dependent variable is pre-merger misconduct instead of post-merger misconduct. Introduced Branch High Misconduct is positive and significant, which should not be possible because it means that peers met in the future affect behavior in the present (or put another way, that the present affects the past). This suggests that, without the fixed effects, there may be selection biases at the firm level affecting our results. In column (2), however, we include merger-firm fixed effects (as a conditional logit model), and the relation between advisor's behavior and the behavior of future peers is not significantly different from zero. This provides support for our identification

strategy based on mergers and firm-level fixed effects within the merger.

4.4 Similarity and Contagion

It is possible that our results in Table 4.3 are driven by other branch-level variation, such as variation in oversight. To address this, we examine effects from peers that are ethnically similar, on the assumption that advisors of the same ethnicity are more likely to associate with one another, and thus have a larger effect on each other’s behavior.⁴ We use a classifier developed by Ambekar, Ward, Mohammed, Male, and Skiena (2009) to determine advisors’ ethnicities from their names, and then construct indicator variables for advisors’ ethnic classifications. We then create an indicator equal to one if the member of an advisor’s introduced branch that are of the same ethnicity engaged in misconduct in the pre-merger period (Same Ethnicity Misconduct). Some advisors cannot be classified, resulting in a somewhat smaller sample size available for tests.

Table 4.5 reports the results of adding ethnicity fixed effects and an interaction between Introduced Branch High Misconduct and Same Ethnicity Misconduct to our model from column 1 Table 4.3. If our results are driven primarily by peer influence and not other factors, we would expect that similar peers would have greater influence than others, and so the interaction with Same Ethnicity Misconduct would be positive and significant. Table 4.5 has exactly this result—the interaction has a coefficient of 0.222 and is statistically significant. Introduced Branch High Misconduct remains economically and statistically significant with a coefficient of 0.257, meaning that less similar peers still have influence, but not as much as those in the same ethnicity. Any explanation of our results that is not based on peer effects must explain not only why introduced peers’ behavior is correlated with an advisor’s misconduct, but also why this is modulated by ethnicity.

4.5 Geographic Variation and Branch Size

Since branches are generally merged due to similar geography, it is possible that advisors are already familiar with introduced peers, and that effects may be confounded by a similar regulatory environment. To address this, we add geographic controls, in the form of merger-firm-state and merger-firm-county fixed effects to our main test. Table 4.6 reports these results in columns (1) and (2), respectively. The estimates are very similar to those in column (1) of Table 4.3; Introduced Branch High Misconduct remains positive and significant with coefficients of 0.290 and 0.321. The coefficients on Original Branch High Misconduct and Pre-Merger Individual Misconduct are also similar to those in our main test. This clearly indicates that geographic variation cannot account for our results.

⁴Bertrand, Luttmer, and Mullainathan (2000) and McPherson, Smith-Lovin, and Cook (2001) find that ethnicity is one of the strongest factors in forming social groups, and Pool, Stoffman, and Yonker (2015) find that stronger peer effects within ethnicities.

It is possible that our size controls fail to account for non-linearity in the effect of branch size on misconduct, such as economies of scale in supervision. This would result in across-branch, within-merger variation that could account for our main effect. To check for this, we replace the merger-firm fixed effects of the main specification with merger-firm-size category fixed effects, allowing for separate effects of large and small branches. Branches are divided into large and small using the median in-sample branch size. The results are reported in column (3) of Table 4.6, and as before they are very similar to the results of the main test.

4.6 Across-Branch Variation in Monitoring and Supervision

The introduced branch misconduct rate may proxy for supervision or monitoring at the branch level, which may vary substantially from one branch to another. To address this, we make a series of modifications to our main specification, the results of which are reported in Table 4.7. At the broadest level, target advisors may simply be adapting to the supervision at the acquiring branch, which the introduced branch's misconduct rate simply reflects. If this is the case, we would expect the effect of the introduced branch to be driven almost entirely by target advisors, with approximately no effect on acquirer advisors. Column (1) adds an interaction between Introduced Branch High Misconduct and an Acquirer indicator. The interaction is positive, but not significant, meaning that peer influence is insignificantly *stronger* for acquirer advisors, which is not consistent with adaptation to new supervision, or, alternatively, to adaptation to the acquiring branch's social norms.

Some studies, such as Agarwal and Hauswald (2010), find that distance from a firm's headquarters is (negatively) correlated with monitoring due to increased independence. To test for this, we introduce an indicator equal to one if a branch is within 100 miles of the firm's headquarters (Close to HQ). The result from adding this indicator and interacting it with Introduced Branch High Misconduct are reported in column (2) Table 4.7. The indicator is positive but not significant, and the interaction is very small (-0.005) and insignificant, meaning that distance from the firm's headquarters cannot plausibly account for our results. The other coefficients are very similar to those from the main test.

In columns (3), (4), and (5) of Table 4.7 we exclude three subsets of observations that could potentially confound our results. In column (3), we exclude observations from post-merger branches where there has been a pre-merger complaint citing a failure to supervise against any advisor, because lax supervision varies from one branch to another and could account for the appearance of influence. In column (4), we exclude observations from post-merger branches where there has been a regulatory action in the pre-merger period to ensure that common exposure to regulatory actions does not drive our results. Finally, in column (5) we exclude from the sample any merger where the same lawyer has represented clients against both the target and the acquirer, to ensure that lawyers targeting particular branches does not drive our

results.⁵ In each of these subsamples, the estimated coefficients are very similar to those in the main test, and their statistical significance is similar. This means that across-branch variation in monitoring and supervision, even external “supervision” from litigators, cannot explain our results.

4.7 Additional Robustness Tests

In order to rule out other possible factors or biases, we conduct a series of robustness tests, reported in Table 4.8 and Table 4.9. Specifically, we exclude misconduct settled by small dollar amounts, divide the sample into early and late periods, consider a delayed post-merger period, include advisors that exited their firm prior to the merger, and estimate our main specification using alternative pre-merger and post-merger periods.

One potential concern is that complaints involving small dollar amounts could represent nuisance complaints that are not indicative of actual misconduct. To address this, exclude complaints settled for less than \$50,000 from all of our misconduct measures and reestimate the main specification in column (1) of Table 4.8. The coefficients are very similar to the main result, so our findings cannot be driven by nuisance complaints that do not represent misconduct.

While our design inherently includes time controls, to ensure that our findings are not driven by common shocks such as the recent financial crisis, we split our sample into two subperiods. In column (2) of Table 4.8 we present results using only mergers from 1999–2005, and in column (3) we use only mergers from 2006–2011. In both periods, the coefficient on Introduced Branch High Misconduct is positive, significant, and economically similar to our main result. It is unlikely, then, that our results are driven by a specific time period.

A key assumption of our tests is that complaints received in the post-merger period is due to post-merger misconduct. However, it is possible that delays in identifying an advisor’s misconduct and filing the appropriate complaint could mean that some post-merger complaints are related to pre-merger behavior, potentially biasing our results. To address this, in column (4) we present results using a delayed post-merger period, starting two years after the post-merger period in the main specification.⁶ The coefficients estimated in column (4) are very similar to those in the main test, meaning that our results were not biased by possibly including misconduct from before the merger.

Advisors who exit their firm prior to the merger could be doing so because they have information about the norms or supervision at the introduced branch. If this is the case, our results could be affected by selection bias, where advisors that choose to stay are similar to their introduced branch. We address this by including advisors that were present at the firm before the merger but were not part of the post-merger firm in our sample, assigning them to the post-merger branch they would have joined

⁵We use the Westlaw International Database to identify cases and lawyers names.

⁶This means that the post-merger period in this test begins two years and one hundred days after the merger.

had they stayed. Presented in column (5) of Table 4.8, these results are very similar to those of our main specification. We find no evidence that advisor 'survival' can account for our results.

Finally, it is possible that our choice of three year windows before and after the merger somehow drives the results of the main specification. To test for this, we create alternate windows of one, two, and five years around the mergers in our sample and reestimate the main specification. We also introduce pre-merger windows of ten years and one hundred years (capturing an advisors' entire history in the industry). Table 4.9 shows the coefficient on Introduced Branch High Misconduct and its z-score for every combination of one, two, three, and five pre-merger and post-merger windows, and the ten and one hundred year pre-merger windows. With the exception of using a one year window on both sides of the merger, every estimated coefficient is positive, significant, and economically similar to our main result. This demonstrates that the choice of window length does not significantly affect the results.

4.8 Tables

Table 4.1: Summary Statistics: Firms, Branches, and Advisors

This table provides summary statistics of firm and advisor characteristics for the merger sample. The sample consists of 477 mergers during the period August 1999 to December 2011. Statistics are reported separately for acquirer and target firms. *Assets under management (AUM)* is summarized using only those observations for which Meridian IQ has data.

	Acquirer	Target
<u><i>Financial Advisory Firms</i></u>		
Number of Firms	477	477
Average Financial Advisors per Firm	192.0	124.2
<u><i>Branches</i></u>		
Number of Branches	5,737	7,693
Average Branches per Firm	12.0	16.1
Average Financial Advisors per Branch	16.0	7.7
<u><i>Individual Financial Advisors</i></u>		
Number of Financial Advisors	91,569	59,264
Experience	9.8	11.0
Assets Under Management (AUM) in \$ millions	80.0	88.1
Male	71.0%	71.1%
Age	39.3	41.6

Table 4.2: Misconduct by Financial Advisors

This table provides summary statistics of misconduct by financial advisors in the merger sample. Panel A tabulates the number of misconduct events and percentage of advisors that commit misconduct in the three year windows before and after the merger. Pre-merger period is the three years prior to the merger. Post-merger period is the three years after the merger. Panel B reports the misconduct statistics at the branch level. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. Panel C presents the Dollar Value Distribution of Settlements and Awards. Settlements are not disclosed for 7.5% of complaints.

Panel A: Individual Misconduct Summary Statistics		
Pre-Merger Individual Misconduct	1,867	1.02%
Post-Merger Individual Misconduct	2,812	1.40%

Panel B: Branch Misconduct Summary Statistics		
Original Branch High Misconduct		12.5%
Introduced Branch High Misconduct		14.7%

Panel C: Dollar Value Distribution of Settlements and Awards		
Mean		\$430,338
10 th Percentile		\$12,500
25 th Percentile		\$22,826
Median		\$50,000
75 th Percentile		\$150,000
90 th Percentile		\$425,294
% Settlement Not Disclosed		7.5%

Table 4.3: Mergers, Introduced Co-Workers, and Financial Misconduct

Columns (1) and (4) report estimates from a conditional logit model grouped by merger-firm. Column (2) reports estimates from a linear model with merger-firm fixed effects. Column (3) reports estimates from a negative binomial model with merger-firm fixed effects. In columns (1), (2) and (4), the dependent variable equals 1 if the advisor commits misconduct in the post-merger period (three year window). In column (3), the dependent variable is the count of misconduct events. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct Dummy* equals 1 if the advisor committed misconduct before the merger (three year window). *Above Median* equals 1 if, among the *Introduced Branch High Misconduct* branches, the advisor's *Introduced Branch* has an above median misconduct rate. The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets in columns one and two. *Z*-scores without clustering are reported in column (3). The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Cond. Logit (1)	Linear F.E. (2)	Count Model (3)	Cond. Logit (4)
Introduced Branch High Misconduct	0.313*** [4.01]	0.005*** [2.80]	0.269*** [3.73]	0.275** [2.12]
Introduced Branch High Misconduct \times Above Median				0.051 [0.29]
Original Branch High Misconduct	0.488*** [7.11]	0.010*** [5.85]	0.482*** [9.34]	0.488*** [7.07]
Pre-Merger Individual Misconduct Dummy	1.392*** [12.17]	0.077*** [8.51]	1.338*** [16.12]	1.392*** [12.12]
Standardized Age	0.040 [1.47]	0.003*** [5.00]	0.032 [1.02]	0.039 [1.46]
Standardized Experience	0.206*** [7.82]	0.003*** [6.20]	0.206*** [7.76]	0.206** [7.85]
Male Dummy	1.306*** [20.26]	0.014*** [7.52]	1.283*** [16.63]	1.306 [20.27]
Standardized AUM	0.023** [2.51]	0.002** [2.13]	0.019** [1.96]	0.023** [2.51]
Missing Demographic Data Dummy	0.493*** [4.55]	0.004*** [5.59]	0.487*** [4.56]	0.493*** [4.55]
Main Office Dummy	-0.695*** [2.71]	-0.004*** [2.59]	-0.657*** [3.72]	-0.696*** [2.70]
Solo Original Branch	-0.214 [1.18]	-0.003 [1.01]	-0.220 [1.42]	-0.701 [3.94]
log(Original Branch Size)	-0.234*** [7.34]	-0.003*** [3.93]	-0.225*** [10.48]	-0.234*** [7.37]
log(Introduced Branch Size)	-0.166*** [5.84]	-0.002*** [5.49]	-0.160*** [7.58]	-0.165*** [5.76]
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes
Number of Observations	113,963	150,746	113,963	113,963

Table 4.4: Placebo Tests of Introduced Co-Workers and Financial Misconduct

Column (1) reports estimates from a logit model of pre-merger misconduct (three year window *before* the merger). Column (2) reports estimates from a conditional logit model of pre-merger misconduct (three year window *before* the merger) with merger-firm fixed effects. For both models, the dependent variable equals 1 if the advisor committed misconduct before the merger (three year window). *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Before Pre-Merger Individual Misconduct Dummy* equals 1 if the advisor committed misconduct before the merger window (any time before the three year pre-merger window). The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Introduced Branch High Misconduct	0.595*** [7.77]	0.123 [1.53]
Original Branch High Misconduct	1.258*** [9.28]	0.548*** [3.76]
Before Pre-Merger Individual Misconduct Dummy	1.178*** [11.51]	1.035*** [12.50]
Advisor and Advisory Firm Controls	Yes	Yes
Merger-Firm Fixed Effects	No	Yes
Number of Observations	150,746	102,691

Table 4.5: Similarity and Contagion

This table reports coefficients from a conditional logit model in which the dependent variable equals 1 if the advisor commits misconduct in the 3 year post-merger period. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct Dummy* equals 1 if the advisor committed misconduct before the merger (three year window). *Same Ethnicity Misconduct Dummy* equals 1 if an advisor's new colleagues of the same ethnicity (classified using the approach from Ambekar, Ward, Mohammed, Male, and Skiena, 2009) committed misconduct before the merger (three year window). $\ln(\text{Merger Same Ethnicity Network Size})$ is the logarithm of the number of advisors of same ethnicity in the *Introduced Branch*. *Same Ethnicity Misconduct Dummy* and $\ln(\text{Merger Same Ethnicity Network Size})$ are included as controls, but not reported. The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Introduced Branch High Misconduct	0.257** [2.50]
Introduced Branch High Misconduct \times Same Ethnicity Misconduct	0.222** [2.03]
Original Branch High Misconduct	0.442*** [5.77]
Pre-Merger Individual Misconduct Dummy	1.280*** [8.85]
Ethnicity Fixed Effects	Yes
Advisor and Advisory Firm Controls	Yes
Merger-Firm Fixed Effects	Yes
Number of Observations	78,234

Table 4.6: Geography and Branch Size

This table reports coefficients from conditional logit models in which the dependent variable equals 1 if the advisor commits misconduct in the 3 year post-merger period. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct Dummy* equals 1 if the advisor committed misconduct before the merger (three year window). In column (1), the model includes Merger-Firm-State Fixed Effects (separate fixed effects for each state in which branches are located for each firm in each merger). In column (2), the model includes Merger-Firm-County Fixed Effects (separate fixed effects for each county in which branches are located for each firm in each merger). In column (3), the model includes Merger-Firm-Median Branch Size Fixed Effects (separate fixed effects for small and large branches for each firm in each merger). The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Geographic		Branch Size
	(1)	(2)	(3)
Introduced Branch High Misconduct	0.290*** [3.00]	0.321** [2.08]	0.288*** [3.62]
Original Branch High Misconduct	0.503*** [6.31]	0.571*** [4.71]	0.483*** [6.39]
Pre-Merger Individual Misconduct Dummy	1.432*** [11.32]	1.492*** [11.05]	1.395*** [12.33]
Advisor and Advisory Firm Controls	Yes	Yes	Yes
Merger-Firm Fixed Effects	Subsumed	Subsumed	Subsumed
Merger-Firm-State Fixed Effects	Yes	No	No
Merger-Firm-County Fixed Effects	No	Yes	No
Merger-Firm-Median Branch Size Fixed Effects	No	No	Yes
Number of Observations	84,457	70,614	102,274

Table 4.7: Across-Branch Variation in Monitoring and Supervision

This table reports coefficients from conditional logit models in which the dependent variable equals 1 if the advisor commits misconduct in the 3 year post-merger period. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. Column (1) includes an interaction of *Introduced Branch High Misconduct* with a dummy equal to one if the advisor initially worked for the Acquiring firm [note: the direct effect of the Acquirer dummy is subsumed by the Merger-Firm Fixed Effects]. Column (2) includes an interaction of *Introduced Branch High Misconduct* with a dummy equal to 1 if the advisor's branch is located within 100 miles of the acquirer's main office. In column (3), the sample excludes all branches with any history of failure to supervise (in the pre-merger window). In column (4), the sample excludes all branches with any history of regulatory actions (in the pre-merger window). In column (5), we exclude all mergers in which the same lawyer represents claims against advisors from both the target firm and from the acquirer firm. The models include Merger-Firm Fixed Effects. The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Introduced Branch High Misconduct	0.244** [2.02]	0.308*** [3.35]	0.271*** [3.24]	0.272*** [2.73]	0.349*** [4.16]
Original Branch High Misconduct	0.493*** [7.11]	0.492*** [7.29]	0.436*** [6.30]	0.425*** [4.83]	0.487*** [6.47]
Pre-Merger Individual Misconduct Dummy	1.394*** [12.04]	1.386*** [11.56]	1.372*** [13.09]	1.385*** [12.79]	1.295*** [15.43]
Introduced Branch High Misconduct \times Acquirer	0.159 [0.80]				
Introduced Branch High Misconduct \times Close to HQ		-0.005 [0.03]			
Close to HQ		0.128 [1.27]			
Advisor and Advisory Firm Controls	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Exclude Branches with History of Failure to Supervise	No	No	Yes	No	No
Exclude Branches with History of Regulatory Actions	No	No	No	Yes	No
Exclude Mergers with Overlap of Complainant Lawyer	No	No	No	No	Yes
Number of Observations	113,963	104,895	102,565	77,023	107,706

Table 4.8: Additional Robustness Tests

This table reports coefficients from conditional logit models in which the dependent variable equals 1 if the advisor commits misconduct in the 3 year post-merger period. *Introduced Branch High Misconduct* equals 1 if an above average percentage of the advisor's new co-workers from the *Introduced Branch* committed misconduct during the pre-merger period. *Original Branch High Misconduct* equals 1 if an above average percentage of the advisor's *Original Branch* co-workers committed misconduct during the pre-merger period. *Pre-Merger Individual Misconduct Dummy* equals 1 if the advisor committed misconduct before the merger (three year window). In column (1), we exclude any complaints with settlements of less than \$50,000 in the calculation of the misconduct variables. In column (2), we include only observations from mergers in 1999-2005. In column (3), we include only observations from mergers in 2006-2011. In column (4), the dependent variable is misconduct in a three year window that starts two years and 100 days after the merger. In column (5), we append to the sample by counterfactually assigning all advisors that exited before the completion of the merger to the post-merger branch joined by the plurality of their *Original Branch* co-workers. The models include Merger-Firm Fixed Effects. The models also include controls for advisor age, experience, gender, assets under management, the number of advisors in the *Original Branch* and *Introduced Branch*, and branch type. *Z*-scores clustered by merger are in brackets. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

45

	Exclude Small \$ Settlements (1)	Early Sample (2)	Late Sample (3)	Delayed Post Period (4)	Include Exiting Advisors (5)
Introduced Branch High Misconduct	0.267*** [2.76]	0.241** [2.21]	0.362*** [3.47]	0.337*** [4.00]	0.351*** [4.11]
Original Branch High Misconduct	0.499*** [9.10]	0.504*** [4.63]	0.488*** [5.44]	0.462*** [6.24]	0.491*** [7.07]
Pre-Merger Individual Misconduct Dummy	1.517*** [12.54]	1.748*** [13.99]	1.097*** [12.49]	1.154*** [9.74]	1.387*** [11.48]
Advisor and Advisory Firm Controls	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of Observations	113,963	48,340	65,623	105,126	116,223

Table 4.9: Alternate Windows

This table reports the *Introduced Branch High Misconduct* coefficient from models of post-merger misconduct using alternate pre-merger and post-merger windows. The models and variable definitions are based on the specification in Table 3 column 1 and other than the window size are otherwise identical. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	<i>Coefficient of Introduced Branch High Misconduct</i>			
	Post-Merger Window Length			
	1-year	2-year	3-year	5-year
1-year Pre-Merger Window	0.217 [1.43]	0.291*** [2.74]	0.308*** [3.41]	0.324*** [3.98]
2-year Pre-Merger Window	0.324*** [2.83]	0.241** [2.48]	0.258*** [3.02]	0.321*** [3.86]
3-year Pre-Merger Window	0.301** [2.55]	0.270*** [3.14]	0.313*** [4.01]	0.345*** [4.54]
5-year Pre-Merger Window	0.376*** [3.37]	0.294*** [3.16]	0.215** [2.17]	0.225** [2.17]
10-year Pre-Merger Window	0.353*** [3.12]	0.335*** [3.24]	0.296*** [2.72]	0.274*** [2.69]
Full Pre-Merger Window	0.418*** [3.20]	0.415*** [4.43]	0.426*** [4.64]	0.406*** [5.12]

Chapter 5

Conclusion

Financial advisors are a large and important part of the financial industry, worth studying in their own right due to their role as intermediaries for retail investment. Additionally, they are a useful setting for studying broader behaviors by individuals and organizations because of the industry structure and disclosure rules.

In Chapter 3, I examine the difference in misconduct rates between brokers and RIAs, which I refer to as the advisor effect, and find that the mix products and services offered by their employers appears to account for the difference, but that fiduciary standards do not appear to affect rates of misconduct. I find that the geographic location of a representative does not explain the advisor effect, either at the zip code level or when accounting for location in a state that imposes fiduciary duties on brokers. Further, the advisor effect cannot be explained by firm or branch factors such as size or how closely the representative is supervised.

Recent discussions in the media (Stulberg, 2015, for example) and a proposal by the US Department of Labor (introduced in February of 2015 and finalized in July of 2016) to impose fiduciary duties on brokers that handle retirement products make understanding how a fiduciary standard affects misconduct valuable. If, as Gough (2014) suggests, the advisor effect is due to the fiduciary standard lowering the bar for successful customer complaints, then investors might benefit significantly from stricter standards of conduct for brokers. However, I find that brokers and advisors are frequently engaged in different activities, and while there is overlap between the two groups, the advisors' higher rate of customer complaints is likely due to different products and services offered by advisors relative to brokers. This suggests that the benefits of imposing a fiduciary standard on brokers will be small, and that stricter professional standards are unlikely to have an economically significant effect on brokers' misconduct rates.

Finally, advisors receive complaints at a higher rate than brokers in several important areas, such as mutual fund sales, and advisors employed at advisory firms have a much higher rate of complaints than others. This indicates that efforts to address misconduct by brokers is missing the larger problem of misconduct by registered investment advisors, which appears to be more severe.

In Chapter 4, I—in conjunction with Stephen Dimmock and William Gerken—examine the effects of peer interactions on misconduct rates. Using mergers between

firms and the branch structure prevalent in the industry, we are able to distinguish between contagion—causal peer effects on one another—and endogenous selection or homophily—people’s preference for associating with others that are similar. We find that advisors influence each others’ propensity to engage in misconduct, and that working with peers that have a history of misconduct increases an advisor’s likelihood of engaging in misconduct in the future. This effect is stronger for individuals that have a similar ethnic background, indicating that regulatory or supervisory mechanisms cannot explain our results. Further, our results are robust to a variety of alternative specifications and controls, such as geography, monitoring and supervision, functional form, and the length of the time periods used.

Our results are relevant to firms, investors, and regulators. Firms desiring better oversight and management must consider how peer effects fit into their supervision and compliance policies. Investors who seek an honest financial advisor would be well served by examining advisors’ peers, who have strong influence over each others’ behavior and misconduct. Finally, because contagion is a negative externality, regulators seeking optimal penalty structures must account for peer effects; optimal penalties are higher in the case of economically significant peer effects in order to account for the additional damage misconduct does by spreading itself. Contagion also highlights the value of suspension and expulsion from the industry: Removing unethical advisors is likely more effective at preventing the spread of misconduct than monetary fines.

References

- Agarwal, Sumit, and Robert Hauswald, 2010, Authority and information, *Working Paper*.
- Ambekar, Anurag, Charles Ward, Jahangir Mohammed, Swapna Male, and Steven Skiena, 2009, Name-ethnicity classification from open sources, in *Proceedings of the 15th ACM SIGKDD International Conference on Knowledge Discovery and Data Mining* KDD '09 pp. 49–58 New York, NY, USA. ACM.
- Becker, Gary, 1968, Crime and punishment: An economic approach, *Journal of Political Economy* 76, 169–217.
- Bertrand, Marianne, Erzo F. P. Luttmer, and Sendhil Mullainathan, 2000, Network effects and welfare cultures, *Quarterly Journal of Economics* 115, 1019–1055.
- Bizjak, John, Michael Lemmon, and Ryan Whitby, 2009, Option backdating and board interlocks, *Review of Financial Studies* 22, 4821–4847.
- Brown, Jennifer, and Dylan Minor, 2013, Misconduct in credence good markets, *NBER Working Paper 18608*.
- Brown, Jennifer L, 2011, The spread of aggressive corporate tax reporting: A detailed examination of the corporate-owned life insurance shelter, *The Accounting Review* 86, 23–57.
- Brown, Jeffrey R., Zoran Ivković, Paul A. Smith, and Scott Weisbenner, 2008, Neighbors matter: Causal community effects and stock market participation, *Journal of Finance* 63, 1509–1531.
- Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller, 2011, Robust inference with multiway clustering, *Journal of Business & Economic Statistics* 29.
- Dimmock, Stephen G, and William C Gerken, 2012, Predicting fraud by investment managers, *Journal of Financial Economics* 105, 153–173.
- , and Nathaniel P Graham, 2016, Is fraud contagious? career networks and fraud by financial advisors, *Working Paper*.
- Eaglesham, Jean, and Rob Barry, 2014, How troubled brokers cluster, often among elderly investors, *Wall Street Journal* published Nov 12, 2014.

- Easterbrook, Frank H, and Daniel R Fischel, 1993, Contract and fiduciary duty, *Journal of Law and Economics* pp. 425–446.
- Egan, Mark, Gregor Matvos, and Amit Seru, 2016, The market for financial adviser misconduct, *Working Paper*.
- Emons, Winand, 1997, Credence goods and fraudulent experts, *The RAND Journal of Economics* pp. 107–119.
- Finke, Michael, Sandra Huston, and William Waller, 2009, Do contracts influence comprehensive financial advice?, *Financial Services Review* 18, 177.
- Glaeser, Edward L., Bruce Sacerdote, and Jose A. Scheinkman, 1996, Crime and social interactions, *Quarterly Journal of Economics* 111, 507–548.
- Glaeser, Edward L, Bruce I Sacerdote, and Jose A Scheinkman, 2003, The social multiplier, *Journal of the European Economic Association* 1, 345–353.
- Gough, Jeffrey M, 2014, Certification and fiduciary liability in the us financial advisor industry, Ph.D. thesis Florida State University.
- Hankins, Kristine W, Mark J Flannery, and Mahendrarajah Nimalendran, 2008, The effect of fiduciary standards on institutions’ preference for dividend-paying stocks, *Financial Management* 37, 647–671.
- Harris, Milton, and Artur Raviv, 1979, Optimal incentive contracts with imperfect information, *Journal of Economic Theory* 20, 231–259.
- Hong, Harrison, Jeffrey D. Kubik, and Jeremy C. Stein, 2004, Social interaction and stock-market participation, *Journal of Finance* 59, 137–163.
- Hvide, Hans K, and Per Östberg, 2015, Social interaction at work, *Journal of Financial Economics* 117, 628–652.
- Kedia, Simi, Kevin Koh, and Shivaram Rajgopal, 2015, Evidence on contagion in earnings management, *The Accounting Review* 90, 2337–2373.
- La Porta, Rafael, Florencio Lopez-de Silanes, and Andrei Shleifer, 2006, What works in securities laws?, *Journal of Finance* 61, 1–32.
- Lerner, Josh, and Ulrike Malmendier, 2013, With a little help from my (random) friends: Success and failure in post-business school entrepreneurship, *Review of Financial Studies* 26, 2411–2452.
- Litan, Robert, and Hal Singer, 2015, Good intentions gone wrong: The yet-to-be-recognized costs of the department of labor’s proposed fiduciary rule, *Report prepared by Economists Incorporated*.
- Lusardi, Annamaria, and Olivia S. Mitchell, 2011, Financial literacy and planning: Implications for retirement wellbeing, *NBER Working Paper 17078*.

- Manski, Charles F., 1993, Identification of endogenous social effects: The reflection problem, *Review of Economic Studies* 60, 531–542.
- , 2000, Economic analysis of social interactions, *Journal of Economic Perspectives* 14, 115–136.
- McCann, Craig, Chaun Qin, and Mike Yan, 2016, How widespread and predictable is stock broker misconduct?, *Working Paper*.
- McPherson, Miller, Lynn Smith-Lovin, and James M Cook, 2001, Birds of a feather: Homophily in social networks, *Annual Review of Sociology* 27, 415–444.
- Mehran, Hamid, and René M Stulz, 2007, The economics of conflicts of interest in financial institutions, *Journal of Financial Economics* 85, 267–296.
- Parsons, Christopher, Johan Sulaeman, and Sheridan Titman, 2015, The geography of financial misconduct, *NBER Working Paper 20347*.
- Pool, Veronika K, Noah Stoffman, and Scott E Yonker, 2015, The people in your neighborhood: Social interactions and mutual fund portfolios, *Journal of Finance* 70, 2679–2732.
- Posner, Richard A., 1997, Social norms and the law: An economic approach, *American Economic Review* 87, 365–369.
- PriceMetrix, 2015, The state of retail wealth management, *5th Annual Report*.
- Qureshi, Hammad, and Jonathan Sokobin, 2015, Do investors have valuable information about brokers?, *FINRA Working Paper*.
- Schanzenbach, Max M, and Robert H Sitkoff, 2007, Did reform of prudent trust investment laws change trust portfolio allocation?, *Journal of Law and Economics* 50, 681–711.
- Shalizi, Cosma R., and Andrew C. Thomas, 2011, Homophily and contagion are generically confounded in observational social network studies, *Sociological Methods & Research* 40, 211–239.
- Shue, Kelly, 2013, Executive networks and firm policies: Evidence from the random assignment of mba peers, *Review of Financial Studies* 26, 1401–1442.
- Spence, Michael, 1977, Consumer misperceptions, product failure and producer liability, *The Review of Economic Studies* pp. 561–572.
- , and Richard Zeckhauser, 1971, Insurance, information, and individual action, *American Economic Review* pp. 380–387.
- Stulberg, Ariel, 2015, A new obama rule could save ordinary investors billions in hidden fees, *Vox.com*.

- van Rooij, Maarten, Annamaria Lusardi, and Rob Alessie, 2011, Financial literacy and stock market participation, *Journal of Financial Economics* 101, 449–472.
- Wooldridge, Jeffrey M, 1997, Quasi-likelihood methods for count data, *Handbook of Applied Econometrics* 2, 352–406.

Vita

Nathaniel Phillip Graham

Place of Birth

Harbor City, CA

Educational Institutions Attended and Degrees Already Awarded

University of Kentucky, Bachelors of Business Administration, Finance & Economics

Professional Positions

Visiting Assistant Professor of Finance, Trinity University

Scholastic and Professional Honors

Gatton Fellowship

Max Steckler Fellowship

Lockett Fellowship

Departmental honors in Finance

Departmental honors in Economics