Empirical Investigation of the Causes and Effects of Misconduct in the U.S. Securities Industry

by

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Abstract

I examine how individuals and organizations interact to cause and respond to misconduct. To improve identification of the causes and effects of misconduct, I build a dataset of the instances of misconduct of a sample of approximately 10,000 stockbrokers employed in 3,600 brokerage firms in the U.S. securities industry from the archives of the Financial Industry Regulatory Authority (FINRA) from 1974 to 2013. This dataset allows me to analyze both the individual and organization levels simultaneously. I first empirically investigate the long-standing question of "bad apples" (i.e., rogue individuals) versus "bad barrels" (i.e., rogue firms) which often arises in the aftermath of misconduct and examine how much of individual-level misconduct should be attributed to individuals versus their organizations. Addressing this question has implications for who to punish and how to avoid misconduct in the first place. Using the econometrics of linked employee-employer data, I find that persistent individual differences account for two to five times more of the variation in misconduct than do persistent organizational differences. I also find evidence for a mismatch on ethics (where ethical individuals match with rogue firms and unethical individuals match with ethical firms) and show that this mismatch on ethics explains up to 20% of variation in misconduct, outweighing the contribution of either of individual or firm differences. Second, I examine the long-term, rather than commonly debated and demanded short-term, consequences of misconduct and address the variation in who gets punished for misconduct. I find that customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not. I also show that higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct. I also find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers. Third, I analyze repeat firm-level misconduct and address why some firms learn and change after misconduct while others do not. Using negative binomial models, I find that firm-level misconduct increases with past misconduct, but this relationship is weakened the longer is the elapsed time since last misconduct.

Keywords: misconduct; the U.S. securities industry; econometrics

To my family for all their love and support!

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Chapter 1.

Introduction

The potential consequences of individual- and organization-level misconduct can be enormous as we are reminded by the scandals and crises in the recent decades. Misconduct in the financial industry, in particular, is of significant concern as the integrity of financial markets has important implications for the functioning of economies nationally and globally (Coffee 2006). In this respect, the financial meltdown of 2008, fueled in part by misconduct by subprime mortgage lenders, investment banks, and ratings agencies (Lewis 2010), precipitated the recent great recession, and thus offers an unambiguous illustration of the danger of rampant financial misconduct. Better understanding of the causes and effects of financial misconduct should inform efforts to design and maintain more effective regulatory systems for capital markets that ultimately improve nations' overall economic health. Thus, research that can help prevent or mitigate the effects of misconduct can be of direct and significant benefit to society.

Indeed, interest in understanding misconduct, corruption, and unethical behavior in or by organizations has led to a substantial body of research, including some experimental, survey-based, and archival studies (Palmer, Greenwood & Smith-Crowe, 2016; Muzio, Faulconbridge, Gabbioneta, Greenwood, 2016; Palmer, 2013; Greve, Palmer & Pozner 2010; Tenbrunsel & Smith-Crowe, 2008; Trevino, Weaver & Reynolds, 2006) with laboratory-based and self-reported survey-based papers outweighing papers with behavioral field evidence (Pierce & Balasubramanian, 2015). An inherent difficulty in research on misconduct lays in the difficulty in collecting data on individual- or organization-level misconduct. Data over time is even harder to come by. In light of limited examples of studies of misconduct using archival field evidence (such as Yenkey, 2017; Aven, 2015; Palmer & Yenkey, 2015; Pierce, Snow, & McAfee, 2015; and Edelman & Larkin, 2014), prominent scholars in this field call for additional systematic and objective analysis of misconduct using panel data from actual organizations over a long period of time examining both individual and organizational antecedents and consequences of organizational misconduct (Mitchell, Reynolds, & Trevino, 2017; Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014; Craft, 2013; Kish-Gephart, Harrison, & Trevino, 2010; Tenbrunsel & Smith-Crowe, 2008).

To make progress on this opportunity, my dissertation includes three studies that systematically investigate the causes and effects of misconduct using novel datasets from an actual organizational setting over time. Specifically, each of these three studies will address one of the following research questions:

- *Is misconduct by an individual in the context of an organization more explained by individual or organizational differences?*
- *Are visible instances of misconduct by an individual in the context of an organization associated with a higher or lower likelihood of exiting the profession and being able to leave one's current employer for a new employer?*
- *Do prior instances of misconduct by an organization increase or decrease its rate of misconduct in the future?*

In particular, the first study, addresses a common debate that arises in the aftermath of scandals involving misconduct around the question of "bad apples" versus "bad barrels."[1] The second study addresses an ambiguity in our understanding of the career consequences of misconduct where some anecdotal evidence post-2008 crisis seem to question the basic expectation that misconduct impairs future labor market

[1] A version of this study is published as: Assadi, P., & von Nordenflycht, A. (2013). Bad apples or bad barrels? Individual and organizational heterogeneity in professional wrongdoing. *Academy of Management Proceedings*, (1) 17401; Assadi, P., & von Nordenflycht, A. (2016). Ethics of sorting talent on Wall Street. *Academy of Management Proceedings*, (1) 15270.; Assadi, P. (2017). Human Capital of Misconduct in the US Securities Industry. *Academy of Management Proceedings*, (1) 16576.

opportunities.[2] The third study addresses the prevalent and yet less understood dynamics of repeat misconduct by organizations.[3] These studies will also delve deeper into some of the mechanisms involved and offer additional nuances into matching on ethics and variation in punishment for misconduct depending on tenure and gender. In doing so, my studies draw from and contribute to organization and management theories including the fields of organizational misconduct, behavioral ethics, and strategic human capital.

To empirically examine my research questions, I construct and use longitudinal panels of data on stockbrokers and brokerage firms from the U.S. securities industry, including information on the instances of misconduct. This setting allows me to observe variation in misconduct at both individual and firm levels over time which will then allow me to test my hypotheses. For the first two studies, I analyze the career histories of two random samples of U.S. stockbrokers between 1974 and 2013 using econometric techniques. For the third study, I analyze the life cycles of a panel of 648 brokerage firms between 1990 and 2004.

These datasets are useful and allow for enhanced empirical analysis of misconduct, not only because they offer longitudinal field evidence from actual organizations but also because data on individual misconduct in and across organizational contexts allows for analysis of the interaction of individuals *and* organizations in explaining misconduct, whereas existing organizational misconduct research focuses largely on the individuals *or* organizations. In addition, observing individuals in different organizational context allows for better establishment of causal relationships and empirical separation of individual effects from organization effects. Furthermore, the measures of misconduct that I employ in my studies are not subject to the same degree of regulator bias and non-reporting that limits much existing misconduct research.

[2] A version of this study is published as: Assadi, P., & von Nordenflycht, A. (2015). Does it matter if stockbrokers get caught cheating? Consequences of misconduct on careers. *Academy of Management Proceedings*, (1) 17361.

[3] A version of this study is published as: Assadi, P. (2015). Running towards or running away? The patterns of repeat organizational misconduct in the U.S. securities industry. *Proceedings of the Eastern Academy of Management Conference*, 2115-2139.

Beyond theoretical and empirical implications for academics, the findings from my dissertation should have important practical implications for regulators, managers, and those who are active in the securities industry in the United States. Specifically, these findings should help answer such questions as whether regulators and managers should focus more of their resources on organizations or individuals in preventing or penalizing misconduct, which types of firms and individuals are likely to pose the greatest risks of cheating the investing public, whether misconduct has any adverse impacts on individual stockbrokers' careers, and whether firm-level misconduct generates a vicious cycle of repeat effect that firms cannot escape.

In what follows, I will introduce each of the three essays of my dissertation. Chapter 2, entitled *"Bad Apples, Bad Barrels, Redux: Empirically Estimating the Relative Influence of Individuals versus Organizations on Organizational Misconduct in the U.S. Securities Industry"* addresses a debate that often arises when misconduct is committed by an organization or by its members in the course of their work for the organization: whether it resulted from the actions of a few bad apples or from the characteristics of the organization as a whole. In this essay, I seek to estimate the relative importance of individual versus organizational characteristics in explaining the likelihood of misconduct. To do so, I exploit the licensing database of the U.S. securities industry's self-regulatory authority to build a useful dataset of the careers of 10,000 U.S. stockbrokers, including information on their 3,600 employers as well as instances of organizational misconduct. I apply two-way fixed effects models and variance decomposition techniques to estimate the percentage of variation in misconduct that can be attributed to fixed effects of individuals versus fixed effects of firms. My analyses across two different random samples of stockbrokers suggest that the variation in organizational misconduct is largely explained by individual differences rather than organizational differences – i.e., misconduct by the stockbrokers in the context of brokerage firms is more a product of "bad apples" rather than "bad barrels." Specifically, I find that persistent individual differences account for two to five times more of the variation in misconduct than do persistent organizational differences. I also find evidence for a mismatch on ethics, with bad apples match with employment at more ethical firms and ethical individuals match with rogue firms. I show that this mismatch on ethics explains up to 20% of variation in misconduct, outweighing the contribution of either individual or firm differences.

Chapter 3, entitled *“Does it Matter if Stockbrokers Get Caught Cheating? Consequences of Misconduct on Careers in the Securities Industry*”, investigates the consequences of misconduct on the careers of U.S. stockbrokers where the basic expectation is that, besides official penalties, individual-level misconduct results in reputational damage and impaired future labor market opportunities. However, the consequences of misconduct seem mild on Wall Street, where employers may perceive misconduct as a sign of aggressiveness or a cost of doing business. To address this ambiguity, I investigate the career consequences of one form of Wall Street misconduct where stockbrokers cheat their customers by generating higher fees through conducting unnecessary, unsuitable, or unauthorized transactions. Specifically, I examine whether visible instances of misconduct are associated with higher/lower likelihood of exiting the profession and being able to leave one’s current employer for another employer. I also examine whether a stockbroker’s tenure moderates the variation in the consequences of misconduct as misconduct may be a weaker signal to the market the more experienced the stockbroker is. I further examine the role of gender in light of research that documents harsher punishment for misconduct for women. I use the records of the Financial Industry Regulatory Authority (FINRA) which include stockbrokers’ employment history and any involvement in formal disputes with customers. I measure misconduct as disputes resulting in settlements or restitution payments to customers, or as regulatory sanctions. My sample includes 4,675 stockbrokers randomly selected from FINRA’s population of 1.3 million stockbrokers with employment spells at 1,877 brokerage firms between 1984 and 2013. Using robust linear probability models, I find that customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not. I also show that higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct. Furthermore, I find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers. These findings advance our understanding of the consequences of misconduct and offer insights into the variation in who gets (and does not get) punished in the aftermath of misconduct. They also offer nuance to enhance our understanding of how gender affects variation in punishment for misconduct.

Chapter 4 entitled "*Running Towards or Running Away? The Patterns of Repeat Organizational Misconduct in the U.S. Securities Industry*", investigates the patterns of repeat organizational misconduct in the U.S. securities industry. In doing so, in this essay, I address a debate on whether misconduct by Wall Street firms increases or decreases with the number of their past instances of misconduct (i.e., whether firms "run towards" more of their tainted past or they "run away" from it). In fact, repeat instances of misconduct by firms on Wall Street are of significant concern to law makers and the public. A recent analysis by the *New York Times* documents 51 repeat violations of antifraud laws by 19 large Wall Street firms between 1996 and 2011 and criticizes the regulators' practice of pursuing civil, monetary settlements where the offending firms neither admit nor deny any misconduct – which might then encourage repeat misconduct. However, it is not clear to what extent this anecdotal evidence reliably reflects what is going on in this industry as a whole – beyond its largest players. In this respect, I systematically analyze the information on instances of misconduct, as measured by firms' arbitration losses to their clients, across 648 brokerage firms between 1990 and 2004 to understand how past misconduct might facilitate or inhibit future misconduct. I also examine the moderating effect of the time that has elapsed since firms' last engagement in misconduct. In doing so, I draw from organization and management theories that inform how executives who act on behalf of a firm respond to instances of misconduct and adjust their future behavior, and test two competing hypotheses. Using panel negative binomial models, I find that misconduct increases with the number of past misconduct (i.e., support for "running towards" hypothesis) and decreases with the time that has elapsed since the last misconduct. I also find that the positive relationship between past and future misconduct is weakened the longer the time it has elapsed since the last misconduct. Together, these findings contribute to our understanding of the dynamics of repeat organizational misconduct. In addition to their theoretical and empirical contributions, these findings also have important implications for law makers, regulators, and executives who aim to understand and manage the consequences of organizational misconduct over time.

I will conclude this thesis in Chapter 5 by providing a summary of my studies along with their limitations and contributions.

Chapter 2.

Bad Apples, Bad Barrels, Redux: Empirically Estimating the Relative Influence of Individuals versus Organizations on Organizational Misconduct in the U.S. Securities Industry

2.1. Abstract

When misconduct is committed by an organization or by its members in the course of their work for the organization, there is often a debate about whether it resulted from the actions of a few bad apples or from the characteristics of the organization as a whole. I seek to estimate the relative importance of individual versus organizational characteristics in explaining the likelihood of misconduct. To do so, I exploit the licensing database of the U.S. securities industry's self-regulatory authority to build a useful dataset of the careers of 10,000 U.S. stockbrokers, including information on their 3,600 employers as well as instances of organizational misconduct. I apply two-way fixed effects models and variance decomposition techniques to estimate the percentage of variation in misconduct that can be attributed to fixed effects of individuals versus fixed effects of firms. My analyses across two different random samples of stockbrokers suggest that the variation in organizational misconduct is largely explained by individual differences rather than organizational differences – i.e., misconduct by the stockbrokers in the context of brokerage firms is more a product of "bad apples" rather than "bad barrels." Specifically, I find that persistent individual differences account for two to five times more of the variation in misconduct than do persistent organizational differences. I also find evidence for a mismatch on ethics, with rogue individuals matching with employment at more ethical firms and ethical individuals match with rogue firms. I show that this mismatch on ethics explains up to 20% of variation in misconduct and, in this way, outweighs the contribution of either individual or firm differences.

2.2. Introduction and Theoretical Framework

In the aftermath of scandals involving organizational misconduct – any illegal, unethical, or socially irresponsible behavior by individuals in the context of organizations (Greve, Palmer, & Pozner, 2010) – a common debate often arises around the question of “bad apples” versus “bad barrels”, namely should we pin the blame on individuals or on the organizations that employ them? In fact, this question arises throughout organizational life (e.g., financial industry, academia, the military) and it has drawn attention in both the financial press and academic research (e.g., organization theory).

For instance, in the wake of the 2008 financial crisis, the financial press has debated whether the blame lays with rogue individuals or corrupt organizational cultures – with different answers suggesting different approaches to punishment and future prevention (Schmidt & Wyatt, 2012; McCarty, Poole & Rosenthal, 2013; da Costa, 2014; Eaglesham & Barry, 2014; Eavis, 2014). One the one hand, the press reports that the U.S. government’s post-2008 strategy of pursuing settlements with firms instead of prosecutions of individuals has been criticized for its potential to encourage future misconduct by removing individual accountability (Schmidt & Wyatt, 2012) and advocates for pursuing criminal charges for individuals in the instances of organizational misconduct (da Costa, 2014; Eaglesham & Barry, 2014). On the other hand, the press criticizes the financial sector’s tendency for going after low-hanging bad apples (McCarty, Poole, & Rosenthal, 2013) where in fact the rotten culture of the firms through unhealthy compensation practices is at the core of the issue (Eavis, 2014). These contradictory approaches to punishment and future prevention are partly present because some pin the blame more on rogue individuals and others pin it more on corrupt organizations instead. A recent film, “The Wolf of Wall Street” by Martin Scorsese depicts these broader influences associated with individuals and organizations vis-à-vis organizational misconduct in the U.S. stock markets. This “bad apples versus bad barrels” debate in the press is not just limited to the financial industry. It extends to academic fraud (Bhattacharjee, 2013) and the U.S. Army scandals (Editorial Board, 2014). Implicit to these views is the notion that the blame rests with certain inherent time-invariant characteristics born into an individual or an organization.

In addition to the mainstream press, this debate occurs in legal theory, too. Most legal scholarship holds individuals accountable for instances of organizational misconduct, arguing that organizations can act only through individuals (Hasnas, 2007; Moohr, 2007; Richter, 2008; Bucy, 2009; Lipman, 2009; Thompson, 2009; Hasnas, 2010; Barrett, 2011; Harlow, 2011; Sepinwall, 2011; Velikonja, 2011; Hasnas, 2012; Schmidt & Wyatt, 2012). However, some legal theorists have more recently made the case for holding organizations accountable, arguing that group culture and dynamics provide a unique context for illegality (Fanto, 2008; Moore, 2009; Fanto, 2010; Sepinwall, 2010; Evans, 2011).

Of course, we know that both individuals and organizations matter in understanding and predicting misconduct in organizational contexts. Research on ethical decision making has shown that the likelihood of individual wrongdoing correlates with variations in psychological and demographic characteristics of individuals, such as cognitive moral development, age, education, and cultural and religious beliefs (Tenbrunsel & Smith-Crowe, 2008; Kish-Gephart, Harrison, & Trevino, 2010; Thoroughgood, Hunter, & Sawyer, 2011, Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013). And organizational misconduct research has shown that the likelihood of engaging in misconduct correlates with characteristics of organizations such as complexity, relative performance, ethical infrastructure/climate, and size (Vaughan, 1999; Pinto, Leana & Pil, 2008; Greve, Palmer, & Pozner, 2010; Palmer, 2012; Craft, 2013; Palmer, Greenwood & Smith-Crowe, 2016). That is, there are both bad apples and bad barrels.

What we know less about, however, is how much individual versus organizational characteristics matter. That is, what is their relative importance in explaining organizational misconduct? Should organizational misconduct be attributed largely to specific rogue individuals or instead to the corrupt organizations by which the individuals are employed – or are they equally to blame? Addressing this question is important because of its implications on who to punish and how to avoid misconduct. Organization and management research on misconduct largely focuses on one or the other dimension, where research on ethical decision making primarily focuses on differences across individuals and research on organizational wrongdoing primarily focuses on differences across organizations. Even where individual and organizational characteristics are observed and measured in the same study, their relative magnitude is not (Baker & Faulkner, 2003; Pierce & Snyder 2008; Kish-Gephart, Harrison, & Trevino 2010; Thoroughgood, Hunter, & Sawyer 2011; Craft, 2013).

There are some exceptions, particularly from experimental work, but they do not offer a consistent message. On the one hand, several renowned social psychological experiments assume that "situational and social forces overwhelm individual differences in explaining ethical behavior" (Bazerman & Gino, 2012, p. 91). On the other hand, other experimental studies of fictional organizational settings report that individual characteristics outweighed organizational conditions in explaining variance in ethical decisions.

In any case, there are also acknowledged limits on extrapolating lab experiments to real organizational contexts (Pierce & Balasubramanian, 2015), including problems with self-perceptions or self-reporting, lack of objective measures and presence of common method bias (Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress & Joseph 2014), use of unrepresentative samples of students (O'Fallon & Butterfield 2005; Craft 2013), and general difficulties in simulating the complexity of real organizational life (Trevino, den Neiuwenboer & Kish-Gephart 2013).

Limited examples of studies of misconduct using archival field evidence in the way of empirically studying individual unethical behavior include Yenkey (2017), Aven (2015), Palmer and Yenkey (2015), Pierce, Snow, and McAfee (2015), and Edelman and Larkin (2014). Thus, we are left with little in the way of empirically driven expectations regarding the relative importance of individual versus organizational influences on misconduct. Yet this question remains important to deciding how misconduct should be punished and prevented in the first place. Not surprisingly, recent reviews of ethical decision making research have called for field research (Mitchell, Reynolds, & Trevino, 2017) that "simultaneously examines different sets of antecedents" (Kish-Gephart, Harrison, & Trevino, 2010, p. 1), connects "the micro and the macro" (Tenbrunsel & Smith-Crowe, 2008, p. 591), and utilizes longitudinal data and methods (Craft, 2013) rather than cross-sectional research which does not allow for causal inferences (Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014).

To advance our empirical understanding of the relative importance of individuals and organizations in explaining organizational misconduct, I construct a novel dataset of U.S. securities firms and individual stockbrokers that identifies organizations, individuals within each organization, and professional misconduct by individuals within those organizations over time. I then exploit this observation of individuals across multiple organizational contexts to estimate the relative contribution of fixed individual effects and fixed organizational effects to explain instances of misconduct. With this approach, I can estimate the total effect of time-invariant characteristics of individuals versus organizations.

The data originates primarily from the registration database maintained by the Financial Industry Regulatory Authority (FINRA, formerly known as NASD), the principal professional association and regulatory body for the U.S. securities industry. I use instances of customer disputes and disciplinary actions in which arbitrators/FINRA rule against a stockbroker as my measurement of misconduct.

I draw on a two-way fixed effects approach to analyze my data (Abowd & Kramarz, 1999a; Abowd & Kramarz, 1999b; Abowd, Kramarz, & Woodcock, 2008; Woodcock, 2011). This approach has been used recently in labor economics to tease apart individual-specific heterogeneity from organization-specific heterogeneity in determination of earnings (Abowd, Kramarz, & Margolis, 1999; Abowd, Kramarz, Lengermann, & Perez-Duarte, 2003; Woodcock, 2003; Abowd, Kramarz, Lengermann, & Roux, 2005), and in education research to attribute student test scores to individual students and schools (Rivkin, Hanushek, & Kain, 2005; Aaronson, Barroe, & Sander, 2007).

I find that both individual and organizational heterogeneity account for statistically significant proportions of the variance in professional misconduct. But I also find that individual effects explain two to five times more of the variance in organizational misconduct than firm effects. In other words, I find that organizational misconduct arises more from bad apples or rogue individuals who commit misdeeds across multiple firms than from bad barrels or rogue organizations that corrupt the individuals that move into and through them.

I also find that, on average, rogue individuals are matched with employment at more ethical firms and ethical individuals are matched with rogue firms. To test the robustness of this finding, I employ several alternative specifications to address and mitigate the potential bias in correlation between stockbroker and firm fixed effects by focusing on sub-samples with higher observed stockbroker mobility – these specifications support the mismatch finding. Furthermore, I find that this mismatch on ethics explains up to 20% of variation in misconduct and, in this way, outweighs the contribution of either individual or firm differences.

In discussing my results, I acknowledge that my setting might condition my findings, where certain characteristics of my setting – readily observable misconduct, high mobility, and high individual discretion in production – might make individual factors more important here than in other settings. For those in this setting – securities regulators and securities firm managers – though, my findings highlight the importance of individual accountability and the importance of firms' selection, training, and monitoring processes.

2.3. Variation in Misconduct in the U.S Securities Industry

The securities industry in the U.S. consists of registered stock brokerage firms and stockbrokers that buy and sell financial securities on behalf of clients. The actions of brokerage firms and individual brokers in this industry are regulated by FINRA, the Financial Industry Regulatory Authority, which expects firms and individuals act in keeping with a set of conduct rules. Organizational misconduct occurs when stockbrokers' behavior contradicts these conduct rules. And to the extent to which some brokers can be responsible for failing to protect clients' interests, either through fraud or negligence (Astarita 2008), the U.S. securities industry provides an appropriate setting in which there is variation in misconduct that individuals engage in. In addition, this variation can be further exacerbated as stockbrokers have different levels of expertise and therefore they can exploit their non-expert clients to varying degrees due to “asymmetry of expertise” based on what the sociological theory of the professions tells us (Parsons 1939; Friedson 1994).

Organizational misconduct in the U.S. securities industry are primarily in the forms of churning (self-interested transactions), unauthorized trading, unsuitability (recommending inappropriate investments), misrepresentation of investments, and negligence (no reasonable diligence) (Astarita 2008). Any of these circumstances, according to FINRA’s conduct rules, is both unacceptable and unethical, and is considered an instance of professional misconduct which can be investigated and penalized through customer-initiated disputes and/or regulator-initiated disciplinary actions.

2.4. Individual versus Organizational Antecedents of Misconduct

As Greve, Palmer and Pozner (2010) point out, our ex ante intuitions about organizational misconduct invoke elements of both, that misconduct is conducted by rogue people or bad apples, but that it also happens in corrupt organizations or bad barrels with overly-strong performance incentives, corrupt climates, and/or lax controls. More broadly, this is consistent with what we know from literatures on behavioral ethics, organizational wrongdoing, and behavioral economics which suggest that organizational misconduct has both individual and organizational antecedents and that every effort is necessary to understand the complexity and multidetermined nature of organizational misconduct (Kish-Gephart, Harrison, & Trevino, 2010).

In this respect, Figure 2-1 illustrates a review of the literature on empirical ethical decision making by Craft (2013) and provides a summary of important time-invariant and time-varying factors concerning individuals and organizations that help explain organizational misconduct. According to this review, prior research finds that both individual factors, such as gender and experience, and organizational factors, such as organization size, explain some variation in misconduct. In what follows, I will discuss the individual and organizational antecedents of misconduct as it might pertain to the U.S. securities industry.

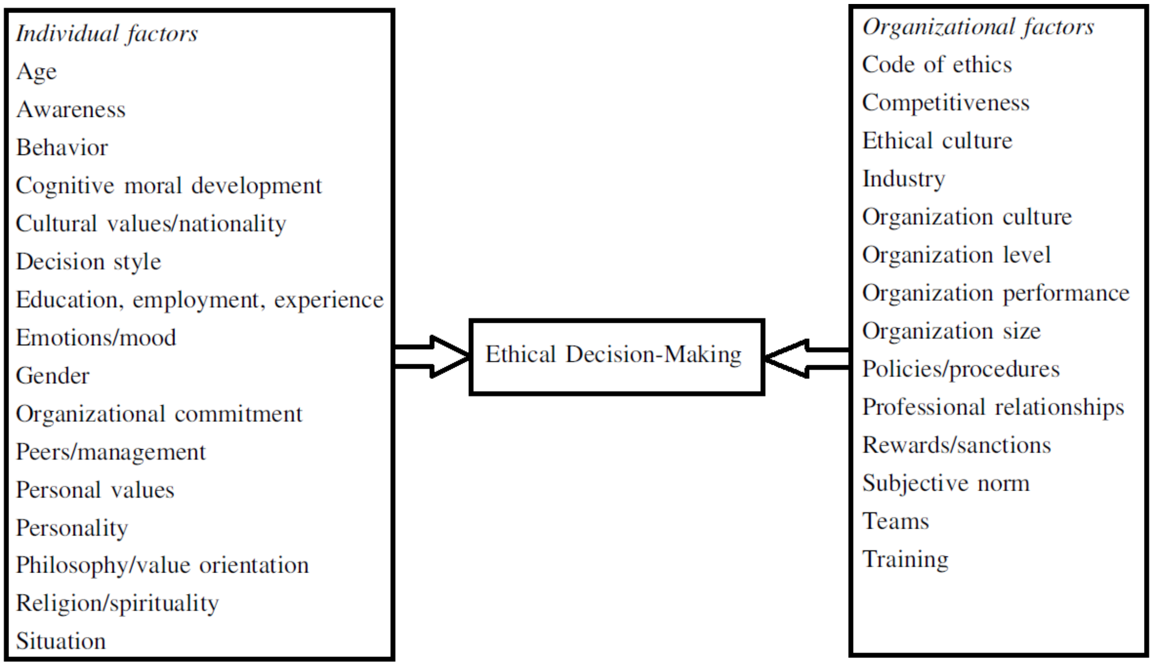

Figure 2-1. Antecedents of organizational misconduct (adopted from Craft, 2013).

2.4.1. Individual Antecedents of Organizational Misconduct

On the one hand, there is a substantial body of research in behavioral ethics which has generated insights into how individual psychological and demographic characteristics facilitate or hinder misconduct, and hence focusing on the role of "bad apples" or "a few unsavory individuals" (Trevino & Youngblood, 1990, p. 378) when it comes to explaining organizational misconduct (Ford & Richardson, 1994; Loe, Ferrell, & Mansfield, 2000; O'Fallon & Butterfield, 2005; Tenbrunsel & Smith-Crowe, 2008; Kish-Gephart, Harrison, & Trevino, 2010; O'Boyle, Forsyth & O'Boyle, 2011; Bazerman & Gino, 2012; Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013). In this respect, individuals' cognitive moral development is negatively related with unethical behavior because higher sophisticated moral reasoning around ethical issues inhibits individuals' desire to act in a way which requires lower level thinking (i.e., unethically) (Kish-Gephart, Harrison, & Trevino, 2010; Craft, 2013). Furthermore, individuals' moral philosophy of relativism is positively related with unethical behavior as individuals with this moral philosophy can view ethical issues as situationally determined and can readily rationalize their (otherwise potentially unethical) behavior (Kish-Gephart, Harrison, & Trevino, 2010; Craft, 2013).

In addition, Machiavellianism and external locus of control lead to greater unethical behavior whereas job satisfaction leads to lesser unethical behavior in the workplace (Kish-Gephart, Harrison, & Trevino, 2010; Craft, 2013). Also, other stable individual features such as moral attentiveness, moral cognition, and moral identity affect unethical organizational behavior (Trevino, den Nieuwenboer, & Kish-Gephart, 2013; Craft, 2013). Moreover, individual cognitive processes, including moral disengagement and loss decision frames, as well as affective processes, such as envy, shame, anger, or fear, are associated with unethical behavior (Tenbrunsel & Smith-Crowe, 2008; O'Boyle, Forsyth & O'Boyle, 2011; Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013; Martin, Kish-Gephart, & Detert, 2014). Lastly, several demographic variables such as gender might affect unethical behavior – although with mixed/null empirical results (Tenbrunsel & Smith-Crowe, 2008; Kish-Gephart, Harrison, & Trevino, 2010; Thoroughgood, Hunter, & Sawyer, 2011).

The individual fixed effects analysis in this study is intended to capture a variety of these time-invariant individual characteristics in explaining misconduct. Consistent with Pierce and Snyder (2008), I argue that a portion of misconduct is explained by individual fixed effect which is persistent throughout different employments.

2.4.2. Organizational Antecedents of Organizational Misconduct

On the other hand, there is a substantial body of research on organizational wrongdoing and behavioral economics, and behavioral ethics which has assessed how organizational characteristics – or ethical infrastructure (Tenbrunsel & Smith-Crowe, 2008; Trevino, den Nieuwenboer, & Kish-Gephart, 2013) – lead to misconduct even if ultimately committed by specific individuals (Ford & Richardson, 1994; Loe, Ferrell, & Mansfield, 2000; O'Fallon & Butterfield, 2005; Pierce & Snyder, 2008; Pinto, Leana & Pil 2008; Tenbrunsel & Smith-Crowe, 2008; Greve, Palmer, & Pozner, 2010; Kish-Gephart, Harrison, & Trevino, 2010; O'Boyle, Forsyth & O'Boyle, 2011; Bazerman & Gino, 2012; Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013), focusing on the role of Trevino and Youngblood's (1990) "bad barrels" effect. For example, low relative performance, strong performance incentives, corrupt climates, and lax controls (Greve, Palmer, & Pozner, 2010; Craft, 2013) as well as the overall corporate governance structure (Henle, 2006) are related to individual level misconduct in organizations.

Additionally, organizational incentives and rules, training and monitoring practices, organizational complexity and constraints, and social pressure and cultural norms tend to affect misconduct by individuals in organizational settings (Pierce & Snyder, 2008; Craft, 2013). Furthermore, more egoistic, less benevolent, and less principled ethical climates are associated with unethical behavior of individuals as these climates represent perceived organizational values with respect to unethical behavior and misconduct (Kish-Gephart, Harrison, & Trevino, 2010; Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013). Strength of ethical culture (i.e., formal and informal organizational systems such as leadership, norms, and reward policies which are designed to control behavior) and both existence and enforcement of codes of conduct are argued to be negatively related with unethical behavior (Henle, 2006; Kish-Gephart, Harrison, & Trevino, 2010; O'Boyle, Forsyth & O'Boyle, 2011; Craft, 2013; Trevino, den Nieuwenboer, & Kish-Gephart, 2013). Other features of organizations, such as their size, are also associated with organizational misconduct (Craft, 2013; Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014).

The firm fixed effects analysis in this study is intended to capture a variety of these time-invariant organizational characteristics in explaining misconduct. In this respect, I argue that a portion of misconduct is explained by organization fixed effect which is persistent over time. That is, I consider the combined effect of some of these mechanisms as an organization fixed effect that is persistent over time.

2.4.3. Individual *versus* Organizational Antecedents of Organizational Misconduct

Although theoretical perspectives from behavioral ethics, and organizational wrongdoing and behavioral economics point to the joint and simultaneous influences of individuals and organizations when it comes to explaining organizational misconduct, they do not necessarily provide insights on the relative magnitude of these influences. Our empirically driven ex ante expectation regarding the relative magnitude of individuals and organizational effects on organizational misconduct is also limited.

In this respect, one the one hand "scholars interested in the study of intentional unethical behavior argue that situational and social forces overwhelm individual differences in explaining ethical behavior" (Bazerman & Gino, 2012, p. 91) as demonstrated by Zimbardo's (1969) Stanford Prison, Milgram's (1974) electric shock experiments, and Zimbardo's (2007) Lucifer effect, but we now know more about the limitations of these experimental studies. On the other hand, in an experimental study in a fictional (rather than an actual) organizational setting, Trevino and Youngblood (1990) find that individual differences explain more of the variance in unethical decision making than do organizational differences, but, again, this experimental study has an admittedly limited organizational construct.

Although we observe that much of research on the antecedents of organizational misconduct involve individual characteristics (for comprehensive reviews, see Ford & Richardson, 1994; Loe, Ferrell, & Mansfield, 2000; O'Fallon & Butterfield, 2005; Craft, 2013), we do still observe research which also involve organizational characteristics.

In this respect, the application of these theoretical and empirical insights raises ambiguity about whether organizational misconduct in the U.S. securities industry is *more* a product of individual time-invariant heterogeneity or organizational time-invariant heterogeneity. In this way, this issue is of interest to scholars of organizational misconduct, is of practical importance to regulators, managers, investors, and clients, and is an open empirical question.

2.4.4. Match effect as an antecedent of misconduct

In addition to exploring the individual and organizational factors, I theorize a third element that might affect the occurrence of misconduct. I specifically draw on a construct from labor economics where at its core the theory is that the match between individuals and organizations help explain economic outcomes (e.g., Woodcock, 2008) such as individual earnings, productivity, and turnover. This literature documents that economic outcomes are determined not just by the separate characteristics of the individual and organization, but also by the degree to which individual and organization are a good match.

In this context, I argue that analogous reasoning may apply to misconduct, such that matches between (i.e., interaction of) individual and organizational propensities for misconduct may have an impact on the rate of misconduct over and above the separate effects of the individual and organizational characteristics. In other words, idiosyncratic fits (or misfits) between an individual and organization – that are unobservable ex ante – may explain some of the performance outcome than the separate qualities of the individual and organization. In this view, there are not only "bad apples" and "bad barrels" but also "bad matches."

Specifically, this theory would suggest that a match based on ethics would foster ethical behavior where more ethical individuals match with more ethical employment opportunities. This would occur because of both pull and push factors seeking for complementarities. That is, to foster ethical behavior and benefit from the subsequent amplifications, an ethical firm might want to be matched with more ethical individuals. Also, ethical individuals might seek matching opportunities with more ethical firms where the ethics of the firm complements the ethics of the individual.

In this context, what fosters unethical behavior (i.e., misconduct) then should be a mismatch based on ethics – where ethical individuals match with rogue firms and rogue individuals match with ethical firms. In other words, one would expect that "mismatch on ethics" explains some of the variation in misconduct, above and beyond the portion of misconduct which is explained by either of individual and firm effects. This is in part due to amplification between individual and firm (time-invariant) characteristics with respect to misconduct, where ethics of a rogue firm influences the ethical individual in the way it fosters misconduct due to the spill-over effects from the firm to the individual as documented by Pierce and Snyder (2008), and where reduced scrutiny afforded by greater structural assurance (McKendall & Wagner, 1997) in an ethical firm might foster unethical behavior for a rogue individual.

In what follows, I will describe my setting of the U.S. securities industry in section 2.5, provide details on my sample, measures, and models in section 2.6, present the results in section 2.7, and discuss my results and their implications in section 2.8.

2.5. The U.S. Securities Industry

I choose the U.S. securities industry as the setting for my empirical analysis because it satisfies several characteristics that facilitate the examination of my research question: well-defined misconduct, relatively cheap mechanisms by which to seek visible adjudication of alleged misconduct, archives of individuals' employment history and records of misconduct, and relatively high mobility across employers (which allows for better estimation of my models). Also, this setting has recently been used by other scholars addressing related questions (Egan, Matvos, & Seru, 2016; Egan, Matvos, & Seru, 2017). In this section, I describe my setting of the U.S. securities industry in more detail and discuss the conduct rules that govern it. I also discuss the processes of arbitration for customer disputes and regulatory actions.

2.5.1. Setting

The securities industry consists of firms that buy and sell financial securities on behalf of clients. This includes not only buying and selling existing securities, but also underwriting new securities issues; hence, the industry includes both stockbrokerages and investment banks. The boundaries of the industry are reasonably well-defined in the U.S. because securities trading is regulated under the provisions of the Securities Exchange Act of 1934. Any company that trades securities for its own account or on behalf of clients is required to register as a "broker/dealer" with the Securities and Exchange Commission (SEC) and with one of the industry's self-regulatory organizations (SROs), either FINRA or a specific stock exchange[4].

Employees who act as agents of broker/dealer firms (i.e., stockbrokers) must also be registered with the SEC and one of the SROs. Hence, they are often referred to as "registered representatives" (RRs). Registration as a stockbroker requires passing an exam to establish knowledge of financial securities, securities order processing, and ethical responsibilities to clients and for acceptable conduct.

[4] von Nordenflycht, A., & Assadi., P., The Public Corporation on Wall Street: Public Ownership and Organizational Misconduct in Securities Brokerage. Working paper.

As part of its mandate to regulate the licensing and professional behavior of securities stockbrokers, FINRA maintains a database of every person who is or has been registered as a securities broker, including their employment history within the securities industry and any involvement in formal customer disputes that entered the mandatory arbitration process and/or disciplinary actions by regulators. This database is publicly available to allow investors to check the licensing, training, and dispute history of a potential stockbroker.

For a given stockbroker, the FINRA database includes information on who the stockbroker has been employed by (as a stockbroker) and for how long. It also includes information on whether the stockbroker has been involved in any customer disputes or regulatory actions, and what the outcomes of such disputes or actions have been.

2.5.2. Conduct Rules

Stockbrokers' actions are governed by a set of conduct rules maintained and enforced by the SROs (principally, FINRA). These rules establish a range of ways in which stockbrokers can be responsible for failing to protect clients' interests, either through fraud or negligence (Astarita, 2008).

The most common bases for disputes between customers and their stockbrokers include customers' claims of: churning, in which stockbrokers transact securities on behalf of clients solely for the purpose of charging commissions; unauthorized trading, in which stockbrokers buy or sell securities without the client's knowledge or approval; unsuitability, in which stockbrokers recommend securities that are not appropriate for the client's age or stated investment objectives; misrepresentation, in which a stockbroker fails to disclose important facts about or even misrepresents the nature of an investment; and negligence, in which a stockbroker has simply "failed to use reasonable diligence in the handling of the affairs of the customer" (Astarita, 2008).

Remedies for alleged violations of these conduct rules may be pursued in two ways: through private action by customers via a mandatory arbitration process or through public investigation and sanction by the regulator, FINRA.

2.5.3. Arbitration of Customer Disputes

Since 1989, standard contracts between customers and their stockbrokers require that disputes be resolved through mandatory binding arbitration rather than through lawsuits in the courts (Choi & Eisenberg, 2010; Choi, Fisch, & Pritchard, 2010). In arbitration, both sides represent their case to a panel of three arbitrators. The panel of arbitrators includes two public arbitrators and one industry arbitrator, where public arbitrators have minimal ties to the securities industry (and are predominantly lawyers) and are intended to bring a neutral perspective, while industry arbitrators are securities industry participants (including stockbrokers or lawyers who also work with securities firms) and are intended to bring expertise (Choi & Eisenberg, 2010; Choi, Fisch, & Pritchard, 2010).

While the decisions of arbitrator panels are likely imperfect, they represent the judgment of a panel of experts as to whether a brokerage firm and/or an individual stockbroker treated a customer in contravention of the profession's conduct code and thus seem a credible signal of whether misconduct occurred. Furthermore, this process is easier and less expensive to initiate than court-based private action. This suggests that customers likely pursue more cases than would be the case in many other settings in which the process is court-based. This then partially mitigates the gap, endemic to misconduct research (e.g., Krishnan & Kozhikode, 2014), that exists between actual versus observed misconduct.

2.5.4. Regulatory Sanctions

According to Section 15A of the Securities Exchange Act of 1934 and FINRA Rule 8310 which is elaborated in FINRA Sanctions (2017), FINRA can impose a variety of sanctions on stockbrokers and securities firms that are found guilty of an infraction, including: limitation (where a respondent's business activities, functions or operations are limited or modified), fine, censure, suspension (where a respondent's business activities are suspended for a specific period of time or until certain act is performed), and bar/expulsion (where a respondent stockbroker or firm is barred from the securities industry).

These sanctions are designed with the aim of protecting the investing public and deterring misconduct in the first place. There are several considerations in determining appropriate sanctions for violations, depending on the facts of a case and the type of violation involved (FINRA Sanctions, 2017). Relevant disciplinary history of a respondent could influence a regulatory sanction.

According to FINRA Sanctions (2017), a few examples of cases that might be penalized by regulatory sanctions include: activity away from associated person's member firm because of the inherent failure to comply with rule requirements, sales of unregistered securities, recordkeeping violations and forgery or falsification of records.

2.6. Samples, Measures, and Models

This section presents more detail on my two samples, my three different but related measurements of organizational misconduct, and the econometric models I used to estimate my effects of interest followed by variance decomposition.

2.6.1. Samples

From FINRA records, I drew two samples through BrokerCheck for my study. BrokerCheck is "a tool from FINRA that can help [the investing public] research the professional backgrounds of brokers and brokerage firms, as well as investment adviser firms and advisers" including information on employment history and any violations for brokers and investment advisors (FINRA, 2017).

First, I drew a random sample (hereafter referred to as the "simple random sample") of 4810 individuals from the population of the 1,301,584 people who were ever registered as a securities broker in the U.S. This sample is random in the sense that each individual active or inactive stockbroker in the sample had the same probability of being selected from the population. These sampled stockbrokers were employed in 1996 stockbrokerage firms during 1974-2013, and 2526 of these stockbrokers moved across firms at least once in my sample timeframe (i.e., 2284 did not). 4.4% of these brokers were shown to have engaged in misconduct in their career. The subsequent panel from this sample includes 51395 broker-year observation, from which 11023 reflect new employment. Table 2-1 summarizes the basic features of my simple random sample.

Table 2-1. Basic features of simple random sample.

Brokers	4,810
Stayers	2,284
Movers	2,526
% brokers with misconduct in their career	4.4%
Firms	1,996
Broker-firm match	10,840
Firm-year match	14,498
Years	1974-2013 (40 years)
Observations	51,395
Observations that reflect a new employment	11,023

However, this simple random sample runs the risk of having only minimal connectedness between sampling frames (i.e., individuals and firms may not necessarily be highly connected through employment relationships). This may be problematic because most statistical analyses on longitudinal linked employer-employee data rely on connectedness between sampling frames for identification of individual and firm effects, meaning that lack of enough connectedness might substantially complicate or prevent identification by traditional methods (Woodcock, 2005).

To counteract this risk of lack of enough connectedness, I also drew a "dense random sample" (Woodcock, 2005). This sample is otherwise equivalent to a simple random sample of observations from one sampling frame of individuals or organizations, meaning all individual stockbrokers have an equal probability of being selected, except that it ensures each sampled stockbroker is connected to at least n other stockbrokers in a reference time period by means of a common employer. To construct a dense random sample, I use Woodcock's (2005) proposed algorithm. To do so, I select a reference period of May 2013 and start from a population of 630,131 stockbrokers and restrict my sample such that each stockbroker is employed at only one brokerage firm at that time (May 2013) and that all firms have at least 9 employees at that time. I do so because firms with 8 or fewer employees will not likely have the critical mass to maintain strong organizational features that would generate significant influence. Then, in that reference period, I sample firms with probabilities that are proportional to their employment, meaning that firms with more employment are more likely to be selected. In the next step, I sample workers within sampled firms, with equal (firm-specific) probabilities. In this way, the probability of sampling a particular stockbroker within a brokerage firm is inversely proportional to the firm's employment in my chosen reference period. The resulting probability of sampling any stockbroker using this algorithm is a constant.

However, to apply the dense sampling approach to my data source, I could only select from the set of currently active stockbrokers (which became my reference period of May 2013). This means that my dense random sample potentially suffers from survivorship bias, if those who engaged in misconduct in the past were more likely to be selected out – hence looking at the career histories of the currently active set of stockbrokers may be less representative of the overall level of misconduct, relative to my simple random sample.

My dense sample is a random draw of 4854 U.S. stockbrokers who were active in May 2013. Of these, 2768 were employed at more than one firm over my sample timeframe (i.e., 2086 were not). These sampled stockbrokers were employed in 1613 stockbrokerage firms during 1974-2013. This is fewer than the 1996 firms involved in the simple random sample, suggesting that the dense random sample is more connected than the simple random sample because relatively same number of brokers with a similar mover percentage are now distributed in lesser number of firms. 4.4% of these brokers were shown to have engaged in misconduct in their career. The subsequent panel from this sample includes 63064 broker-year observation, from which 11752 reflect new employment. Table 2-2 summarizes the basic features of my dense random sample.

Table 2-2. Basic features of dense random sample.

Brokers	4,854
Stayers	2,086
Movers	2,768
% brokers with misconduct in their career	4.5%
Firms	1,613
Broker-firm match	11,521
Firm-year match	11,945
Years	1974-2013 (40 years)
Observations	63,064
Observations that reflect a new employment	11,752

In both samples, I collected the sampled stockbrokers' complete work histories including instances of misconduct through FINRA's BrokerCheck (see an example visual report in Appendix A and a detailed pdf report in Appendix B). I create a panel dataset from 1974 to 2013 – a 40 years period. The FINRA data identifies the dates of employment as a registered representative at any licensed stockbroker/dealer firm; the time when any customer disputes were filed and resolved; the way those disputes were resolved (dismissal, settlement, or monetary judgment against the stockbroker); and the time that any regulatory actions were announced.

My samples are useful because individual stockbrokers and their employers are identified and followed over time, the employment relationship between a stockbroker and his/her employer is continuously monitored, and use of a dense (and yet random) sampling procedure allows for higher connectedness while the use of a simple sampling procedure allows for lower potential survivorship bias (Abowd, Kramarz, & Woodcock, 2008).

2.6.2. Measures

My measurement of organizational misconduct, the dependent variable of this study, is three-fold: (1) the number of instances of customer disputes in which arbitrators rule against a stockbroker (i.e., number of awards or lost cases); (2) the number of instances of lost customer disputes plus the number of settlements – cases where customer and stockbrokers settle (i.e., number of cases where a payment was involved to the client); and (3) and the number of instances of lost customer disputes and settlements, plus regulatory actions (i.e., number of all instances of proven misconduct).

This third measure considers any of regulatory actions, settlements, or awards against a broker as an indicator of misconduct. The other two measures ensure the robustness of my misconduct measure, one that only considers customer disputes resulting in awards to customers (i.e., first measure) and one that considers payments of any sort including awards and settlements (i.e., second measure) as indicators of misconduct. In doing so, I also allow flexibility if there is something qualitatively different in measuring misconduct by considering all available information versus measuring misconduct by only considering awards and/or payments.

In my regression analysis, I control for a number of variables including:

- Industry tenure: I measure industry tenure based on the number of years an individual was employed in the securities industry.

- Firm tenure: I measure firm tenure based on the number of years an individual was employed with a firm.

- Relative firm size: I measure the relative size of the firms in my sample by log of the number of employees that they employ in my sample.

- Frequency of employer change: This variable measures the frequency with which a given broker changes employers. In other words, this variable controls for the number of times that a broker has changed employers.

- All yearly misconduct: I measure the number of brokers shown to have engaged in misconduct on a yearly basis. This measure works similar to controlling for year effects in regression models in the way it captures idiosyncrasies of different years during the course of my analysis – but demands lesser computing power to run the models involved. Hence, depending on computing requirements, I use either of these approaches (i.e., using all yearly misconduct or year dummies).

2.6.3. Models

To analyze my linked employee-employer panel, I use two-way fixed effects models to jointly derive individual and firm fixed effects (Abowd & Kramarz, 1999a; Abowd & Kramarz, 1999b; Abowd, Kramarz, & Woodcock, 2008; Woodcock, 2011). In other words, I seek to decompose the variance in the likelihood of misconduct to its individual and organizational elements. This approach focuses on disentangling time-invariant individual and organizational influences on a given outcome.

I first estimate Equation 2-1:

$$y_{it} = \theta_i + \psi_{J(i,t)} + x_{it}\beta + \varepsilon_{it}$$

Equation 2-1. Two-way regression model.

where the dependent variable is misconduct by individual i at time t (while employed at firm j), the function J(i,t) indicates the employer of stockbroker i at time t, the first component in the right hand side of the equation is the stockbroker fixed effects, the second component is the firm fixed effects, the third component is the time-varying measured characteristics effect (such as firm tenure, industry tenure, relative size, frequency of employer change), and the last component is the statistical residual, orthogonal to all other effects in the model.

For robustness of my estimations, I also control for year fixed effects to account for unobserved shocks over time and include robust standard errors (i.e., Huber/White/sandwich estimates of the covariance matrix) to rule out understated standard errors and overstated statistical significance.

After estimating this regression model, I decompose the variance of organizational misconduct to its fixed individual and firm components to address the question of bad apples versus bad barrels, using Equation 2-2.

$$Var(y_{it}) = Cov(y_{it}, y_{it}) = Cov(y_{it}, \theta_i + \psi_{J(i,t)} + x_{it}\beta + \varepsilon_{it}) \Rightarrow$$
$$Var(y_{it}) = Cov(y_{it}, x_{it}\beta) + Cov(y_{it}, \theta_i) + Cov(y_{it}, \psi_{J(i,t)}) + Cov(y_{it}, \varepsilon_{it})$$

Equation 2-2. Variance decomposition model.

where the component on the left-hand side of the equality is the variance of organizational misconduct, and the components on the right-hand side of the equality from left to right are the contribution of measured time-varying effects, the contribution of individual time-invariant effects (i.e., bad apples effect), the contribution of organizational time-invariant effects (i.e., bad barrels effect), and contribution of residual effects to the overall variation of organizational misconduct.

For estimation of match effect models, I add match fixed effects to the above regression models. That is, I include a dummy for every broker-firm match in my analysis, in addition to dummies for brokers and firms separately (i.e., a full dummy specification). For robustness, I include two-way firm-broker clustered standard errors to rule out overstated statistical significance. Once the match effect models are estimated, I use variance decomposition to decompose the variance of misconduct explained by the match effects as well as by firm and broker fixed effects.

2.6.4. Basic Features and Descriptive Statistics of Samples

In this section, I first provide various descriptive statistics of my data and then illustrate some of its basic features in both simple and dense random samples. These statistics and illustrations are useful in the way they describe some of the basic features of my data.

Table 2-3 presents basic statistics of my variables in both samples. This table shows that my simple random panel consists of 4810 stockbrokers and 1996 firms in which these stockbrokers were employed sometime in their career during 1974-2013. It also shows that my dense random panel consists of 4854 stockbrokers and 1613 firms during the same period.

As Table 2-3 shows, 0.7% of the observations in my simple random sample include instances of misconduct (i.e., lost cases, settlements, plus regulatory actions) while this number is 0.5% in my dense sample – which could reflect the possibility that my dense sample has more of a survivorship bias than my simple random sample by construction. This table also shows that the average industry/firm tenure and firm size is slightly higher in dense random sample than simple random sample.

Table 2-3. Basic statistics in simple random and dense random samples.

	Simple						Dense					
	N	mean	p50	sd	min	max	N	mean	p50	sd	min	max
awards	51,395	0.001	0	0.03	0	2	63,064	0.000	0	0.02	0	1
payments	51,395	0.005	0	0.09	0	7	63,064	0.004	0	0.08	0	6
all misconduct	51,395	0.007	0	0.10	0	7	63,064	0.005	0	0.08	0	6
tenure-ind	51,395	9.8	8	8.1	1	56	63,064	10.4	8	8.1	1	54
tenure-firm	51,395	5.5	4	5.3	1	48	63,064	6	4	5.6	1	54
lnsize	51,395	2.2	2.3	1.5	0	4.9	63,064	2.8	2.7	1.6	0	5.7
freqchange	51,395	1.1	1	1.6	0	13	63,064	1.1	1	1.6	0	14
allyearly	51,395	13.8	15	6.6	0	26	63,064	15.5	10	13.3	0	49
Unique brokers	4,810						4,854					
Unique firms	1,996						1,613					
Year	’74-13						’74-13					

*awards: lost cases
*payments: lost cases + settlements
*all misconduct: lost cases + settlements + regulatory disciplines

Table 2-4 offers the pairwise correlation coefficients between all the dependent and independent variables in my regressions. The immediate line following each row of correlation coefficients report the significance level of each correlation coefficient.

Table 2-4. Pairwise correlations in simple and dense random samples.

	Simple								Dense							
	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
1 awards	1.0								1.0							
2 payments	0.3	1.0							0.2	1.0						
	0.0								0.0							
3 All misconduct	0.3	0.9	1.0						0.2	0.9	1.0					
	0.0	0.0							0.0	0.0						
4 tenure-ind	0.0	0.0	0.0	1.0					0.0	0.0	0.0	1.0				
	0.0	0.0	0.0						0.1	0.0	0.0					
5 tenure-firm	0.0	0.0	0.0	0.6	1.0				0.0	0.0	0.0	0.6	1.0			
	1.0	0.1	0.3	0.0					0.4	0.0	0.0	0.0				
6 lnsize	0.0	0.0	0.0	0.0	0.1	1.0			0.0	0.0	0.0	0.1	0.2	1.0		
	1.0	0.1	0.1	0.0	0.0				0.2	0.0	0.0	0.0	0.0			
7 freqchange	0.0	0.0	0.0	0.5	-0.1	-0.2	1.0		0.0	0.0	0.0	0.5	-0.1	-0.1	1.0	
	0.1	0.0	0.0	0.0	0.0	0.0			0.0	0.0	0.0	0.0	0.0	0.0		
8 allyearly	0.0	0.0	0.0	0.0	0.0	0.2	0.0	1.0	0.0	0.0	0.0	0.1	0.1	0.2	0.1	1.0
	0.3	0.0	0.0	0.0	0.0	0.0	0.0		0.2	0.0	0.0	0.0	0.0	0.0	0.0	

Having reviewed the basic descriptive statistics of my data, I depict the distribution of my sampled stockbrokers' start year in the simple random sample and dense random sample in Figure 2-2 and Figure 2-3 respectively. By construction, the dense random sample includes more stockbrokers with more recent start dates – but otherwise it spans similar to simple random sample over the years.

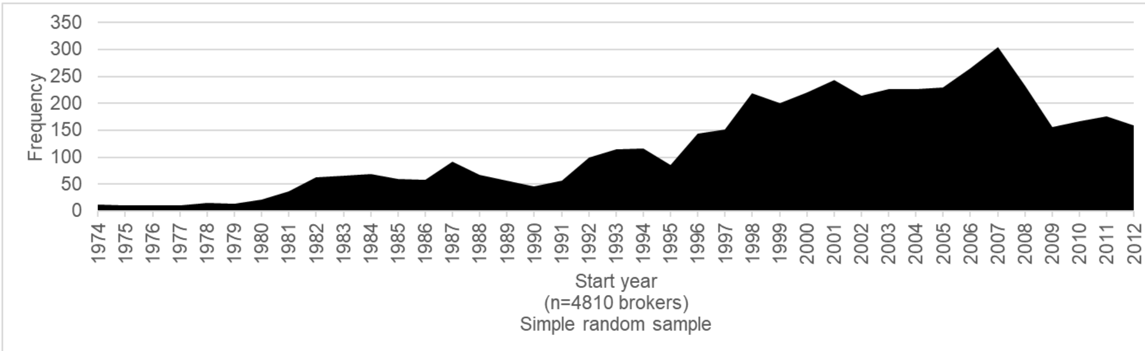

Figure 2-2. Distribution of sampled broker start year in simple random sample.

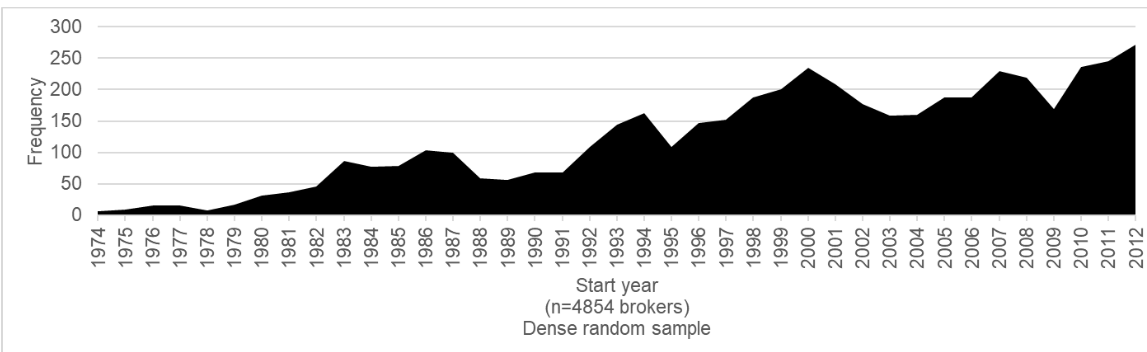

Figure 2-3. Distribution of sampled broker start year in dense random sample.

The distribution of stockbrokers' tenure in the industry is shown in Figures 2-4 and 2-5. The average industry tenure in the dense random sample is slightly higher than the simple random sample – by construction. However, the distributions are otherwise similar.

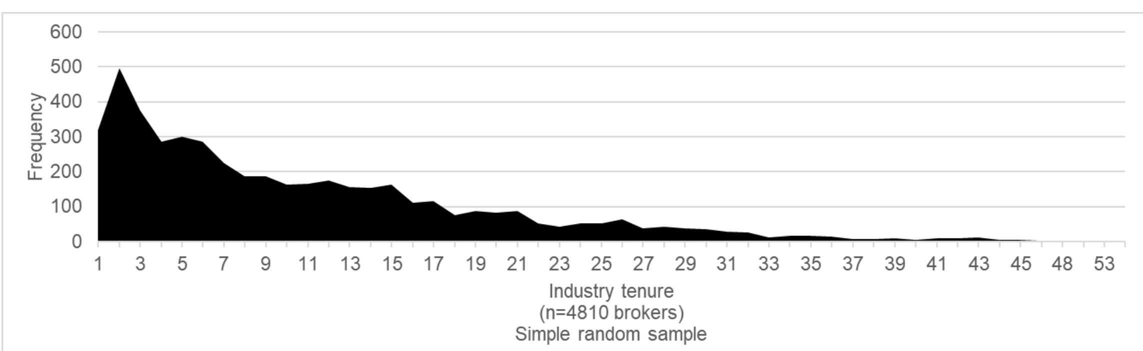

Figure 2-4. Distribution of broker tenure in the industry in simple random sample.

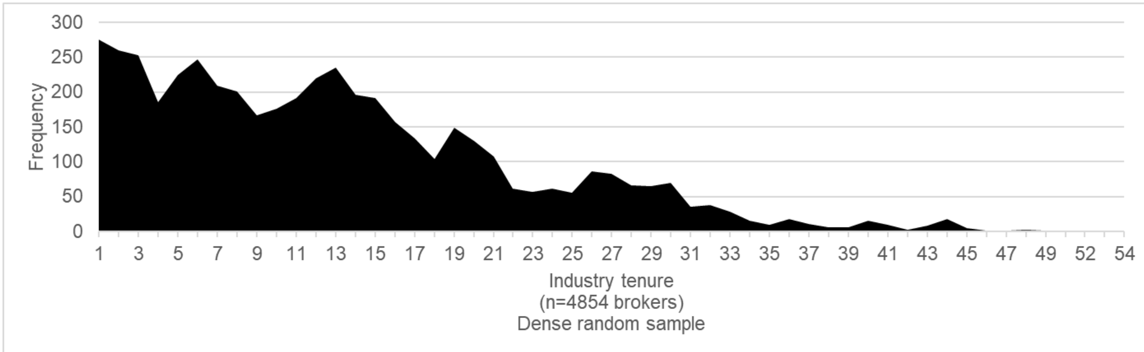

Figure 2-5. Distribution of broker tenure in the industry in dense random sample.

Figure 2-6 and Figure 2-7 illustrate the firm size as measured by the number of sampled brokers in simple and dense random samples respectively. The distributions are similar in the way they show how this industry consists of larger number of small firms.

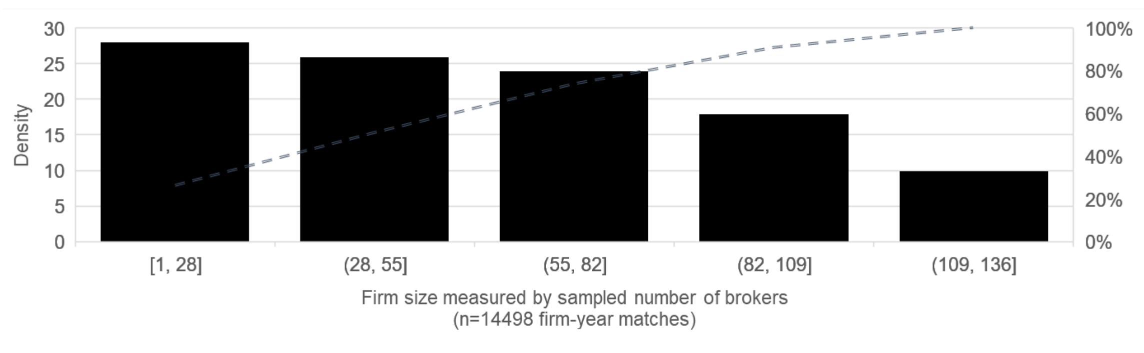

Figure 2-6.Distribution of firm size over the years in simple random sample.

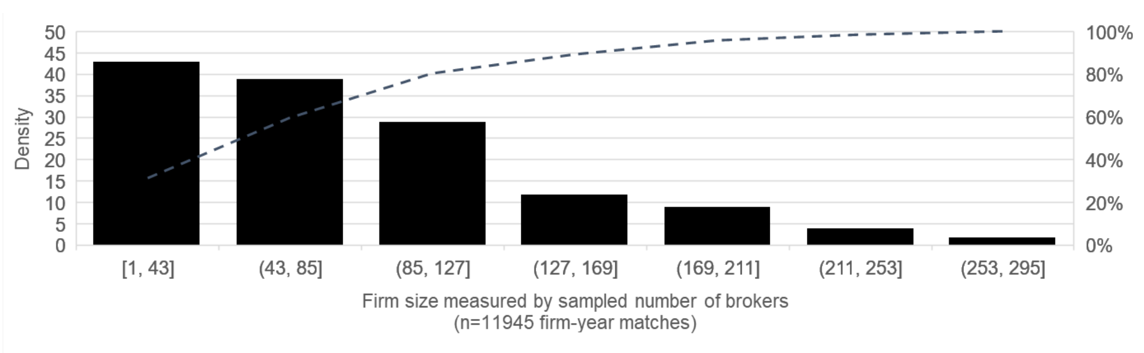

Figure 2-7. Distribution of firm size over the years in dense random sample.

Lastly, Figure 2-8 and Figure 2-9 show the number of stockbrokers with misconduct over the years in both the simple random sample and dense random sample.

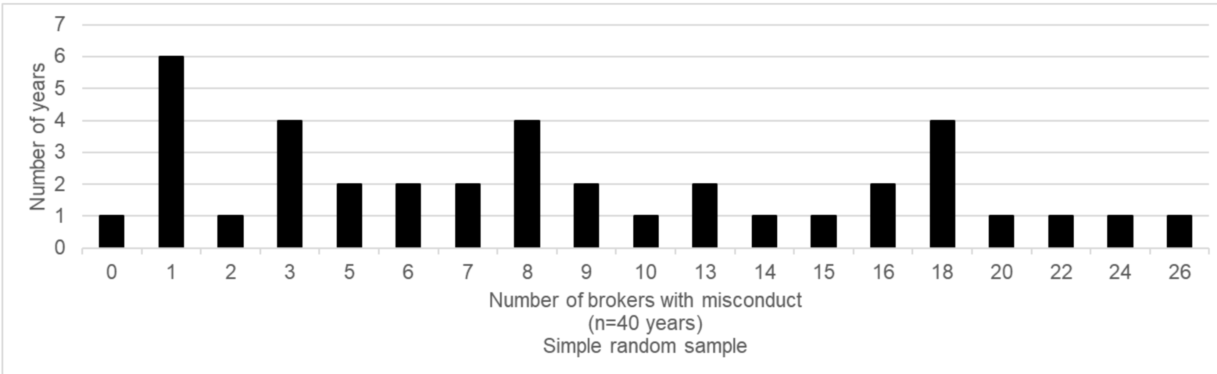

Figure 2-8. Distribution of all yearly misconduct in simple random sample.

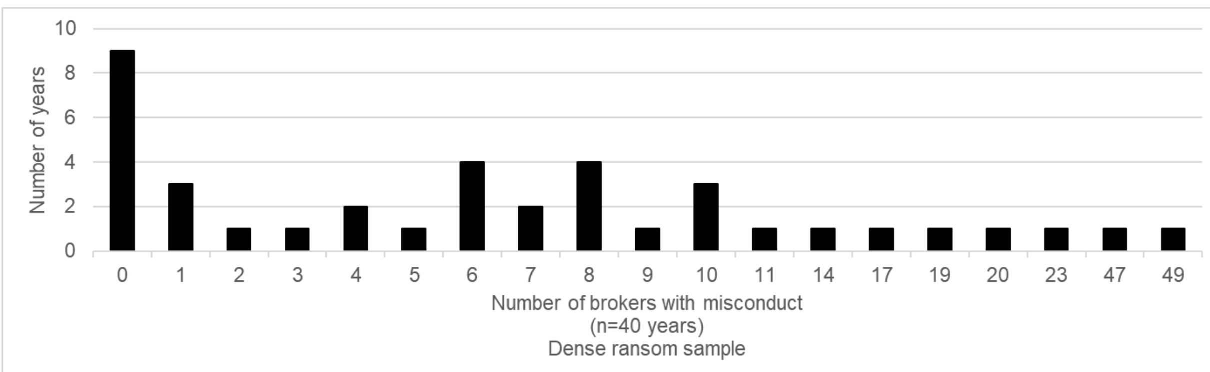

Figure 2-9. Distribution of all yearly misconduct in dense random sample.

Taken together, these descriptive statistics set the stage for a more in-depth analysis of the sample to examine whether it satisfies the requirements for adequately estimating the coefficients of interest in a two-way regression model.

2.6.5. Sample Requirements for Two-way Regression Analysis

Two-way fixed effects models of employee-employer datasets require that (1) employees move (i.e., have more than one employers in their careers), (2) employees be observed multiple times during time, (3) employers employ movers, and (4) the largest connected employee-employer network contain the majority of the employees and employers – to produce reliable estimates of employee and employer fixed effects. The two panels emanating from my simple and dense random samples meet these requirements and are useful for identification purposes in running two-way regression models.

First, employees move in my dataset, meaning that they have more than one employer in their careers. Table 2-5 summarizes the number of firms that workers are employed in. From this table, it is clear that the majority of brokers in each of the simple and dense random samples have been employed in 2 or more firms (because 47.48% and 42.97% of the brokers in simple and dense random samples only ever had one employer).

Table 2-5. Number of firms brokers have been employed in.

	Simple			Dense		
Number of firms	Freq.	Percent	Cum.	Freq.	Percent	Cum.
1	2,284	47.48	47.48	2,086	42.97	42.97
2	1,029	21.39	68.88	1,063	21.9	64.87
3	615	12.79	81.66	709	14.61	79.48
4	379	7.88	89.54	449	9.25	88.73
5	233	4.84	94.39	246	5.07	93.80
6	128	2.66	97.05	144	2.97	96.77
7	54	1.12	98.17	70	1.44	98.21
8	31	0.64	98.81	42	0.87	99.07
9	20	0.42	99.23	24	0.49	99.57
10	20	0.42	99.65	6	0.12	99.69
11	10	0.21	99.85	3	0.06	99.75
12	4	0.08	99.94	5	0.1	99.86
13	2	0.04	99.98	3	0.06	99.92
14	1	0.02	100	2	0.04	99.96
15				2	0.04	100
Total	4,810	100		4,854	100	

Specifically, Table 2-6 shows that 52.52% of brokers in simple and 57.03% of brokers in dense random sample are movers. This satisfies the first requirement of the sample for having movers in the data for estimation purposes.

Table 2-6. Movers vs stayers.

	Simple			Dense		
Mover	Freq.	Percent	Cum.	Freq.	Percent	Cum.
0	2,284	47.48	47.48	2,086	42.97	42.97
1	2,526	52.52	100	2,768	57.03	100
Total	4,810	100		4,854	100	

Second, employees are observed multiple times during time in my dataset. Table 2-7 shows that approximately half of the brokers were observed 8 or more times in the simple random sample and 11 or more times in the dense random sample. This satisfies the second requirement of the sample for my estimation purposes.

Table 2-7. Number of observations per broker.

	Simple			Dense		
Obs. per person	Freq.	Percent	Cum.	Freq.	Percent	Cum.
1	320	6.65	6.65	276	5.69	5.69
2	497	10.33	16.99	260	5.36	11.04
3	374	7.78	24.76	252	5.19	16.23
4	286	5.95	30.71	185	3.81	20.05
5	300	6.24	36.94	224	4.61	24.66
6	289	6.01	42.95	248	5.11	29.77
7	223	4.64	47.59	208	4.29	34.05
8	185	3.85	51.43	201	4.14	38.20
9	188	3.91	55.34	168	3.46	41.66
10	164	3.41	58.75	176	3.63	45.28
11	162	3.37	62.12	190	3.91	49.20
12	175	3.64	65.76	220	4.53	53.73
13	157	3.26	69.02	235	4.84	58.57
14	151	3.14	72.16	196	4.04	62.61
15	164	3.41	75.57	193	3.98	66.58
16	112	2.33	77.90	157	3.23	69.82
17	117	2.43	80.33	132	2.72	72.54
18	75	1.56	81.89	104	2.14	74.68
19	87	1.81	83.70	149	3.07	77.75
20	84	1.75	85.45	130	2.68	80.43
21	88	1.83	87.28	108	2.22	82.65
22	49	1.02	88.30	63	1.30	83.95
23	44	0.91	89.21	57	1.17	85.13
24	56	1.16	90.37	62	1.28	86.40
25	54	1.12	91.50	54	1.11	87.52
26	67	1.39	92.89	88	1.81	89.33
27	41	0.85	93.74	84	1.73	91.06
28	45	0.94	94.68	65	1.34	92.40
29	39	0.81	95.49	67	1.38	93.78
30	36	0.75	96.24	67	1.38	95.16
31	35	0.73	96.96	37	0.76	95.92
32	29	0.60	97.57	41	0.84	96.77
33	13	0.27	97.84	27	0.56	97.32
34	16	0.33	98.17	16	0.33	97.65
35	16	0.33	98.50	11	0.23	97.88
36	7	0.15	98.65	17	0.35	98.23
37	11	0.23	98.88	12	0.25	98.48
38	16	0.33	99.21	8	0.16	98.64
39	38	0.79	100	66	1.36	100
Total	4,810	100		4,854	100	

Third, employers employ movers in my dataset. Table 2-8 shows that 92.08% of the firms in the simple random sample and 99.94% of the firms in the dense random sample did have at least one mover (because only 7.92% of the firms in the simple random sample and 0.56% of the firms in the dense random sample did not have any movers), confirming that the vast majority of the firms did have movers in both of my samples. Again, this allows for better estimation of my models and satisfies the third requirement of the sample for my analysis.

Table 2-8. Number of mover brokers per firm.

	Simple			Dense		
movers per firm	Freq.	Percent	Cum.	Freq.	Percent	Cum.
0	158	7.92	7.92	9	0.56	0.56
1- 5	1,004	50.3	58.22	878	54.43	54.99
6- 10	300	15.03	73.25	214	13.27	68.26
11- 20	235	11.77	85.02	178	11.04	79.29
21- 30	92	4.61	89.63	80	4.96	84.25
31- 50	74	3.71	93.34	82	5.08	89.34
51- 100	63	3.16	96.49	84	5.21	94.54
>100	70	3.51	100	88	5.46	100
Total	1,996	100		1,613	100	

Fourth, the largest connected employee-employer network in my dataset contains the majority of the employees and employers in my data. Table 2-9 shows the groups of firms that are connected through worker mobility for both the simple and dense random samples. By construction, there are 38 connected groups of firms in the simple random sample versus only 3 in the dense random sample – suggesting higher connectedness in the dense sample as expected.

More importantly, Table 2-9 shows that there are 38 exclusive groups within which there is worker mobility and that the largest connected network in my data (Group 1) from the simple random sample includes 1739 firms which employ 4574 brokers (of which 2487 are movers) – that is the majority of the firms and the brokers in my data. 158 firms which employ 187 stayers (Group 0 which regroups firms with no movers) are not connected to any other firms because they do not have any movers. This means no firm effect in Group 0 of firms is identified. 1800 other firm effects are identified (number of firms - number of firms without movers - number of groups excluding Group 0 = 1996 – 158 – 38 = 1800).

For my dense random sample, Table 2-9 shows that there are 3 exclusive groups within which there is worker mobility and that the largest connected network in my data (Group 1) includes 1599 firms which employ 4835 brokers (of which 2766 are movers) – that accounts for the majority of the firms and the brokers in my data. In the dense sample, 9 firms which employ 17 stayers (Group 0 which regroups firms with no movers) are not connected to any other firms because they do not have any movers. This means no firm effect in Group 0 of firms is identified. However, 1601 other firm effects are identified (number of firms - number of firms without movers - number of groups excluding Group 0 = 1613 – 9 – 3 = 1601).

Hence, the fourth requirement for my sampled data is satisfied in both the simple and dense random samples.

Table 2-9. Groups of firms connected by worker mobility.

		Simple				**Dense**		
group	Person-years	Persons	**Sample** Movers	Firms	Person-years	Persons	**Sample** Movers	Firms
0	1,211	187	0	158	115	17	0	9
1	49,600	4,574	2,487	1,739	62,909	4,835	2,766	1,599
2	30	1	1	3	30	1	1	2
3	13	1	1	2	10	1	1	3
4	38	1	1	3				
5	30	1	1	2				
6	37	1	1	4				
7	19	1	1	2				
8	20	2	1	2				
9	16	1	1	4				
10	12	1	1	4				
11	16	1	1	3				
12	23	1	1	2				
13	11	2	1	5				
14	2	1	1	2				
15	7	1	1	2				
16	11	1	1	3				
17	24	1	1	2				
18	19	1	1	4				
19	21	1	1	2				
20	14	1	1	2				
21	25	6	1	2				
22	13	1	1	2				
23	23	2	2	3				
24	15	1	1	3				
25	21	1	1	2				
26	11	1	1	3				
27	6	1	1	2				
28	6	1	1	3				
29	32	3	2	6				
30	11	1	1	2				
31	4	1	1	2				
32	9	1	1	3				
33	10	1	1	2				
34	5	1	1	2				
35	5	1	1	2				
36	8	2	1	2				
37	6	2	1	2				
38	11	1	1	3				
Total	51,395	4,810	2,526	1,996	63,064	4,854	2,768	1,613

Together, the four requirements of the data for better identification of the broker and firm fixed effects in my both simple and dense random samples are satisfied: brokers move and are observed multiple times, brokerage firms employ movers, and the largest connected broker-firm network contains the majority of the brokers and brokerage firms.

2.6.6. Two-Way Fixed Effects Regression Analysis and Variance Decomposition – Bad Apples versus Bad Barrels

I run my estimation models in Stata using the method proposed by Andrews, Schank, and Upward (2006) and Cornelissen (2008) to estimate the individual and firm fixed effects. This method combines the classical fixed-effects model and the least-squares dummy-variable model such that one effect is eliminated by the fixed-effects transformation and the other is included as dummy variables (McCaffrey, Lockwood, Mihaly, & Sass, 2012). While this approach is equivalent to the model with full dummy variables (Abowd, Kramarz, & Margolis, 1999), it requires less memory than the explicit creation and storage of all the dummy variables, especially in the case of high-dimensional fixed effects (Cornelissen, 2008).

Once fixed effects are estimated, I calculate the contribution of broker/firm fixed effect to variance in misconduct through dividing the covariance of the broker/firm fixed effects and the dependant variable by the variance of the dependant variable:

- Cov(DV, broker_fe) / Var(DV)
- Cov(DV, firm_fe) / Var(DV)

The detailed regression results for estimating the fixed effects are reported in Appendix C. Table 2-10 summarizes the main results of my variance decomposition models in both simple and dense random samples with my three different dependent variables (i.e., three different measures of misconduct). Specifications with year fixed effects and robust standard errors is included for robustness check. The table reports results from nine models applied to each sample – resulting 18 models in total. For each model, Table 2-10 reports the percentage contribution of individual fixed effects versus firm fixed effects to explaining the variance in observed misconduct.

Table 2-10. Two-way fixed effects regression and variance decomposition results.

	% of variance in DV explained by broker vs firm effects and the ratio of the variance explained by broker vs firm							
	Simple				**Dense**			
Model #	**Broker**	**Firm**	**Ratio**	**R-sq**	**Broker**	**Firm**	**Ratio**	**R-sq**
1/2. DV: awards (basic model)	9.4%	3.9%	2.41	0.13	5.4%	2.1%	2.57	0.08
3/4. DV: awards (w/ year dummies)	9.3%	3.9%	2.38	0.13	5.4%	2.1%	2.57	0.08
5/6. DV: awards (w/ robust SE)	9.4%	3.9%	2.41	0.13	5.5%	2.1%	2.62	0.08
7/8. DV: payments (basic model)	11.5%	5.9%	1.95	0.18	8.9%	1.9%	4.68	0.11
9/10. DV: payments (w/ year dummies)	11.5%	5.9%	1.95	0.18	8.7%	1.9%	4.58	0.11
11/12. DV: payments (w/ robust SE)	11.5%	5.9%	1.95	0.18	8.9%	1.9%	4.68	0.11
13/14. DV: all misconduct (basic model)	12.6%	6.7%	1.88	0.19	8.9%	2.2%	4.05	0.11
15/16. DV: all misconduct (w/ year dummies)	12.6%	6.7%	1.88	0.19	8.7%	2.2%	3.95	0.11
17/18. DV: all misconduct (w/ robust SE)	12.9%	6.5%	1.98	0.19	8.9%	2.2%	4.05	0.11

F-test that person and firm effects are equal to zero: reject
F-test that person effects are equal to zero: reject
F-test that firm effects are equal to zero: reject
All F-tests reject the hypotheses that fixed effects are jointly 0

In all my models, I find that both time-invariant individual and organizational differences account for statistically significant proportions of the variance in misconduct, as evidenced by the fact that the F-tests reject the hypotheses that individual and/or firm fixed effects are jointly zero. This result complements the findings of prior experimental and self-reported survey-based studies by simultaneously analyzing individual and organizational differences and offering evidence from the field, suggesting that both time-invariant individual and organizational differences do matter in explaining misconduct.

More importantly, I find that individual fixed effects explain two to five times more of the variance in misconduct than do firm fixed effects. In other words, I show that misconduct arises more from bad apples or rogue individuals who commit misdeeds across multiple firms than from bad barrels or rogue organizations that corrupt the individuals that move into and through them. This finding is valuable in the way it informs the question of bad apples versus bad barrels that comes up frequently in the aftermath of misconduct because it has implications for who to punish and how to prevent misconduct in the first place. That would be through focusing more of the available resources on employee selection and training, and monitoring processes as well as on holding individuals accountable rather than merely prosecuting organizations for misconduct. This result also advances the literature on misconduct and behavioral ethics through use of systematic longitudinal data from actual organizational setting and simultaneous analysis of the individual and organizational effects.

These results are consistent across three misconduct measurements, where the first two measures (i.e., awards and payments) serve as robustness checks for the first/main measure of misconduct (i.e., awards, payments, and regulatory sanctions). The r-squared is higher in the latter than the former as there is more variance to be explained in the dependent variable where any of awards, payments, and regulatory sanctions indicates misconduct.

The results are also consistent across the two simple and dense random samples. Because of relative high degree of observed mobility in the simple random sample, the issue of not having enough connectedness in this sample did not pose a serious challenge, and at the same time the dense random sample proved useful as a robustness check tool.

The r-squared ranges from 13% to 19% in the simple and from 8% to 11% in dense random sample. This higher variance explained in the simple sample than the dense sample could partly be due to the fact that there are lesser number of observations with misconduct in the dense sample, a feature of how it was created to offer higher connectedness at the cost of slightly greater survivorship bias.

2.6.7. Matching on Ethics – Bad Matches

In addition to exploring how individual and organizational factors affect the occurrence of misconduct, I also consider a third factor: “matching” of individuals and organizations in explaining misconduct. In this respect, I argue that matches between individual and organizational propensities for misconduct may have an impact on the rate of misconduct over and above the separate effects of the individual and organizational characteristics. That is, there are not only “bad apples” and “bad barrels” but also “bad matches.”

More specifically, intuition and theory suggests that a match based on ethics would foster ethical behavior where more ethical individuals match with more ethical employment opportunities. This is a positive matching expectation (i.e., matching of the likes) and it would occur because of both pull and push factors seeking for complementarities. What fosters unethical behavior (i.e., misconduct) then should be a mismatch based on ethics – where ethical individuals match with rogue firms and rogue individuals match with ethical firms. This is a negative matching expectation.

To test whether this expectation is supported with data, I examine the correlation between individual and firm fixed effects from the aforementioned two-way fixed effects models. Table 2-11 summarizes these correlation coefficients between broker and firm fixed effects in all the 18 models analyzed in the previous section. In all models and across both simple and dense random sampled, I find that the broker fixed effects and firm fixed effects correlate negatively.

Table 2-11. Correlation between broker and firm fixed effects.

	Correlation	
Model #	**Simple**	**Dense**
1/2. DV: awards	-0.557	-0.551
3/4. DV: awards (w/ year dummies)	-0.552	-0.549
5/6. DV: awards (w/ robust SE)	-0.556	-0.551
7/8. DV: payments	-0.715	-0.392
9/10. DV: payments (w/ year dummies)	-0.715	-0.390
11/12. DV: payments (w/ robust SE)	-0.715	-0.392
13/14. DV: all misconduct	-0.647	-0.421
15/16. DV: all misconduct (w/ year dummies)	-0.646	-0.419
17/18. DV: all misconduct (w/ robust SE)	-0.652	-0.419

All F-tests reject the hypotheses that fixed effects are jointly 0

From this correlational analysis, I find support for negative matching in the market – matching of the unlike. Specifically, I find that, in fostering misconduct, on average, bad apples (i.e., rogue employees) are matched with employment at less misconduct-facilitating firms, and that ethical employees are matched with rogue firms. This seems to offer some correlational support for the case of "mismatch on ethics."

I also employ several robustness checks to mitigate a potential bias in deriving the correlation between stockbroker and firm fixed effects. Andrews, Schank, and Upward (2006) show that the correlation between employee and employer effects in an analysis of large-scale employee-employer data could be biased because of an econometric estimation error. They show that if the employee and employer dummy variables are estimated with error in the first place, a situation which is likely when one is estimating a large number of fixed effects in a model, then it is also plausible that the estimated correlation between employee and worker fixed effects also be biased. Andrews, Schank, and Upward (2006) further show that this bias in estimating correlation between employer and worker fixed effects is larger for situations with lower observed employee mobility between employers. Therefore, they suggest that after estimation, one should impose certain requirements to select employee and employer fixed effects that meet a minimum number of movers per employer or a minimum number of observations per employee. To address such potential bias, then, I focus my analysis on sub-samples with higher observed employee mobility, in the following scenarios:

- Largest connected network and movers per firm>10
- Largest connected network and start year>1985
- Largest connected network and observation per person>10
- Largest connected network and max tenure industry>10

The first sub-sample limits the original sample of stockbrokers and firms to firms that have more than 10 movers in them. The second sub-sample limits the original sample to stockbrokers who started their careers after 1985. The third sub-sample limits the original sample to stockbrokers for whom we have more than 10 observations. The fourth sub-sample limits the original sample to stockbrokers whose tenure in the industry exceeds 10 years. In all these scenarios, we expect higher than average mobility rates which should mitigate the potential biases which might arise in studying the correlation between stockbroker and firm fixed effects when observed mobility is lower. These scenarios all yield a negative correlation between the broker fixed effects and firm fixed effects – i.e., mismatch on ethics persists.

Once these negative correlations are established, I turn to examining their consequences. In other words, I test the matching expectation through regression analysis and decomposition of variance to assess whether and to what extent employee-firm matches explain variance in misconduct. To do so, in keeping with the literature in labor economics (e.g., Woodcock, 2008), I run 6 additional models where I add match fixed effects to the regression models – that is, I include a dummy for every broker-firm match in my analysis, in addition to dummies for brokers and firms separately (i.e., a full dummy specification). All these models include clustered standard errors. These 6 additional models reflect regressions for my three dependent variables across two simple/dense random sample. Models 19-24 in Appendix C summarize the regression results. Table 2-12 shows the percentage of variance in misconduct which is explained by the mismatch between ethical employees and rogue firms and vise versa.

Table 2-12. Misconduct stemming from mismatch on ethics.

	% of variance in DV explained by *mismatch on ethics*, as well as by firm and broker fixed effects							
	Simple				**Dense**			
Model #	**match**	**broker**	**firm**	**r-sq**	**match**	**broker**	**firm**	**r-sq**
19/20. DV: awards (w/ firm/broker cluster and match effects)	*11.7%*	9.4%	3.9%	0.25	*11.8%*	5.5%	2.1%	0.19
21/22. DV: payments (w/ firm/broker cluster and match effects)	*20.4%*	11.5%	5.9%	0.38	*16.6%*	8.9%	1.9%	0.28
23/24. DV: all misconduct (w/ firm/broker cluster and match effects)	*18.2%*	12.9%	6.5%	0.38	*15.2%*	8.9%	2.2%	0.27

All F-tests reject the hypotheses that fixed effects are jointly 0

These results show that time-invariant broker-firm match effects account for statistically significant proportions of the variance in misconduct, as evidenced by the fact that the F-tests reject the hypotheses that fixed effects are jointly zero. Furthermore, I find that these match effects account for practically significant proportion of the variance in misconduct – ranging from 11.7% to 20.4% across six different models. That is, the mismatch on ethics of brokers and firms have demonstrably significant correlation with the variance of misconduct.

In fact, match effects (reported in Table 2-12) explain more of the variation in misconduct than do either of individual fixed effects or firm fixed effects (reported in Table 2-10). This is in part due to amplification between individual and firm time-invariant characteristics with respect to misconduct, where ethics of a rogue firm influences the ethical individuals in the way it fosters misconduct due to the spill-over effects from the firm to the individuals as documented by Pierce and Snyder (2008), and where reduced scrutiny afforded by greater structural assurance (McKendall & Wagner, 1997) in an ethical firm might foster unethical behavior for a rogue individual.

In addition, Figure 2-10 summarizes the percentage of variance in misconduct that is explained by the firm, broker, and match effects for four scenarios: “good” broker in a “good” firm (gG), “good” broker in a “bad” firm (gB), and “bad” broker in a “good” firm (bG), “bad” broker in a “bad” firm (bB) based on the variance decomposition results from the simple random sample where all information is used to measure misconduct (other options reveal similar patterns). This figure shows that the ethical mismatch between the broker and firm fixed effects in bG and gB scenarios explain more of the variance in misconduct than do the ethical match between broker and firm fixed effects in the bB and gG scenarios. Furthermore, a “bad-broker good-firm” match seems to be most consequential in explaining variance in misconduct.

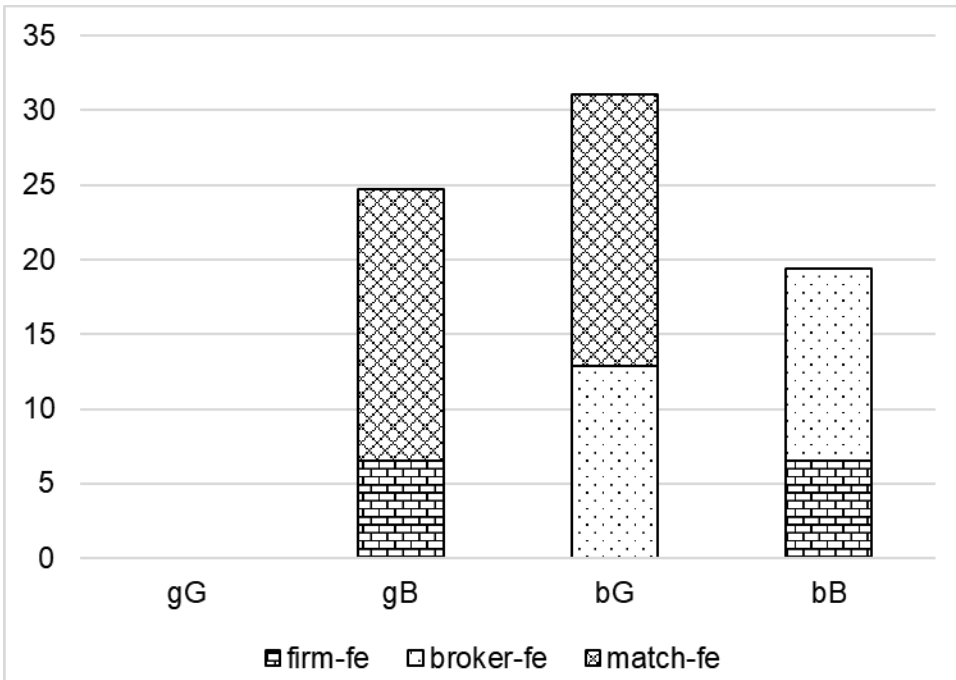

Figure 2-10. % variance explained by firm, broker, and match effects.

There are two caveats in interpreting and generalizing these match results: (1) that these findings reflect correlational rather than causal effects and (2) they pertain to the time-invariant characteristics of the broker, firm, and match effects. Nonetheless, these findings highlight the usefulness of an approach that accounts for match effects as well as individual and firm effects in examining unethical behavior in organizations.

2.7. Discussion, Limitations, and Implications

Using the two-way fixed effects models, across my two samples, I address the debate on the simultaneous and relative influence of both individuals and organizations on organizational misconduct and find that time-invariant individual heterogeneity explains relatively more of the variance in organizational misconduct than time-invariant firm heterogeneity. In other words, I find evidence that, while both individual and organizational characteristics matter, misconduct by individuals in the context of organizations arises more from bad apples or rogue individuals who commit misdeeds across multiple firms than from bad barrels or bad organizations that corrupt the individuals that move into and through them. I also find evidence for a mismatch on ethics where ethical individuals match with rogue firms and unethical individuals match with ethical firms. Furthermore, I show that this mismatch on ethics explains up to 20% of variation in misconduct and, in this way, outweighs the contribution of either individual or firm differences.

There are caveats when interpreting the findings of my study. First, my study is subject to the same challenges that are endemic to all organizational misconduct research, including the facts that not all misconduct is discovered/punished, that some misconduct is settled outside the formal process (thus cannot be observed), that clients might go to arbitration more in loss situations, and that certain client bases tend to litigate more than others. However, I do not have any evidence to believe these challenges are systematic in the way they significantly affect and alter the findings of my study. Second, my setting might condition some of my findings, where certain characteristics of my setting – readily observable misconduct, high mobility, and high individual discretion in production – might make individual factors more important here than in other settings. However, my findings should be relevant for those in this setting – securities regulators and securities firm managers – and those in similar industries with similar characteristics, such as the professional services industry.

Notwithstanding these challenges, the main contribution of my study is offering the first explicit estimate of the relative importance of individual versus organizational differences in accounting for variation in misconduct. This is a question that has not been addressed or answered. In addressing this question, my study contributes to academic research on organizational misconduct because my dataset has been built to allow separation of individual from organizational effects, with less bias and under-reporting of misconduct than in existing research, and has provided a much-needed behavioral field evidence.

Specifically, my evidence from the field contributes to our knowledge from the limited prior experimental investigation of the joint influences of individual and organizational factors. In this respect, my study supports Trevino and Youngblood's (1990) experimental study of a fictional setting where they find that heterogeneity in individual moral development outweighs the variance in organizational conditions in explaining ethical decisions. Similarly, my study, through offering evidence from the field, complements Thoroughgood, Hunter, and Sawyer's (2011) experimental study of a fictional organization involving undergraduate participants where they find *both* individual (leader's gender) and organizational (organization's climate and financial performance) factors important in explaining followers' views of an ethically unaccepted behavior (i.e., destructive leadership) – though they do not explicitly offer much in the way of comparing the relative influence of individual and organizational factors.

My study also builds on and contributes to the limited large-scale archive-based investigation of these joint effects. For instance, I build on Pierce and Snyder's (2008) analysis of a large sample of automobile emissions inspectors and inspection stations showing that organization-specific levels of cheating positively influence the likelihood of cheating by individual inspectors – although they do not examine their relative magnitudes. Additionally, I advance Kish-Gephart, Harrison, and Trevino's (2010) empirical meta-analysis where they find evidence that both individual and organizational characteristics help explain unethical choices of individuals within organizations – although they too fail to offer any insights on their relative magnitudes.

By providing evidence on the relative magnitude of individual and organizational influences on organizational misconduct from actual organizations using longitudinal data, my study also makes a number of broader contributions to the literature in the field of organizational misconduct and ethical decision making. In this respect, I address the “need for research that simultaneously examines different sets of antecedents” (Kish-Gephart, Harrison, & Trevino, 2010, p. 1), which connects “the micro and the macro” (Tenbrunsel & Smith-Crowe, 2008, p. 591) and, in particular, address the call for a “renewed focus on organizational variables” (Craft, 2013, p. 256) in research on organizational misconduct. I also address the need for additional quantitative studies in this domain (for reviews, see Ford & Richardson, 1994; Loe, Ferrell, & Mansfield, 2000; O’Fallon & Butterfield, 2005; Craft, 2013) and address the specific “need for more longitudinal research in ethical decision making” (Craft, 2013, p. 255) rather than cross-sectional research, which does not allow for causal inferences (Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014). In addition, my study addresses the call “to extend the results of laboratory research to field methodologies to insure generalizability of the findings to complex organizational environments” as the “realities of working inside an organization are difficult to capture” with experimental studies (Trevino, den Nieuwenboer, & Kish-Gephart, 2013, p. 654) and addresses the need to utilize objective measures of unethical behavior rather than self-perceptions or self-reports (Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014). Lastly, my study addresses the need for the use of representative samples to the hypothesized population rather than the current dominant use of student samples (O’Fallon & Butterfield, 2005; Craft, 2013) – where the use of student samples has increased from 40% before 2004 to 53% of the studies in the 2004-2011 research (Craft, 2013).

My analysis of this debate also has important practical and policy implications both for whom should be held responsible and punished for misconduct as well as for how misconduct might best be avoided in the first place. Specifically, in light of the findings of this study, to prevent misconduct, *more* of the securities firms’ resources should be allocated towards their selection, training, and monitoring processes at the individual level, rather than broader firm-level processes. For securities regulators, my research should aid the design of systems and rules to prevent, regulate, and punish organizational misconduct by highlighting the higher relative importance of individual (rather than collective) accountability.

In addition, my findings provide systematic empirical evidence to support the otherwise anecdotal evidence and legal arguments that pertain to the responsibility of individuals for incidences of misconduct in the context of organizations (Moohr, 2007; Sepinwall, 2011). Such findings also support the notion that organizations “can only act through individuals and not independently” (Lipman, 2009, p. 389) and offer some support for those who advocate for prosecuting individuals more frequently than organizations in the aftermath of misconduct (Schmidt & Wyatt, 2012). Additionally, my findings provide some support to those arguing against merely prosecuting organizations (rather than rogue individuals) in the aftermath of misconduct (Lipman, 2009; Moohr, 2009) arguing that it imposes unwarranted costs against organizations (Hasnas, 2007; Richter, 2008; Bucy, 2009; Thompson, 2009; Barrett, 2011; Velikonja, 2011) in particular in the context of organizations with highly complex operations (Barrett, 2011) and in the context of the competitive global marketplace (Richter, 2008; Bucy, 2009).

My findings, however, do not lend as much support to the notion that “group dynamics pose unique opportunities for illegality” (Evans, 2011, p. 22) and that misconduct in organizational contexts should not be reduced to the actions of individuals (Sepinwall, 2010). Similarly, my findings reject the notion that merely focuses on addressing group malfunctioning and pathological organizational culture for any meaningful reforms to inhibit misconduct (Fanto, 2008). Lastly, my findings warn regulators that, for any meaningful prevention and punishment of organizational misconduct, they should not just go after organizations (even if they have deeper pockets) and that they should try to overcome the difficulty of linking individual actions to misconduct (Schmidt & Wyatt, 2012).

In conclusion, I suggest a few avenues in the way of future research. Frist, one potential avenue would be to replicate the analysis offered in this study in other organizational settings with different degrees of mobility and misconduct to determine what the boundary conditions are and/or to determine how robust the current study’s broker to firm ratio is in explaining misconduct. A second pathway for future research could be to add additional time-varying observable variables to the model to determines how much such time-varying observable characteristics contribute to explaining variation in misconduct. A third possibility for research is to conduct interviews with a sample of brokers and firms with and without misconduct to better understand the dynamics though which misconduct occurs.

Chapter 3.

Does it Matter if Stockbrokers Get Caught Cheating? Consequences of Misconduct on Careers in the Securities Industry

3.1. Abstract

This analysis investigates the consequences of misconduct on the careers of U.S. stockbrokers. The basic expectation is that, besides official penalties, individual-level misconduct results in reputational damage and impaired future labor market opportunities. However, the consequences of misconduct seem mild on Wall Street, where employers may perceive misconduct as a sign of aggressiveness or a cost of doing business. To address this ambiguity, I investigate the career consequences of one form of Wall Street misconduct: where stockbrokers cheat their customers by generating higher fees through conducting unnecessary, unsuitable, or unauthorized transactions. Specifically, I examine whether visible instances of misconduct are associated with higher/lower likelihood of exiting the profession and being able to leave one's current employer. I also examine whether a stockbroker's tenure moderates the variation in the consequences of misconduct, as misconduct may be a weaker signal to the market the more experienced the stockbroker is. I further examine the role of gender in light of research that documents harsher punishment for misconduct for women. I use the records of the Financial Industry Regulatory Authority (FINRA), which include stockbrokers' employment history and any involvement in formal disputes with customers. I measure misconduct as disputes resulting in settlements or restitution payments to customers, or as regulatory sanctions. My sample includes 4,675 stockbrokers randomly selected from FINRA's population of 1.3 million stockbrokers with employment spells at 1,877 brokerage firms between 1984 and 2013. Using robust linear probability models, I find that customer-initiated misconduct is punished by the

labor market, but regulator-initiated misconduct is not. I also show that higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct. Furthermore, I find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers. These findings advance our understanding of the consequences of misconduct below executive level and offer insights into the variation in who gets (and does not get) punished in the aftermath of misconduct. They also enhance our understanding of how gender affects variation in punishment for misconduct.

3.2. Introduction

Ex ante, the career consequences of misconduct on Wall Street are ambiguous. On the one hand, in a review of organizational misconduct research, Greve, Palmer and Pozner (2010) summarize and articulate a basic expectation that organizations and individuals who are judged to have committed wrongdoing will suffer two types of punishments: an “official” monetary or symbolic penalty, as well as impaired future prospects, either in the form of withdrawal of business partners for organizations or limited labor market opportunities for individuals. This occurs in part due to the reputational damage and negative stigma associated with misconduct. In fact, recent empirical studies indicate that officers and directors of firms implicated in accounting fraud suffer loss of positions with the focal firm and diminished subsequent job opportunities (Pozner 2008; Arthaud-Day & Certo 2006; Srinivasan 2005).

On the other hand, there are reasons to doubt this baseline expectation for financial services professionals. We have seen complaints in recent business press post-2008 financial crisis, where for all the appearance of rotten behavior, there is a concern that individuals who are caught cheating their clients are not being punished. That is, in the case of misconduct on Wall Street specifically, there has been a groundswell of concern that the consequences are mild at best. While the U.S. government has extracted settlements and fines from financial firms, the amounts are seen as a slap on the wrist, dwarfed by the overall size of the banks’ profits. Furthermore, few individuals at the implicated firms have been penalized, either monetarily or via criminal prosecutions (Frontline 2014), raising concerns that there are no consequences for individuals and that punishment is borne only by shareholders (Rushton 2014).

In fact, recent work by Roulet (2014) offers interesting theory and evidence suggesting that the behavior that is criticized by society at large might be rewarded by a specific industry. In particular, he finds that investment banking firms that are more criticized by the press tend to get more business. This finding suggests that we should not expect negative consequences of misconduct for individuals if the firms in the securities industry on Wall Street do not negatively stigmatize those individuals and perhaps view misconduct as a favorable sign of aggressiveness.

These contradictory arguments and evidence, then, portray an open question when it comes to the consequences of misconduct for individuals on Wall Street. In addition, our understanding of whether and how severely individuals are punished in the aftermath of misconduct, however, is limited by a lack of data for individuals lower down in the organization, particularly below the officer and director level. Specifically, Greve, Palmer and Pozner (2010) note that "more work also needs to be done on how organizational misconduct affects organizational members below the top management level" (Greve, Palmer, & Pozner, 2010, p. 91). They point to the substantial variance in who does or does not get punished as an opportunity for valuable research insights.

To advance our understanding of the consequences of misconduct particularly for those below the top management level, I investigate the career consequences of one form of Wall Street misconduct: stockbrokers cheating their customers by generating higher fees through conducting unnecessary, unsuitable, or unauthorized transactions. Being caught cheating customers may damage the reputation of both the stockbroker and her employer, which could lead to adverse future labor market outcomes. But it could alternatively be perceived by current and potential employers in a positive light – a sign of aggressiveness – or at least a neutral light – a cost of doing business or an unlucky experience with a disgruntled client.

My primary question, then, is whether visible instances of misconduct have an impact on stockbroker careers. In particular, are they associated with higher or lower likelihood of exiting the profession and/or of being able to leave one's current employer for another employer? Exiting the industry is considered as an unfavorable outcome and being able to leave one's current employer for another employer is considered a favorable outcome for individuals (Marx & Timmermans, 2014) in the securities industry where generally high mobility is expected and is associated with higher pay.

I also address Greve, Palmer and Pozner's (2010) question about sources of variance in the consequences of misconduct. In this respect, Arnold and Hagen (1992), for instance, show that client complaints against lawyers are more likely to be prosecuted the less experienced the lawyer is. This finding suggests that misconduct may be a weaker signal to the market the more experienced the stockbroker is. My second question, then, is whether a stockbroker's tenure moderates the impact of misconduct on the likelihood of exiting the industry or changing current employer.

Lastly, considering recent research that shows women are targets of more severe punishment than men following misconduct at work (e.g., Kennedy, McDonnell, & Stephens, 2017), my third question examines whether the moderating effect of tenure on the relationship between misconduct and career consequences is different for men versus women. This is a three-way interaction.

To empirically examine my research questions, I draw on records of the Financial Industry Regulatory Authority (FINRA), the professional association and regulatory body for the U.S. securities industry. FINRA maintains records of every registered securities stockbroker. These records include employment history and any involvement in formal disputes with customers. I measure misconduct as disputes with customers that result in settlements, stockbrokers (and/or their employers) making restitution payments to customers, or regulators sanctioning brokers. I refer to the later as regulator-initiated misconduct and the two former as customer-initiated misconduct.

My sample includes 4,675 stockbrokers randomly selected from FINRA's population of 1.3 million stockbrokers. The resulting panel runs yearly from 1984 to 2013 and includes employment spells at 1,877 brokerage firms.

Using robust linear probability models, I find that customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not. I also show that higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct. Furthermore, I find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers.

I next provide a theoretical background for my investigation in section 3.3, describe the setting of my empirical study in more detail in section 3.4, provide details on my data and estimation model in section 3.5, present the results in section 3.6, and discuss my results and their implications in section 3.7.

3.3. Theoretical Framework

To theorize about the career consequences of misconduct on Wall Street, I draw from two sets of literatures that seem to offer contradictory insights – the literature on organizational misconduct and the literature on institutional logics.

On the one hand, the longstanding arguments in the organizational misconduct literature seem to suggest that organizations and individuals who engage in misconduct will be penalized in two ways upon getting caught. First, they suffer an official monetary or symbolic penalty, imposed on them by a "social control agent" such as the government or a regulatory body (Greve, Palmer, & Pozner, 2010). Second, they suffer impaired future prospects, either in the form of withdrawal of business partners for organizations or limited labor market opportunities for individuals (Greve, Palmer, & Pozner, 2010). Recent empirical studies support this expectation in the way they find that officers and directors of firms implicated in accounting fraud suffer loss of positions with the focal firm and diminished subsequent job opportunities (Pozner 2008; Arthaud-Day & Certo 2006; Srinivasan 2005).

While the former punishment in the form of official penalties is of interest to the field of law, the latter punishment in the form of limited labor market opportunities is of significant interest to scholars in organizational studies. In this respect, these scholars have proposed various theoretical mechanisms to explain the negative career consequences of misconduct. In one line of reasoning, Lorsch and MacIver (1989), for example, argue that misconduct signals to the market certain inadequacies, including unfavorable performance and quality, which will then limit future labor market opportunities for the individuals involved. In another line of reasoning, Pozner (2008), for instance, argues that to the extent to which misconduct represents deviation from accepted rules, regulations, and norms in general, it comes with reputational damage and negative stigma. The resulting stigma in turn reduces the social acceptability of those who are involved with misconduct (Carter & Feld, 2004; Kurzban & Leary, 2001) in a way that would limit their subsequent career opportunities, as others seek to dissociate themselves to lessen the threat to their identities and image (Pozner 2008).

This line of reasoning further suggests that the more controllable is the deviation from the acceptable norms, the greater will be the extent to which an individual faces stigmatization (Goffman, 1986). That is to say, if the market perceives an individual to be in control of the act of misconduct, the greater will be the extent to which the market would seek to dissociate.

Taken together, these arguments seem to suggest that stockbrokers who are caught cheating their clients (i.e., misconduct involves the client, henceforth "customer-initiated misconduct") should suffer negative consequences in two specific ways career-wise.

First, they are more likely to exit the industry because the perceived inadequacies in their performance as it pertains to the clients will lessen their market value and because they seek to "avoid difficult interactions with the untainted" (Pozner, 2008, p.145) in the future.

Second, they are less likely able to change employers because other brokerage firms do not wish to associate with them – particularly because stockbrokers have high level of discretion/control in what they do and therefore their act of misconduct involving clients will be of a greater negative signal. Hence:

Hypothesis 1a: stockbrokers' visible instances of customer-initiated misconduct are associated with higher likelihood of exiting the profession.

Hypothesis 1b: stockbrokers' visible instances of customer-initiated misconduct are associated with lower likelihood of being able to leave current employer for a new employer.

These arguments can also inform Greve, Palmer and Pozner's (2010) question about sources of variance in the consequences of misconduct. In particular, these arguments seem to further suggest that the negative consequences of visible misconduct involving clients (i.e., customer-initiated misconduct) are weakened for those stockbrokers with higher tenure for two reasons.

First, misconduct may be a weaker signal of inadequacies to the market the more experienced the stockbroker is as the market has more historical information on the performance and qualities of a more experienced individual to go by. Second, in a similar fashion, misconduct may be a weaker stigmatizing signal to the market for more experienced stockbrokers suggesting that these brokers have been around long enough to know better, so there must have been something else that facilitated misconduct above and beyond the control of the experienced individual. In addition, misconduct early in the career can also signal incompetence (on top of malfeasance) which could then strengthen the likelihood of punishment for client-initiated misconduct. Also, Arnold and Hagen's (1992) finding provides some support for these arguments as they show that client complaints against lawyers are more likely to be prosecuted the less experienced the lawyer is. Hence:

Hypothesis 2a: higher tenure weakens the positive relationship between stockbrokers' visible instances of customer-initiated misconduct and likelihood of exiting the profession.

Hypothesis 2b: higher tenure weakens the negative relationship between stockbrokers' visible instances of customer-initiated misconduct and likelihood of being able to leave current employer.

On the other hand, the literature on institutional logics provides reasons to doubt the baseline expectation around the negative consequences of misconduct for financial services professionals on Wall Street. In this respect, for example, Roulet (2014) suggests that behavior in an industry that is criticized by the society at large might be rewarded by that industry itself. In doing so, he notes that "if loyalty to resistant logics is valued enough by crucial groups of stakeholders, it might be better for an actor to preserve the vilified logics rather than change" (Roulet, 2014, p. 26). He in fact finds that investment banking firms that are more criticized by the press for their societally perceived questionable behavior tend to get more business. At the core of this line of reasoning is the argument that when there is conflict between behavioral norms that an actor can adapt, the actor will benefit most from adapting to the norm that is local to them as opposed to the norm that is distant but is perhaps more universal (i.e., being loyal for better evaluation by peers).

These arguments seem to suggest that we should not expect negative but rather expect positive career consequences of regulator-initiated misconduct for individuals in the securities industry. In this respect, the more universal yet distant norms that a regulator tries to establish though sanctions might not be detrimental to the career of a broker. Indeed, such sanctions should help advance the career of a broker because they could be perceived by current and potential employers in a positive light – a sign of aggressiveness – or at least a neutral light – a cost of doing business. That is to say, regulator-initiated misconduct should have a positive effect on the career of the broker and a negative effect on the likelihood of punishment. Hence:

Hypothesis 3a: stockbrokers' visible instances of regulator-initiated misconduct are associated with lower likelihood of exiting the profession.

Hypothesis 3b: stockbrokers' visible instances of regulator-initiated misconduct are associated with higher likelihood of being able to leave current employer for a new employer.

As for Greve, Palmer and Pozner's (2010) question about sources of variance in the consequences of misconduct, these arguments seem to further suggest that the positive consequences of regulator-initiated misconduct are weakened for those stockbrokers with higher tenure. That is to say, misconduct early in the career will provide a greater signal of aggressiveness and loyalty to the local norms and ultimately will enhance future labor market opportunities, whereas misconduct later in the career will provide a lesser signal of aggressiveness and will raise doubt on the loyalty of the individual involved to the local norms (i.e., it is too late to signal one's aggressiveness and loyalty to the local norms later during the career). Therefore:

Hypothesis 4a: higher tenure weakens the negative relationship between stockbrokers' visible instances of regulator-initiated misconduct and likelihood of exiting the profession.

Hypothesis 4b: higher tenure weakens the positive relationship between stockbrokers' visible instances of regulator-initiated misconduct and likelihood of being able to leave current employer for a new employer.

These theoretical arguments highlight a fundamental difference between customer-initiated and regulator-initiated misconduct in the way they predict that the careers of brokers are only negatively affected if they are involved in cases of misconduct which are initiated by the customers which are key to the success of the firms in this industry. However, brokers careers will not negatively be impacted, and in fact might be positively impacted, if they are involved in cases of misconduct which are brought against them by the regulator. In this case the brokers involved might be positively perceived as aggressive by the firms in this industry.

3.4. Empirical Setting

To empirically make progress on testing these hypotheses, I investigate the career consequences of one form of Wall Street misconduct, namely stockbrokers cheating their customers by generating higher fees through conducting unnecessary, unsuitable, or unauthorized transactions, in the context of the U.S. securities industry

I chose the U.S. securities industry as the setting for my empirical analysis because it satisfies several characteristics that facilitate the examination of my research questions: well-defined misconduct, relatively cheap mechanisms by which to seek visible adjudication of alleged misconduct, archives of individuals' employment history and records of misconduct, and relatively high mobility across employers.

At its core, the securities industry consists of firms that buy and sell financial securities on behalf of clients. This includes not only buying and selling existing securities, but also underwriting new securities issues; hence, the industry includes both stockbrokerages and investment banks. The boundaries of the industry are reasonably well-defined in the U.S. because securities trading is regulated under the provisions of the Securities Exchange Act of 1934. Any company that trades securities for its own account or on behalf of clients is required to register as a "broker/dealer" with the Securities and Exchange Commission (SEC) and with one of the industry's self-regulatory organizations (SROs), either FINRA or a specific stock exchange[5].

Employees who act as agents of broker/dealer firms (i.e., stockbrokers) must also be registered with the SEC and one of the SROs. Hence, they are often referred to as "registered representatives" (RRs). Registration as a stockbroker requires passing an exam to establish knowledge of financial securities, securities order processing, and ethical responsibilities to clients and for acceptable conduct.

As part of its mandate to regulate the licensing and professional behavior of securities stockbrokers, FINRA maintains a database of every person who is or has been registered as a securities broker, including their employment history within the securities industry and any involvement in formal customer disputes that entered the mandatory arbitration process and/or disciplinary actions by regulators. This database is publicly available, to allow investors to check the licensing, training, and dispute history of a potential stockbroker. Presumably, in a similar way, the employers review these records when they are recruiting.

[5] von Nordenflycht, A., & Assadi., P., The Public Corporation on Wall Street: Public Ownership and Organizational Misconduct in Securities Brokerage. Working paper.

For a given stockbroker, the FINRA database includes information on who the stockbroker has been employed by (as a stockbroker) and for how long. It also includes information on whether the stockbroker has been involved in any customer disputes or regulatory actions, and what the outcomes of such disputes or actions have been.

Within the U.S. securities industry, stockbrokers' actions are governed by a set of conduct rules maintained and enforced by the SROs (principally, FINRA). These rules establish a range of ways in which stockbrokers can be responsible for failing to protect clients' interests, either through fraud or negligence (Astarita, 2008). The most common bases for disputes between customers and their stockbrokers include customers' claims of: churning, in which stockbrokers transact securities on behalf of clients solely for the purpose of charging commissions; unauthorized trading, in which stockbrokers buy or sell securities without the client's knowledge or approval; unsuitability, in which stockbrokers recommend securities that are not appropriate for the client's age or stated investment objectives; misrepresentation, in which a stockbroker fails to disclose important facts about or even misrepresents the nature of an investment; and negligence, in which a stockbroker has simply "failed to use reasonable diligence in the handling of the affairs of the customer" (Astarita, 2008).

Remedies for alleged violations of these conduct rules may be pursued in two ways: through private action by customers via a mandatory arbitration process (i.e., customer-initiated) or through public investigation and sanction by the regulator, FINRA (i.e., regulator-initiated).

Since 1989, standard contracts between customers and their stockbrokers require that disputes be resolved through mandatory binding arbitration rather than through lawsuits in the courts (Choi & Eisenberg, 2010; Choi, Fisch, & Pritchard, 2010). In arbitration, both sides represent their case to a panel of three arbitrators. The panel of arbitrators includes two public arbitrators and one industry arbitrator, where public arbitrators have minimal ties to the securities industry (and are predominantly lawyers) and are intended to bring a neutral perspective, while industry arbitrators are securities industry participants (including stockbrokers or lawyers who also work with securities firms) and are intended to bring expertise (Choi & Eisenberg, 2010; Choi, Fisch, & Pritchard, 2010).

While the decisions of arbitrator panels are likely imperfect, they represent the judgment of a panel of experts as to whether a brokerage firm and/or an individual stockbroker treated a customer in contravention of the profession's conduct code and thus seem a credible signal of whether misconduct occurred. Furthermore, this process is easier and less expensive to initiate than court-based private action. This suggests that customers likely pursue more cases than would be the case in many other settings in which the process is court-based. This then partially mitigates the gap, endemic to misconduct research (e.g., Krishnan & Kozhikode, 2014), that exists between actual versus observed misconduct.

According to Section 15A of the Securities Exchange Act of 1934 and FINRA Rule 8310, FINRA can impose a variety of sanctions on stockbrokers and securities firms that are found guilty of an infraction, including limitation (where a respondent's business activities are limited or modified), fine, censure, suspension (where a respondent's business activities are suspended for a specific period of time or until certain act is performed), and bar/expulsion (where a respondent stockbroker or firm is barred from the securities industry).

3.5. Data, Measures, and Models

This section presents more detail on my data, my three different but related measurements of organizational misconduct, and the econometric models I used to estimate my effects of interest.

3.5.1. Data

From FINRA records, I drew a random sample of 4808 individuals from the population of the 1,301,584 people who were registered with FINRA as a securities broker in the U.S. I then collected the sampled stockbrokers' complete work histories including instances of misconduct. With this information, I create a panel dataset of brokers with their employment spells at 1877 brokerage firms from 1984 to 2013 (a 30-year period).

As shown in Table 3-1, gender information is available for 4675 brokers (out of the 4808 sampled brokers) where 29.24% of the brokers are female and 70.76% are male. 2243 brokers (out of the 4808 sampled brokers) only had one employer during their career in this industry (i.e., stayers) while 2432 had more than one employer in their careers (i.e., movers).

The FINRA data identifies the dates of employment as a registered representative at any licensed stockbroker/dealer firm; the time when any customer disputes were filed and resolved; the manner in which those disputes were resolved (e.g., settlement, or monetary judgment against the stockbroker); and the time that any regulatory actions were announced.

Table 3-1. Basic features of the sample.

Brokers with gender information	4,675
female	1,367 (29.24%)
male	3,308 (70.76%)
Stayers	2,243
Movers	2,432
Firms	1,877
Years	1984-2013 (30 years)
Observations	48,384
Observations that reflect a new employment	10,480

This sample is useful because individual stockbrokers and their employers are identified and followed over time and the employment relationship between a stockbroker and his/her employer is continuously monitored. This allows for a more effective identification of the effects of misconduct.

3.5.2. Measures

As I discussed earlier, stockbrokers can cheat their clients by fraud or negligence. There are two ways that misconduct can be investigated and enforced. The first way is through formal complaints by clients (i.e., customer-initiated) which can either result in restitution payments after an arbitration hearing (if claim is not dismissed) or result in a settlement. That is, client disputes might result in some kind of payment if not dismissed. The second way is through regulatory investigation (i.e., regulator-initiated) which can result in limitation of activities, censure, suspension, and bar. I summarize these processes in Figure 3-1.

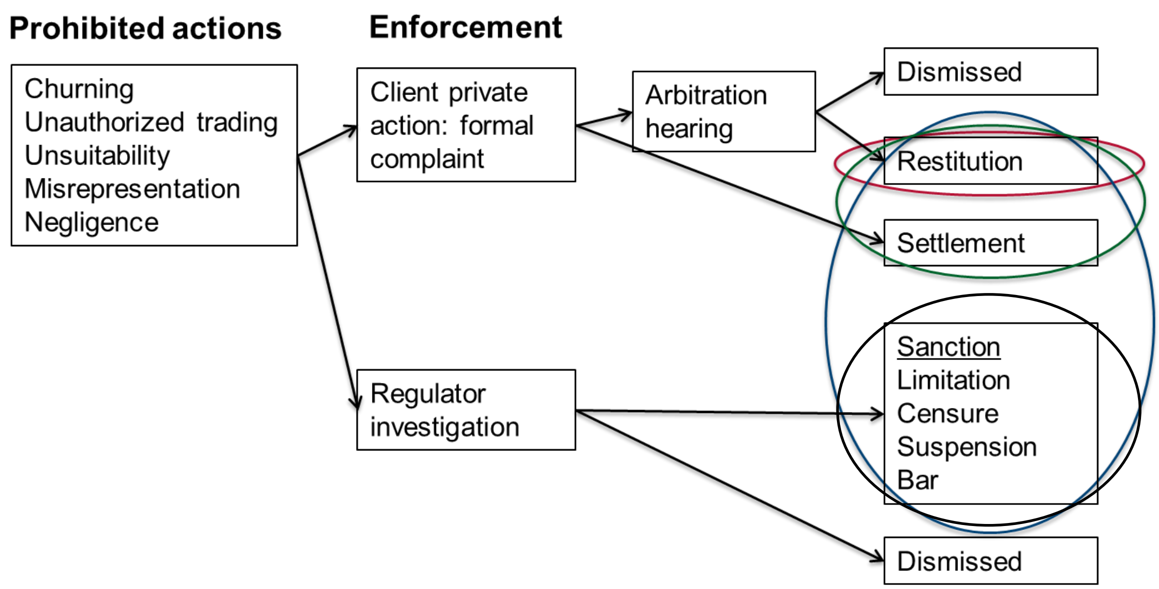

Figure 3-1. Measurement of misconduct.

I measure misconduct – my independent variable – in four ways. The fourth measure considers any of regulatory actions, settlements, or awards against a broker as an indicator of misconduct. To ensure the robustness of my misconduct measure, I also create three additional variables, one that only considers awards (i.e., first measure), one that considers payments of any sort including awards and settlements (i.e., second measure), and one that only considers regulatory actions (i.e., third measure) as indicators of misconduct. I do so to allow flexibility in the case there is something qualitatively different in measuring misconduct by considering all available information versus measuring misconduct by only considering awards, payments, and/or regulatory sanctions. These four measures include:

- Award: whether or not there are disputes with customers which result in stockbrokers (and/or their employers) making restitution payments to customers (i.e., customers receive awards) in three years prior to any given year for each individual. Small/red circle in Figure 3-1 reflects this measure. This measure is coded as pastaward3.

- Payment (award or settlement): whether or not there are disputes with customers which result in settlements or stockbrokers (and/or their employers) making restitution payments to customers in three years prior to any given year for each individual. Medium/green circle in Figure 3-1 reflects this measure. This measure is coded as pastpayment3.

- Regulatory: whether or not there are regulatory actions against a stockbroker in three years prior to any given year for each individual. Black circle in Figure 3-1 reflects this measure. This measure is coded as pastreg3.

- All (award, settlement or regulatory sanction): whether or not there are regulatory actions, settlements, or awards against a stockbroker in three years prior to any given year for each individual. Large/blue circle in Figure 3-1 highlights this measure. This measure is coded as pastall3.

I adopt a three-year perspective in measuring misconduct to address a potential concern about reverse causality where one could argue that perhaps people first form intentions – e.g., "I'm going to leave this job or the profession soon" – then act accordingly – e.g., "since I'm going to leave, I can throw caution and cheat to make money without regard for future opportunities." I also measure misconduct as a dichotomous variable in this study to isolate the qualitative effect of misconduct.

I also measure two specific career outcomes – my dependant variables – as shown in Figure 3-2:

- Exit is set to 1 for an individual in the year beyond which I do not observe that individual in my dataset, and is set to 0 for that individual prior to that year.

- Employer change is set to 1 for an individual in every year when she moves to a new employer, and is set to 0 for that individual in other years.

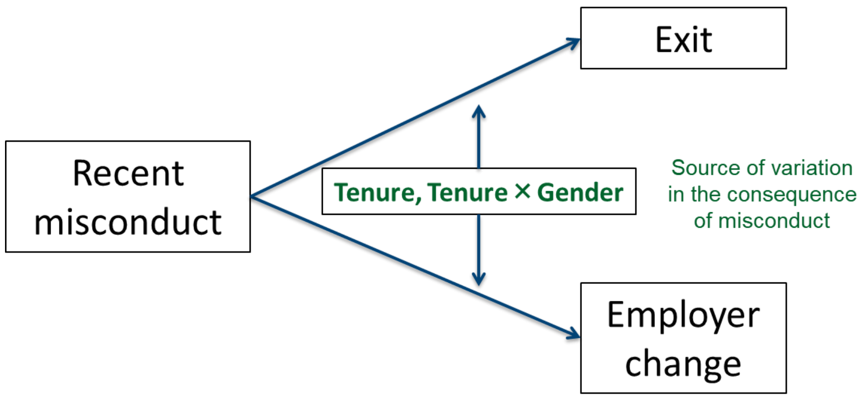

Figure 3-2. Career effect model.

There is a caveat in using these outcomes where it is not clear why individuals exit and whether employer change is categorically favorable (versus not). A fuller understanding of the reasons behind exit and employer change in my future work can further enhance this analysis. Nonetheless, this choice is useful in advancing our understanding of the effects of misconduct, particularly where prior research documents that exiting the industry is generally considered as an unfavorable outcome and being able to leave one's current employer for another employer is considered as a favorable outcome for individuals especially in industries where high mobility is generally expected and is associated with higher pay. Specifically, research in several industries show that wage growth is more likely to be gained through job change rather than by accumulating firm specific capital by staying with a firm (Marx & Timmermans, 2014; Fuller, 2008; Fujiwara-Greve & Greve, 2000; Wegener, 1991; Halaby, 1988; Bartel & Borjas, 1981).

I measure firm tenure based on the number of years an individual was employed with a firm. I code gender of brokers in my sample based on their name and, where necessary and available, based on other information such as profile pictures. To use names for gender determination, consistent with prior research (e.g., Ewens & Townsend, 2017), I run the first names in my sample through the genderize.io API to extract the probability that a first name is female versus male. A gender value of 1 reflects male and a gender value of 0 reflects female in the code.

Lastly, to control for firm size, I measure relative firm size the firms in my sample by log of the number of employees that they employ in my sample.

3.5.3. Estimation model

To test my hypotheses, I use linear probability models with robust standard errors to estimate the drivers of the variance in my dichotomous dependent variables (i.e., exit and employer change). I do so because (a) for large number of observations it is a relatively close approximation of logistic regression which would be the alternative method to this and (b) it is unbiased and does not suffer incidental parameter problem which is common for logistic models with many fixed effects (Bennett, Pierce, Snyder, & Toffel, 2013). I also account for individual, firm, and time unobserved heterogeneity when relevant to my models (Abowd & Kramarz, 1999a; Abowd & Kramarz, 1999b; Abowd, Kramarz, & Woodcock, 2008; Woodcock, 2011).

In this context, I estimate the tenure effect on the relationship between misconduct and career outcomes using Equation 3-1:

$$y_{it} = \theta_i + \psi_{J(i,t)} + T_t + misconduct_{it}\beta + tenure_{it}\beta^{'} + misconduct_{it}tenure_{it}\beta^{''} + \varepsilon_{it}$$

Equation 3-1. Career effect linear probability regressions with tenure.

where the dependent variable is exit/change in year t for individual I (0 or 1 dichotomous variable), the first component in the right hand side is the stockbroker fixed effects, the second component is the firm fixed effects where the function J(i,t) indicates the employer of stockbroker i at time t, the third component is the year fixed effects, the fourth component is the effect of misconduct (a dichotomous variable reflecting award, payment or all misconduct in three years prior to year t for individual i as discussed in the previous section), the fifth component reflects the firm tenure effect, the sixth component is the interaction of misconduct and firm tenure (number of years in the firm), and the last component is the statistical residual, orthogonal to all other effects in the model.

To estimate how the difference in punishment of misconduct across tenure might depend on the gender of the stockbroker involved, I use Equation 3-2:

$$y_{it} = \psi_{J(i,t)} + misconduct_{it}\beta_1 + tenure_{it}\beta_2 + gender_{it}\beta_3 + \\ misconduct_{it}tenure_{it}\beta_4 + tenure_{it}gender_{it}\beta_5 + misconduct_{it}gender_{it}\beta_6 + \\ misconduct_{it}tenure_{it}gender_{it}\beta_7 + \varepsilon_{it}$$

Equation 3-2. Career effect linear probability regressions with tenure and gender.

where the dependent variable is exit/change in year t for individual I (0 or 1 dichotomous variable), the first component in the right-hand side is the firm fixed effects where the function J(i,t) indicates the employer of stockbroker i at time t, the second component is the effect of misconduct, third is the effect of tenure, fourth is the effect of gender. The fifth, sixth, and seventh components show the two-way interactions of misconduct, tenure, and gender. The eights component is the three-way interaction which is to show whether the moderating role of tenure in punishment of misconduct is different for men versus women, and the last component is the statistical residual, orthogonal to all other effects in the model.

3.6. Results

3.6.1. Basic Characteristics of the Sampled Data

My panel consists of 48384 person-year observations of 4675 brokers (29.24% female, 52.02% movers) employed in 1877 firms during the course of 1984 to 2013.

Table 3-2 summarizes the number of firms that workers are employed in. From this table, 47.98% of the brokers only ever had one employer.

Table 3-2. Number of firms that workers are employed in.

Number of firms	Freq.	Percent	Cum.
1	2,243	47.98	47.98
2	998	21.35	69.33
3	612	13.09	82.42
4	372	7.96	90.37
5	221	4.73	95.10
6	111	2.37	97.48
7	50	1.07	98.55
8	27	0.58	99.12
9	16	0.34	99.47
10	14	0.3	99.76
11	6	0.13	99.89
12	3	0.06	99.96
13	1	0.02	99.98
14	1	0.02	100
Total	4,675	100	

Table 3-3 show that the majority of brokers in my sample are movers, 52.02%. In other words, the majority of brokers in my sample have been employed in 2 or more firms. This is a useful feature in estimation of my models involving individual and firm fixed effects.

Table 3-3. Movers vs stayers.

Mover	Freq.	Percent	Cum.
0	2,243	47.98	47.98
1	2,432	52.02	100
Total	4,675	100	

In addition, Table 3-4 shows that approximately half of the brokers were observed 8 or more times in the sample. This is another effective characteristic of the data for estimation purposes.

Table 3-4. Number of observations per broker.

Obs. per person	Freq.	Percent	Cum.
1	309	6.61	6.61
2	479	10.25	16.86
3	360	7.7	24.56
4	281	6.01	30.57
5	292	6.25	36.81
6	284	6.07	42.89
7	209	4.47	47.36
8	185	3.96	51.32
9	180	3.85	55.17
10	160	3.42	58.59
11	161	3.44	62.03
12	173	3.7	65.73
13	157	3.36	69.09
14	152	3.25	72.34
15	160	3.42	75.76
16	108	2.31	78.07
17	117	2.5	80.58
18	81	1.73	82.31
19	88	1.88	84.19
20	90	1.93	86.12
21	94	2.01	88.13
22	64	1.37	89.50
23	47	1.01	90.50
24	55	1.18	91.68
25	52	1.11	92.79
26	69	1.48	94.27
27	36	0.77	95.04
28	54	1.16	96.19
29	178	3.81	100
Total	4,675	100	

When examining the firms, Table 3-5 shows that the vast majority of the firms (91.48%) have movers (because only 8.52% of the firms in the sample did not have any movers). This allows for better estimation of my models.

Table 3-5. Number of mover brokers per firm.

Movers per firm	Freq.	Percent	Cum.
0	160	8.52	8.52
1- 5	943	50.24	58.76
6- 10	283	15.08	73.84
11- 20	221	11.77	85.62
21- 30	81	4.32	89.93
31- 50	63	3.36	93.29
51- 100	58	3.09	96.38
>100	68	3.62	100
Total	1,877	100	

Lastly, Table 3-6 shows the groups of firms that are connected through worker mobility. As you can see, the largest connected network in my data involves the majority of the firms and brokers in my sample. Specifically, 160 firms which employ 188 stayers (Group 0 which regroups firms with no movers) are not connected to any other firms because they do not have any movers. This means no firm effect in Group 0 of firms is identified. Instead, 1678 other firm effects are identified (number of firms - number of firms without movers - number of groups excluding Group 0 = 1877 – 160 – 39 = 1678). This table shows that there are 39 exclusive groups within which there is worker mobility and that the largest connected network in my data includes 1618 firms which employ 4434 brokers, of which 2392 are movers (Group 1).

Table 3-6. Groups of firms connected by worker mobility.

Group	Person-years	Persons	Movers	Firms
0	1,230	188	0	160
1	46,591	4,434	2,392	1,618
2	29	1	1	3
3	13	1	1	2
4	29	1	1	2
5	19	1	1	2
6	29	1	1	3
7	20	2	1	2
8	8	1	1	3
9	11	1	1	3
10	16	1	1	3
11	11	2	1	5
12	2	1	1	2

13	7	1	1	2
14	11	1	1	3
15	8	1	1	2
16	32	2	1	2
17	24	1	1	2
18	20	1	1	4
19	19	1	1	4
20	21	1	1	2
21	14	1	1	2
22	25	6	1	2
23	13	1	1	2
24	23	2	2	3
25	15	1	1	3
26	21	1	1	2
27	11	1	1	3
28	6	1	1	2
29	6	1	1	3
30	32	3	2	6
31	11	1	1	2
32	4	1	1	2
33	9	1	1	3
34	5	1	1	2
35	5	1	1	2
36	9	3	1	2
37	8	2	1	2
38	6	2	1	2
39	11	1	1	3
Total	48,384	4,675	2,432	1,877

Taken together, these characteristics of the data allow for better identification of broker and firm effects in explaining the career effects of misconduct – where both individual and firm unobserved heterogeneity is controlled for.

3.6.2. Basic Descriptive Statistics

Table 3-7 presents basic statistics of the variables in my sample. This table shows that on average, 9.5% of the stockbrokers exit the industry every year while 21.3% of the stockbrokers change employers each year. Also, 1.61% of the stockbrokers are shown to have committed misconduct of the kinds discussed earlier in

3-year periods. The average firm tenure is 5.5 years. 75% of the observations include data from male stockbrokers.

Table 3-7. Basic descriptive statistics.

	N	mean	p50	sd	min	max
exit	48,384	0.095	0	0.29	0	1
new spell	48,384	0.213	0	0.41	0	1
award	48,384	0.002	0	0.04	0	1
payment	48,384	0.013	0	0.11	0	1
regulatory	48,384	0.005	0	0.07	0	1
all	48,384	0.02	0	0.13	0	1
tenure	48,384	5.51	4	5.10	1	48
gender	48,384	0.74	1	0.44	0	1
lnsize	48,384	2.27	2.3	1.51	0	4.88

Table 3-8 offers the pairwise correlation coefficients between all the dependent and independent variables in my regressions. The immediate line following each row of correlation coefficients report the significance level of each correlation coefficient.

Table 3-8. Pairwise correlations.

		1	2	3	4	5	6	7	8	9
1	exit	1.00								
2	new_spell	-0.06	1.00							
		0.00								
3	pastaward3	-0.01	0.00	1.00						
		0.12	0.85							
4	pastpayment3	-0.01	-0.01	0.34	1.00					
		0.04	0.05	0.00						
5	pastreg3	-0.01	0.01	0.16	0.13	1.00				
		0.18	0.12	0.00	0.00					
6	pastall3	-0.01	0.00	0.30	0.88	0.53	1.00			
		0.02	0.50	0.00	0.00	0.00				
7	tenure_firm	0.02	-0.30	-0.01	0.02	-0.01	0.01	1.00		
		0.00	0.00	0.23	0.00	0.19	0.00			
8	gender	-0.03	-0.01	0.01	0.04	0.02	0.05	0.06	1.00	
		0.00	0.00	0.07	0.00	0.00	0.00	0.00		
9	lnsize	0.06	-0.06	-0.01	0.01	-0.05	-0.01	0.10	-0.07	1.00
		0.00	0.00	0.27	0.02	0.00	0.00	0.00	0.00	

3.6.3. Descriptive Analysis

Figure 3-3 shows the percentage of observations with male versus female brokers over the 1984-2013 period. Overall the industry has recently seen more female brokers involvement compared to 1984.

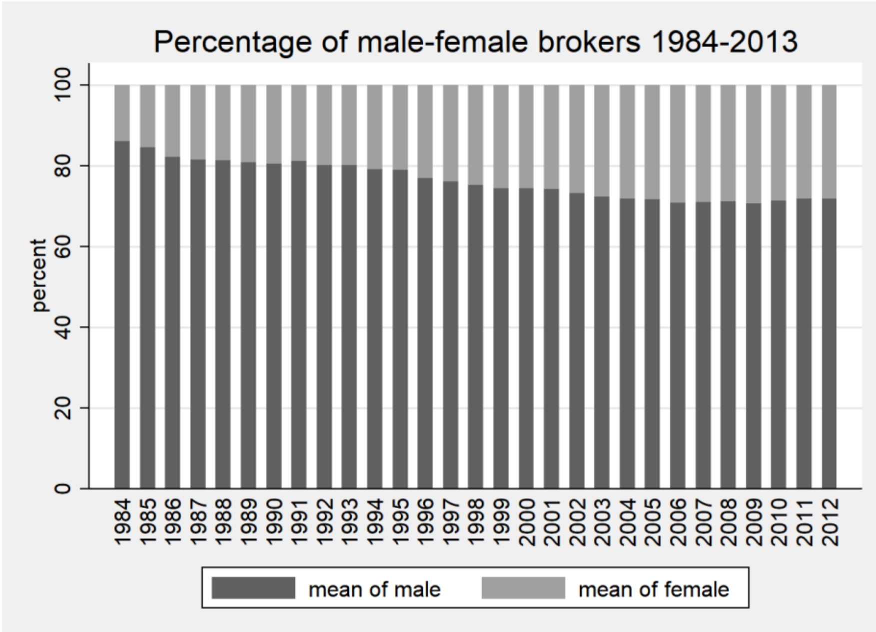

Figure 3-3. Brokers by gender over the sample period.

Figure 3-4 compares the male-female composition of those without misconduct (i.e., misconduct=0) with those with misconduct (i.e., misconduct=1) in their careers. The bars representing male and female within each of these categories (i.e., misconduct=0 or misconduct=1) add to 100%. The male-female percentage gap is larger for those with misconduct as compared to those without misconduct – illustrating a positive correlation between gender and misconduct where male brokers account for more of the misconduct than female brokers.

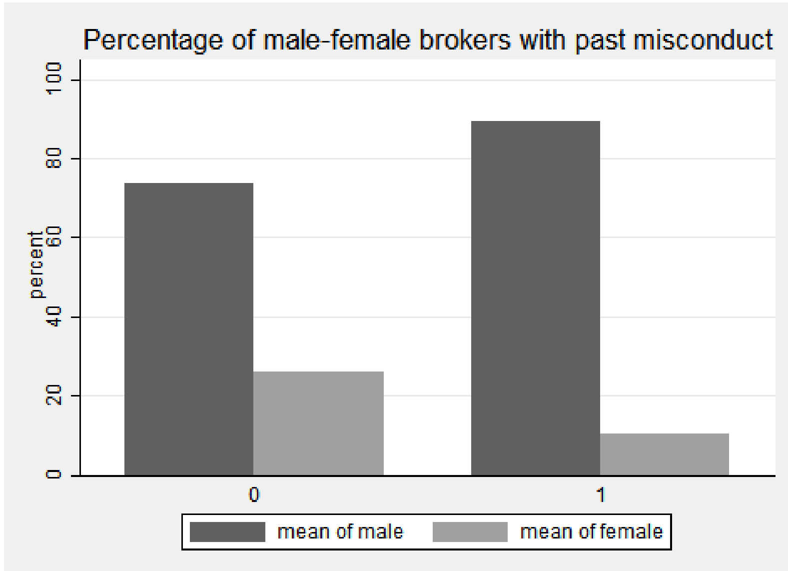

Figure 3-4. Gender and past misconduct interaction.

Table 3-9 provides a basic descriptive interaction of misconduct (i.e., pastall3 which includes recent misconduct in the form of awards, settlements, or regulatory sanctions), tenure, and gender. For example, male brokers are more likely to experience higher tenure (a positive correlation). And brokers with higher tenure tend to have lesser misconduct (a negative correlation).

Table 3-9. Interaction of misconduct, firm tenure, and gender.

	gender			pastall3		
tenure	0	1	Total	0	1	Total
1	2,677	7,329	10,006	9,842	164	10,006
2	2,154	5,725	7,879	7,758	121	7,879
3	1,590	4,215	5,805	5,711	94	5,805
4	1,199	3,174	4,373	4,315	58	4,373
5	946	2,532	3,478	3,422	56	3,478
6	747	2,038	2,785	2,743	42	2,785
7	608	1,645	2,253	2,222	31	2,253
8	486	1,396	1,882	1,852	30	1,882
9	405	1,180	1,585	1,564	21	1,585
10	329	1,003	1,332	1,311	21	1,332
11	269	856	1,125	1,103	22	1,125
12	224	739	963	948	15	963
13	163	620	783	769	14	783
14	138	515	653	644	9	653
15	113	431	544	535	9	544

16	95	368	463	452	11	463
17	79	296	375	366	9	375
18	59	266	325	315	10	325
19	48	223	271	265	6	271
20	41	186	227	224	3	227
21	33	167	200	195	5	200
22	29	142	171	168	3	171
23	26	124	150	148	2	150
24	21	111	132	130	2	132
25	15	95	110	108	2	110
26	10	85	95	92	3	95
27	8	71	79	76	3	79
28	7	63	70	66	4	70
29	6	54	60	56	4	60
30	4	44	48	45	3	48
31	2	32	34	33	1	34
32	1	26	27	26	1	27
33	1	23	24	23	1	24
34	1	20	21	20	1	21
35	1	12	13	13	0	13
36	1	10	11	11	0	11
37	1	8	9	9	0	9
38	1	7	8	8	0	8
39	1	4	5	5	0	5
40	0	2	2	2	0	2
41	0	1	1	1	0	1
42	0	1	1	1	0	1
43	0	1	1	1	0	1
44	0	1	1	1	0	1
45	0	1	1	1	0	1
46	0	1	1	1	0	1
47	0	1	1	1	0	1
48	0	1	1	1	0	1
Total	12,539	35,845	48,384	47,603	781	48,384

Figure 3-5 provides a descriptive look at how exit rate varies by the interaction of misconduct, gender, and tenure. There are four sub-graphs in this figure, each illustrating the percentage (or rate between 0 and 1) of those who exit the industry over the course of the tenure variable for 4 categories of female with no past misconduct, male with no past misconduct, female with past misconduct, and male with past misconduct. For instance, in the first sub-graph, you can see that 100% of female brokers with no past misconduct at the 39-year tenure mark exit the industry (note that n=1 for this sub-category). Also, as another example, the bottom two sub-graphs of Figure 3-5 compare the exit rates of female with past misconduct with male with past misconduct over the course of their tenure and show that misconduct later in the career for men is correlated with higher exit rates than for women.

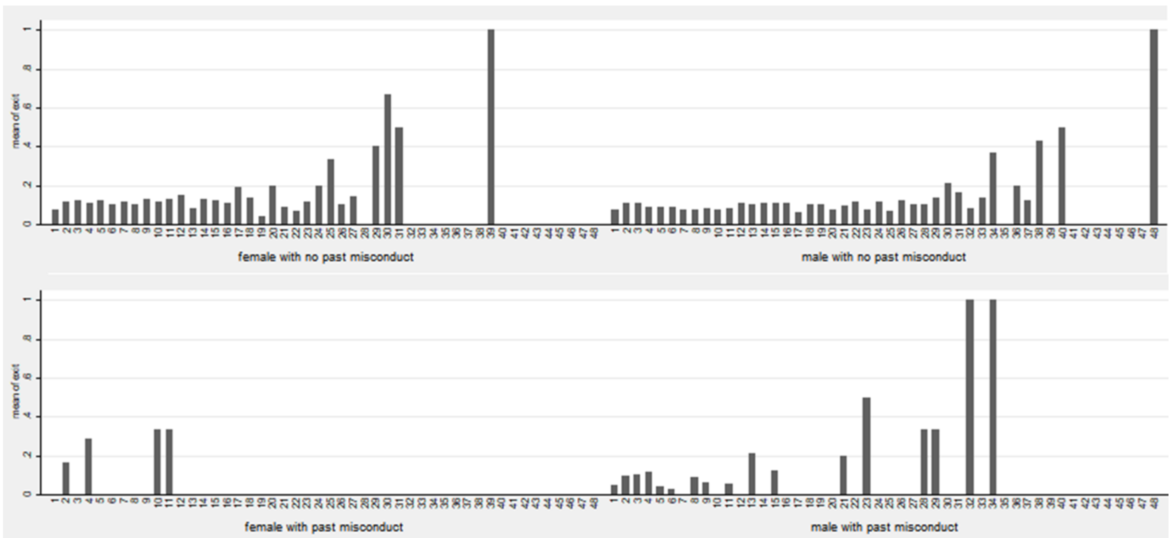

Figure 3-5. Exit rate by misconduct, gender, and tenure.

Figure 3-6 provides a descriptive overview of how employer change (i.e., new employment spell) rate varies by the interaction of misconduct, gender, and tenure. There are four sub-graphs in this figure, each illustrating the percentage (or rate between 0 and 1) of those who change employers over the course of the tenure variable for 4 categories of female with no past misconduct, male with no past misconduct, female with past misconduct, and male with past misconduct.

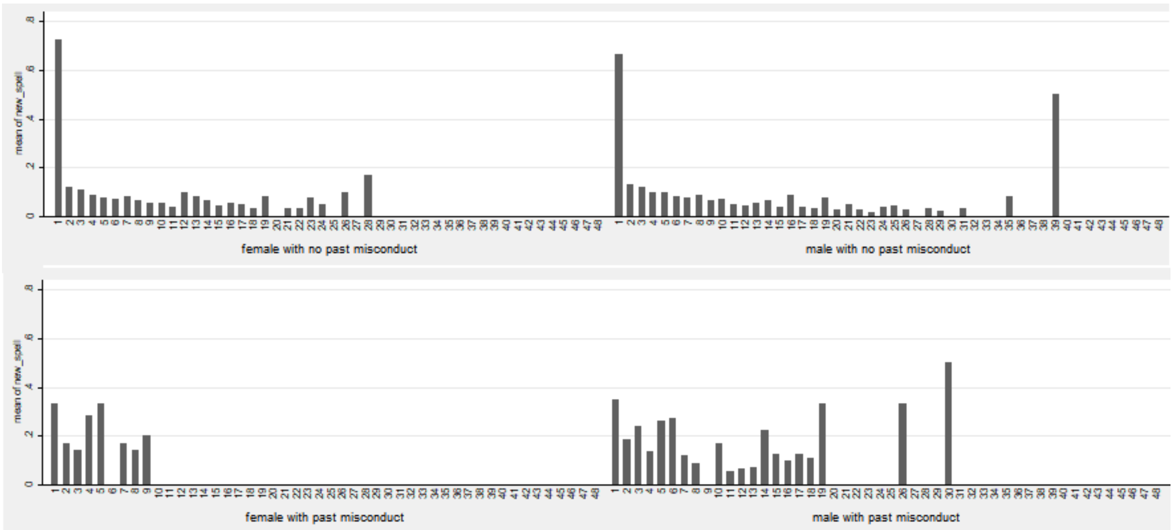

Figure 3-6. New spell/employer change rate by misconduct, gender, and tenure.

These basic descriptive statistics do not control for observed and un-observed heterogeneity but they set the stage for the subsequent regression analysis.

3.6.4. Linear Probability Regression Analysis

This section offers the results of my regression analysis. The first set of results show how punishment of misconduct might vary depending on the tenure levels of brokers and depending on whether the case of misconduct was initiated by the customer or the regulator. The second set of results demonstrate how punishment of misconduct across tenure might vary by gender.

Variation of punishment of customer-initiated misconduct across tenure

Tables 3-10, 3-11, and 3-12 summarize the main results of regression models for when customer-initiated misconduct is measured as award, payment, or payment and regulatory sanctions in the past three years, respectively. Each table reports results from two models applied to the sample. Model 1 reports the results for exit dependent variable. Model 2 reports the results for employer change dependent variable. These regressions include robust standard errors as well as broker fixed effects, firms fixed effects, and year fixed effects to control for unobserved heterogeneity. Also, F-tests reject the hypotheses that broker and/or firm fixed effects are jointly zero.

Table 3-10. Misconduct measured as restitution payment.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	-0.00006		-0.0338	**
	0.00266		0.0065	
tenure_firm	0.00066	+	-0.0115	**
	0.00035		0.0008	
pastaward3	0.04282	+	-0.1104	+
	0.02653		0.0695	
tenure_firm* pastaward3	-0.00378	+	0.0197	+
	0.00258		0.0115	
	.		.	
Robust	Yes		Yes	
Person FE	Yes		Yes	
Firm FE	Yes		Yes	
Time FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	
r-squared	0.67397		0.26691	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

Table 3-11. Misconduct measured as restitution payment or settlement.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	-0.00004		-0.0338	**
	0.00260		0.0065	
tenure_firm	0.00073	*	-0.0118	**
	0.00035		0.0008	
pastpayment3	0.02283	*	-0.0684	*
	0.01157		0.0271	
tenure_firm* pastpayment3	-0.00302	**	0.0121	**
	0.00109		0.0022	
	.		.	
Robust	Yes		Yes	
Person FE	Yes		Yes	
Firm FE	Yes		Yes	
Time FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	
r-squared	0.67401		0.26725	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

Table 3-12. Misconduct as restitution payment, settlement, or regulatory sanction.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	-0.00006		-0.0339	**
	0.00266		0.0065	
tenure_firm	0.00073	*	-0.0118	**
	0.00035		0.0008	
pastall3	0.02880	*	-0.0562	*
	0.01153		0.0254	
tenure_firm* pastall3	-0.00275	**	0.0113	**
	0.00105		0.0021	
	.		.	
Robust	Yes		Yes	
Person FE	Yes		Yes	
Firm FE	Yes		Yes	
Time FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	
r-squared	0.67402		0.26724	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

From Table 3-7, the baseline exit and employer change levels are 9.5% and 21.3% respectively. That is, 9.5% of the stockbrokers exit the industry every year while 21.3% of the stockbrokers change employers each year. As tables 3-10, 3-11, and 3-12 show, I find that stockbrokers with recent customer-initiated misconduct suffer negative labor market consequences. Particularly, stockbrokers who experience awards in the form of restitution payments are 4.3% more likely to exit the industry (45% of the baseline 9.5% rate) and 11.0% less likely to be able to change employers (52% of the baseline 21.3% rate) over the next three years than those without such judgments. Similarly, stockbrokers who experience payments of any kid (i.e., restitution or settlement) are 2.3% more likely to exit the industry (24% of the baseline 9.5% rate) and 6.8% less likely to be able to change employers (32% of the baseline 21.3% rate) over the next three years than those without such judgments. Lastly, stockbrokers who experience payments or regulatory sanctions 2.9% (31% of the baseline 9.5% rate) more likely to exit the industry and 5.6% less likely to be able to change employers (26% of the baseline 21.3% rate) over the next three years than those without such judgments. These results offer support for hypotheses 1a and 1b.

I further find that tenure does appear to moderate the effect of customer-initiated misconduct. In particular, I find that higher tenure in the firm dampens the positive relationship between misconduct and exit by 0.37%, 0.30%, and 0.28% when misconduct is measured as awards, payments, and payments or regulatory sanctions respectively. In addition, I find that higher firm tenure dampens the negative relationship between misconduct and employer change by 2.0%, 1.2%, and 1.1% when misconduct is measured as awards, payments, and payments or regulatory sanctions respectively. Although the magnitude of the effect varies slightly across three measurements of misconduct, these results consistently show that higher tenure weakens the negative effects of customer-initiated misconduct. These results offer support for hypotheses 2a and 2b.

Together, these results seem to suggest that customer-initiated misconduct has negative consequences – that brokers are more likely to have to exit the industry and less likely to be able to find new employment in the aftermath of misconduct. However, these negative consequences seem to be weaker for brokers with higher tenure – that customer-initiated misconduct later in the career is punish less severely than customer-initiated misconduct early in the career.

Variation of punishment of customer-initiated misconduct across tenure by gender

Tables 3-13, 3-14, and 3-15 summarize the estimates of linear probability models on how the difference in punishment of customer-initiated misconduct across tenure might depend on the gender of the stockbroker involved. Each table corresponds with a different way of measuring misconduct as discussed earlier. Each table reports results from two models applied to the sample. Model 1 reports the results for exit dependent variable. Model 2 reports the results for employer change dependent variable. These regressions include robust standard errors and firms fixed effects. Also, F-tests reject the hypotheses that firm fixed effects are jointly zero.

Table 3-13. Misconduct measured as restitution payment.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
Lnsize	0.08081	**	0.0153	**
	0.00880		0.0059	
tenure_firm	0.00488	**	-0.0285	**
	0.00101		0.0017	
gender (male=1)	-0.00370		-0.0407	**
	0.00464		0.0092	
pastaward3	-0.00181		-0.1330	
	0.01191		0.1406	
tenure_firm* pastaward3	-0.01058	**	0.0237	
	0.00230		0.0249	
gender* pastaward3	0.01885		-0.0325	
	0.03849		0.1501	
gender* tenure_firm	-0.00222	**	0.0089	**
	0.00071		0.0014	
gender* tenure_firm*pastaward3	0.00795	*	-0.0002	
	0.00377		0.0280	
	.		.	
Robust	Yes		Yes	
Firm FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

Table 3-14. Misconduct measured as restitution payment or settlement.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	0.08080	**	0.0154	**
	0.00880		0.0059	
tenure_firm	0.00505	**	-0.0288	**
	0.00102		0.0018	
gender (male=1)	-0.00290		-0.0401	**
	0.00464		0.0093	
pastpayment3	0.00757		-0.1383	**
	0.04539		0.0471	
tenure_firm* pastpayment3	-0.00942	**	0.0178	**
	0.00274		0.0035	
gender* paypayment3	-0.02584		0.0194	
	0.05162		0.0505	
gender* tenure_firm	-0.00241	**	0.0088	**
	0.00072		0.0015	
gender* tenure_firm*pastpayment3	0.01063	**	-0.0028	
	0.00316		0.0039	
	.		.	
Robust	Yes		Yes	
Firm FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

Table 3-15. Misconduct as restitution payment, settlement, or regulatory sanction.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	0.08079	**	0.0153	**
	0.00879		0.0059	
tenure_firm	0.00500	**	-0.0288	**
	0.00101		0.0018	
gender (male=1)	-0.00319		-0.0399	**
	0.00466		0.0093	
pastall3	0.00297		-0.0822	+
	0.03675		0.0511	
tenure_firm* pastall3	-0.00662	*	0.0155	**
	0.00294		0.0038	
gender* pastall3	-0.01157		-0.0209	
	0.04336		0.0536	
gender* tenure_firm	-0.00235	**	0.0088	**
	0.00071		0.0015	
gender* tenure_firm*pastall3	0.00701	*	-0.0017	
	0.00332		0.0042	
	.		.	
Robust	Yes		Yes	
Firm FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

These tables show that[6], across different ways of measuring misconduct, the dampening effect of higher tenure on punishment for customer-initiated misconduct is

[6] Harsher punishment for misconduct for female brokers is not statistically significant.

weaker for men than women – that is, men suffer greater career consequences than women for customer-initiated misconduct later in the career. This is particularly true when we examine exit as dependent variable. Men with misconduct later in their career are more likely to exit than women with misconduct later in their career. For new spell dependent variable, we do not observe statistically significant results for the three-way interaction terms – that is, gender does not seem to play a significant role in the way tenure affects employer change opportunities in the aftermath of misconduct. However, the signs of estimated three-way interaction term coefficients are negative – in line with the broader notion.

Are the effects of regulator-initiated misconduct qualitatively different from those of customer-initiated infractions?

To address whether the career effects of regulator-initiated misconduct are different, I turn to Roulet's (2014) finding where he shows that firms that are more criticized by the press and the public tend to get more business in investment banking. In that setting, other client firms' judgement of a focal firm's behavior is more relevant for getting more business than the judgement of the press and the criticism by the society at large. This, in the case of securities brokerage misconduct, raises the question: whether customer-initiated infractions are taken more seriously (i.e., punished more) by the firms in this industry than the regulator-initiated sanctions – because other prospective clients' judgement of a focal broker's behavior is more relevant for getting more business in the future than the judgement of the regulator (of course except in the case of being barred from the industry).

In addressing this question, I limit my measure of misconduct to include regulatory sanctions of brokers by the regulator (i.e., regulator-initiated misconduct). I further examine the role of tenure and gender to examine whether the previously discovered relationships persist.

Table 3-16 summarizes the results of my regression models for exit (model 1) and new spell (model 2) dependent variables when misconduct is measured as regulatory sanctions. Robust standard errors and firm effects are incorporated.

Table 3-16. Effect of regulatory vs customer-initiated infractions.

Model	**1**		**2**	
Dependent variable	Exit		New_Spell	
lnsize	0.08081	**	0.0154	**
	0.00880		0.0059	
tenure_firm	0.00482	**	-0.0285	**
	0.00100		0.0017	
gender (male=1)	-0.00390		-0.0405	**
	0.00465		0.0092	
pastreg3	-0.26691	**	0.0903	
	0.09529		0.1711	
tenure_firm* pasreg3	0.05247	*	-0.0014	
	0.02128		0.0246	
gender* pastreg3	0.28693	**	-0.1440	
	0.10038		0.1786	
gender* tenure_firm	-0.00216	**	0.0089	**
	0.00071		0.0014	
gender* tenure_firm*pastreg3	-0.05578	**	0.0091	
	0.02156		0.0252	
	.		.	
Robust	Yes		Yes	
Firm FE	Yes		Yes	
# observations	48,384		48,384	
# persons	4,675		4,675	
# firms	1,877		1,877	
# mover persons	2,432		2,432	
FE F-test significant?	Yes		Yes	

Notes: Figures in smaller type are estimated robust standard errors.
+ p<0.15; * p<0.05; ** p<0.01

As this table shows, when I measure misconduct by whether or not a stockbroker experienced a regulatory action in the past three years, I find that past regulatory sanctions decrease, rather than increase, the exit rate (support for hypotheses 3a) – a finding which is the reverse of what I have shown in the case of customer-initiated infractions and in line with the broader expectation laid out by Roulet (2014) and my hypothesized effects. This effect is weaker for brokers with higher tenure (support for hypotheses 4a) – that is, regulatory sanctions later in the career are punished more – which is again the reverse of what I have shown in customer-initiated infractions. Lastly, male with higher tenure seem to be punished less for regulatory sanctions than highly tenured women – another reverse finding to the case of customer-initiated misconduct. New spell as dependent variable does not reveal any differences across these dimensions (partial support for hypotheses 3b and 4b).

Taken together, I find that: customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not; higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct; and male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers.

3.7. Discussion and Implications

Using robust linear probably analyses of a random sample of stockbrokers, I address an ambiguity in our understanding of the career consequences of misconduct on Wall Street and find that customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not – results that provide support for the hypothesized effects. I also show that higher tenure weakens the punishment after customer-initiated misconduct but strengthens the punishment after regulator-initiated misconduct. Furthermore, I find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers.

One interpretation of the latter effect is in keeping with the expectations of the role congruity theory which suggests that the positive evaluation of an entity occurs when it behaves according to its the typical social roles (Eagly & Diekman, 2005). In this view, women during their tenure tend to garner trustworthiness and warmth (Gervais & Hillard, 2011) whereas men garner competence during their tenure (Eagly & Karau, 2002) in keeping with their typical social roles. Therefore, when a highly tenured female gets involved in a customer-initiated misconduct, it might be seen as an oversight whereas when a highly tenured male engages in customer-initiated misconduct, this might be seen as a sign of overly aggressive behavior. And thus, a highly tenured woman would be punished to a lesser extent than a highly tenured man in the aftermath of customer-initiated misconduct. In the case of regulator-initiated misconduct – where the labor market stakes are lower – these effects are reversed.

My study contributes to academic research on organizational misconduct in a number of ways. In addressing my research questions, the study validates Greve, Palmer, and Pozner's (2010) articulated baseline expectations and adds additional nuance to them – by providing evidence from below top-management level and by identifying sources of variance in the consequences of misconduct. It also highlights the difference between customer-initiated versus regulator-initiated misconduct and shows that the actions of a public actor might not be consequential with respect to the careers of those involved in an industry that overlook public actor actions. My study also advances our understanding of the role of gender in the dynamics involving punishment for misconduct. More broadly, my study addresses the stated need in the field of organizational misconduct by offering objective analysis of panel data from actual organizations over a long period of time (Smith-Crowe, Tenbrunsel, Chan-Serafin, Brief, Umphress, Joseph, 2014; Craft, 2013; Kish-Gephart, Harrison, & Trevino, 2010; Tenbrunsel & Smith-Crowe, 2008).

Chapter 4.

Running Towards or Running Away? The Patterns of Repeat Organizational Misconduct in the U.S. Securities Industry

4.1. Abstract

In this paper, I investigate the patterns of repeat organizational misconduct in the U.S. securities industry. In doing so, I address a debate on whether misconduct by Wall Street firms increases or decreases with the number of their past instances of misconduct (i.e., whether firms "run towards" more of their tainted past or they "run away" from it). In fact, repeat instances of misconduct by firms on Wall Street are of significant concern to law makers and the public. A recent analysis by the *New York Times* documents 51 repeat violations of antifraud laws by 19 large Wall Street firms between 1996 and 2011 and criticizes the regulators' practice of pursuing civil, monetary settlements where the offending firms neither admit nor deny any misconduct – which might then encourage repeat misconduct. However, it is not clear to what extent this anecdotal evidence reliably reflects what is going on in this industry as a whole – beyond its largest players. In this respect, I systematically analyze the information on instances of misconduct, as measured by firms' arbitration losses to their clients, across 648 brokerage firms between 1990 and 2004 to understand how past misconduct might facilitate or inhibit future misconduct. I also examine the moderating effect of the time that has elapsed since firms' last engagement in misconduct. In doing so, I draw from organization/management theories that inform how executives who act on behalf of a firm respond to instances of misconduct and adjust their future behavior, and test two competing hypotheses. Using panel negative binomial models, I find that misconduct increases with the number of past misconduct (i.e., support for "running towards" hypothesis) and decreases with the time that has elapsed since the last misconduct. I

also find that the positive relationship between past and future misconduct is weakened the longer the time it has elapsed since the last misconduct. Together, these findings contribute to our understanding of the dynamics of repeat organizational misconduct. In addition to their theoretical and empirical contributions, these findings also have important implications for law makers, regulators, and executives who aim to understand and manage the consequences of organizational misconduct over time.

4.2. Introduction and Theoretical Background

Too often, I've seen Wall Street firms violating major antifraud laws because the penalties are too weak and there is no price for being a repeat offender.

– President Barack Obama, December 6, 2011

Repeat instances of organizational misconduct by firms on Wall Street are of significant concern to law makers, regulators, courts, executives, investors, and the public (Wyatt, 2012a; 2012b; 2012c). For example, in a recent $285 million settlement with the U.S. Securities and Exchange Commission (SEC) over a mortgage security marketed in 2007, Citigroup pledged not to violate the same antifraud law in 2011 that they did in 2010, 2005, and 2000 – that is, "promising not to do something that the law already forbids" (Wyatt, 2011a). The Citigroup case is not the only example of recidivistic behavior in this industry, as "nearly all of the biggest Wall Street firms have settled fraud cases by promising never to violate a law that they had already promised not to break, usually multiple times" (Wyatt, 2012c).

The significance of repeat misconduct in part is due to the commonly held assumption that misconduct breeds misconduct, in other words, misconduct increases with a higher number of past misconduct. A recent analysis by the *New York Times* documents 51 repeat violations of antifraud laws by 19 large Wall Street firms between 1996 and 2011 and criticizes the regulators' practice of pursuing civil, monetary settlements where the offending firms neither admit nor deny any misconduct (Wyatt, 2011a). As for the Citigroup example, a federal judge unprecedentedly blocked the 2011 settlement with the SEC because of the lack of admission to and accountability of misconduct (Wyatt, 2011b), but this decision was overruled three years later by an appeals court that argued that “consent decrees are primarily about pragmatism”, unlike “trials [which] are primarily about the truth” (Protess & Goldstein, 2014). Since then, there have been increasing calls for bringing criminal cases before the Justice Department rather than pursuing civil cases to inhibit repeat instances of firms’ violations of the law on Wall Street (da Costa, 2014) – again, highlighting a core expectation that, unless costs are elevated, a higher number of past misconduct correlates positively with future misconduct.

However, it is not clear to what extent such anecdotal arguments and evidence reliably reflect what is going on in this industry as a whole – beyond its largest players. In fact, there are reports in the business press to the contrary, with some showing how past misconduct, as a sign of performance and quality inadequacies, initiates a search for best practices – including practices around corporate social responsibility – which in turn inhibit future misconduct.

Theories of organizational misconduct also lend more ambiguity to this debate. On the one hand, some behavioral theories of misconduct suggest that an organization’s prior engagement in misconduct reduces its engagement in subsequent misconduct. In this line of reasoning, an organization guilty of an infraction seeks to leave behind the unsavory situation created by past misconduct (i.e., “run away”). In this view, misconduct comes with negative consequences and costs – beyond its direct and legal implications: it has negative reputational and status effects (Greve, Palmer, & Pozner, 2010), it negatively disturbs the internal moral balance of an organization (Bazerman & Gino, 2012), and it stigmatizes the organization and its associates (Pozner, 2008). In this respect, then, misconduct provides a learning opportunity that organizations use to avoid being put again in this unsavory situation, i.e., they learn not to re-engage in misconduct.

On the other hand, other behavioral and economic theories of misconduct suggest that prior engagement in misconduct increases future misconduct by an organization. In this line of reasoning, an organization guilty of an infraction is unable or simply refuses to leave behind the unsavory situation created by prior misconduct (i.e., “run towards”). In this view, an organization might maintain an already chosen but wrongful course of action (i.e., routines) due to the escalation of commitment or because it might suffer from declining morale associated with misconduct (Tenbrunsel & Smith-Crowe, 2008). In this view, prior misconduct breeds additional misconduct, in particular when the benefits of past misconduct outweigh its costs (Greve, Palmer, & Pozner, 2010). Ongoing work will enhance these theoretical frameworks.

To make progress on this theoretical and empirical opportunity, I investigate the patterns of repeat organizational misconduct in the U.S. securities industry. I specifically address a debate on whether misconduct by Wall Street firms increases or decreases with the number of their past instances of misconduct (i.e., whether firms "run towards" more of their tainted past or "run away" from it). I also examine the moderating effect of the time that has elapsed since firms' last engagement in misconduct.

Theoretically, I draw from organization/management theories that inform how firms and their executives respond to instances of misconduct and adjust their future behavior. Empirically, I systematically analyze the information on instances of misconduct, as measured by firms' arbitration losses to their clients, across 648 brokerage firms between 1990 and 2004 to understand how past misconduct might facilitate or inhibit future misconduct.

Using panel negative binomial models with various random effects, fixed effects, and population average specifications, I find that misconduct increases with the number of past misconduct (i.e. support for "running towards hypothesis") and decreases with the time that has elapsed since the last misconduct. I also find that the positive relationship between past and future misconduct is weakened (and possibly reversed) the longer the time has elapsed since the last misconduct. This shows that longer disengagement of firms from misconduct lessens their propensity to engage in misconduct in the future – suggesting that firms might "forget" routines that encourage misconduct the longer those routines are unused.

Together, these findings contribute to our understanding of the dynamics of repeat organizational misconduct. In addition to their theoretical and empirical contributions, these findings also have important implications for law makers, regulators, and executives who aim to understand and manage the consequences of organizational misconduct over time.

I next describe the setting of my empirical study in more detail in section 4.3, provide details on my sample and specification models in section 4.4, present the results in section 4.5, and discuss my results and their implications in section 4.6.

4.3. Setting: the U.S. securities industry

The securities industry consists of firms that buy and sell financial securities on behalf of clients. The boundaries of the industry are reasonably well defined in the U.S. because securities trading is regulated under the provisions of the Securities Exchange Act of 1934. Any company that trades securities for its own account or on behalf of clients is required to register as a "broker/dealer" with the Securities and Exchange Commission (SEC) and with one of the industry's self-regulatory organizations (SROs). The primary SRO is the Financial Industry Regulatory Authority (FINRA). Trading of securities includes not only buying and selling existing securities but also the underwriting of new securities issues. Thus, the industry includes both stock brokerages and investment banks. The employees who act as agents of broker/dealer firms are stockbrokers who must also be registered with the SEC and one of the self-regulating organizations (SROs)[7].

The actions of stockbrokers are governed by a set of conduct rules maintained and enforced by FINRA. These rules establish a range of ways in which brokers can be responsible for failing to protect clients' interests, either through fraud or negligence (Astarita 2008), which include churning, unauthorized trading, unsuitability, misrepresentation, and neglecting to use reasonable diligence.

Third-party arbitrations of customer complaints are a primary mechanism by which the aforementioned misconduct is identified and penalized in the U.S. securities

[7] von Nordenflycht, A., & Assadi., P., The Public Corporation on Wall Street: Public Ownership and Organizational Misconduct in Securities Brokerage. Working paper.

industry (Choi et al 2010). The arbitration process is initiated by a customer filing a complaint against the brokerage firm and specifying a monetary claim for restitution. At any point in the process prior to the arbitration panel's decision, the parties can agree to a settlement, ending the arbitration process. Barring a settlement, the parties agree on a panel of three arbitrators and present their arguments in writing and during an in-person hearing. The panel includes two "public" arbitrators and one "industry" arbitrator, for both neutrality and industry expertise (Choi et al 2010, Kondo 2009). The panel decides whether or not the brokerage (and/or its brokers) violated the conduct standards and decides how much money the brokerage will pay as restitution to the customer and penalty to the brokerage.

FINRA administers ninety percent of the industry's arbitrations, and the rest is administered by a stock exchange or the American Arbitration Association (Kondo 2009). Thus, FINRA's arbitration archives constitute the best record of client-focused securities misconduct in the U.S. In addition to this, FINRA's arbitration records offer several other benefits as a basis for measuring brokerage misconduct. For example, while the decisions of arbitrator panels are likely imperfect, they represent the judgment of a panel of neutrals and experts as to whether a brokerage mistreated a customer in contravention of the profession's conduct code and thus seem a credible signal of whether or not cheating occurred in instances in which it was suspected. In addition, the arbitration process does not require initiation by a single regulatory body and is intended to be cheaper and faster than court-based litigation. This makes it easier for customers to initiate and pursue claims, which suggests that more brokerage activity is subject to this adjudication process than would be the case in court-driven adjudication or regulatory enforcement. This in turn partially addresses the concern that: not all misconduct is even suspected, much less pursued by clients, so the arbitration records do not capture all misconduct – an issue in virtually all research on crime and misconduct based on archival records (Krishnan & Kozhikode, 2014; Mishina et al 2010, Clinard & Yeager 1980, McKendall & Wagner 1997, McKendall & Jones-Rikkers 2002).

Overall, the U.S. securities industry along with FINRA arbitrations provides an appropriate setting to test my two primary hypotheses around the relationship between past and future misconduct at the firm level.

4.4. Sample, Measures, and Specification Strategies

This section summarizes my data, measurement of various elements of my hypotheses, and specification strategies which I will use to analyze my data.

4.4.1. Sample

The sample for this analysis includes 648 firms that were active in the U.S. securities industry between 1990 and 2004 – from 1,369 firms listed in the Securities Industry Association Yearbook (SIA Yearbook) during that time. The SIA is one of the main professional associations for the U.S. securities industry. The annual Yearbook lists most of the SIA's members, along with information on their size and ownership status. Approximately 400 firms are listed each year. The Yearbook indicates that its listed members account for about 60% of the U.S. securities industry's total capital base. They also account for 60% of the industry's arbitration cases during the sample period.

The SIA Yearbooks provide information on the number of stockbrokers for each firm, in two categories: retail (services provided to individual investors) and institutional (services provided to companies). Firms that cater to retail rather than to institutional customers are more at risk for arbitration cases mainly because they are likely to have more customers as a whole. To focus data collection and data validation efforts, I omitted firms that had no retail stockbrokers and those for which there was no information on whether their stockbrokers were retail or institutional. This reduced the sample to 706 firms. Missing data further reduced the sample to 648 firms.

4.4.2. Measures

The dependent variable for my analysis is arbitration awards. To measure rates of misconduct at retail securities firms, I utilize a database of arbitrations from 1990 to 2004, compiled by Kondo (2009) from NASD archives available on LexisNexis. The LexisNexis archives include almost all arbitration cases administered by FINRA. This database identifies, for each firm in each year, the total number of arbitration cases filed against the firm along with the number dismissed in favor of the brokerage and the number ultimately upheld in favor of the clients, resulting in monetary awards paid to the clients.

In this respect, I measure misconduct by a firm's annual count of "lost" cases (i.e., awards). These are cases in which the arbitrator panel judges against the firm and awards the client some remuneration. Awards are coded in the year in which the complaint was filed, rather than the year in which the award was decided, so that the measurement matches as closely in time as possible to the characteristics of the firm when the misconduct occurred.

The independent variables of my study are: sumlawards, which is the sum of the count of all awards in the years prior to the current year for any given firm, and sumelapsed, which is the time that has elapsed since the last misconduct by any given firm. I also include the interaction term of these two variables in my analysis.

The analysis includes a number of control variables which are likely to have an impact on cross-firm and cross-year differences in the number of arbitration awards: yr_awards, which is the total annual awards experienced by all the firms in the sample in each year; lnemp, which is firm size as measured by the natural log of the firm's number of stockbrokers; pctret, which is percentage retail stockbrokers for any given firm averaged across all of a firm's years in the sample (time-invariant); pct_rr, which measures the brokerage as percentage of overall firm business (divide the number of the firm's stockbrokers by the number of the firm's employees); foreign, which measures whether a firm is a subsidiary of foreign companies; and lastly pub, which codes for whether a firm is publicly traded or owned by a publicly traded parent.

Table 4-1 presents sample statistics.

Table 4-1. Basic sample statistics.

stats	awards	yr_awards	lnemp	pctret	pct_rr	foreign	pub	sum lawards	sum elapsed	awards* elapsed
N	4,110	4,110	4,009	4,110	4,110	4,110	4,110	4,110	4,110	4,110
mean	0.74	200.76	4.59	0.79	0.52	0.09	0.29	3.56	3.50	0.88
sd	3.89	86.44	2.11	0.28	0.26	0.28	0.45	19.72	3.31	4.94
min	0.00	3.00	0.00	0.02	0.02	0.00	0.00	0.00	0.00	0.00
max	97	362.00	10.37	1.00	1.00	1.00	1.00	442.00	15.00	222.00

Table 4-2 shows pairwise correlations.

Table 4-2. Pairwise correlations.

	yr_awards	lnemp	pctret	pct_rr	foreign	pub	sum lawards	sum elapsed	awards* elapsed
yr_awards	1.00								
lnemp	0.01	1.00							
pctret	0.01	-0.10	1.00						
pct_rr	0.01	-0.40	0.28	1.00					
foreign	0.00	0.14	-0.32	-0.14	1.00				
pub	0.02	0.54	-0.10	-0.17	0.32	1.00			
sumlawards	0.02	0.36	0.08	-0.07	-0.01	0.21	1.00		
sumelapsed	0.07	-0.20	-0.13	-0.03	0.05	-0.12	-0.18	1.00	
awards* elapsed	0.00	0.12	0.05	-0.03	0.04	0.07	0.13	0.01	1.00

4.4.3. Specification Strategy

The dependent variable, awards, is a count variable whose standard deviation exceeds its mean (i.e., a case of over-dispersion), so I use a negative binomial model (Barron, 1992) – consistent with prior research on misconduct (Krishnan & Kozhikode, 2014). I use generalized population average with exchangeable correlation (which assumes two distinct observations from the same firm have the same correlation coefficient) and generalized population average with first-order autoregressive correlation structure (AR1) specifications as my primary models. But I also report the results for random effects and fixed effects estimations.

I do so because a generalized population average specification has advantages over other specifications – where random effects models cannot fully address unobserved heterogeneity and fixed effects models drop many observations for firms that show no variation in the dependent variable over time (this eliminates 60% of the observations and 73% of the firms in my sample). A generalized population average specification is efficient and can address unobserved heterogeneity (Krishnan & Kozhikode, 2014; Hardin & Hilbe, 2003; Katila & Ahuja, 2002) and allows robust standard errors with various within group correlation structures (e.g., exchangeable and autoregressive).

4.5. Results

I summarize the results of my analysis in four tables in keeping with four different specification strategies that I adopt. Table 4.5.1.1 shows a generalized population average panel negative binomial model with AR1 correlation, Table 4.5.1.2 shows a generalized population average panel negative binomial model with exchangeable correlation, Table 4.5.2.1 shows a fixed effects panel negative binomial model with oim (observed information matrix) standard errors, and Table 4.5.2.2 shows a random effects panel negative binomial model with oim standard errors.

The first model in each table includes the control variables and the independent variable sumlawards. The second model in each table includes the control variables and two independent variables sumlawards and sumelapsed. The third model in each table includes the control variables, the two independent variables, and their interaction effect.

For each model, I report the coefficients and their significance levels. I also report the percentage change in incidences of awards ([exp^coef-1]%) predicted by one unit increase in my independent variable. I do so because a coefficient of a negative binomial regression means: “for a one unit change in the predictor variable, the difference in the logs of expected counts of the response variable is expected to change by the respective regression coefficient, given the other predictor variables in the model are held constant” (IDRE, 2014). Note that the interpretation of the continuous by continuous interaction effects in negative binomial models is more complicated.

4.5.1. Main results

Across two specifications of a generalized population average panel negative binomial model with AR1 correlation and a generalized population average panel negative binomial model with exchangeable correlation, I find that misconduct increases with past misconduct. Particularly, one unit increase in the count of past awards predicts a 0.6% increase in incidences of future awards. I also find that misconduct decreases with the time elapsed since last infraction. In particular, I find that one year increase in the amount of time that has elapsed since last award predicts 99.3-99.6% reduction in incidences of future misconduct.

Additionally, as illustrated by negative and significant coefficients for the interaction effect in both specifications in Tables 4-3 and 4-4, I find that the positive correlation between past and future misconduct is weakened the longer it is the time that has elapsed since last misconduct.

Table 4-3. Generalized population average panel negative binomial with autoregressive1 correlation.

Model	1			2			3		
	coef	%change	sig	coef	%change	sig	coef	%change	sig
yr_awards	0.006	0.6%	**	0.003	0.3%	**	0.003	0.3%	**
Lnemp	0.770	116.0%	**	0.334	39.7%	**	0.335	39.7%	**
Pctret	2.363	962.2%	**	0.661	93.6%		0.665	94.5%	
pct_rr	0.602	82.5%	+	0.467	59.5%	+	0.462	58.7%	+
Foreign	-0.677	-49.2%	+	-0.428	-34.8%	**	-0.426	-34.7%	**
Pub	-0.166	-15.3%		0.040	4.1%		0.040	4.1%	
sumlawards	0.007	0.7%	**	0.006	0.6%	**	0.006	0.6%	**
sumelapsed				-5.218	-99.5%	**	-4.974	-99.3%	**
awards*elapsed							-0.149	-13.9%	**
_cons	-9.118		**	-2.829		**	-2.838		**
Standard error	robust			robust			robust		
Number of obs	2,597			2,597			2,597		
Number of firms	366			366			366		

Note: + p<0.10; * p<0.05; ** p<0.01

Table 4-4. Generalized population average panel negative binomial with exchangeable correlation

Model	4			5			6		
	coef	%change	sig	coef	%change	sig	coef	%change	sig
yr_awards	0.005	0.5%	**	0.002	0.2%	**	0.002	0.2%	**
lnemp	0.818	126.6%	**	0.329	39.0%	**	0.329	39.0%	**
pctret	2.791	1530.0%	**	0.623	86.5%	+	0.625	86.8%	+
pct_rr	0.652	91.9%	*	0.482	61.9%	*	0.481	61.7%	*
foreign	-0.742	-52.4%	*	-0.398	-32.8%	**	-0.397	-32.8%	**
pub	-0.169	-15.5%		-0.021	-2.0%		-0.021	-2.0%	
sumlawards	0.001	0.1%		0.006	0.6%	**	0.006	0.6%	**
sumelapsed				-5.626	-99.6%	**	-5.489	-99.6%	**
awards*elapsed							-0.095	-9.1%	**
_cons	-9.47		**	-2.651		**	-2.66		**
Standard error	robust			robust			robust		
Number of obs	4,009			4,009			4,009		
Number of firms	648			648			648		

Note: + p<0.10; * p<0.05; ** p<0.01

The majority of my control variables predicted some portion of the variance in awards in an statistically significant manner.

4.5.2. Robustness checks

As robustness checks, I have also estimated and included the results for a fixed effects panel negative binomial model with oim (observed information matrix) standard errors, and a random effects panel negative binomial model with oim standard errors in tables 4-5 and 4-6. The results are consistent with the results of my main models – save one.

The coefficient for sumlawards is significant and negative (rather than positive). Specifically, one unit increase in the count of past awards predicts a 0.2-0.4% decrease in incidences of future awards based on these models. But as I discussed earlier I believe that these results are not as reliable as the results of my main models.

Table 4-5. Fixed effects panel negative binomial.

Model	7			8			9		
	coef	%change	sig	coef	%change	sig	coef	%change	sig
yr_awards	0.005	0.5%	**	0.003	0.3%	**	0.003	0.3%	**
lnemp	0.526	69.3%	**	0.433	54.1%	**	0.433	54.2%	**
pctret	1.362	290.6%		1.144	213.9%		1.145	214.3%	
pct_rr	0.524	68.8%	+	-0.052	-5.1%		-0.052	-5.0%	
foreign	-0.766	-53.5%	**	-0.611	-45.7%	**	-0.611	-45.7%	**
pub	0.361	43.5%	*	0.106	11.2%		0.108	11.4%	
sumlawards	-0.003	-0.3%	**	-0.004	-0.4%	**	-0.004	-0.4%	**
sumelapsed				-4.970	-99.3%	**	-4.640	-99.0%	**
awards*elapsed							-0.202	-18.3%	
_cons	-5.537		**	-1.814			-1.819		
Standard error	oim			oim			oim		
Number of obs	1,629			1,629			1,629		
Number of firms	175			175			175		
Log likelihood	-1,301.80			-804.01			-803.63		

Note: + p<0.10; * p<0.05; ** p<0.01

Table 4-6. Random effects panel negative binomial.

Model	10			11			12		
	coef	%change	sig	coef	%change	sig	coef	%change	sig
yr_awards	0.005	0.5%	**	0.003	0.3%	**	0.003	0.3%	**
lnemp	0.750	111.6%	**	0.350	41.9%	**	0.350	41.9%	**
pctret	2.474	1086.5%	**	0.929	153.3%	**	0.930	153.5%	**
pct_rr	0.737	108.9%	**	0.343	40.9%		0.343	40.9%	
foreign	-0.935	-60.7%	**	-0.494	-39.0%	**	-0.494	-39.0%	**
pub	0.046	4.7%		-0.024	-2.3%		-0.024	-2.3%	
sumlawards	-0.003	-0.3%	**	-0.002	-0.2%	**	-0.002	-0.2%	**
sumelapsed				-5.505	-99.6%	**	-5.409	-99.6%	**
awards*elapsed							-0.071	-6.8%	
_cons	-7.863		**	-1.292		**	-1.294		**
Standard error	oim			oim			oim		
Number of obs	4,009			4,009			4,009		
Number of firms	648			648			648		
Log likelihood	-2,019.59			-1,245.93			-1,245.85		

Note: + p<0.10; * p<0.05; ** p<0.01

4.6. Discussion and Implications

Using various specifications of negative binomial models, I find that misconduct increases with past misconduct such that a one unit increase in the count of past awards predicts a 0.6% increase in incidences of future awards. This finding lends support to the "running towards" hypothesis. But at the same time, I find that the positive correlation between past and future misconduct is weakened the longer the time has elapsed since the last misconduct. This suggests that firms are not trapped in a vicious cycle of misconduct and the longer the time has elapsed since their last misconduct will reduce the rate of future misconduct.

A caveat in interpreting the findings of my study is that they rely on observation of outcomes of the arbitration process, rather than on direct observation of misconduct. More in-depth research into the arbitration process and firm arbitration strategies could help address this limitation.

Despite this challenge, my study contributes to academic research on organizational misconduct by shedding some light on the dynamics of significant but less examined repeat organizational misconduct. More broadly, my study provides a more systematic/objective analysis of panel data from actual organizations over a long period of time to inform the anecdotal and societal conversation around recidivism when it comes to misconduct on Wall Street.

Chapter 5.

Conclusion

My dissertation includes three studies that empirically investigate the causes and effects of misconduct. In doing so, it draws from and contributes to the fields of organizational misconduct, behavioral ethics, and strategic human capital.

In the first study, I focus on understanding the causes of misconduct. This study addresses a debate that often arises when misconduct is committed by an organization or by its members in the course of their work for the organization: whether it resulted from the actions of a few bad apples or from the characteristics of the organization as a whole. In this essay, I seek to estimate the relative importance of individual versus organizational characteristics in explaining the likelihood of misconduct. To do so, I exploit the licensing database of the U.S. securities industry's self-regulatory authority to build a useful dataset of the careers of 10,000 U.S. stockbrokers, including information on their 3,600 employers as well as instances of organizational misconduct. I apply two-way fixed effects models and variance decomposition techniques to estimate the percentage of variation in misconduct that can be attributed to fixed effects of individuals versus fixed effects of firms. My analyses across two different random samples of stockbrokers suggest that the variation in organizational misconduct is largely explained by individual differences rather than organizational differences – i.e., misconduct by the stockbrokers in the context of brokerage firms is more a product of "bad apples" rather than "bad barrels." Specifically, I find that persistent individual differences account for two to five times more of the variation in misconduct than do persistent organizational differences. I also find evidence for a mismatch on ethics, with bad apples match with employment at more ethical firms and ethical individuals match with rogue firms. I show that this mismatch on ethics explains up to 20% of variation in misconduct, outweighing the contribution of either individual or firm differences.

In the second study, I focus on the effects of misconduct on individual careers. This study investigates the consequences of misconduct on the careers of U.S. stockbrokers where the basic expectation is that, besides official penalties, individual-level misconduct results in reputational damage and impaired future labor market opportunities. However, the consequences of misconduct seem mild on Wall Street, where employers may perceive misconduct as a sign of aggressiveness or a cost of doing business. To address this ambiguity, I investigate the career consequences of one form of Wall Street misconduct where stockbrokers cheat their customers by generating higher fees through conducting unnecessary, unsuitable, or unauthorized transactions. Specifically, I examine whether visible instances of misconduct are associated with higher/lower likelihood of exiting the profession and being able to leave one's current employer for another employer. I also examine whether a stockbroker's tenure moderates the variation in the consequences of misconduct as misconduct may be a weaker signal to the market the more experienced the stockbroker is. I further examine the role of gender in light of research that documents harsher punishment for misconduct for women. I use the records of the Financial Industry Regulatory Authority (FINRA) which include stockbrokers' employment history and any involvement in formal disputes with customers. I measure misconduct as disputes resulting in settlements or restitution payments to customers, or as regulatory sanctions. My sample includes 4,675 stockbrokers randomly selected from FINRA's population of 1.3 million stockbrokers with employment spells at 1,877 brokerage firms between 1984 and 2013. Using robust linear probability models, I find that customer-initiated misconduct is punished by the labor market, but regulator-initiated misconduct is not. I also show that higher tenure weakens the punishment after customer-initiated misconduct but it strengthens the punishment after regulator-initiated misconduct. Furthermore, I find evidence that male brokers later in their careers are punished more for customer-initiated misconduct and punished less for regulator-initiated misconduct than female brokers later in their careers. These findings advance our understanding of the consequences of misconduct and offer insights into the variation in who gets (and does not get) punished in the aftermath of misconduct. They also offer nuance to enhance our understanding of how gender affects variation in punishment for misconduct.

In the third study, I focus on the effects of misconduct on organizations. This study investigates the patterns of repeat organizational misconduct in the U.S. securities industry. In doing so, in this essay, I address a debate on whether misconduct by Wall Street firms increases or decreases with the number of their past instances of misconduct (i.e., whether firms “run towards” more of their tainted past or they “run away” from it). In fact, repeat instances of misconduct by firms on Wall Street are of significant concern to law makers and the public. A recent analysis by the *New York Times* documents 51 repeat violations of antifraud laws by 19 large Wall Street firms between 1996 and 2011 and criticizes the regulators’ practice of pursuing civil, monetary settlements where the offending firms neither admit nor deny any misconduct – which might then encourage repeat misconduct. However, it is not clear to what extent this anecdotal evidence reliably reflects what is going on in this industry as a whole – beyond its largest players. In this respect, I systematically analyze the information on instances of misconduct, as measured by firms' arbitration losses to their clients, across 648 brokerage firms between 1990 and 2004 to understand how past misconduct might facilitate or inhibit future misconduct. I also examine the moderating effect of the time that has elapsed since firms’ last engagement in misconduct. In doing so, I draw from organization and management theories that inform how executives who act on behalf of a firm respond to instances of misconduct and adjust their future behavior, and test two competing hypotheses. Using panel negative binomial models, I find that misconduct increases with the number of past misconduct (i.e., support for “running towards” hypothesis) and decreases with the time that has elapsed since the last misconduct. I also find that the positive relationship between past and future misconduct is weakened the longer the time it has elapsed since the last misconduct. Together, these findings contribute to our understanding of the dynamics of repeat organizational misconduct. In addition to their theoretical and empirical contributions, these findings also have important implications for law makers, regulators, and executives who aim to understand and manage the consequences of organizational misconduct over time.

Taken together, my dissertation has important theoretical and empirical implications for academics, as well as practical implications for regulators, managers, and society. Specifically, I contribute to the academic research on organizational misconduct because my datasets have been built to allow specification of individual and organizational effects, with less bias and under-reporting of misconduct than in existing research. In addition, my studies specifically address a need in the field of organizational misconduct and offer a systematic/objective analysis of panel data from actual organizations over a long period of time, examining both individual and organizational antecedents and consequences of organizational misconduct. My studies add additional nuance to the literature on organizational misconduct by providing evidence from below top management level and by identifying sources of variance in the consequences of misconduct. My studies also contribute to academic research on organizational misconduct by shedding some light on the dynamics of significant but less examined repeat organizational misconduct.

As for practice and policy, for the managers of financial firms, my studies provide evidence regarding the importance of individual accountability and significance of firms' selection processes when it comes to inhibiting individual-level misconduct within organizations in the context of the U.S. securities industry. For those actively involved in this industry, my studies highlight the negative career consequences of misconduct in customer-initiated cases – in a way that might adjust their incentives to engage in misconduct. For regulators, my studies provide suggestions as to how they might be able to manage recidivism when it comes to misconduct in the U.S. securities industry.

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Appendix A.

Sample Stockbroker Visual Report

This figure shows an actual example of a BrokerCheck visual report for a given stockbroker.

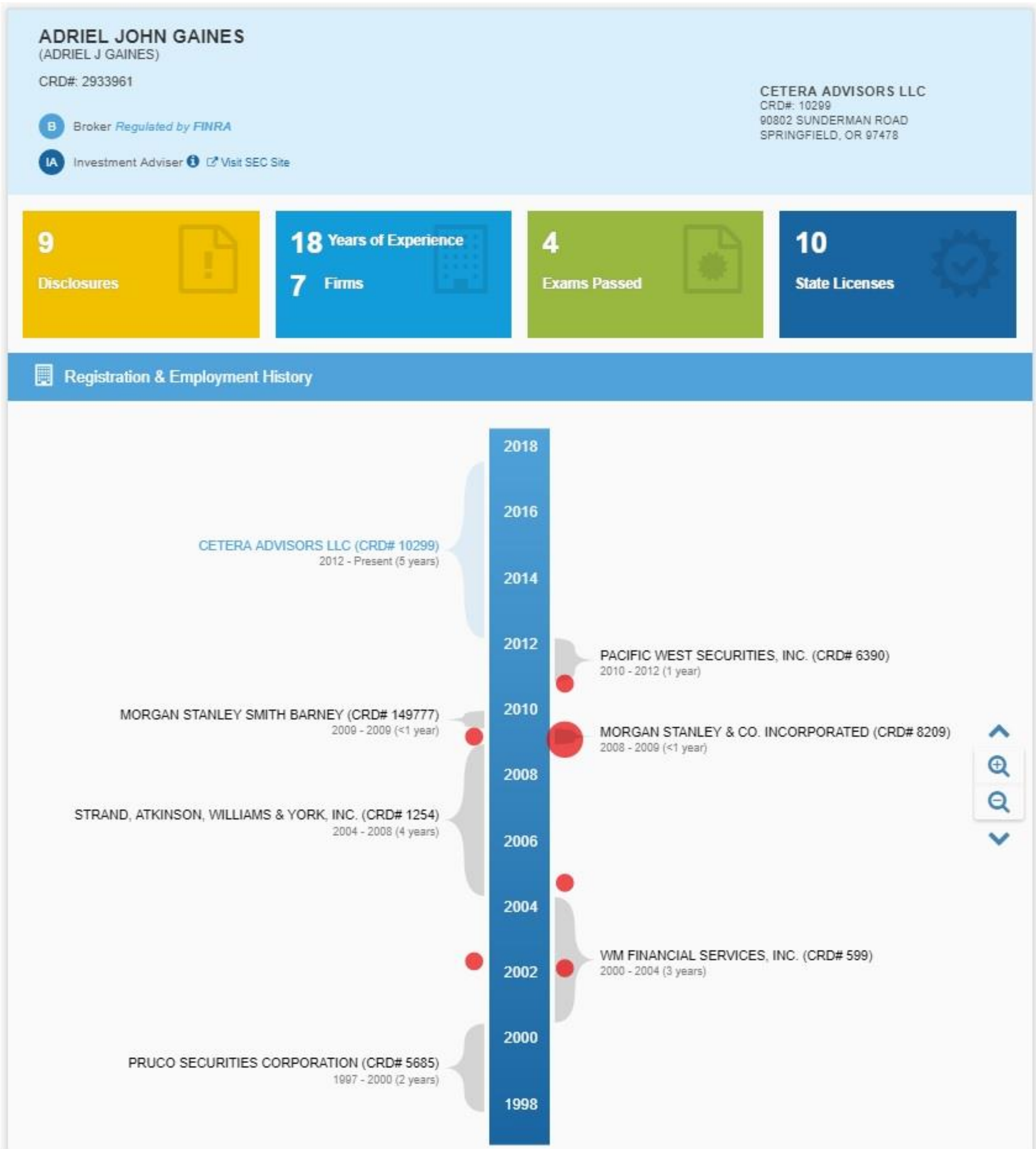
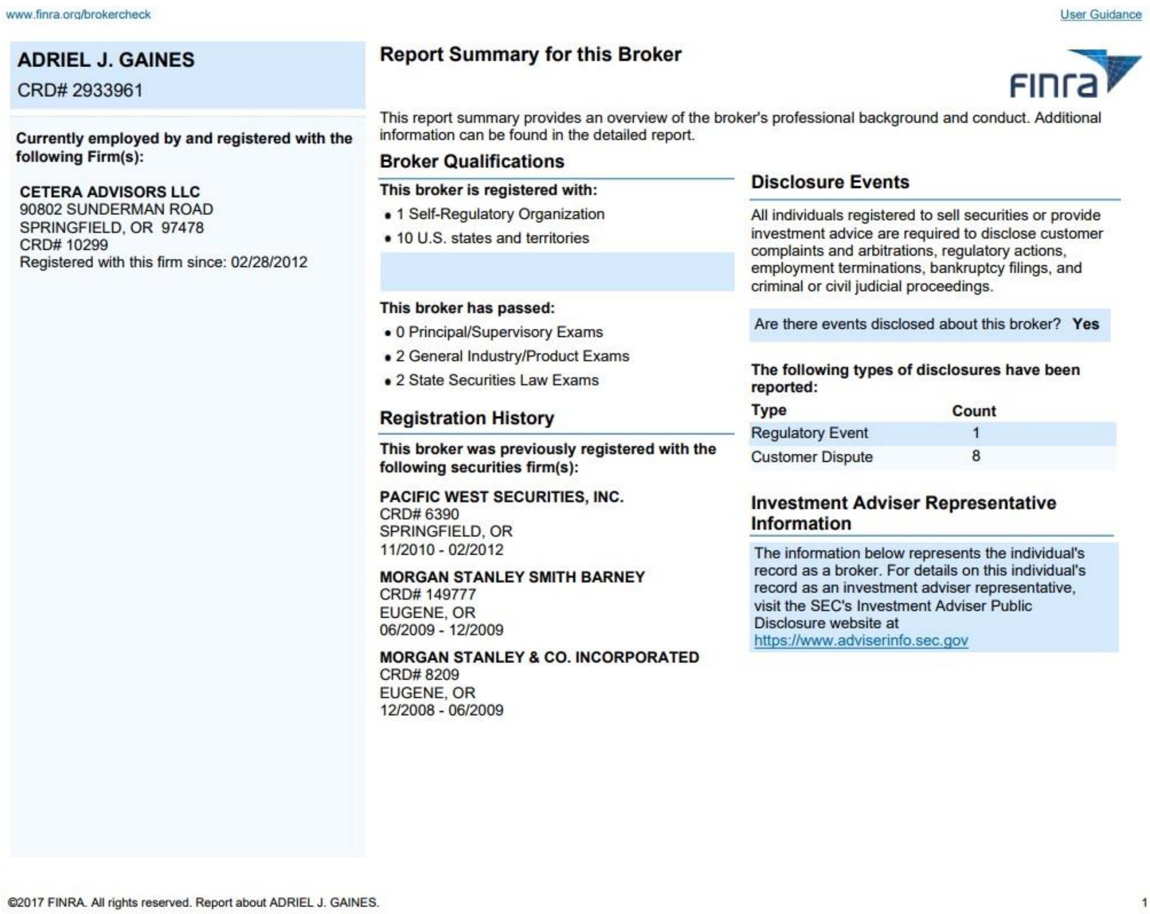


Figure 5-1. Sample stockbroker visual report

Appendix B.

Sample Stockbroker Pdf Report

This figure represents an example of the first page of a detailed BrokerCheck pdf report for a given stockbroker.



www.finra.org/brokercheck

User Guidance

ADRIEL J. GAINES
CRD# 2933961

Currently employed by and registered with the following Firm(s):

CETERA ADVISORS LLC
90802 SUNDERMAN ROAD
SPRINGFIELD, OR 97478
CRD# 10299
Registered with this firm since: 02/28/2012

Report Summary for this Broker

FINRA

This report summary provides an overview of the broker's professional background and conduct. Additional information can be found in the detailed report.

Broker Qualifications

This broker is registered with:

- 1 Self-Regulatory Organization
- 10 U.S. states and territories

This broker has passed:

- 0 Principal/Supervisory Exams
- 2 General Industry/Product Exams
- 2 State Securities Law Exams

Registration History

This broker was previously registered with the following securities firm(s):

PACIFIC WEST SECURITIES, INC.
CRD# 6390
SPRINGFIELD, OR
11/2010 - 02/2012

MORGAN STANLEY SMITH BARNEY
CRD# 149777
EUGENE, OR
06/2009 - 12/2009

MORGAN STANLEY & CO. INCORPORATED
CRD# 8209
EUGENE, OR
12/2008 - 06/2009

Disclosure Events

All individuals registered to sell securities or provide investment advice are required to disclose customer complaints and arbitrations, regulatory actions, employment terminations, bankruptcy filings, and criminal or civil judicial proceedings.

Are there events disclosed about this broker? **Yes**

The following types of disclosures have been reported:

Type	Count
Regulatory Event	1
Customer Dispute	8

Investment Adviser Representative Information

The information below represents the individual's record as a broker. For details on this individual's record as an investment adviser representative, visit the SEC's Investment Adviser Public Disclosure website at https://www.adviserinfo.sec.gov

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Figure 5-2. Sample stockbroker pdf report

Appendix C.

Regression results for models in Chapter 2

In this appendix, I summarize the regression results for models in Chapter 2 of this thesis. Models 1 to 18 show the regression results for three dependent variables, with three different specifications, across two simple/dense random samples. Models 19 to 24 reflect the regression results with match fixed effects for three dependent variables across two simple/dense random samples.

Table 5-1. Regression results – Model 1.

N=51395	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.000002	0.00005	-0.04	0.97
tenure_firm	0.000013	0.00007	0.19	0.848
lnsize	0.000275	0.00042	0.65	0.516
freqemployerchange	0.000065	0.00017	0.38	0.706
allyearly	0.000060	0.00002	2.96	0.003

Table 5-2. Regression results – Model 2.

N=63064	Coef.	Robust Std. Err.	t	P>t
tenure_ind	-0.00003	0.00002	-0.12	0.906
tenure_firm	0.00002	0.00002	1.12	0.263
lnsize	-0.00021	0.00026	-0.81	0.418
freqemployerchange	0.00022	0.00017	1.28	0.2
allyearly	0.00001	0.00000	1.85	0.064

Table 5-3. Regression results – Model 3.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.000134	0.00010	1.32	0.187
tenure_firm	0.000014	0.00007	0.21	0.833
lnsize	0.000334	0.00047	0.71	0.477
freqemployerchange	0.000082	0.00018	0.47	0.64
year fixed effect				

Table 5-4. Regression results – Model 4.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00003	0.00003	0.94	0.346
tenure_firm	0.00002	0.00002	1.19	0.234
lnsize	-0.00017	0.00028	-0.64	0.524
freqemployerchange	0.00023	0.00019	1.27	0.205
year fixed effect				

Table 5-5. Regression results – Model 5.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00000	0.00007	0	0.997
tenure_firm	0.00000	0.00009	0.08	0.935
lnsize	0.00029	0.00049	0.6	0.550
freqemployerchange	0.00005	0.00020	0.28	0.776
allyearly	0.00006	0.00002	2.64	0.008

Table 5-6. Regression results – Model 6.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00000	0.00003	0.31	0.756
tenure_firm	0.00001	0.00002	1.02	0.31
lnsize	-0.00030	0.00031	-0.8	0.422
freqemployerchange	0.00020	0.00017	1.16	0.245
allyearly	0.00001	0.00000	1.74	0.082

Table 5-7. Regression results – Model 7.

N=51395	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00043	0.00027	1.57	0.117
tenure_firm	-0.00024	0.00029	-0.83	0.405
lnsize	0.00026	0.00115	0.23	0.816
freqemployerchange	-0.00084	0.00119	-0.71	0.48
allyearly	0.00031	0.00006	5.02	0

Table 5-8. Regression results – Model 8.

N=63064	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00012	0.00016	0.76	0.449
tenure_firm	0.00002	0.00015	0.16	0.873
lnsize	-0.00014	0.00099	-0.15	0.883
freqemployerchange	0.00049	0.00060	0.82	0.414
allyearly	0.00026	0.00004	5.65	0

Table 5-9. Regression results – Model 9.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00047	0.00054	0.88	0.378
tenure_firm	-0.00024	0.00030	-0.81	0.418
lnsize	0.00037	0.00123	0.3	0.763
freqemployerchange	-0.00081	0.00121	-0.68	0.499
year fixed effect				

Table 5-10. Regression results – Model 10.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00069	0.00027	2.62	0.009
tenure_firm	0.00004	0.00016	0.3	0.762
lnsize	0.00005	0.00103	0.05	0.957
freqemployerchange	0.00064	0.00063	1.03	0.302
year fixed effect				

Table 5-11. Regression results – Model 11.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00035	0.00028	1.26	0.207
tenure_firm	0.00000	0.00031	-0.52	0.601
lnsize	0.00039	0.00149	0.26	0.792
freqemployerchange	-0.00050	0.00124	-0.44	0.657
allyearly	0.00031	0.00009	3.46	0.001

Table 5-12. Regression results – Model 12.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00015	0.00022	0.73	0.468
tenure_firm	0.00000	0.00019	-0.04	0.967
lnsize	-0.00010	0.00121	-0.09	0.929
freqemployerchange	0.00043	0.00067	0.63	0.528
allyearly	0.00026	0.00007	3.61	0

Table 5-13. Regression results – Model 13.

N=51395	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00028	0.00029	0.97	0.333
tenure_firm	-0.00015	0.00031	-0.49	0.622
lnsize	0.00127	0.00137	0.93	0.354
freqemployerchange	-0.00060	0.00129	-0.47	0.639
allyearly	0.00034	0.00006	4.99	0

Table 5-14. Regression results – Model 14.

N=63064	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00009	0.00018	0.52	0.603
tenure_firm	0.00000	0.00016	0.05	0.961
lnsize	0.00038	0.00106	0.36	0.72
freqemployerchange	0.00072	0.00064	1.13	0.258
allyearly	0.00026	0.00004	5.55	0

Table 5-15. Regression results – Model 15.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00024	0.00057	0.43	0.669
tenure_firm	-0.00015	0.00032	-0.49	0.625
lnsize	0.00155	0.00146	1.07	0.286
freqemployerchange	-0.00061	0.00131	-0.47	0.64
year fixed effect				

Table 5-16. Regression results – Model 16.

	Coef.	Robust Std. Err.	t	P>t
tenure_ind	0.00066	0.00031	2.14	0.032
tenure_firm	0.00002	0.00017	0.17	0.863
lnsize	0.00060	0.00111	0.54	0.588
freqemployerchange	0.00088	0.00067	1.33	0.184
year fixed effect				

Table 5-17. Regression results – Model 17.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00021	0.00031	0.69	0.488
tenure_firm	0.00000	0.00034	-0.24	0.807
lnsize	0.00139	0.00186	0.75	0.453
freqemployerchange	-0.00040	0.00139	-0.27	0.791
allyearly	0.00035	0.00010	3.47	0.001

Table 5-18. Regression results – Model 18.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00011	0.00023	0.49	0.626
tenure_firm	0.00000	0.00022	-0.09	0.926
lnsize	0.00046	0.00130	0.36	0.722
freqemployerchange	0.00069	0.00073	0.94	0.346
allyearly	0.00027	0.00007	3.71	0

Table 5-19. Regression results – Model 19.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00002	0.00007	0.34	0.737
tenure_firm	0.00003	0.00007	0.45	0.655
lnsize	-0.00030	0.00047	-0.64	0.52
freqemployerchange	0.00007	0.00020	0.38	0.703
allyearly	0.00007	0.00003	2.59	0.01

Table 5-20. Regression results – Model 20.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00036	0.00041	0.88	0.381
tenure_firm	-0.00040	0.00042	-0.86	0.39
lnsize	0.00016	0.00031	0.52	0.6
freqemployerchange	-0.00020	0.00016	-1.2	0.23
allyearly	0.00000	0.00000	1.46	0.143

Table 5-21. Regression results – Model 21.

	Coef.	Std. Err.	t	P>t
tenure_ind	-0.00020	0.00062	-0.24	0.808
tenure_firm	0.00051	0.00063	0.81	0.418
lnsize	-0.00001	0.00157	-0.01	0.994
freqemployerchange	0.00096	0.00142	0.68	0.499
allyearly	0.00027	0.00010	2.72	0.007

Table 5-22. Regression results – Model 22.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00122	0.00216	0.57	0.572
tenure_firm	-0.00120	0.00215	-0.54	0.587
lnsize	0.00181	0.00136	1.33	0.183
freqemployerchange	-0.00006	0.00242	-0.02	0.982
allyearly	0.00025	0.00007	3.33	0.001

Table 5-23. Regression results – Model 23.

	Coef.	Std. Err.	t	P>t
tenure_ind	-0.00030	0.00063	-0.46	0.648
tenure_firm	0.00063	0.00062	1.01	0.313
lnsize	0.00066	0.00197	0.34	0.737
freqemployerchange	0.00057	0.00135	0.42	0.673
allyearly	0.00031	0.00011	2.77	0.006

Table 5-24. Regression results – Model 24.

	Coef.	Std. Err.	t	P>t
tenure_ind	0.00083	0.00223	0.37	0.712
tenure_firm	-0.00090	0.00222	-0.38	0.702
lnsize	0.00251	0.00151	1.66	0.097
freqemployerchange	0.00127	0.00346	0.37	0.714
allyearly	0.00026	0.00007	3.49	0