Ethics of Sorting Talent on Wall Street

Pooria Assadi
Wharton School of Business - University of Pennsylvania
passadi@wharton.upenn.edu

Andrew von Nordenflycht
Beedie School of Business - Simon Fraser University
vonetc@sfu.ca

Abstract

Competition necessitates sorting of talent into varying employment opportunities. While labor economics traditionally focuses on economic gains in explaining talent sorting in the market and the subsequent quality of the match between the employee and the employer, more recent theoretical work and empirical evidence call for examining the ethics of sorting talent as well. In this respect, intuition and theory suggest that more ethical talent will be sorted into more ethical employment opportunities and that less ethical talent (i.e., "bad apples") will be sorted into less ethical employment opportunities (i.e., "bad barrels"), a positive sorting expectation - matching of the likes. To test this hypothesis, I exploit the licensing database of the U.S. securities industry's self-regulatory authority to build a unique dataset of the careers of 10,000 U.S. stockbrokers, including information on their employers as well as instances of misconduct. Using two-way fixed effects models, I find support for negative, rather than positive, sorting in the market - matching of the unlikes. Specifically, I find that, on average, stockbrokers with lower levels of misconduct are sorted into employment at firms with higher levels of misconduct and that stockbrokers with higher levels of misconduct are sorted into employment at firms with lower levels of misconduct. Controlling for this sorting effect, I also find that the variation in misconduct is more a product of "bad apples" effect rather than "bad barrels" effect. My future analysis will refine and expand these preliminary findings by examining whether these results are robust to varying industries, business cycles, and geographies.

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 to pin the blame on either individuals or organizations that employ them. In fact, this question arises throughout organizational life. 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). Similarly, recent academic fraud has triggered a debate about the rule breaking nature of the individuals versus the roles of poor incentive and control systems (Bhattacharjee, 2013). And the U.S. Army is reported to be seeking "a balance between blaming individuals for misconduct while taking responsibility for putting individuals in situations or a culture that could set the stage for misbehavior" (Editorial Board, 2014).

This debate occurs in legal theory, too. Most legal scholarship holds individuals accountable for instances of organizational misconduct, arguing that organizations can only act 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 organizational misconduct. Ethical decision making research 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). 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 for which the individuals are employed – or are they equally to blame? 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, Bazerman and Gino (2012) cite classic social psychological experiments, such as Zimbardo's (1969) Stanford Prison, Milgram's (1974) electric shock experiments, and Zimbardo's (2007) Lucifer effect, to argue that scholars typically assume that "situational and social forces overwhelm individual differences in explaining ethical behavior" (Bazerman & Gino, 2012, p. 91). On the other hand, in an experimental study of a fictional organizational setting, Trevino and Youngblood (1990) 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, 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).

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 research 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 explaining instances of misconduct. With this approach, I can estimate the total effect of time-invariant characteristics of individuals versus organizations. I acknowledge, though, that this approach may under-measure time- varying characteristics.

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 professional 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.

Also, while intuition and theory suggest that more ethical talent will be sorted into more ethical employment opportunities, I find support for negative, rather than positive, sorting in the market - matching of the unlikes. Specifically, I find that, on average, stockbrokers with lower levels of misconduct are sorted into employment at firms with higher levels of misconduct and that stockbrokers with higher levels of misconduct are sorted into employment at firms with lower levels of misconduct. 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.

I next describe my setting of the U.S. securities industry in more detail, provide details on my sample, measures, and models, present the results, and lastly discuss my results and their implications.

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 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). In this section, I describe in more detail my setting of the U.S. securities industry and discuss the conduct rules that govern it. I also discuss the processes of arbitration for customer disputes and regulatory actions.

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.

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, in order 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.

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.

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.

Regulatory Sanctions

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).

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.

Samples

From FINRA records, I drew two samples for my study. First, I drew a random sample (hereafter referred to as the "simple random sample") of 5,065 individuals from the population of the 1,301,584 people who were registered as a securities broker in the U.S. during 1990-2013. 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 1,750 stockbrokerage firms during this period, and 50.82% of these stockbrokers moved across firms at least once in my sample timeframe.

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 I assume that 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 sample.

My dense sample is a random draw of 5,065 U.S. stockbrokers active in May 2013. Of these, 55.41% were employed at more than one firm over my sample timeframe. These sampled stockbrokers were employed in 1,431 stockbrokerage firms during 1990- 2013. This is fewer than the 1,750 firms involved in the simple random sample, suggesting that the dense random sample is more connected than the simple random sample. But the difference is fairly small, so the risk of unconnectedness in the simple sample may have been avoided.

In both samples, I collected the selected stockbrokers' complete work histories including instances of misconduct. I create a panel dataset from 1990 to 2013. 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 (dismissal, settlement, or monetary judgment against the stockbroker); and the time that any regulatory actions were announced.

My samples are unique 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 sampling procedure allows for high connectedness while the use of a simple sampling procedure allows for lower potential survivorship bias (Abowd, Kramarz, & Woodcock, 2008).

Measures

My measurement of organizational misconduct is three-fold: (1) the number of instances of customer disputes in which arbitrators rule against a stockbroker; (2) the number of instances of lost customer disputes plus the number of settlements (cases where customer and stockbrokers settle); and (3) and the number of instances of lost customer disputes and settlements, plus regulatory actions.

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 this equation:

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

where the dependent variable is organizational misconduct, the function J(i,t) indicates the employer of stockbroker i at time t, the first component is the stockbroker fixed effects, the second component is the firm fixed effects, the third component is the measured characteristics effect (which I do not estimate at this time), and the last component is the statistical residual, orthogonal to all other effects in the model.

After estimating the first equation, I decompose the variance of organizational misconduct to answer the question of bad apples versus bad barrels, using a second equation as follows:

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

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

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 effects (which I do not estimate at this time), the contribution of individual time-invariant effects, the contribution of organizational time-invariant effects, and contribution of residual effects to the variation of organizational misconduct.

Results

Basic Descriptive Statistics

Table 1 presents basic statistics of my variables in both samples. This table shows that my simple random panel consists of 4,805 stockbrokers for whom the data was available (from the 5,056 sampled stockbrokers) and 1,750 firms in which these stockbrokers were employed sometime in their career during 1990-2013. It also shows that my dense random panel consists of 4,854 stockbrokers for which the data was available (from the 5,056 sampled stockbrokers) and 1,431 firms.

As Table 1 shows, 0.9% of the observations in my simple random sample include instances of misconduct (i.e., lost cases, settlements, plus regulatory actions) while this number is 0.6% in my dense sample – which could be a reflection of the possibility that my dense sample has more of a survivorship bias than my simple random sample.

Table 1. Basic statistics in simple random and dense random samples.

	Simple Random Sample				Dense Random Sample			
Variable	**# Observations**	**Mean**	**Min**	**Max**	**# Observations**	**Mean**	**Min**	**Max**
Broker	46,243 (4,805 unique)				57,733 (4,854 unique)			
Firm	46,107 (1,750 unique)				57,687 (1,431unique)			
Year	46,224		1990	2013	57,726		1990	2013
Lost cases	46,243	.00067	0	2	57,733	.00029	0	1
Lost cases + settlements	46,243	.00594	0	7	57,733	.00494	0	6
Lost cases + settlements + disciplines	46,243	.00906	0	7	57,733	.00592	0	6

Besides what this table shows, I find that 6.7% of the stockbrokers in my simple random sample got involved in formal customer disputes that entered the mandatory arbitration process sometime during their career, and of these, 57.4% lost their case or settled (i.e., 3.9% of the sampled stockbrokers lost or settled a case). I also find that 2.9% of the stockbrokers were disciplined by the regulator sometime during their career.

Appendix1 summarizes more descriptive details.

Two-Way Fixed Effects Regression Analysis

I run my estimation models in Stata using the “felsdvreg” command (Cornelissen, 2008). The way this command works is that it combines the classical fixed-effects (FE) model and the least-squares dummy-variable model (LSDV) such that one effect is eliminated by the fixed-effects transformation and the other is included as dummy variables (McCaffrey, Lockwood, Mihaly, & Sass, 2012); hence, Andrews, Schank, and Upward (2006) name this technique as the “FEiLSDVj” method.

Table 2 summarizes the main results of my regression models for the largest connected networks in both simple and dense random samples with my three different dependent variables. The table reports results from four models applied to each sample. The first three models utilize the three different measures of misconduct. The fourth adds year dummies. For each model, Table 2 reports the percentage contribution of individual effects versus firm effects to explaining the variance in observed misconduct.

In all my models save one, I find that both time-invariant individual and organizational differences account for statistically significant proportions of the variance in professional misconduct, as evidenced by the fact that the F-tests reject the hypotheses that individual and/or firm fixed effects are jointly zero.

In addition, I find that individual effects explain two to five times more of the variance in organizational misconduct than firm effects. This suggests that misconduct stems more from individual characteristics than organizational ones. The r-squared for the final model (model #4) in the simple and dense random samples is 21.26% and 12.53% respectively.

Also, while intuition and theory suggest that more ethical talent will be sorted into more ethical employment opportunities, we find support for negative, rather than positive, sorting in the market - matching of the unlikes. Specifically, we find that, on average, stockbrokers with lower levels of misconduct are sorted into employment at firms with higher levels of misconduct and that stockbrokers with higher levels of misconduct are sorted into employment at firms with lower levels of misconduct.

Appendix2 summarizes these results.

Table 2. Two-way fixed effects regression results.

	% of variance in DV explained by *broker* vs *firm* differences	
Model #	**Simple Random Sample**	**Dense Random Sample**
1. DV: lost cases	11.2% versus 4.6%	6.7% versus 1.3% (*)
2. DV: lost cases + settlements	12.8% versus 6.5%	9.9% versus 2.0%
3. DV: lost cases + settlements + disciplines	14.7% versus 6.5%	10.1% versus 2.3%
4. DV: lost cases + settlements + disciplines (w/ year dummies)	14.7% versus 6.5%	10.1% versus 2.3%
r-squared for Model #4	21.26%	12.53%
	(*) F-test cannot reject the hypotheses that fixed effects are jointly zero	

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.

There are caveats when interpreting the findings of my study. First, at this time I do not include any observable factors in my model. Inclusion of observable characteristics might affect the results of my study – although I do not expect this change to significantly alter the results on the time-invariant characteristics that I report in this manuscript. Second, both of my samples potentially suffer from survivorship bias – a common problem in studies of misconduct – although my simple random sample suffers much less from this potential problem. Third, I use linear models for my estimations because they are more stable when one includes many fixed effects, but I can try to adopt non-linear poisson or negative binomial models as a robustness check. Lastly, my study suffers from some of the endemic issues to the organizational misconduct research, including the facts that not all misconduct is discovered/punished, that some misconduct are settled which cannot be observed, that clients might go to arbitration more in loss situations, and that certain client base tend to litigate more than others. My future work will refine and expand my analyses along these lines.

Notwithstanding these challenges, 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. In addition, my study specifically addresses several calls by prominent scholars in the field of organizational misconduct and offers a systematic/objective analysis of panel data from actual organizations over a long period of time examining both individual and organizational antecedents of organizational misconduct. My study also contributes to the practice and policy by providing evidence regarding the importance of individual accountability and significance of firms' selection processes.

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Appendix1

Simple random sample

Table 1.1 summarizes the number of firms workers are employed in. From this table, it is clear that 50.82% of the brokers have been employed in 2 or more firms – that is, 50.82% of the brokers are movers, those who make the estimation of our models possible.

Table 1.1 Number of firms brokers have been employed in in simple random sample.

Number of	Freq.	Percent	
1	2,361	49.18	49.18
2	1,058	22.04	71.21
3	634	13.21	84.42
4	383	7.98	92.40
5	197	4.10	96.50
6	88	1.83	
7		98.33	
8	39	0.81	99.15
Total	4,801	100.00	

Table 1.2 shows that approximately half of the brokers were observed 8 or more times in our sample.

Table 1.2. Number of observations per broker in simple random sample.

Obs. per	Freq.	Percent	
1	324	6.75	6.75
2	501	10.44	17.18
3	376	7.83	
4		25.02	
5	290	6.04	31.06
6	305	6.35	37.41
7	292	6.08	
8		43.49	
9	227	4.73	
10		48.22	
11	195	4.06	
12		52.28	
13	192	4.00	
14		56.28	
15	164	3.42	
		59.70	
Total	4,801	100.00	

Table 1.3 shows that only 10% of the firms did not have any movers, meaning 90% did. Again, this broker mobility allows for better estimation of our models.

Table 1.3. Number of mover brokers per firm in simple random sample.

Movers per	Freq.	Percent	
0	177	10.11	10.11
1- 5	850	48.57	
6- 10		58.69	
11- 20	262	14.97	
21- 30		73.66	
31- 50	201	11.49	85.14
Total	1,750	100.00	

In addition, Table 1.3 shows that 177 firms which employ 207 brokers (Group 0) 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, 1531 other firm effects are identified (number of firms - number of firms without movers - number of groups excluding Group 0 = 1750 – 177 – 42). This table also shows that there are 42 exclusive groups within which there is worker mobility and that the largest connected network in our data includes 1464 firm which employ 4541 brokers, of which 2397 are movers (Group 1).

Table 1.4. Groups of firms connected by broker mobility in simple random sample.

group_ide	N(_	N(_	sum(_	N(_
0	1,330	207	0	177
1	44,200	4,541	2397	1,464
2	23	1	1	2
3	23	1	1	4
4	13	1	1	2
5	11	1	1	4
6	15	1	1	2
7	23	1	1	3
8	20	2	1	2
9	16	1	1	3
10	23	1	1	2
11	22	1	1	4
12	11	2	1	5
13	7	1	1	3
14	2	1	1	2
15	7	1	1	2
16	11	1	1	3
17	23	1	1	2
18	14	1	1	4
19	19	1	1	4
20	5	1	1	2
21	16	1	1	2
22	21	1	1	2
23	20	1	1	2
24	11	1	1	2
25	25	6	1	2
26	13	1	1	2
27	23	2	2	3
28	15	1	1	3
29	21	1	1	2
30	11	1	1	3
31	6	1	1	2
32	6	1	1	3
33	32	3	2	6
34	11	1	1	2
35	4	1	1	2
36	9	1	1	3
37	10	1	1	2
38	5	1	1	2
39	5	1	1	2
40	8	2	1	2
41	6	2	1	2
42	11	1	1	3
Total	46,107	4,801	2440	1,750

Table 1.5. Main results in simple random sample.

Dependent variable: Disputes + settlements + regulatory actions (with year dummies)
N=44200

	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
Iyear 199	007952	0056561	1 41	0 160	- 0031339	019038
Iyear 199	009654	0068032	1 42	0 156	- 0036803	02298
Iyear 199	008281	0047209	1 75	0 079	- 0009715	017534
Iyear 199	006138	0039089	1 57	0 116	- 0015228	01380
Iyear 199	003606	0037919	0 95	0 341	- 0038253	011039
Iyear 199	002608	0035071	0 74	0 457	- 0042659	00948
Iyear 199	004512	0034918	1 29	0 196	- 0023317	011356
Iyear 199	006565	0037952	1 73	0 084	- 0008734	01400
Iyear 199	0091661	0040643	2 26	0 024	0012	017132
Iyear 200	00840	0035882	2 34	0 019	0013721	01543
Iyear 200	0123706	0037176	3 33	0 001	0050841	019657
Iyear 200	013519	0038738	3 49	0 000	0059263	0211117
Iyear 200	0119949	0037658	3 19	0 001	0046139	019375
Iyear 200	009068	0033911	2 67	0 007	0024219	015715
Iyear 200	0113648	0035811	3 17	0 002	0043458	018383
Iyear 200	00780	0031658	2 46	0 014	0015969	01400
Iyear 200	008877	0032697	2 72	0 007	0024687	015286
Iyear 200	012873	0036637	3 51	0 000	0056921	02005
Iyear 200	0110765	0035247	3 14	0 002	004168	01798
Iyear 201	0132374	0037857	3 50	0 000	0058173	020657
Iyear 201	012024	0037908	3 17	0 002	0045945	019454
Iyear 201	008398	0033516	2 51	0 012	0018289	014967
_cons	-	.	.	.	.	.

The regression constant is understood as descriptive statistic for the grand mean, its standard error is not computed.

F-test that person and firm effects are equal to zero: F(6003,38174)=1.71 Prob > F = 0

F-test that person effects are equal to zero: F(4540,38174)=1.41 Prob > F = 0

F-test that firm effects are equal to zero: F(1463,38174)=2.83 Prob > F = 0

If the covariances are positive, the following may indicate the importance in explaining the variance of all:

Cov(all, xb) / Var(all): .00028893

Cov(all, broker_fe) / Var(all): .14736118

Cov(all, firm_fe) / Var(all): .06495355

Cov(all, res) / Var(all): .78739635

Dense random sample

Table 2.1 summarizes the number of firms workers are employed in. From this table, it is clear that 55.41% of the brokers have been employed in 2 or more firms – that is, 55.41% of the brokers are movers, those who make the estimation of our models possible.

Table 2.1. Number of firms brokers have been employed in in dense random sample.

Number of	Freq.	Percent	
1	2,164	44.59	44.59
2	1,108	22.83	67.42
3	726	14.96	82.38
4	432	8.90	91.28
5	215	4.43	95.71
6		112	2.31
7			98.02
8	49	1.01	
9		99.03	
	[illegible]	0.68	99.71
Total	4,853	100.00	

Table 2.2 shows that approximately half of the brokers were observed 12 or more times in our sample.

Table 2.2. Number of observations per broker in dense random sample.

Obs. per	Freq.	Percent	
1	279	5.75	5.75
2	262	5.40	11.15
3	255	5.25	16.40
4	185	3.81	20.21
5	225	4.64	
6		24.85	
7	250	5.15	
8		30.00	
9	207	4.27	34.27
10	202	4.16	
11		38.43	
12	172	3.54	41.97
13	178	3.67	
14		45.64	
15	189	3.89	
		49.54	
Total	4,853	100.00	

Table 2.3 shows that less than 1% of the firms did not have any movers, meaning more than 99% did. Again, this broker mobility allows for better estimation of our models.

Table 2.3. Number of mover brokers per firm in dense random sample.

Movers per	Freq.	Percent	
0	11	0.77	0.77
1- 5	778	54.37	55.14
6- 10	178	12.44	
11- 20		67.58	
21- 30	160	11.18	78.76
31- 50	77	5.38	84.14
Total	1,431	100.00	

In addition, Table 2.4 shows that 11 firms which employ 26 brokers (Group 0) 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, 1417 other firm effects are identified (number of firms - number of firms without movers - number of groups excluding Group 0 = 1431 – 11 – 3). This table also shows that there are 3 exclusive groups within which there is worker mobility and that the largest connected network in our data includes 1415 firm which employ 4825 brokers, of which 2687 are movers (Group 1). As compared to the simple random sample, the number of firms is smaller while the numbers of employed brokers and broker movers are higher in the dense random sample.

Table 2.4. Groups of firms connected by broker mobility in dense random sample.

group_ido	N(_	N(_	sum(_	N(_
0	296	26	0	11
1	57,358	4,825	2687	1,415
2	23	1	1	2
3	10	1	1	3
Total	57,687	4,853	2689	1,431

Table 2.5. Main results in dense random sample.

Dependent variable: Disputes + settlements + regulatory actions (with year dummies)
N=57358

	Coef.	Robust Std. Err.	t	P>|t|	[95% Conf.	Interval]
Iyear 199	.0015659	.0051108	0.31	0.759	-.0084513	.011583
Iyear 199	-.001338	.0045157	-0.30	0.767	-.0101887	.007512
Iyear 199	-.0015768	.0042731	-0.37	0.712	-.009952	.00679
Iyear 199	-	.004376	-0.52	0.607	-.0108308	.00632
Iyear 199	-.0028516	.0041279	-0.69	0.490	-.0109424	.00523
Iyear 199	-	.004088	-0.34	0.731	-.0094183	.00660
Iyear 199	-.0013444	.0041174	-0.33	0.744	-.0094146	.006725
Iyear 199	-	.0038493	-1.11	0.267	-.0118132	.00327
Iyear 199	-	.003940	-0.71	0.480	-.0105072	.0049
Iyear 200	-.0016037	.0039097	-0.41	0.682	-.0092667	.00605
Iyear 200	-	.0038856	-0.33	0.739	-.0089106	.006321
Iyear 200	.002222	.0041591	0.53	0.593	-.0059295	.010374
Iyear 200	.00401	.0041775	0.96	0.337	-.0041779	.012197
Iyear 200	-.0002713	.003845	-0.07	0.944	-.0078075	.00726
Iyear 200	-	.0039371	-0.09	0.926	-.0080825	.00735
Iyear 200	-	.0037968	-0.12	0.902	-.0079092	.00697
Iyear 200	.000847	.0037979	0.22	0.823	-.0065965	.008291
Iyear 200	.0107261	.004551	2.36	0.018	.0018061	.019646
Iyear 200	.009959	.0044356	2.25	0.025	.0012653	.018652
Iyear 201	.002994	.0039052	0.77	0.443	-.0046598	.010648
Iyear 201	.003262	.0038737	0.84	0.400	-.0043298	.010855
Iyear 201	.0019019	.003801	0.50	0.617	-.0055481	.00935
_cons	-	.	.	.	.	.

The regression constant is understood as descriptive statistic for the grand mean, its standard error is not computed.

F-test that person and firm effects are equal to zero: F(6238,51097)=1.16 Prob > F = 0

F-test that person effects are equal to zero: F(4824,51097)=1.05 Prob > F = .006> 4

F-test that firm effects are equal to zero: F(1414,51097)=1.23 Prob > F = 0

If the covariances are positive, the following may indicate the importance in explaining the variance of all:

Cov(all, xb) / Var(all): .0014474

Cov(all, broker_fe) / Var(all): .10099268

Cov(all, firm_fe) / Var(all): .02295212

Cov(all, res) / Var(all): .8746078

Appendix2. Ethics of Sorting

Competition necessitates sorting of talent into varying employment opportunities. While labor economics traditionally focuses on economic gains in explaining talent sorting in the market and the subsequent quality of the match between the employee and the employer, more recent theoretical work and empirical evidence call for examining the ethics of sorting talent as well. Intuition and theory suggest that more ethical talent will be sorted into more ethical employment opportunities, a positive sorting expectation of matching of the likes.

When we test this hypothesis, we find support for negative, rather than positive, sorting in the market - matching of the unlikes. Specifically, we find that, on average, stockbrokers with lower levels of misconduct are sorted into employment at firms with higher levels of misconduct and that stockbrokers with higher levels of misconduct are sorted into employment at firms with lower levels of misconduct. Our future analysis will refine and expand this preliminary and surprisingly finding by examining whether this result is robust to varying industries, business cycles, and geographies.

Here are specifics:

Simple random sample

The broker fixed effects and firm fixed effects also correlate negatively in the simple random sample with a disputes plus settlements dependent variable (-0.7225).

The broker fixed effects and firm fixed effects also correlate negatively in the simple random sample with a disputes, settlements, plus regulatory actions dependent variable (-0.6657).

Dense random sample

The broker fixed effects and firm fixed effects also correlate negatively in the simple random sample with a disputes, settlements, plus regulatory actions dependent variable (-0.6657).

The broker fixed effects and firm fixed effects also correlate negatively when controlled for time effects in the simple random sample with a disputes, settlements, plus regulatory actions dependent variable (-0.6637).