The Self-Regulation of Enforcement:

Evidence From Investor-Broker Disputes at the NASD

Jiro E. Kondo*
Northwestern University
Kellogg School of Management
j-kondo@kellogg.northwestern.edu

Fall 2009

Abstract

This paper investigates whether allocating more control rights to industry groups in customer-firm enforcement proceedings (i.e., the self-regulation of enforcement) leads to greater industry bias and expertise in enforcement. The approach used in this paper focuses on a particular way that bias and expertise can arise in enforcement: through the selection of adjudicators to enforcement panels. Using novel data on customer-firm enforcement run by a specific self-regulatory organization (the National Association of Securities Dealers (NASD)), I document that pro-industry arbitrators are selected to arbitration panels more often than pro-investor ones (selection on bias) and that experts are selected more frequently to cases (selection on expertise). To assess whether the NASD is *responsible* for these patterns, I examine the impact of a change in regulation that moved much of the control rights over arbitrator selection from the NASD to investors and brokers jointly. Following this change, the allocation of expertise to cases declined dramatically while selection on bias increased. These findings suggest that the NASD is not responsible for selection on bias but that it increases selection on expertise. Thus, I do not find support of industry favoritism at the NASD and find evidence that it adds expertise to enforcement. Moreover, these results suggest that alternative control structures commonly used in enforcement have important drawbacks (e.g., jury selection mechanisms). The extent to which these findings are robust and generalizable is discussed in this paper as well.

*I am grateful to my advisors, Antoinette Schoar (Chair), Dirk Jenter and Steve Ross, for advice, encouragement and suggestions. I also thank Manuel Adelino, Nittai Bergman, Tara Bhandari, John Cox, Joseph Doyle, Isil Erel, Carola Frydman, Ilan Guedj, Adam Kolasinski, Gustavo Manso, Stew Myers, Vasia Panousi, Dimitris Papanikolaou, Michel Robe, Eric Van den Steen, Jialan Wang and presentation participants at the Chicago GSB, Columbia GSB, Duke, Lehman Brothers, MIT Sloan, Northwestern Kellogg, Northwestern Law, Ohio State, Princeton, the RS-DeGroote Conference on Market Structure and Market Integrity, Stanford GSB, U of Alberta, UIUC, UNC, UT-Austin, U of Toronto, Wisconsin, Wharton, and Yale for helpful comments. Financial support from the Lehman Brothers Fellowship for Research Excellence in Finance is gratefully acknowledged. All errors are my own.

1 Introduction

The enforcement of regulatory and private contracts between firms and their customers is important to the proper functioning of product markets. This is particularly true in the markets for professional services where substantial information asymmetries, externalities, and commitment problems exist. A key characteristic of an enforcement mechanism is how it allocates control rights between parties involved in a dispute and various third parties. In dispute resolution circles, a clear distinction is made between enforcement mechanisms that assign more joint control to customers, firms, and independent parties (e.g., public courts) and those that assign more control to industry groups that are set up to represent the interests of their member firms (e.g., industry arbitration). The latter control structure is a form of self-regulation: the industry maintains control over its member firms' regulation by exercising control over enforcement proceedings against these members. These industry groups are widely referred to as self-regulatory organizations (SROs).

In practice, self-regulation plays a substantial role in the enforcement process for many product markets (e.g., in the medical and accounting professions). For instance, in retail financial service markets in the United States, many enforcement duties are heavily controlled by SROs like the National Association of Securities Dealers (NASD) and the New York Stock Exchange (NYSE).[1] Since these SROs are owned and organized by securities firms, there are potential advantages and disadvantages associated with their control over enforcement. If SROs maximize the interests of their member firms, they may prefer to underenforce rules violations if they are ineffectively monitored through the regulation channel (e.g., by the Securities and Exchange Commission (SEC)) or through the reputation channel (e.g., by retail investors). On the other hand, SROs have specialized industry knowledge and may face lower monitoring and verification costs. Thus, self-regulation could be associated with a trade-off between industry bias and expertise. Despite the prevalence of self-regulation in financial and other professional services markets, no empirical research has attempted to examine the desirability of this form of regulation.

This paper starts filling this gap by exploring the behavior of a specific SRO, the NASD, in the resolution of disputes between retail investors and securities brokers. Since these disputes proceed in an arbitration forum run by the NASD, this environment provides an opportunity to study the trade-off between industry bias and expertise associated with self-regulation. This setting is particularly promising because the entire range of possible enforcement outcomes is reported. More importantly, a recent rules change introduced in 1998 reduced the NASD's control over the enforcement process. Using a unique hand-collected database of arbitration cases, I provide evidence that is consistent with the presence of expertise and industry bias in SRO enforcement. I then investigate whether the decline in NASD control led to a drop in this expertise and industry favoritism. Overall, the findings are consistent with a fall in expertise, but do not provide evidence that bias decreased. In fact, the

[1] On July 2007, the enforcement divisions of the NASD and NYSE merged and are now known as the Financial Industry Regulatory Authority (FINRA). This organization is still an SRO.

results suggest the opposite change: bias *increases* following the reduction in self-regulation.

Ideally, enforcement institutions should be designed to maximize accuracy in interpreting facts (i.e., expertise) and minimize deviation from the social optimum when translating these facts into decisions (i.e., bias).[2] However, because optimal punishments are not observed in the data, it is impossible to evaluate an enforcement mechanism on either of these dimensions while only studying the level or variance of decisions. Instead, this paper examines the endogenous selection of arbitrators to cases. Using observed arbitration decisions to classify arbitrators as more or less pro-industry, I look for bias in enforcement by verifying whether relatively more pro-industry arbitrators are selected more frequently than pro-investor ones. Likewise, I classify the expertise of arbitrators on the basis of their professional backgrounds and case experience and explore expertise in enforcement by asking whether relatively more expert arbitrators are also selected more often to cases. These two arbitrator selection patterns are referred to as *selection on bias* and *selection on expertise*, respectively.

Endogenous arbitrator selection illustrates a specific way that bias and expertise can arise in enforcement. For instance, if some arbitrators are more pro-industry than others, an arbitrator selection process that "favors" pro-industry arbitrators will induce average outcomes that benefit the industry. Ex-ante, it is reasonable to focus on selection on bias and expertise in this setting for two reasons. First, there is likely to be widespread arbitrator heterogeneity. Variation along the pro-claimant/pro-respondent dimension has been documented in other judicial forums (e.g., Kling, 2006; and Chang and Schoar, 2006) and should be even more pronounced in securities arbitration because arbitrators have more discretion than judges. Second, the NASD exercises control over the arbitrator selection process. Namely, it manages the arbitrator pool and, prior to the rules change, hand-picks lists of potential arbitrators that are sent to parties. After receiving these lists, investors and brokers have limited discretion in selecting the final panel.[3]

In the first part of the analysis, I document general patterns in arbitrator selection and find evidence of selection on bias and expertise even after controlling for an arbitrator's degree of availability and the characteristics of other potential arbitrators. Moreover, these patterns vary across measures of case importance and complexity. Selection on bias is more pronounced in important cases: when the respondent list includes a large brokerage firm or when the requested compensatory damage by investors is large. Selection on bias is also greater when cases involve more complex financial instruments and strategies. This is consistent with the allocation of pro-industry arbitrators being *targeted* to cases that most benefit industry either because the returns to bias are larger (important cases) or

[2] An optimal enforcement institution also minimizes enforcement costs (e.g., Bernardo, Talley and Welch, 2000). Since NASD case dockets do not contain sufficient information on these costs, I cannot explore this dimension in the analysis. However, many observers have argued that alternative dispute resolution mechanisms, like arbitration, have lower enforcement costs than more formal ones, like courts.

[3] This selection procedure is quite different from judge trials because judges are usually *randomly* drawn from a regional pool. With the exception of the NASD's role in selection, arbitrator selection shares common features with the selection of jurists in jury trials (e.g., both parties to a dispute have a fixed number of peremptory strikes and can issue challenges for cause).

bias is more easily masked into decisions (complex cases). Meanwhile, there is moderate evidence that the correlation between arbitrator expertise and selection is strongest in complex cases which have a large number of distinct allegations and involve complex instruments. This suggests that expertise is not only used but *managed* in a way to increase the precision of enforcement. This interpretation is reinforced by the fact that this pattern is only associated with scarce forms of expertise, namely experience in cases with similar allegation profiles, and not with more widely available expertise, such as being a lawyer or having experience as a chairperson on arbitration panels.

However, since all parties (not just the NASD) are involved in selection, two *distinct* factors can induce these patterns in selection on bias. While selection on bias is consistent with the presence of industry favoritism within the NASD, an alternative hypothesis is that brokerage firms do better in the arbitrator selection process because of other comparative advantages. For example, brokerage firms have more experience in arbitration and in selecting arbitrators and have more information about arbitrators. This latter view does not imply that SRO control of arbitration leads to weaker enforcement. In the second and *main* part of the analysis, I attempt to separate these two hypotheses by taking advantage of the 1998 rules change mentioned earlier. This change only affected rules governing the arbitrator selection process and primarily removed the NASD's discretion in picking lists of potential arbitrators to send to parties. Surprisingly, I find that selection on bias increased and, in some specifications, only becomes statistically significant *after* this reduction in NASD control over selection. While some of this difference can be explained by time variation in case characteristics, point estimates of selection on bias are always larger and significantly greater than zero following the rules change even after accounting for observable variation in cases. This supports the hypothesis that other comparative advantages are at least partially responsible for selection on bias and casts some doubt on the widespread view that SROs lead to reduced enforcement. Moreover, selection on expertise *declines* following this change. This is consistent with the view that the NASD actually helped improve the allocation of expertise to arbitration cases and thus increased the precision of enforcement.

A potential concern in interpreting both parts of the analysis is the presence of unobservable case quality and other omitted variables. In particular, due to the endogeneity of panel selection, proxies for pro-industry bias may be capturing unobservable aspects of arbitrator expertise. Thus, the correlation between arbitrator bias and selection frequency may be spurious. To mitigate this concern, I use additional institutional features of securities arbitration to verify that the pro-industry proxies reflect differences in opinion and judgement patterns across arbitrators. For example, the NASD does not allow class action lawsuits or require the application of precedent. This allows me to identify a subsample of cases that have similar quality, due to a common source of wrongdoing, and are subject to rulings by different arbitrators. I find that the outcomes of these cases are correlated with my measures of bias. I also look at open disagreement between members of arbitration panels by analyzing dissent patterns and find that larger *within* tribunal dispersion in the bias proxies predicts a higher likelihood of dissent. Other robustness checks are also presented to address concerns like the misclassification of expertise. Overall, results do not seem to be driven by the misclassification of either arbitrator bias or expertise.

Beyond serving as a unique laboratory to study the relationship between self-regulation and enforcement, dispute resolution between investors and brokers is also relevant for financial markets because, in theory, it should substantially effect broker incentives. Broker behavior is likely to have a real impact on investment because full-service brokers are major providers of professional financial advice (e.g., ICI and SIA, 2005).[4] While this delegation to brokers can be desirable because of economies of scale in information production and monitoring (e.g., Diamond, 1984) and investors' behavioral biases (e.g., Odean, 1999; and Barber and Odean, 2000), investors will only realize these gains if brokers act in their interest. However, as described in the Tully Commission Report (SEC, 1995), the prevalence of commissions-based compensation in the industry encourages brokers to recommend excessive trading and overinvestment in proprietary products. Thus, if enforcement is ineffective, the agency problem between investors and brokers may be sufficiently severe that investors reduce their use of advice from brokerage firms.[5] This could contribute to costly distortions in portfolio allocation that are observed in the data, like limited stock market participation or overinvestment in familiar assets and other negative effects on financial markets.

The remainder of the paper is structured as follows. Section 2 discusses the literature relating to the design and enforcement of financial regulation. Section 3 provides a background on important institutional characteristics including the change in arbitrator selection procedures. Section 4 introduces the data and some descriptive statistics. Section 5 documents general patterns in arbitrator selection and section 6 uses the rules change to see whether the NASD is responsible for these patterns. Section 7 contains a number of robustness checks. Section 8 concludes.

2 Related Literature

Starting with Becker (1968), there is an extensive theoretical literature on enforcement as a means of deterring inefficient behavior. Most relevant to the debate on self-regulation is the issue of *who* should enforce rules. Much of the research into this question has centered on the choice of public versus

[4]Given recent changes in public policy, this influence is expected to grow substantially. For example, the Pension Protection Act (HR2830), which amends the Employee Retirement Income Securities Act (ERISA) by lowering the legal liability of employers who hire outside advisors to provide investment advice to their 401(k) plan participants, is expected to dramatically increase the provision of professional advice in the defined-contribution channel of retirement investing (e.g., "Trolling for 401(k) Treasures" in *Registered Representative Magazine*, 11/01/05.). Another potential policy change, that would have an even greater impact on the size of the market, is the privatization of Social Security.

[5]This concern is particularly relevant given recent criticisms of securities arbitration by investor groups and the media. See, e.g. "Walled Off From Justice?" in *Business Week*, 03/22/04; "Judging Wall Street" in *Newsweek*, 09/06/04; "Rough Justice: Wall Street Panels for Settling Fights Draw Renewed Fire" in *Wall Street Journal*, 03/17/05; and "Is This Game Already Over?" in *The New York Times*, 06/18/06. There have also been political hearings on securities arbitration held by the U.S. House of Representatives' Committee on Financial Services (03/17/05) and the North American Securities Administrators Association (07/20/04).

private enforcement.[6] Focusing mainly on the incentives of enforcers, Becker and Stigler (1974) argued that private parties, compensated with the fines they collect, would implement optimal enforcement and that the market for private enforcement would ensure low enforcement costs. However, others have suggested that private enforcement has limitations that can lead to either overenforcement (Landes and Posner, 1975) or underenforcement (Polinsky, 1980) and that public enforcement can be favorable even if private enforcers have a cost advantage. Looking within public enforcement, Glaeser, Johnson and Shleifer (2001) explore adjudicator heterogeneity and argue that the choice of enforcer reduces to a trade-off between adjudicators who are impartial but unmotivated (judges) versus those who are biased but highly incentivized (regulators).[7] More closely related to this paper, DeMarzo, Fishman and Hagerty (2005) explicitly focus on self-regulation and argue that it may be preferred over direct government regulation because of SROs' relative expertise in detecting rules violations even though this leads to sub-optimal levels of enforcement.[8] Further, they advocate a hierarchical structure of self-regulation with government oversight because it achieves cost-effective enforcement while also inducing SROs to increase enforcement in order to avoid direct involvement by the government.[9,10]

There is also a large empirical literature that explores the effects of legal rules restricting the behavior of corporate insiders and financial intermediaries. Using cross-country variation in these rules, research in law and finance highlights a strong positive correlation between laws protecting minority shareholders (e.g., La Porta et al., 1997; 1998; and 2002), mandating disclosure, and facilitating private enforcement (La Porta, Lopez-de-Silanes and Shleifer, 2006) and measures of financial development such as equity market size, ownership concentration, and firm valuation. Related work also studies variation in specific rules within a country to learn about the impact of rules changes. For example, recent research has recognized that certain rules changes that stengthened mandatory

[6]Roughly, public enforcement is undertaken by government institutions while private enforcement is undertaken by private parties. Since enforcement is a multi-stage process, most enforcement institutions are characterized as a mixture of these two extreme organizational forms where investigations are privately triggered (e.g., by victims) and undertaken by either private (e.g., lawyers) or public (e.g., regulators) parties, while decisions are made by a public (e.g., regulator or judge) or private (e.g., self-regulator) enforcer. The existing literature generally focuses on public vs. private involvement in the latter two stages of the enforcement process.

[7]Regulators are highly motivated but biased because they are more subject to political and career concerns. However, the corollary that regulators gather and interpret more information than judges requires that the information provided by other parties across these two regimes be the same. As pointed out by Dewatripont and Tirole (1999), after taking the other parties' incentives into account, this may not be the case.

[8]Low levels of enforcement are used by SROs to provide excessive moral hazard rents to member firms even though compete to attract customers (see Pirrong (1995) for an alternative story based on customer heterogeneity). This view that SROs exercise monopoly power to capture rents for their members can be interpreted as an extreme form of regulatory capture (Stigler, 1971).

[9]Similar trade-offs between expertise and bias have been discussed in other economic environments. For instance, managing this trade-off has been mentioned as an important consideration in the choice between insiders and outsiders in corporate boards (e.g., Raheja, 2005).

[10]Relatedly, the financial contracting literature has begun exploring *what* laws should be enforced given the characteristics (expertise) of the enforcer (e.g., Ayotte and Yun, 2005) and *how* optimal contracting (provision of incentives) responds to corruption at the enforcer-level (e.g., Bond, 2004).

disclosure had a differential impact across corporations and exploited this to provide further evidence that mandatory disclosure increases firm value (e.g., Greenstone, Oyer and Vissing-Jorgensen, 2006; and Hochberg, Sapienza and Vissing-Jorgensen, 2006). Like this paper, Chang and Schoar (2006) look at variation across judges in the application of laws, but do so in the context of bankruptcy and reorganization. However, their focus is quite different from mine because they look at ex-post effects of more pro-debtor application of rules while this paper documents how adjudicator selection and heterogeneity impact ex-ante *effective* rules (i.e., rules after taking their enforcement into account).

Focusing on enforcement, Bhattacharya and Daouk (2002) also look at effective rules in financial markets. In particular, they use cross-country panel data on insider trading laws and their enforcement and find that the cost of capital in a country only falls after the first enforcement of these laws rather than the date of their passing. This provides support for the intuition that a good law requires good enforcement to have real impact. Bhattacharya, Galpin and Haslem (2006) study corporate litigation and document that domestic firms fare better than international ones when sued in U.S. federal courts. Their paper is similar to mine in that it looks at *bias in enforcement*. However, my paper focuses on the disciplining of financial intermediaries which, most importantly, sheds light on a common feature of the regulation of financial markets, self-regulation, because enforcement falls under the control of SROs rather than the public. This paper also adds to the empirical literature on arbitration, which has mainly looked at labor disputes (e.g., Ashenfelter and Bloom, 1984; Bloom and Cavanagh, 1986; and Ashenfelter, 1987), by analyzing disputes in the fastest growing segment of arbitration: commercial arbitration.

Regarding the securities brokerage industry, evidence on the link between conflicts of interest and broker behavior is limited due to the unavailability of micro-data on most broker actions. Recent work provides some suggestive evidence on this link using data on mutual fund flows that is disaggregated by distribution channel. This research finds that broker-distributed funds charge higher fees and perform worse than those that are distributed directly (Bergstresser, Chalmers and Tufano, 2006) and, looking within this channel, that redemptions from funds using less conflicted brokers (unaffiliated brokers) are more closely associated with poor future performance while those from funds using more conflicted brokers (captive brokers) are more likely to be reallocated within the same fund family (Christoffersen, Evans and Musto, 2006). Using more detailed data from the real estate market, Levitt and Syverson (2006) also document self-interested behavior by real estate brokers. Given further evidence of conflicts of interest and misbehavior in the financial services profession (e.g., Christie and Schultz, 1994; Lin and McNichols, 1998; Michaely and Womack, 1999; and Geczy and Yan, 2006), it seems that managing broker incentives should be a priority for investors and policymakers. This paper complements the emphasis on compensation-based incentives by exploring the other component of incentive constraints: enforcement-based incentives.

This paper also loosely relates to the literature on frictions in portfolio choice by exploring an institutional component of retail investor trust in financial intermediaries. In particular, if investors do not trust brokers in their role as financial advisors, they may respond by undertaking investment more independently which can lead to distortions in portfolio allocation.[11] For example, Guiso, Sapienza and Zingales (2006) provide evidence that investor concerns about being cheated in financial markets lead to limited stock market participation. This is particularly likely in the case of mistrust in financial advisors because of increased participation costs associated with researching investments individually (e.g., Vissing-Jorgensen, 2002). Other systematic patterns of underdiversification by individual investors could also be partially explained by a lack of trust in brokers. These include holding too few stocks in one's portfolio to save on information acquisition costs, domestic and international home bias (e.g., Grinblatt and Keloharju, 2001; Ivkovic and Weisbenner, 2005; Poterba and French, 1991; and Bailey, Kumar and Ng, 2005) and overconcentration of portfolios in own company stock (e.g., Benartzi, 2001; and Cohen, 2006) because investments in familiar assets are likely to require less information acquisition costs or advice. These distortions can also have equilibrium implications on the level of securities prices (e.g., Mankiw and Zeldes, 1991; and Vissing-Jorgensen, 1999) and the covariance structure of returns (e.g., Pirinsky and Wang, 2006).

3 Institutional Background on Securities Arbitration

Securities arbitration was initiated by the NYSE in 1872 and the NASD in 1968. The NASD is by far the dominant forum for resolving disputes between investor and brokerage firms with over 90% of cases filed in its forum. Almost all customer brokerage contracts include predispute arbitration agreements that force investors to opt out of litigation in courts by binding them to adjudicate their claims in securities arbitration. However, it was not until 1987 that the Supreme Court ruled that these agreements were enforceable.[12] It was this ruling that effectively made SRO arbitration the default dispute resolution mechanism between investors and brokers instead of commercial courts.

3.1 Disputes

The most common conflict between retail investors and securities brokers arises from the latter's incentive to encourage inappropriate and unnecessary trading by their clients in order to increase commissions revenue. Consequently, most investor-broker cases allege actions by brokers that either *directly* or *indirectly* generate excessive commissions. The legal basis for these claims comes from federal securities laws, most notably the Securities Exchange Act of 1934, especially section 10(b)-5 of this Act (Hazen, 2003). Allegations of direct actions mainly comprise of *churning* and *unauthorized trading* claims. Churning occurs when a broker has control of a customer account and makes trades more frequently than necessary with the purpose of generating commissions. Control over an account

[11] Alternatively, investors could also respond to their lack of faith in professional advisors by relying on their peers for investment advice. This might explain some of the social interaction effects that have been documented in the behavior of retail investors (e.g., Duflo and Saez, 2002; and Hong, Kubik and Stein, 2004).

[12] For a historical legal background on these agreements, see Appendix A.

can either be explicit, as is the case in discretionary accounts, or implicit. Implicit controls arises when an investor relies heavily on the broker's recommendations. Unauthorized trading involves the placement of transactions in non-discretionary accounts by the broker without obtaining prior approval from the client.

Meanwhile, indirect actions cover behavior that is meant to mislead investors to undertaking unnecessary trades. This mainly consists of manipulating the disclosure of information about investments or making faulty recommendations by misinterpreting or ignoring facts. The manipulation of information involves either *misrepresentation* or *omission*. Misrepresentation occurs when the broker makes mistakes or is untruthful in disclosing material facts to the client. Omission of information consists of failing to disclose facts that are material to the customer's decision to invest. The manipulation of investment recommendations falls under *unsuitability* claims. These indirect actions can also result from other types of agency problems between the two parties, like insufficient broker effort, which are particularly likely when actions are client-specific. For example, a broker may be liable under these claims if he fails to learn and address the particular financial needs of his client. Insufficient broker effort can also lead to a *failure in following customer instructions*.[13] Other broad claims, like *breach of fiduciary duty* and *negligence*, provide additional information on the nature of the relationship between the investor and the broker or the alleged actions.

Brokerage firms are almost always the primary respondents in investor-broker disputes because a company is liable for the actions of its employees (when it profits from these actions) and employers have deeper pockets than their employees. Beyond the actions of individual brokers, the brokerage firm or the broker's supervisor, who is usually a retail branch manager, may also be liable for *failing to supervise* the broker's activity.

3.2 Arbitrators

Arbitrators have more discretion than judges. While many observers claim that this discretion is necessary to ensure the flexibility and effectiveness of arbitration (e.g., Perino, 2002), others believe that it allows arbitrator bias to influence case outcomes.[14] The most notable source of this discretion is the limited grounds for overturning arbitration awards.[15] These grounds do not include instances where arbitrators misunderstood or misapplied the law, only those where it is established that they must have been aware of the law and chose to disregard it.[16] Furthermore, arbitrators alone decide whether evidence is relevant to a case and their decisions cannot be vacated on the basis of their

[13]Securities arbitration is also used to resolve disputes between registered representatives (employees) and brokerage firms as well as conflicts between brokerage firms. We do not focus on either of these disputes in this paper.

[14]See, e.g. *Wilko v. Swan*, 346 U.S. 427, 438 (1953).

[15]See Chapter 1, Section 10 of the Federal Arbitration Act.

[16]See, e.g. *Montes v. Shearson Lehman Brothers, Inc.* (1997) 128 F.3d 1456 for an example (of an employee-firm case) where this standard was applied in vacating a securities arbitration award.

determination of facts.[17] Arbitrators are not even required to follow precedent from case law or previous arbitration decisions. Judicial review is further handicapped because, unlike judges, arbitrators are not required (and rarely choose) to provide written opinions in their decisions.[18] Unsurprisingly, the vacatur of investor-broker arbitration awards is extremely rare.

At the NASD, there are two major classifications for arbitrators: public and industry. Public arbitrators are supposed to have no ties to the securities industry while industry arbitrators have current or recent professional associations either as registered representatives or attorneys doing business with brokerage firms.[19] Depending on the amount of damages claimed, an investor-broker case will either have one public arbitrator or a three-member panel consisting of two public arbitrators and one industry arbitrator. Once a panel is selected, one of the arbitrators (usually a public one) is assigned as the chairperson. The special duties of the chairperson include presiding over the pre-hearing conference (where discovery and other issues are resolved), maintaining order in case proceedings, and taking a lead in questioning disputants.

Public arbitrators make up the majority of a panel in order to preserve the appearance of impartiality and reduce the risks associated with undisclosed conflicts of interest by industry arbitrators. However, critics have pointed out that certain public arbitrators are subject to conflicts as well: some are retired brokers and attorneys or have non-professional links to industry.[20] Furthermore, if brokerage firms have substantial influence in selection, public arbitrators may also avoid giving investors large awards to ensure future selection to panels. Given their diverse backgrounds, public arbitrators also vary in their ability to precisely determine the merits of a case. As a result, an argument can be made that arbitration outcomes are more sensitive to which public, rather than industry, arbitrators are selected.

Meanwhile, the inclusion of industry arbitrators on panels has received a great deal of public scrutiny. Observers contend that industry arbitrators induce a bias in decisions by, for example:

> "sanction[ing] industry practices that have become institutionalized and apply[ing] the standard of their own practices, rather than [mandated practices]."[21]

On the other hand, industry arbitrators have more expertise in the material issues of a case. As Perino (2002) points out:

> "[t]his is one of the key benefits of arbitration because expertise theoretically allows arbitrators to render more accurate rulings on complex, technical, and often arcane questions.

[17] Rule 10323 of the NASD's Uniform Code of Arbitration states: "The arbitrators shall determine the materiality and relevance of any evidence proffered and shall not be bound by rules governing the admissibility of evidence."

[18] In 2005, the NASD proposed a rule change (SR-NASD-2005-032) that would give either party in a dispute the right to request that arbitrators provide reasoned decisions at an additional cost. These decisions would "stat[e] the reasons that each alleged cause of action was granted or denied" but would not need to explain specific damage calculations.

[19] For a precise definition of an industry arbitrator, see Rule 10308(4) of the NASD's Uniform Code of Arbitration.

[20] See, e.g. "Rough Justice: Wall Street Panels for Settling Fights Draw Renewed Fire" in *Wall Street Journal*, 03/17/05.

[21] See the Public Investors Arbitration Bar Association's statement submitted at the House Committee on Financial Services's hearing on securities arbitration (03/17/05).

Such expertise typically comes from working in or with the industry."

3.3 Arbitrator Selection and the NLSS Rules Change

The arbitrator selection process involves investors, brokerage firms, and the NASD. By most accounts, this process is extremely adversarial. Solin (2004) remarks that:

> "[t]here is nothing more important than the selection of the arbitrators who will hear [the] dispute... [C]onsiderable effort is expended by securities lawyers to determine whatever they can about prospective arbitrators... [e.g.] obtaining copies of prior awards by each proposed arbitrator... [and] contacting attorneys who participated in hearings before that arbitrator."

Indeed, under the current selection regime (described below), parties often fail to reach consensus on a tribunal in the first round of selection. As a result, the analysis of panel selection seems well-placed to address the question of industry favoritism and overall expertise in enforcement. As mentioned in the introduction, there was a substantial change to this process during the sample period and I describe the selection of three-member panels under both the old and new procedures. Since public and industry arbitrators are not substitutes in selection (there are 2 public and 1 industry arbitrators selected), the selection of public and industry arbitrators should be considered as separate selection processes (although they are governed by the same rules).

Under the old procedure, the NASD had full discretion in proposing an initial panel of three arbitrators. Each party would be able to dismiss one arbitrator by exercising a peremptory strike. If necessary, the NASD would also choose replacement arbitrators and, afterwards, parties could only request dismissal of further arbitrators through challenges for cause. Decision to grant this request belonged to the NASD. As remarked in the Ruder Commission Report (NASD, 1996), parties only had a limited opportunity to participate in the selection of arbitrators under this process.

In November 1998, a new selection process (called the *Neutral List Selection System* (NLSS)) was implemented in response to recommendations by a securities arbitration task force. This task force was formed by the NASD in September 1994 to study general issues in securities arbitration. While recommendations were made in January 1996, the proposed changes in arbitrator selection could not be implemented until approval was granted by the SEC almost three years later. As a result, the timing of this event had an exogenous component. The recommendations were made to address investor concerns regarding NASD control and their own lack of control over selection, but were not a response to evidence of bias, including selection on bias, under existing rules. Under the NLSS, two computer-generated lists are sent to both parties. The first list contains 10 public arbitrators and the second contains 5 industry arbitrators. Parties can initially strike any number of arbitrators from each list and rank the remaining ones. These rankings are used, without staff discretion, to choose arbitrators. If a full panel has not been determined after this first stage, additional arbitrators are selected by computer algorithm and rounds of challenges for cause (as in the old system) are played.

Practioners generally agree that the main impact of this rule change was a significant reduction of NASD involvement in selection and increased input by claimants and respondents.

4 Data Construction and Descriptive Results

The data in this paper comes from three sources: (i) The NASD arbitration awards folder of Lexis-Nexis's federal securities library, (ii) the Securities Industry Yearbooks which are published annually by the SIA, and (iii) the Central Registration Depository (CRD) which is available online through the NASD BrokerCheck search engine.

I obtain information on securities arbitration cases from NASD arbitration decisions. To get these awards, I perform a search of NASD decisions in Lexis-Nexis using the keyword "award" (which shows up in the header of all decision files). This search covers the period from January 1991 to December 2004 and yields 21,031 cases. In order to deal with the enormous output of this search, I create a computer program to parse through each file and extract all formulaic and standardized entries in the text. This includes basic information about the case (e.g., location and dates), claim type dummies, and the identities of participants in the case (including the arbitrators). Whenever relevant, arbitrator names are cleaned of errors by cross-checking suspicious entries using hearing location and signatures from original documents (available online through the SAC-CCH Awards Network). It is not possible to accurately extract the level of damages (both compensatory and punitive) that are requested and awarded using this program.[22] This information, along with other decisions like counterclaims or third-party claims (by respondents) and dismissal or expungement (by arbitrators), is gathered manually. All cases that do not involve retail investors suing brokerage firms are removed from the main sample.[23] This leaves 15,983 cases. The award-to-claim ratio is available for 13,915 cases and all other covariates are available for 15,306 cases.

I report summary statistics of the case characteristics in the main sample in Panel A of Table 1. Monetary claim, denoted as $Claim_i$, is defined as compensatory damages claimed (winsorized at the 95th percentile). This variable does not include requests for interest and attorney fees. Throughout the paper, the subscript i will be used to index cases. The distribution of $Claim_i$ exhibits substantial positive skewness with a mean (239,864 dollars) that roughly equals the 75th percentile value (250,000 dollars). The median claim value is 73,002 dollars, suggesting that, from the standpoint of damages claimed, most securities arbitration cases are important to investors and registered representatives but are unlikely to have a direct impact on the profitability of brokerage firms. Punitive damages are requested in 56 percent of cases. Disputes include an average of 3.01 allegation types among

[22] This is principally due to two reasons. First, unlike most case summaries, damage requests and awards are written in natural rather than legal language and, therefore, are not standardized enough to accurately extract by algorithm. Second, further complications arise from the fact that we do not include requests and awards of interest or attorney fees in our measures (because precise numbers for these are not always provided by claimants and arbitrators).

[23] These include: registered representative v. brokerage firm (vice versa), firm v. firm, investor v. registered representative (but no firm included) and firm v. investor.

those listed in Section 3.1. As shown in Panel B, twelve percent (41 percent) of these allegations claim that the broker directly (indirectly) generated excess commissions. Breach of fiduciary duty and negligence each represent about 15 percent of allegations, respectively. Failure to supervise the account manager represents 10 percent of allegations. Failure to follow instructions represents about 6 percent of allegations.

Various measures of case outcomes are recorded from the award dockets. The measure that is used in the analysis is $Decision_i$ which equals the monetary award-to-claim ratio.[24] Monetary awards are calculated using the same convention as $Claim_i$. The distribution of $Decision_i$ is heavily censored, with 47.2 percent (10.8 percent) of the observations equal to zero (one), and its average is 0.284. The other measures of case outcome, which are also summarized in Table 1, are punitive award grants, dismissals, and expungements of public disciplinary records. $PunitiveAwd_i$ is a dummy equal to one if punitive damages are awarded. Such awards are rare, they are granted less than 7 percent of the time when requested. Thirty-seven percent of cases are dismissed while 20 percent of cases that include employees are cleared from that person's central registration depository (CRD) record.

As a first proxy for arbitrator bias, I compute claim-weighted averages of $Decision_i$ over each arbitrator's case load. This is denoted by $Decision_j$ where j indexes arbitrators. One important weakness of this proxy is its failure to account for systematic differences in case quality across arbitrators. An attempt is made to control for this by estimating expected decisions using the least squares regression,

$$Decision_i = \alpha_{ct} + \alpha_b + \mathbf{\Theta} \cdot \mathbf{X}_i + \epsilon_i, \tag{1}$$

where α_{ct} and α_b are city×year and firm fixed-effects, respectively, and $\mathbf{X}_i$ is a vector of observable case characteristics.[25] The inclusion of brokerage firm fixed-effects is meant to capture variation in firm-wide practices and, as suggested in McCaffrey and Hart (1998), differences in the aggressiveness of legal defense teams across firms. In order to properly specify brokerage firm fixed-effects, data on mergers and acquisitions and name changes in the brokerage industry are collected from the Securities Industry Yearbooks published during the sample period. Following the suggestion of practitioners, action-state claims with breach of fiduciary and/or negligence claims are allowed to differ in average quality from those without such claims. This is accomplished by including all interactions between these two groups of dummies in $\mathbf{X}_i$. Estimates of this regression and related ones are reported in Table 2. I then define the pro-industriness of a case's outcome as the difference between $E[Decision_i]$ and $Decision_i$ where the expectation is winsorized at the 5th and 95th percentiles. I winsorize to avoid negative expected decisions while preserving symmetry. I then construct two additional measures of arbitrator bias by taking claim-weighted averages of case pro-industriness from specifications

[24]Since compensatory awards occasionally include payment for interest, $Decision_i$ is higher than 1 in less than one percent of cases. In reported results, I cap the value of $Decision_i$ at 1 (though doing this does not affect any results).

[25]All of the results in the paper remain qualitatively unchanged when estimating expected decisions using censored Tobit rather than least squares specifications for decisions.

estimated with and without brokerage firm fixed-effects. These proxies are denoted by $ProInd_j$ and $ProInd_j^{FE}$, respectively. Since I cannot control for unobservable case characteristics, it is possible that these proxies are partly driven by arbitrators being systematically assigned to cases of different quality. I attempt to address this possibility in section 7.1 by verifying that the proxies are correlated with observable differences in opinion.

Meanwhile, three measures of arbitrator expertise are constructed. Since individuals with a legal background are more likely to know how to apply the nuances of the law to a given case, I use a lawyer dummy, $Lawyer_j$, as my first measure of expertise. In particular, I classify an arbitrator as a lawyer by examining the inclusion of Esq and JD suffixes to arbitrator names in award documents. I also use two other proxies for expertise that vary over time. The first one, $ChairExperience_{ij}$ equals one if an arbitrator has served as a chairperson in a case previous to i's filing. Such experience is believed to capture expertise because chairpersons go through additional arbitrator training (through the NASD) and being a chairperson is more demanding for arbitrators. The second time-varying measure quantifies how a current case matches up with each arbitrator's *past* case load. This measure, $CaseExperience_{ij}$, is defined as the proportion of allegations in case i that arbitrator j has had experience with in the past.

Table 3 reports information on the distribution of arbitrator characteristics. In addition to the proxies for arbitrator bias and expertise, the number of selections, tenure length, home state and public/industry classification are inferred. $Selections_j$ denotes the number of cases that j sits on where either a decision is rendered by the arbitration panel or a stipulated award (or other type of observable settlement) is agreed on by the parties and provided to the arbitrators. The average number of observed selections is 4.96, with substantial variation, and is higher for public (5.61) than for industry arbitrators (4.03). One concern regarding this measure of selection is that the sample of publicly available selections suffers from selection bias due to unobserved settlements. In section 7.3, I make an effort to address this issue by making use of observed settlements in the data. Public and industry classifications are almost always included in case dockets and I classify an arbitrator as industry (i.e., $Industry_j = 1$) if he is ever listed as one in the sample. Forty-one percent of arbitrators are classified as industry. $Tenure_j$, measured as the length of time between the filing date of j's first case and the decision date of his last case, is equal to 5.4 years on average. Home city is the city where the arbitrator listens to the majority of his cases. It is only entered if over half of j's cases occur in that city which leaves out approximately 400 arbitrators. Finally, this data is augmented with information on the professional backgrounds of industry arbitrators by attempting to match their names with the CRD database. This determines whether an arbitrator was employed as a registered representative (in the two years prior to November 2005 when this search was performed) and if he has been subject to any disciplinary actions. Among industry arbitrators, 41 percent are brokers and 15 percent of them have been subject to disciplinary actions.

Panel D of Table 3 provides some preliminary evidence on arbitrator selection patterns. After sorting arbitrators into selection quartiles, I find that arbitrators who are selected more frequently are, on average, more pro-industry. These differences are statistically and economically significant. For instance, arbitrators in the 4^{th} quartile rule between an average of 2 to 7 percent more in favor of industry than those in the 1^{st} quartile. This represents an increase of between 8 to 29 percent of the average award. There is also a noticeable pattern in arbitrator backgrounds across these quartiles. For example, arbitrators who are selected more often are more likely to be lawyers. In particular, 57 percent of arbitrators in the top selection quartile are lawyers compared to only 37 percent in the bottom quartile. This loosely suggests that both bias and expertise matter in the selection process. The next two sections of the paper explore these patterns in more detail.

5 General Patterns in Arbitrator Selection

As a first step in the analysis, this section documents general patterns in the arbitrator selection. The first subsection uses data pooled at the arbitrator-level to provide evidence of selection on bias and expertise. The second subsection analyzes more detailed data on case-level selections and goes beyond the analysis of section 5.1 by looking at how selection on bias and expertise vary across different types of cases.

5.1 Arbitrator-Level Evidence

In this subsection, I assume that the number of times an arbitrator is selected to cases follows a negative binomial model subject to some adjustments. This specification extends the Poisson regression model by including random individual-effects. In particular, the conditional distribution of $Selections_j$ is:

$$\Pr(Selections_j = S|\mathbf{A}_j, N_j, \tilde{\alpha}_j, \alpha_s) = \frac{\exp(-\tilde{\mu}_j)\cdot\tilde{\mu}_j^S}{S!} \tag{2}$$

where

$$\begin{aligned} \tilde{\mu}_j &\equiv \tilde{\alpha}_j \cdot N_j \cdot \exp(\mathbf{\Theta}\cdot\mathbf{A}_j + \alpha_c) \\ &= \tilde{\alpha}_j \cdot \exp(\mathbf{\Theta}\cdot\mathbf{A}_j + \ln N_j + \alpha_c), \end{aligned} \tag{3}$$

$\mathbf{A}_j = (Bias_j, Expertise_j, Controls_j)$, N_j is the number of cases filed in j's home city during his tenure, $\tilde{\alpha}_j$ represents unobservable arbitrator characteristics, and α_c are home city fixed-effects. In order to identify the model and maintain a closed-form likelihood function, α_j is assumed to be independently drawn from the gamma distribution:

$$\tilde{\alpha}_j \sim g(\alpha) = \frac{\delta^\delta\cdot\alpha^{\delta-1}\cdot\exp(-\delta\alpha)}{\Gamma(\delta)}, \tag{4}$$

where δ is a parameter to be estimated. This specification assumes that the probability of j being selected to a case is equal to $\exp(\mathbf{\Theta} \cdot \mathbf{A}_j + \alpha_c)$ and that selection is independent across cases during j's tenure. In addition to concerns of neglected heterogeneity, this model is chosen over the Poisson regression because of overdispersion in the selections data.[26] This overdispersion is not surprising given the unconditional mean and standard deviation of $Selections_j$ reported in Table 3.

Since potential arbitrators who are never selected to panels are not included in the dataset, the estimation procedure accounts for truncation at $Selections_j = 0$. This model is estimated by maximum likelihood of the zero-truncated negative binomial model (see Cameron-Trivedi, 2005) with:

$$\mu_j = \tilde{\alpha}_j \cdot \exp(\mathbf{\Theta} \cdot \mathbf{A}_j + \beta \cdot \ln N_j + \alpha_s) \tag{5}$$

and the constraint $\beta = 1$. Reported standard errors account for clustering of the error term at the home city-level. Allowing β to differ from 1 would permit the average probability of being selected to a case to vary over j's tenure in such a way that it increases (decreases) with N_j when $\beta > 1$ ($\beta < 1$). The first column of Table 4 shows that the constraint is rejected when estimating the unrestricted model and suggests that the probability of being selected is not constant across cases (in particular, along a time dimension). Nevertheless, results with $\beta = 1$ imposed are presented because the unconstrained model does not bound probabilities to be less than or equal to one.[27] I delay analysis that accounts for within arbitrator variation in selection probabilities to the next subsection which discusses case-level evidence.

The second to fourth columns of Table 4 indicate that relatively more pro-industry arbitrators are selected to a larger number of cases than relatively pro-investor ones, regardless of the bias proxy used. Evaluating all other variables at their means, a decrease from the 75^{th} to the 25^{th} percentile in $Decision_j$ (an increase in bias) is associated with a 7.8 percent increase in the expected number of selections. However, the third and fourth columns show that this sensitivity to bias falls noticeably when controlling for observable case characteristics in the construction of bias measures. This estimated marginal effect is 4.5 percent when using the $ProInd_j$ measure and 1.7 percent when using $ProInd_j^{FE}$. The fifth and sixth columns run these regressions separately for public and industry arbitrators and document that selection on bias is stronger and only statistically significant in the public sample. Using $ProInd_j^{FE}$, the estimated marginal effects for public and industry arbitrators are 2.2 and 0.7 percent, respectively. Table 4 also shows that lawyers are selected to cases more often (columns 1 to 4) and that this pattern is driven entirely by public arbitrators (columns 5 and 6). Being a lawyer is associated with about a 14.2 percent increase in selection probability. The

[26]The Poisson distribution implies that the mean of a random variable equals its variance, while the negative binomial model allows the variance to be higher than the mean (overdispersion). Estimates from Poisson regressions are qualitatively identical to those of the obtained from Negative Binomial regressions.

[27]Estimates from the unconstrained model are qualitatively identical and quantitatively similar.

corresponding estimates in the public and industry sample are 21.1 and -0.9 percent, respectively.

The documented pattern of selection on bias in the sample of public arbitrators is consistent with the view that brokerage firms have an advantage over investors and exploit this to select arbitrators that have a tendency to rule in their favor. There are several explanations for the lack of evidence on this pattern in the selection of industry arbitrators. First, since industry arbitrators may be more homogeneous than public ones and are selected fewer times, the pro-industriness proxies for this group may be driven by noise rather than bias (evidence of this will be presented in section 7.1). Second, bias may be better captured by unobserved components in the backgrounds of industry arbitrators. The $RepRep_j$ dummy is not a good proxy for this because it only identifies arbitrators who currently are or recently were securities brokers while most other industry arbitrators held similar positions in the *past* or have other significant ties to brokerage firms. It is also likely to be correlated with another important control: availability. Finally, the pattern may not exist because investors put most of their efforts in "fighting" brokerage firms on the selection of industry arbitrators and, in turn, neglect to do so for public ones. The finding that public arbitrators who are lawyers get selected more often to cases than non-lawyers supports the view that there is selection on expertise. However, this result may partially be driven by lawyers being more available to arbitrate than non-lawyers. I will attempt to address this in the case-level evidence by using alternative measures of expertise. The fact that this pattern does not show up in the industry subsample is not surprising because non-lawyers are almost universally considered "financial experts" on the basis of their work experience in industry.

Although the marginal effects reported above are informative, they do not provide a direct estimate of the influence of selection on bias on the the pro-industry bias of tribunals. In order to do this, I compute the expected bias of an arbitrator under the fitted distribution and a corresponding one that is free of selection on bias. Specifically, for the fitted case, I obtain the expected number of selections for each arbitrator, $ESelections_j$, and define the expected bias as:

$$\overline{Bias}^* = \sum_{j=1}^{J} \left(\frac{ESelections_j}{ESelections_1 + ... + ESelections_J} \right) \cdot Bias_j \tag{6}$$

where the term in brackets is the inferred probability that arbitrator j is selected and J denotes the total number of arbitrators. $\overline{Benchmark}$ is calculated similarly using a distribution that has a coefficient of 0 on $Bias_j$, but is otherwise identical to the fitted one. This distribution is meant to approximate a setting where there is no selection on bias. My estimate of the effect of selection on bias is the difference between these two measures (expressed as a percentage of the mean decision),

$$\overline{Bias} = 100 \cdot \frac{\overline{Bias}^* - \overline{Benchmark}}{\text{mean}(Decision_i)}. \tag{7}$$

Table 4 reports that $\overline{Bias}$ ranges from 1.0 to 3.8 percent across specifications with this expected increase in bias due to endogenous panel selection mostly coming from public arbitrators. While these estimates are quite small, it is difficult to judge how much they are downward biased due to measurement error in the pro-industriness proxies.[28]

In summary, the arbitrator-level evidence suggests that there is both selection on bias and expertise. While point estimates indicate that expertise plays a greater role in selection and that selection on bias does not substantially lower average enforcement levels, this may be due to imperfections in the constructed proxies for bias (e.g., measurement error). Furthermore, estimates may be understated because the decision to file cases is endogenous: investors who are most hurt by selection on bias may not file cases. Another important caveat is that the bias proxies only attempt to capture *relative* bias, not *absolute* bias. Thus, even if we ignore imperfections in these proxies, the findings in this subsection (and throughout the paper) only imply that selection on bias leads to lower enforcement relative to an alternative with no selection on bias. I cannot rule out the possibility that a significant *absolute* bias in favor of *either* party exists since this would occur if all arbitrators were generally pro-industry (or pro-investor). Consequently, welfare implications of selection on bias cannot be determined without making assumptions about the population distribution of bias.[29]

5.2 Case-Level Evidence

Next, I create a case-level selections dataset that, for each case, keeps track of the arbitrators who were selected to that case, those who were available but were not selected, and each of their arbitrator-case characteristics. Since information on arbitrator availability is not publicly disclosed, I only classify arbitrators as available in their home city and starting the day after their first selection in that city until the day before their last decision. Observations on the date of an arbitrator's first selection are not included since the arbitrator is, by definition, selected on a case that day. In each city, all cases filed within the year following the first filing in that city or decided within the year before the last decision in that city are omitted. I exclude these cases because the availability proxy most understates the number of available arbitrators towards the beginning and the end of my sample. For instance, in the first filing in any city, the arbitrator selected will be, by construction, the only available arbitrator since no one else's tenure window will have begun. In all, the data used for estimation contains 9,983 cases and, on average, over 100 public and 40 industry arbitrators available for selection.

In addition to the independent variables used in section 5.1, five more arbitrator and arbitrator-case characteristics are added in this analysis. Two of these variables are used as additional proxies for an arbitrator's expertise. The first, $ChairExperience_{ij}$, is a dummy that equals one if an arbitrator has served as a chairperson in the past. The second, $CaseExperience_{ij}$, is a measure of how well an arbitrator's case experience matches up to the allegations of the current case. It is defined as the

[28]Indeed, it is not even clear that there is even a downward bias in the estimates of $\overline{Bias}$. While it is likely that measurement error will pull estimates of selection on bias towards zero, it also increases the variance of the bias distribution. The first effect lowers $\overline{Bias}$ while the second increases it.

[29]For instance, selection on bias is welfare destroying if the average arbitrator has an absolute bias in favor of *industry* but may actually be welfare improving if the average arbitrator has an absolute bias in favor of *investors*.

fraction of case i's allegations (out of the list described in section 3.1) that have also been alleged in at least one of arbitrator j's *previous* cases. Meanwhile, $Length_j$ denotes the average amount of time (in years) needed to resolve cases where j is selected as an arbitrator. Since the length of a case is heavily driven by an arbitrator's availability in scheduling hearing dates, this variable captures an arbitrator's role in resolving cases quickly. $Tenure_{ij}$ measures the length of time, as of case i's filing date, that has elapsed since j's first selection. It is included because arbitrators may become more or less available (or more knowledgeable) over their tenure as suggested in the first column of Table 4. Finally, I proxy for variation in the *degree* of availability among arbitrators by keeping track of which arbitrators are already sitting on panels for other cases when case i is filed. This dummy variable is denoted by $Panel_{ij}$.

I model the arbitrator selection process using a logistic distribution with case fixed-effects (Chamberlain, 1980). The main advantage of this distribution over a standard logistic model is that it conditions the likelihood function on the number of arbitrators selected to a case.[30] Specifically, arbitrator j is selected to case i (i.e., $Selected_{ij} = 1$) if and only if:

$$U_{ij} = \alpha_i + \mathbf{\Theta} \cdot \mathbf{A}_{ij} + \epsilon_{ij} \geq 0, \tag{8}$$

where $\mathbf{A}_{ij} = (Bias_j, Expertise_{ij}, Controls_{ij})$ and ϵ_{ij} follows a logistic distribution. Conditional on a panel of size n_i, this implies that the probability of observing a panel p is given by:

$$\Pr(Panel_i = p) = \frac{J_i(p)}{\sum_{p' \in \mathcal{P}_i} J_i(p')}, \tag{9}$$

where $\mathcal{P}_i$ is the set of all possible panels of size n_i and $J_i(p) = \prod_{j \in p} \exp(\mathbf{\Theta} \cdot \mathbf{A}_{ij})$. Since the selection of public and industry arbitrators is done separately in practice, I estimate a selection model for each group. When $n_i = 1$ (i.e., almost all industry arbitrator selections and all single arbitrator panels), this specification is equivalent to the classical random utility model of McFadden (1974). For $n_i > 1$, it satisfies a number of intuitive properties. Most importantly, the likelihood of being selected increases with characteristic a_{ij} if and only if the coefficient on this characteristic is positive. Furthermore, when the number of potential arbitrators is large relative to n_i, this function can be shown to be a good approximation for the likelihood function obtained by a generalization of the random utility model that incorporates the selection of multiple alternatives. I report standard errors that take into account clustering at the home city×year level.

As a first step, I replicate the results from section 5.1 at the case-level. Table 5 confirms the earlier evidence by showing that there is statistically significant selection on bias and expertise for public arbitrators, but only statistically significant selection on expertise for industry arbitrators. The economic effects of changes in the bias measures and $Lawyer_j$ on selection probabilities are similar

[30] Qualitatively identical results are obtained using the standard logistic regression.

to those from the earlier analysis (from 3.1 to 5.4 percent and around 30 percent, respectively).[31] Selection on expertise is also documented using the new measures of expertise. Arbitrators with experience as chairpersons are around 46 percent more likely to be selected to cases while a move from the 25^{th} to the 75^{th} percentile of case experience is associated with between a 37.4 and 48.1 percent increase in selection probability. The strong correlation between $ChairExperience_{ij}$ and selection probability may be explained by the fact that arbitrators are generally only picked as chairpersons when *both* parties believe they have sufficient expertise to adequately perform the additional duties required in this role. Other measures that affect selection probability are the size of panels and the number of arbitrators available for selection (both of which are built directly into the likelihood function) as well as $Length_j$, $Tenure_{ij}$, and $Panel_{ij}$. The coefficient on $Length_j$ is negative and consistent with the view that parties value the timely resolution of cases. Arbitrators are also more likely to be selected as their tenure increases which confirms the finding that $\beta \neq 1$ in the arbitrator-level results. The negative relationship between $Panel_{ij}$ and selection probabilities suggests that less available arbitrators are less likely to be selected to cases (or to accept selection) because arbitrators who are already on panels are likely to be more available. This is likely due to the fact that they can try to scheduling hearings for multiple cases on common visits to NASD hearing offices. It should be noted that the low pseudo-R^2 values are expected given the imperfect proxies for availability and the fact that all arbitrators have a very low probability of being selected (since there are so many arbitrators to choose from).

One of the main advantages of the case-level analysis is that it also allows for the exploration of how selection on bias and expertise vary across different types of cases. This analysis is done by adding interactions between the bias (expertise) proxies and observable case characteristics. Given the earlier findings, one might predict that selection on bias is stronger in cases that are more important to brokerage firms. This prediction relies on the assumption that the ability to select biased arbitrators is limited. If this were not the case, firms would simply select biased arbitrators in every case without a need to allocate them where the marginal benefit of bias is highest. Such a constraint is reasonable because there is a limited number of pro-industry arbitrators and influencing panel selection to one's advantage may require costly effort. Furthermore, the SEC oversees SRO arbitration and it is more likely to exercise its formal authority if bias is very pronounced (e.g., DeMarzo, Fishman, and Hagerty, 2005). This view is consistent with the relatively small coefficients on bias in the specifications to date.

I consider three primary proxies for case importance: (i) the size of the brokerage firm being sued, (ii) the firm's direct financial stake in a case, and (iii) the firm's reputational stake in the

[31] Throughout the remainder of the paper, I define the *economic effect* of a change in a variable x as the answer to the following question: "If two otherwise identical arbitrators have $x = x_L$ and x_H, how much more (or less) likely is H to be selected to a 1-member panel?" If the effect is normalized by L's likelihood of selection, the answer to this question has a simple form, $\exp(\theta_x \cdot (x_H - x_L))$ - 1, where θ_x is the estimated coefficient on x in the model (hence the first term is a scaled odds-ratio).

case. The first, $LargeBrok_i$, equals one if a brokerage firm is listed among the top ten employers of retail brokers in the Securities Industry Yearbooks in over 80% of the years in the sample in which they operate independently (using publications between 1990-91 and 2004-05).[32] Large brokerage firms are included as the main respondent in almost one-third of the sample. This variable could either be capturing variation in influence over the NASD, legal resources, experience in arbitration, or reputational capital across brokerage firms. The second, $HiClaim_i$, is a dummy equal to 1 if the amount of compensatory damages requested is greater than or equal to the 75^{th} percentile value across cases (250,000 dollars). The last proxy, $Supervision_i$, equals one if it is alleged that a firm failed to supervise its employee. This measure is likely to be correlated with case importance because it involves *firm behavior* rather than the actions of a particular broker. As a result, such a case should have a greater effect on firm reputation and, if successful, could lead to other similar complaints against the firm.

Similarly, selection on expertise should be stronger in complex cases. The first measure of case complexity, $ManyClaims_i$, keeps track of whether many allegations are made in a case. It is a dummy that equals to one if the total number of allegations made by the investor (among those listed in section 3.1) is greater than the mean number of allegations across cases. The second measure, $MargLev_i$, is a dummy variable set to one if a case involves margin or leveraged transactions. These transactions are considered more complicated than simple purchases and sales of securities and are more likely to involve complex financial instruments, like options.[33]

Table 6 reports results on the allocation of bias and expertise across cases. Since selection on bias is only documented in the selection of public arbitrators, this table (along with the analysis in the next section) only focuses on selection in this group. The first three columns sort on the primary measures of case importance. All three specifications confirm or suggest that selection on bias is substantially higher in (and almost entirely driven by) cases that are classified as important. The coefficient on bias is 0.054 and insignificant for small brokerage firms and 0.305 for large ones with the difference being significant at the 5-percent level. The corresponding estimates when sorting on $HiClaim_i$ and $Supervision_i$ are 0.077 to 0.279 and 0.108 to 0.217, respectively (though the differences are statistically weaker with $HiClaim_i$ being significant at the 10-percent level). Economically, these changes are substantial. While arbitrators at the 75^{th} and 25^{th} percentile of $ProInd_j^{FE}$ are about equally likely to be selected in unimportant cases, the pro-industry ones are 5.0 to 7.8 percent more

[32]These firms are Merrill Lynch, Shearson Lehman Hutton (acquired by Smith Barney in July, 1993), Morgan Stanley (also known as Morgan Stanley Dean Witter and operating as Dean Witter Reynolds prior to May, 1997), Citigroup (also known as Salomon Smith Barney and operating as Smith Barney and Smith Barney Harris Upham prior to July, 1997) Prudential Securities (formerly known as Prudential-Bache), UBS Financial (also known as UBS Paine Webber and operating as Paine Webber prior to November, 2000), A.G. Edwards, Edward D. Jones (also known as Edward Jones), Charles Schwab, and Fidelity.

[33]Allegations involving the use of options are widely perceived as being particularly complex. However, since product classifications are infrequently reported in the NASD awards, tests using this measure have insufficient power. In cases with available product classifications, I find that cases with $MargLev_i = 1$ are five times more likely to involve options than those with $MargLev_i = 0$.

likely to be selected in the important cases. As a more nuanced check of the relationship between case importance and selection on bias, I look at how this relationship varies when an employee is included as a defendant in a case. This inclusion has an ambiguous effect on the importance of a case to brokerage firms because of two opposing factors. On the one hand, individual brokers are valuable resources to these firms because customers maintain a direct relationship with these individuals rather than with firms. Furthermore, public disclosure practices are such that a loss in securities arbitration negatively impacts the CRD record of the registered representative while usually leaving the firm's disclosure unchanged.[34] On the other hand, firms may be *less* concerned in these cases because they can use the employee as a scapegoat for the violation (blame it on a "bad apple"). To decouple these two factors, I distinguish between cases that involve large and small brokerage firms. Presumably, large brokerage firms can more convincingly attribute violations to a small number of rogue employees rather than firm-wide practices, while individual employees contribute a larger share of business to a small brokerage firm. Column 4 on Table 6 supports this view. When an employee is included in a case, the coefficient on bias increases from -0.142 to 0.109 for small firms and decreases from 0.393 to 0.279 for large firms. The first difference is statistically significant at the 5-percent level[35]

For the $ChairExperience_{ij}$ measure of expertise, the first three columns of Table 6 also indicate a strong and uniform pattern in the allocation of expertise across measures of case importance: selection on expertise is weaker in important cases. The fifth and sixth columns of the table suggest that the allocation of expertise is stronger in complex cases, but only under the $CaseExperience_{ij}$ measure. The coefficient on $CaseExperience_{ij}$ increases from 0.342 to 0.526 when $ManyClaims_i$ equals one, with the difference being significant at the 1-percent level. This difference is also economically meaningful: moving from the 25^{th} to 75^{th} percentile of $CaseExperience_{ij}$ increases selection probability by 48.4 percent in complex cases compared to 29.3 percent in the other cases. Likewise, when sorting on $MargLev_i$ the coefficient goes from 0.407 to 0.526 which implies a similar economic effect as in $CaseExperience_{ij}$. However, despite the economic significance of this change, this latter difference is not statistically significant at traditional levels (p-value of 0.182). The coefficients on the other expertise measures, $ChairExperience_{ij}$ and (in unreported regressions) $Lawyer_j$, do not vary uniformly across case complexity. This does not necessarily refute the view that expertise is allocated optimally across cases because public arbitrators who are lawyers or have experience as chairpersons are quite common (61 and 63 percent, respectively), only those with case experience that match specific cases are scarce.[36]

[34]In particular, disclosure in the central registration depository only provides a "no" or "maybe" answer to whether or not an employee or a firm has been subject to a disclosure (disciplinary) event. Most individuals have a "no" in this entry while almost every brokerage firm has some disclosure event in its past.

[35]This finding suggests that securities arbitration can, to an extent, be exploited by brokerage firms to manipulate the reputations of their brokers and that smaller brokerage firms take advantage of this opportunity. Similar (though more incriminating) patterns have been uncovered in the disclosure of research analyst histories in the I/B/E/S database (Ljungqvist, Malloy and Marston, 2006).

[36]If a desirable resource (e.g., arbitrator expertise) is available in abundant supply, it will not be forgone on individual cases regardless of case characteristics (e.g., complexity). As a result, such a resource should have a similar impact on

To summarize, these results provide further evidence that the patterns uncovered in the arbitrator-level evidence truly represent selection on bias and expertise. In particular, they extend the observation of "limited" selection on bias and expertise by showing that both bias and scarce forms of expertise are allocated across cases in ways that are consistent with economic theory.

6 Is the NASD Responsible for these Patterns?

While section 5 provides evidence of selection on bias and expertise, it does not identify what is responsible for these patterns. With respect to selection on bias, two channels are consistent with the evidence: (i) pro-industry favoritism *within* the NASD and (ii) other comparative advantages that are enjoyed by brokerage firms. The latter advantages could take the form of a lower marginal cost in inducing bias through selection (because brokerage firms are frequent participants in arbitration or have better information about arbitrators) or a higher marginal benefit of bias (due to a firm's reputation stake in a case or the possibility of future complaints being triggered by the loss of a case).[37]

In order to distinguish between these two channels, I look at how selection on bias and expertise differ before and after the NLSS rules change. This event helps determine whether the NASD is responsible for these patterns because each channel makes different predictions about how selection on bias and expertise change after the NLSS switch. Under the industry favoritism hypothesis, the drop in NASD control over panel selection is expected to reduce or even eliminate selection on bias. However, if other comparative advantages drive selection on bias, such a change should not occur. In fact, if the NASD used its discretion in arbitrator selection to *help* investors by picking initial and replacement lists on the basis of arbitrator fairness, the magnitude of selection on bias could increase because computer-generated lists would contain more variation in arbitrator bias.[38] This increase in heterogeneity could be exploited by brokerage firms to create even more bias in their favor. Likewise, regarding selection on expertise, if the NASD played a special role in improving the quality of arbitration, the NLSS switch might also be associated with a decrease in selection on expertise.

Table 8 supports the view that the NASD is *not* responsible for selection bias. Using the $ProInd_j$ measure of bias, columns 1 and 2 of the table show that selection on bias increases after the NLSS switch and that the magnitudes of the coefficients before and after the change are quite different.

selection probability regardless of case characteristics. However, when this resource is in more limited supply (e.g., an arbitrator cannot sit on too many panels), it may be "saved" for use when it is most desirable (e.g., complex cases) rather than being assigned on a first-come-first-serve basis. This optimal restraint will produce a difference in the effect across cases (e.g., complex vs. "not" complex).

[37]In section 5, it would only have been possible to disentangle NASD favoritism from other comparative advantages if information on the NASD's hand-picked lists was available. In this case, the favoritism channel could be isolated by looking at selection to these list rather than selection to panels (which incorporates both NASD and investor-broker actions).

[38]This is due to two factors: (i) NASD behavior before the change in selection and (ii) the fact that initial list go from a size of 3 to 15 after the rules change.

Economically, a move from the 25^{th} to 75^{th} percentile of $ProInd_j$ leads to slightly more than a 3 percent increase in expected selections in the pre-NLSS period compared to almost 9 percent post-NLSS. Columns 3 and 4 show similar results using $ProInd_j^{FE}$ with the pre-NLSS estimates not being significant. However, due to low power, the differences are not statistically significant in column 4 (though the post-NLSS estimate of selection on bias is significant).[39] Overall, the evidence strongly suggests that selection on bias did not *decrease* after the NLSS switch and generally supports the view that it *increased* after the switch, but I cannot uniformly reject the hypothesis that it remained the same during both periods. Nonetheless, the results indicate that the NASD is at least not *entirely* responsible for selection on bias since this pattern is significant following the rules change.[40]

This suggests that at least some of the selection on bias is due to brokerage firm comparative advantages. Consistent with the view that the NASD was helping investors mitigate this advantage, Figure 1 shows that investors started seeking more help from other sources, namely lawyers, after the rules change. The year after the change, the use of professional representation by investors in cases with three arbitrators jumped from a relatively steady 80 percent to nearly 85 percent and by 2003 had risen to around 90 percent (the change was even larger in cases with one arbitrator). Table 9 generally confirms this pattern in logit regressions that study the determinants of hiring professional representation. These regressions include a post-NLSS dummy and other case characteristics as controls. The coefficients on $PostNLSS_i$ (columns 1, 3, and 5) are positive and significant at the 1- to 5-percent levels in all specifications except in the 3-member subsample (which may be due to low power, especially in column 3). As shown in the second, fourth, and sixth columns of this table, this finding is relatively robust to replacement of the post-NLSS dummy with time-trend variables. Specifically, these regressions find no evidence of increasing professional representation prior to the rules change and, with the exception of column 4, suggest that an increasing trend appears in the post-NLSS period.

Regarding expertise, columns 1 through 4 of Table 8 show that selection on expertise *fell* following the change. These changes are dramatic (with differences that are always significant at the 1-percent level). While an arbitrator with experience as a chairperson was about 80 percent more likely to be selected than an arbitrator without this experience prior the the NLSS switch, this difference essentially disappeared after the change (the drop was significant but not as pronounced with the lawyer measure of expertise). These changes are equally dramatic for $CaseExperience_{ij}$: the difference in expected selections between the 25^{th} and 75^{th} percentiles falls from between 55 to 82 percent to less

[39] Table 19 performs a similar analysis to Table 8 with four alternative bias measures (described in the table notes) and finds qualitatively identical results. In fact, the results are statistically stronger with the analog to column 4 being significant at the 5-percent level or better for 3 of the 4 measures.

[40] Table 7 provides further support for the view that NASD control may have been helping investors. In particular, it finds that average decisions fell following the NLSS switch. When including the $PostNLSS_i$ variable in the regressions on $Decision_i$ from Table 2 (and removing the time fixed-effects), I find a negative and highly significant coefficient of -0.051 to -0.058 (though the estimates are smaller when including broker fixed-effects). I also find that the declining trend in $Decision_i$ only appears following the rules change (the trend is estimated at 0.006 before and -0.024 after the switch).

than 6 percent. In fact, selection on this measure of expertise is no longer statistically significant in the post-NLSS period. Consequently, it seems that the NASD uses its discretion to help parties assign knowledgeable arbitrators to cases and is more successful in doing so than investors and firms (who may neglect arbitrator expertise in the process of fighting over selection along the bias dimension).

Given the surprisingly weak evidence of selection on expertise following the NLSS switch, it is natural to wonder whether arbitrator selection mechanisms that give more joint control to parties in a dispute do a poor job in selecting expert adjudicators to panels. Theory suggests that this might be the case. In particular, consider an environment where these disputants agree on the quality of the case. In this setting, one of three belief structures exists: (i) both parties agree that the customer has a good case, (ii) both parties agree that the firm has a good case, or (iii) both parties agree that the case is a toss-up. The important theoretical observation is the following: Conditional on litigation, cases falling in (i) or (ii) will involve a party that prefers not having experts as adjudicators. In particular, absent risk-aversion issues, the party with the weaker case prefers a decision-maker who is likely to make errors. This is due to the fact that he benefits more from errors than the other party (since awards are bounded between roughly zero and the amount claimed). Thus, if frictions exist that induce cases falling in (i) or (ii) to be litigated frequently enough (e.g., relative to Priest and Klein (1984)), there will be a set of cases where disputant control over arbitrator selection performs poorly because of the "weaker case" party's incentives. This is a fundamental observation that also applies to contexts outside of arbitrator selection, most notably, prominent adjudicator selections mechanisms like jury selection in the United States.

Building mainly on Table 8, this section has made several claims about the role of the NASD in inducing patterns of selection on bias and expertise in arbitrator selection. Of course, there are important concerns with attributing a causal interpretation to the simple analysis of Table 8. In the remainder of this section, I investigate three alternatives to the causal interpretation and argue that none of them can explain all the changes in selection on bias and expertise after the NLSS's implementation. The alternative explanations that I consider are: (i) potential time-variation in case characteristics, (ii) the presence of time-trends in selection patterns, and (iii) the endogeneity of the rules change. Consideration of a fourth alternative, time-variation in external monitoring, is delayed to section 7.4 because it involves a different methodology than the one employed here (duration analysis of arbitrator tenure).

6.1 Time-Variation in Case Characteristics

Given the evidence that selection on bias varies across cases, it is possible that some of the difference in selection on bias before and after the NLSS switch is driven by changes in case characteristics over time. Mean case characteristics over both sample periods are shown in Table 10 (for cases with three panel members). While most allegations appear with similar frequency over the two periods, the three case characteristics that were found to be correlated with selection on bias in Table 6 (claim size, $LargeBrok_i$, and $Supervision_i$) are larger or more common after the NLSS rules change. However, this variation is unlikely to be caused by the rules change since none of the characteristics increase substantially immediately after the switch. They are likely due to other gradual changes in the securities brokerage market. For example, since the post-NLSS period coincides with the rise and fall of the technology bubble (late 1990s and early 2000s), the post-NLSS period should exhibit an increase in claim sizes because customers are wealthier. This prediction is supported by Figure 2 which indicates that claim size closely tracks the market (with a lag): average compensatory damages rise from about 350,000 dollars in 1998 to a high of over 500,000 dollars in 2001.

In order to verify whether the increased frequency of important cases explains all (or even reverses) the change in selection on bias, I augment the specification from Table 8 by estimating selection on bias before and after the rules change in both high and low case importance groups. For each case importance dummy, Table 11 reports estimates of selection on bias across four groups of cases: (i) high importance in the Pre-NLSS period, (ii) high importance in the Post-NLSS period, (iii) low importance in the Pre-NLSS period, and (iv) low importance in the Post-NLSS period. All models use $ProInd_j^{FE}$ as the bias measure.[41] Models 1a and 1b use $HiClaim_i$ as a measure of case importance and find that selection on bias is insignificant and does not change after the NLSS switch for low claim cases. However, in high claim cases, the estimate of selection on bias increases from 0.398 before to 0.704 after the rules change in model 1a which uses $Lawyer_j$ as the expertise measure. This difference implies an increase in the economic effect from 11.9 to 22 percent and is significant at the 5-percent level. In model 1b, which uses $ChairExperience_{ij}$ as the expertise measure, the estimated coefficient increases from 0.182 to 0.440 which represents an increase in the economic effect from 4.4 to 10.9 percent.[42] Models 2a and 2b, which use $LargeBrok_i$, produce a similar change in selection on bias for high importance cases. For example, in model 2b, the coefficient increases from 0.153 (insignificant) to 0.384 (significant at the 1-percent level) or, equivalently, the economic effect increases from 3.7 to 9.4 percent. Using $Supervision_i$, models 3a and 3b produce similar changes, but they also indicate an increase for low importance cases (with a coefficient increasing from 0.056 to a statistically significant 0.199 after the rules change for model 3b). However, since only one of the differences from Table 11 is significant (while another is borderline significant with a p-value of 0.119), the results should be interpreted as weakening the evidence from Table 8 that selection on bias *increased* after the rules change.[43] Nonetheless, it should be highlighted that these findings continue to indicate that NASD favoritism is not *entirely* responsible for selection on bias since this pattern is still significant in high importance cases (and certain low importance ones) following the rules change.

[41] Results are qualitatively identical with $ProInd_j$ used a the bias measure.

[42] This result is not due to differences in claim distribution *within* either of the claim size groups. In fact, differences before and after the change are even more pronounced when using the continuous variable $\ln Claim_i$ as an interaction term instead of $HiClaim_i$.

[43] Given the economic effects from Tables 8 and 11, it is clear that this drop in significance is due to low power rather than a drop in the coefficient estimates.

6.2 Time-Trend in Selection Patterns

Another concern with attributing changes in selection patterns to the NLSS switch is that these changes may reflect a gradual time-trend in selection patterns rather than the impact of this event. Figures 3 and 4 suggests that a time-trend is not responsible for the results on bias and on one of the measures of expertise. Specifically, these figures report results from fixed-effects logistic regressions over one-year long windows relative to the NLSS switch date starting 4 years before the switch and ending 4 years after the switch (eight regressions in total).[44] Each figure plots the coefficients on bias or expertise in the period between t and $t+1$ years after the change (or before the change if t is negative) for $t = -4, ..., 3$. Figure 3 suggests that there was no trend in selection on bias prior to the rules change and there is a noticeable jump in selection on bias immediately following the NLSS switch (though it is not statistically significant). Even with these noisier estimates, selection on bias is generally statistically significant from $t = 1$ to 3. To my knowledge, no other significant event occurred around this period that could have produced this pattern. Meanwhile, Figure 4 suggests that a general decline in the $CaseExperience_{ij}$ coefficient is a more plausible explanation for the change in selection on expertise for this measure. This may reflect gradual adjustments in arbitrator training made by the NASD over time. Nonetheless, the drop in selection on $ChairExperience_{ij}$ between $t = -1$ and $t = 0$ is sufficiently dramatic that it is unlikely to only reflect a downward time-trend: the most reasonable interpretation is that much of this drop is due to the rules change.

6.3 Endogeneity of the Rules Change

While the timing of the change in arbitrator selection has a random component (due to administrative delays and frictions in implementation), the choice by the task force of which change to propose and the timing of the task force's initiation were endogenous. As a result, the initiation and proposal may have been made in response to unobservable changes in the enforcement environment. If changes in the environment (e.g., increase in firms' legal resources) were expected to increase selection on bias, this could explain the results documented in Table 8. Indeed, the rules change may actually have lowered selection on bias in this case, just not enough to outweigh the change in environment.

However, if this were the case, the increase in bias should have appeared around the *initiation or proposal* dates rather than the implementation date which occurred almost three years later. It is unlikely that these unobservable changes would just happen to start influencing arbitrator selection immediately following the rules change. Since this was the case (see Figure 3), endogeneity is unlikely to be driving the change in selection patterns. It is also difficult to imagine that the findings are driven by an increase in investor influence over the NASD (which could also explain the initiation of the task force) because this type of development would be expected to lead to a fall rather than an increase in selection on bias. Indeed, this type of endogeneity probably makes it tougher to detect an

[44]This window roughly corresponds to the largest symmetric window with a common government oversight regime. I define a government oversight regime as a period with the same SEC Chairman (in this case, Arthur Levitt).

increase in selection on bias that is due to the change in arbitrator selection rules.[45]

6.4 Discussion: Findings and Open Questions

To summarize, the results presented in this section point to a story where the NASD plays a positive role in enhancing the efficiency of enforcement. Consistent with existing theory on the advantages of self-regulation, the NASD is found to improve expertise in enforcement by increasing selection on expertise. Meanwhile, the change in selection on bias does not support the hypothesis that self-regulation leads to lax enforcement of rules. In fact, the evidence suggests that NASD control may be associated with stronger enforcement (though, statistically, the analysis is inconclusive on this front). Nonethless, these findings cast some doubt on the widespread view that self-regulation involves a trade-off between expertise and bias.

The lack of evidence on this trade-off suggests that it may not be in the NASD's interest to favor brokerage firms over investors in the selection of arbitrators (relative to the NLSS regime). To the extent that the NASD takes actions that are in the collective interest of member firms, this points to a natural tension between ex-ante and ex-post incentives for individual brokerage firms. Specifically, after being sued by an investor, brokerage firms want to minimize their liability by trying to get pro-industry arbitrators selected to their case. They do this because of an externality: they capture all the gains from influencing selection but only bear part of the social cost of reduced enforcement quality.[46] As in other public good problems, this can make all firms worse off ex-ante. One view that is consistent with the evidence in this section, but stands in stark contrast to existing discussions of SROs, is that the NASD is an institutional solution to this problem. Namely, to the extent that the NASD is designed to be isolated from member influence, it may allow the industry to *commit* to better enforcement by reducing influence activities ex-post.

Of course, the view that the NASD is an effective enforcer is highly speculative at this point. While theoretically sensible and loosely supported by the data in this study, it is important to gather more evidence that either supports or rejects this view. In particular, it is necessary to recognize that one size does not fit all with self-regulation (and enforcement mechanisms in general). An SRO may serve as effective enforcer in one setting but may be ineffective in another. For instance, the ability of investors and the government to monitor SRO behavior should matter. If an SRO's behavior can be effectively observed from public disclosures the reputation channel should work well and the SRO will be better able to commit to good enforcement. This logic suggests that control rights alone do not pin down the effectiveness of an enforcement institution. In the case of private control (e.g., self-regulation), transparency and disclosure should matter as well. This intuition suggests that SROs might do a good job when transparency exists but might do a poor job when disclosures are

[45] It is also unclear how endogeneity would be expected to lead to the decrease in selection on expertise.

[46] This social cost can take the form of lower customer demand (product market discipline) or additional costly monitoring and exercise of control in enforcement by the SEC. This externality is likely to be strongest in settings where monitors focus on keeping track of collective reputations (Tirole, 1986) rather than individual reputations.

withheld. This is loosely consistent with anecdotal evidence within the NASD. While the NASD has not displayed clear industry favoritism in broker enforcement, it has experienced some notable enforcement mishaps in trading regulations (e.g., dealer collusion as documented by Christie and Schultz (1994)). The notable difference in the structure of monitoring between these two settings: triggering enforcement in the broker case better guarantees the production of a public signal than in the trading case since all securities arbitration cases that go to an award are disclosed to the public.

Moving to more targeted observations, the presence of selection on bias following the NLSS switch also suggests that it may be desirable to reduce the impact of other brokerage firm comparative advantages on arbitrator selection. One way to accomplish this may be to incorporate asymmetry in arbitrator selection rules in a way that favors the weaker party (the investor) by limiting the stronger party's ability to exercise peremptory strikes or challenges for cause. While such asymmetry may induce some ex-post inefficiency, it may improve ex-ante enforcement by eliminating some of the bias in selection that hurts investors. The intuition behind this proposal is similar to the motivation for biased legal presumptions (e.g., Bernardo, Talley and Welch, 1998) and is related to some of the arguments made in literature on auctions design with asymmetric bidders (e.g., McAfee and McMillan, 1989; and Povel and Singh, 2006).[47] An alternative is to provide additional help to investors who are most disadvantaged in the hope that this reduces the comparative advantage of brokerage firms.[48] This suggestion may also be applicable to adjudicator selection rules in other litigation settings.

Finally, it is also valuable to carefully explore the idea that disputant control over adjudicator selection might have poor expertise selection properties. As mentioned earlier, this hypothesis can be supported by economic introspection based on ex-post party incentives. Given the prominent role of this control structure in many litigation environments (e.g., many arbitration forums and courts where awards are determined by juries), a better understanding of these incentives and patterns could highlight some of the costs associated with these types of mechanisms.

7 Robustness Checks

The main concerns regarding the findings in sections 5 and 6 that remain unaddressed are due to imperfections in the bias, expertise and selections measures. To address these concerns and a few others, I perform several robustness checks in this section. In order to reduce the effect of measurement

[47] Unfortunately, heterogeneity in the degree of asymmetry between investors and brokerage firms complicates the implementation of the optimal asymmetric selection process. This is due to the fact that the asymmetry in rules depends on the specific parties to a dispute. Unlike an auction setting where the seller of a good is likely to have a good idea of the degree of asymmetry among parties and has an interest in setting the optimal (i.e., revenue-maximizing) asymmetry, determining the appropriate mechanism designer in our enforcement environment is not as straightforward.

[48] An existing set of programs that partially tries to accomplish this are the securities arbitration clinics. These clinics, which are joint initiatives of the NASD and various law schools, provides free legal representation to less wealthy and often elderly investor claimants. However, while these programs are surely worthwhile, one might wonder how effective they are in reducing broker comparative advantages which were found to be strongest in large and more important cases.

error in the bias proxies, all the regressions in section 7.1 restrict the sample to arbitrators with at least 5 selections. These arbitrators represent 72 percent of the selections in the data. For the most part, the use of alternative selections thresholds, or none at all, does not affect the reported findings (whenever it does, I point it out below).

7.1 Misclassification of Bias

Because of potential misclassification in the bias measures, it is possible that the documented relationship between bias and selection probability is due to an omitted arbitrator characteristic that is picked up by the bias proxy. For instance, variation in this proxy could capture differences in unobservable arbitrator skill if cases differ in unobservable quality *and* arbitrators with expertise are, on average, assigned to cases of lower ex-ante quality.[49] If this were the case, the earlier results should be interpreted as further evidence of selection on expertise rather than selection on bias. In order to partially mitigate this concern, I verify that the bias proxies are related to differences in opinion across arbitrators.

Prior to doing this, it is necessary to show that the measures of bias help explain case outcomes. If this were not the case, it would be difficult to argue that they capture arbitrator heterogeneity along the bias dimension. In order to do this, I regress the bias of a panel on the pro-industriness of a case's outcome (defined as the residual from equation (1.1) on p.12). To avoid a mechanical relationship, the panel bias for case i is defined as the average individual bias of a panel's members where the individual biases are computed as in $ProInd_j$ and $ProInd_j^{FE}$ but with the outcome of case i *removed* from the sample. Table 12 shows that the coefficients on this regression are positive and highly statistically significant for all the bias measures. Point estimates suggest that a unit increase in the measures of panel bias are associated with between a 0.12 and 0.14 unit increase in the pro-industriness of a case's outcome. There are two reasons why this coefficient could be less than one: (i) there is measurement error in the panel bias proxy, and (ii) arbitrator bias, though persistent, is not constant over time. As columns 2 and 5 demonstrate, these coefficients do not increase significantly before and after the NLSS change. This reduces concern that selection on bias appears stronger in the post-NLSS period because of reduced measurement error in the bias proxies over this period. If anything, point estimates indicate that measurement error may be more prevalent after the rules change which biases against finding an increase in selection on bias. Columns 3 and 6 documents that this predictive power of the panel's bias is driven by the public arbitrators in the panel. This is consistent with the view that more variation in the bias measure is due to noise for industry arbitrators and further reinforces the decision to focus on public arbitrator selection patterns.

However, as mentioned earlier, the results of Table 12 could be attributable to arbitrators re-

[49] The latter pattern is reasonable because ex-ante quality should be related to case complexity. In particular, since the outcomes of "easy" cases are often known to all parties, investors will only file these cases if they know they are likely to win them (high quality). Meanwhile, "tough" cases will be comprised of both low and high quality cases and, therefore, will have a lower ex-ante quality.

ceiving systematically different cases. To address this concern, it would be ideal to observe multiple arbitrators making decisions on the *same* case to see whether differences in those decisions are correlated with the bias proxies. I attempt to approximate this ideal by exploiting the fact that securities arbitration prohibits the filing of class action suits. Instead, investors are required to file their cases *individually* which induces a sequence of repeated cases decided on by distinct arbitrators. I focus on one particular set of repeated cases: those filed against the analyst Jack Grubman and Citigroup alleging misrepresentation and conflicts of interest in the research coverage of Worldcom. In these cases, the alleged wrongdoing is relatively homogeneous (common analyst reports) and, given the fact that the same law firm represented many of the claimants in my sample, these cases are likely to have similar quality. The first two columns of Table 13 perform the same regressions as Table 12 on this much smaller sample (131 cases with available panel bias measures) and reports that the coefficients are still positive and statistically significant (at the 5- and 10-percent levels of significance). Since investors are usually only aware of wrongdoing in their own accounts, it is difficult for them to coordinate legal actions and, in turn, very few other repeated cases can be identified in the data. I find an additional 17 groups of repeated cases, for a total of 55 cases, using the following screening algorithm: two cases are classified as part of the same repeated group if they contain similar allegations, are filed within two days of each other in the same state against the same brokerage firm, and either include the same individual broker as respondent or members of the same family as claimants.[50] As shown in columns 3 and 4 of Table 13, adding these cases to the Grubman sample and running the same regressions from the first two columns (with fixed-effects for each repeated group) produces virtually identical results which are slightly more significant.

An alternative strategy to identify differences in opinions takes advantage of the fact that many decisions are made by panels rather than individual arbitrators. Though arbitration panels usually find a middle ground when deciding case outcomes, they are not always successful in doing so and an arbitrator occasionally dissents from the majority. This is a public display of difference in opinion. If the measures of bias are adequate, they should predict the probability of such disagreements. To investigate this, I model dissent using a logistic model with *within* panel dispersion in bias and other controls as explanatory variables. The other controls are $AvgLawyer_i$ and $AvgChairExperience_i$ (which are averages of $Lawyer_j$ and $ChairExperience_{ij}$ over the panel, respectively), claim characteristics (including claim type dummies), and the pro-industriness of a case squared (which measures how unusual a decision is given observables). Columns 1 and 3 in Table 15 indicate that dispersion in $ProInd_j$ within a panel, defined as:

$$DispProInd_i = \max_{j \in \mathcal{P}_i} ProInd_j - \min_{j \in \mathcal{P}_i} ProInd_j, \tag{10}$$

[50]One exception to this rule is a set of cases filed against Merrill Lynch regarding misrepresentations in an investment fund (the Focus 20 Fund).

is positively correlated to the probability of dissent as predicted (significance at the 1- and 5-percent levels). When an alternative measure of within panel dispersion in bias is used, namely a dummy equal to 1 if $DispProInd_i$ or $DispProInd_i^{FE}$ is above its 90th percentile, identical and statistically stronger results are obtained.

Overall, these results suggest that the measures of arbitrator bias used in Section 5 capture differences in opinion across arbitrators. Thus, pure misclassification is unlikely to be driving my findings. Nevertheless, it is possible that the bias proxy, while being adequate, is correlated with unobservable arbitrator expertise and that it is this correlation, rather than bias itself, that produces the results.[51] However, I believe that this omitted variables problem is unlikely to explain my findings. If it did, the coefficients on the bias measures would be expected to closely follow those on the observable expertise measures. As Tables 6 and 8 show, this is not the case.

7.2 Misclassification of Expertise

There are also potential problems with a causal interpretation of the positive correlation between my measures of arbitrator expertise and selection to panels. As in the case of the arbitrator bias proxies, one concern is the possibility of misclassification of expertise. In order to address this, I verify that my measures of expertise predict the likelihood of selection as a chairperson. As mentioned earlier, selection as a chairperson is expected to be influenced by expertise because it imposes additional duties on arbitrators that require expertise to be undertaken effectively. This selection is also unlikely to be influenced by bias because both parties can veto an arbitrator's selection as a chairperson.

To investigate the determinants of chairperson selection, I construct a selection model similar to the one used in the case-level analysis of sections 5 and 6. However, in this setting only arbitrators who have been selected to a case are considered as potential chairpersons. I estimate this model using the logit model with case fixed-effects which, in this setting, is identical to a random-utility model because only one chairperson is selected. Table 16 confirms that each of the expertise measures are significant in predicting the likelihood of selection as a chairperson. These results add credibility to the use of these measures as proxies for expertise. Finally, it is important to note that the coefficients on the pro-industriness measures are insignificant. This further reduces concerns that the bias proxies measure unobserved arbitrator expertise.

[51] Two attempts were made to directly rule this out. First, using arbitrator characteristics that are presumably orthogonal to expertise, namely sex and race, an optimal instrument was constructed following Amemiya (1974) but it was not sufficiently strong. The fact that this instrument is weak is not surprising given that there is no a priori reason to believe that men are more biased than woman (vice-versa) or that bias differs systematically across race. Second, data was collected on arbitrators who were dropped from panels due to accident, death or illness. If unobserved expertise is driving my findings, then the bias of the dropped arbitrator should still be positively correlated with the case's outcome. Table 14 suggests that this is not the case though low power (due to the small number of dropped arbitrators identified in the data) makes it impossible to draw conclusions from these results.

7.3 Settlements

The presence of unobserved settlements can also bias my results. For example, if pro-industry arbitrators have a higher propensity to settle cases, then the earlier analysis will understate the extent of selection on bias. On the other hand, if pro-investor arbitrators are more likely to sit on cases that settle, the evidence of selection on bias may simply be a reflection of the fact that the selections data is more downward biased for pro-investor arbitrators than for pro-industry ones. In order to verify whether either scenario is reasonable, I investigate whether the measures of panel bias and expertise predict the probability of *observed* settlements. While observed and unobserved settlements are not necessarily governed in the same way, one might expect arbitrator characteristics to more strongly influence observed settlements where arbitrators usually play a more meaningful role in shaping the terms of the settlement.

Table 17 reports the coefficients of logistic regressions of settlement on arbitrator and case characteristics. It is clear from this table that bias does not have a significant correlation with settlement rates. In fact, the sign on bias is positive when using $ProIndPan_i$ and sometimes negative when using $ProIndPan_i^{FE}$. Interestingly, the sign on expertise tends to be negative and significant. This implies that the earlier analysis may overstate how much more often experts are selected to cases. The only other controls that seem to influence the likelihood of settlement is the inclusion of an employee as a respondent and claim size. The employee inclusion relationship is not surprising: employees are likely to find it in their interests to settle because doing so increases the chance that they avoid public disclosure of the lawsuit in their CRD records. There is also an increase in settlement activity over time (though this does not occur suddenly around the change in arbitrator selection rules).

Overall, there is no evidence that arbitrator bias influences the rate of even observed settlements. This partially reduces concerns that incomplete measurement of arbitrator selections, due to settlement activity, is generating the evidence of selection on bias.

7.4 Arbitrator Tenure

The evidence of selection on bias indicates that *conditional* on being in the list of potential arbitrators, pro-industry arbitrators are more likely to be selected to arbitration panels. However, there are other ways that an industry bias can be introduced into securities arbitration. For instance, this could be achieved if pro-investor arbitrators exited the list of potential arbitrators more quickly than pro-industry ones.

To investigate whether such an exit pattern exists, I employ a standard technique from survival analysis. In particular, I estimate a Cox proportional hazard model (Cox, 1975) with the following semi-parametric specification for the hazard function:

$$h(t) = h_o(t) \cdot \exp\left(\alpha_s + \beta_0 \cdot Bias_j + \beta_1 \cdot PostNLSS_t \times Bias_j + \beta_2 \cdot Lawyer_j\right), \quad (11)$$

where the baseline hazard function, $h_0(t)$, need not be specified because the model is estimated by conditioning out $h_0(t)$ using the partial likelihood approach.[52] Standard errors are clustered at the state level. This specification allows for a change in the hazard rate's sensitivity to arbitrator bias following the NLSS switch. In modeling arbitrator tenure, there are several advantages to using duration models. For example, these models are sufficiently flexible to account for the fact that many arbitrators are still on the NASD list at the end of the sample period. I assume that arbitrators whose tenure windows end in the last year of the sample have not exited the NASD list. This induces individual-specific censoring in about half of the observations.[53]

The first four columns of Table 18 report results from the sample of public arbitrators. Columns 1 and 3 show that more pro-industry arbitrators have lower instantaneous probabilities of leaving the pool of potential arbitrators under both bias measures (since the coefficients on $ProInd_j$ and $ProInd_j^{FE}$ are negative at the 1- and 5-percent levels). Arbitrators at the 75^{th} percentile of $ProInd_j$ ($ProInd_j^{FE}$) have exit rates that are 8.6 percent (6.4 percent) lower than those at the 25^{th} percentile. Furthermore, unlike the analysis from section 5, this pattern also obtains in the sample of industry arbitrators. Using $ProInd_j^{FE}$, industry arbitrators at the 75^{th} percentile of bias have exit rates that are 9.1 percent lower than those at the 25^{th} percentile. Since the NASD has formal control over the list of potential arbitrators, one might be tempted to view these results as evidence of favoritism within the NASD. However, while the NASD exercises control in admitting new arbitrators, it claims not to forcibly remove someone from the arbitrator pool unless that person is rarely available to sit on cases. Consequently, many tenures may be ending at the discretion of the individual arbitrator. Thus, the relationship between exit rates and bias may also be explained by factors not directly related to NASD behavior. For example, it has been reported that arbitrators occasionally receive benefits from brokerage firms through avenues other than selection to cases (e.g., by serving as expert witnesses for them in other legal disputes). If such benefits are only provided to pro-industry arbitrators and only while they are members of the arbitrator pool, then pro-industry arbitrators will have an incentive to extend their tenures longer than pro-investor arbitrators. Furthermore, to the extent that arbitration is more favorable to industry than investors, pro-investor arbitrators may choose to leave the pool early out of frustration for not being selected (regardless of whether or not the NASD is responsible for this industry favoritism).

Table 18 also attempts to rule out the possibility that the increase in selection on bias after the NLSS switch is due to a drop in external monitoring. Specifically, given increased participation in selection by investors and brokerage firms, the NASD may have found it easier to avoid direct suspicion for bias in arbitrator selection following the rules change. Though it is unclear why such a drop in accountability would impact selection on expertise, it could explain the increase in selection on bias. In particular, because the NASD continued to have limited control over the selection process (discretion in granting challenges for cause), it could have responded to weaker accountability by

[52] For a small $dt > 0$, $h(t)dt$ can be interpreted as the probability of exiting between t and $t + dt$ given survival until t.

[53] Reported results are not sensitive to the particular rule used to determine censoring of individual observations.

becoming more aggressive in using this discretion to induce an industry bias. However, if this were the case, one might also expect an increase in the sensitivity of exit to bias in the arbitrator tenure regressions during the post-NLSS period. As columns 2 and 4 of Table 18 show, there is no evidence that exit on bias increased.[54]

7.5 Other Robustness Checks

I perform additional robustness checks to address some other imperfections in the analysis. Since each robustness check involves repeating all the regressions from section 5 and 6 (either with a different samples or with new independent variables), I only describe the relevant results rather than reporting all coefficients in the tables.

Since the data is generated using a snapshot of arbitration awards over a fixed interval of time, bias may be induced by using measures constructed with incomplete histories on arbitrators who are selected to panels *prior* to the beginning of the sample period. In order to see whether this is the case, I redo the analysis after dropping the arbitrators who are most likely to have been selected to cases prior to the beginning of the sample period: those who are selected to cases within a year (or two years) following the filing of the first case in my sample. All the findings on selection on bias and expertise remain unchanged.

A potentially more serious issue exists with the $CaseExperience_{ij}$ measure. Specifically, by construction, this measure of expertise is likely to be correlated with the number of times an arbitrator has been selected in the *past*. Since the number of past selections can reflect both bias and past availability (which is likely to predict future availability), this can lead to problems in determining whether selection on $CaseExperience_{ij}$ really captures the influence of expertise on selection patterns. To address this, I create an alternative measure of case experience, denoted as $CaseExperience^{\epsilon}_{ij}$, defined as the residual from the regression:

$$CaseExperience_{ij} = \alpha + \beta_1 \cdot PastSelections_{ij} + \beta_2 \cdot Tenure_{ij} + \beta_3 \cdot Bias_j + \epsilon_{ij}. \qquad (12)$$

I then redo the analysis from Tables 5, 6 and 8 with $CaseExperience^{\epsilon}_{ij}$ used in place of $CaseExperience_{ij}$ and $PastSelections_{ij}$ included in all specifications. Again, all the findings from sections 5 and 6 remain qualitatively unchanged. The only notable difference is that coefficients on $CaseExperience^{\epsilon}_{ij}$ are around 30 percent smaller in magnitude than those on $CaseExperience_{ij}$. Interestingly, the difference in allocation of case experience across case complexity becomes even more pronounced with this alternative measure (though, as in Table 6, the difference when using $MargLev_i$ as a measure of

[54] On the other hand, one might argue that the implementation of the NLSS only made it easier for the NASD to avoid suspicion in the selection of arbitrators and not for its list management. In this case, post-NLSS effects would only be expected to show up in selection on bias (as is the case in the data). However, such a differential effect requires monitors to separately keep track of the arbitrator pool and arbitrator selections which is unlikely given that the NASD list is not publicly disclosed.

complexity is still marginally insignificant with a p-value of 0.102). As expected from the discussion above, the coefficient on $PastSelections_{ij}$ is also positive and highly significant.

8 Conclusion

Self-regulatory organizations play an important role in the regulation of many professional service industries, particularly in the implementation of enforcement. In this paper, I attempt to evaluate SRO enforcement in the full-service securities brokerage industry by analyzing whether self-regulation has the benefit of leading to more expertise and/or the cost of leading to more bias in enforcement.

Using data on securities arbitration cases at the NASD, I focus on an important stage of this enforcement process: arbitrator selection. In the first stage of the analysis, I document general patterns in arbitrator selection and provide evidence that arbitrators who are classified as pro-industry or as having more expertise are selected more often to arbitration panels (selection on bias and expertise, respectively). Furthermore, I provide evidence that arbitrator bias is allocated across cases to benefit industry by showing that selection on bias is stronger in more important cases, as proxied by a brokerage firm's financial and reputational stake in a case. The largest brokerage firms also enjoy substantially more bias than other firms. Meanwhile, selection on scarce forms of expertise is stronger when cases are more complex, as measured by the number of different types of allegations made in a case. This is consistent with arbitrator expertise being targeted to cases where it is most likely to lead to an increase in precision of punishments.

In the second and main part of the analysis, I explore the relationship between self-regulation and arbitrator selection. In particular, I ask whether the NASD is responsible for these patterns by studying how selection on bias and expertise change following a rules change that reduced NASD control over arbitrator selection (the NLSS switch). I find that selection on bias increases and, in some cases, is only statistically significant after this rules change. These findings are relatively robust to accounting for time-variation in case characteristics and other time-trends and endogeneity concerns. This suggests that the NASD is not entirely responsible for selection on bias and is even consistent with the view that the NASD exercised its influence to reduce (rather than increase) bias in enforcement. Moreover, I show that selection on expertise decreased following this event which supports the view that SRO control increases expertise in enforcement. Thus, the evidence is most consistent with a view where the NASD makes enforcement more investor-friendly relative to when investors and broker jointly have more control over enforcement. One explanation for this evidence is that brokers have comparative advantages over investors in the enforcement process (e.g., due to repeated participation) and that the NASD serves as an institutional solution to the public goods problem in enforcement among brokerage firms.

Of course, such a positive assessment of the NASD remains speculative. This is primarily due to limitations in test design. Given the data that was publicly available and the rules change that occurred at the NASD, the most informative tests involved the study of arbitrator selection patterns.

While arbitrator selection is considered very important in the enforcement process, it still only represents one stage of this process and I cannot rule out the possibility that the NASD induces bias in other stages. As mentioned in the paper, one stage where this could occur is in arbitrator list management. The analysis of tenure patterns from section 7.1 does not establish NASD motives in the management of the arbitrator list.[55] Even more importantly, this paper cannot address whether other forms of self-regulation are effective, particularly in cases where disclosure and transparency of SRO behavior is more limited, or estimate the real impact of self-regulation on industry and investor behavior. Thus, while the results in this paper should move priors concerning the benefits and costs of SRO control over enforcement (at least in the setting studied), there is substantial scope for further analysis on this topic.

Appendix

A. The Rise of Securities Arbitration

Almost all customer brokerage contracts include predispute arbitration agreements. The Federal Arbitration Act of 1925 provides that such a clause to arbitrate future disputes is "valid, irrevocable and enforceable, save upon such grounds as exist at law or in equity for the revocation of any contract."[56] Yet, despite this broad statutory mandate, the Supreme Court held in *Wilko v. Swan* (1953) that claims arising under the Securities Act of 1933 (SA), which protects investors from fraud in public offerings but not in secondary market transactions, could not be compelled to arbitration via contract.[57]

Specifically, the Court considered the right to recover under the SA to be a "special right", that differed from the common law rights of recovery and precluded predispute arbitration agreements, because of two reasons (Heinemann, 1986). First, section 12(a)(2) of the Act placed the burden on the issuer and intermediary to prove lack of scienter and provided the investor with a wide choice of venues for resolving disputes. Second, section 14 stated that "any condition, stipulation or provision binding any person acquiring any security to waive compliance with any provision" of the Securities Act was unenforceable. In essence, the Court believed that compelling arbitration violated the inalienability of the choice of venues provision and that:

> "[the] effectiveness in application (of the Act's provisions) is lessened in arbitration as compared to judicial proceedings... As [the] award may be made without explanation of [the] reasons and without a complete record of [the] proceedings, the arbitrators conception of the legal meaning of such statutory requirements as 'burden of proof,' 'reasonable care' or 'material fact,' cannot be examined."

It also added that such arbitration agreements should be voided given the investor's bounded rationality when:

> "surrender[ing] one of the advantages the Act... at a time when he is less able to judge the weight of the handicap the Securities Act places upon his adversary."

[55]In unreported analysis, I have tried to look at a specific aspect of list management that is viewed by investor groups as highly pro-industry: the inclusion of industry arbitrators on 3-member panels. Using regression discontinuity techniques that exploit (time-varying) claim thresholds for moving from 1- to 3-member panels, I find that investors actually do *better* in cases with industry arbitrators. However, while the results do not seem to be driven by the endogeneity of claim size, it is difficult to reliably gauge whether they are due to the inclusion of the industry arbitrator or the presence of an additional public arbitrator.

[56]See Chapter 1, Section 2 of the Federal Arbitration Act, Title 9, US Code, Section 1-14 (1925).

[57]See *Wilko v. Swan*, 346 U.S. 427, 438 (1953).

Based on section 27 and 29(a) of the SEA, whose wordings are similar to sections 12(a)(2) and 14 of the SA, lower courts extended this ruling to Exchange Act claims and, as a result, most investor-broker disputes were being resolved in public courts.

However, things changed dramatically following the Supreme Court's 5-4 decision in *Shearson v. McMahon* (1987) which formally established the enforceability of arbitration agreements for Exchange Act claims.[58] The Court found that the foundations of the Wilko ruling either did not hold for SEA claims or were no longer accurate. Regarding choice of venue, it found that:

> "... the antiwaiver provision of [section] 29(a) forbids [the] enforcement of agreements to waive 'compliance' with the provisions of the statute. But [section] 27 does not impose any duty with which persons trading in securities must 'comply.' By its terms, 29(a) only prohibits waiver of the substantive obligations imposed by the Exchange Act. Because 27 does not impose any statutory duties, its waiver does not constitute a waiver of 'compliance with any provision' of the Exchange Act under 29(a)."

Furthermore, on the ineffectiveness of arbitration enforcing investors' statutory rights, it argued that:

> "... the mistrust of arbitration that formed the basis of the *Wilko* opinion... is difficult to square with the assessment of arbitration that has prevailed since that time. This is especially so in light of the intervening changes in the regulatory structure of securities laws. Even if *Wilko*'s assumptions regarding arbitration were valid at the time..., most certainly they do not hold true today for arbitration procedures subject to the SEC's oversight authority."

Following this decision, the Wilko doctrine, as it applied to SA claims, was reversed by the Court in *Rodriguez de Quijas v. Shearson/American Express Inc.* (1989) and, practically overnight, the role of securities arbitration in enforcement of broker misbehavior had grown exponentially.[59]

[58]See *Shearson/American Express Inc. v. McMahon*, 482 U.S. 220 (1987).
[59]See *Rodriguez de Quijas v. Shearson/American Express Inc.*, 490 U.S. 477 (1989).

References

[1] Amemiya, Takeshi, 1974, The Nonlinear Two-Stage Least Squares Estimator, *Journal of Econometrics* **2**, 105-110.

[2] Ayotte, Kenneth, and Hayong Yun, 2005, Matching Bankruptcy Laws to Legal Environments, *Working Paper*, Columbia University.

[3] Bailey, Warren, Alok Kumar, and David Ng, 2005, Home Bias of U.S. Individual Investors: Causes and Consequences, *Working Paper*, Cornell University.

[4] Barber, Brad and Terrance Odean, 2000, Trading is Hazardous to Your Wealth: The Common Stock Investment Performance of Individual Investors, *Journal of Finance* **55**, 773-806.

[5] Becker, Gary S., 1968, Crime and Punishment: An Economic Approach, *Journal of Political Economy* **76**, 169-217.

[6] Becker, Gary S., and George J. Stigler, 1974, Law Enforcement, Malfeasance, and Compensation of Enforcers, *Journal of Legal Studies* **3**, 1-18.

[7] Bergstresser, Daniel B., John Chalmers, and Peter Tufano, 2006, The Costs and Benefits of Brokers in the Mutual Fund Industry, *Working Paper*, Harvard University.

[8] Bernardo, Antonio E., Eric Talley, and Ivo Welch, 2000, A Theory of Legal Presumptions, *Journal of Law, Economics, and Organization* **16**, 1-49.

[9] Bhattacharya, Utpal, and Hazem Daouk, 2002, The World Price of Insider Trading, *Journal of Finance* **57**, 75-108.

[10] Bhattacharya, Utpal, Neal Galpin, and Bruce Haslem, 2006, The Home Court Advantage in International Corporate Litigation, *Working Paper*, Indiana University.

[11] Bloom, David, and Christopher Cavanagh, 1986, An Analysis of the Selection of Arbitrators, *American Economic Review* **76**, 408-422.

[12] Bond, Philip, 2004, Contracting in the Presence of Judicial Agency, *Working Paper*, University of Pennsylvania.

[13] Cameron, A. Colin, and Pravin K. Trivedi, 2005, Microeconometrics: Methods and Applications, *Cambridge University Press*.

[14] Chamberlain, Gary, 1980, Analysis of Covariance with Qualitative Data, *Review of Economic Studies* **47**, 225-238.

[15] Chang, Tom, and Antoinette Schoar, 2006, Judge-Specific Effects in Ch. 11 and Firm Outcomes, *Working Paper*, Massachusetts Institute of Technology.

[16] Christoffersen, Susan, Richard Evans, and David Musto, 2006, The Economics of Mutual Fund Brokerage: Evidence from the Cross Section of Investment Channels, *Working Paper*, University of Pennsylvania.

[17] Christie, William, and Paul Schultz, 1994, Why do NASDAQ Market Makers Avoid Odd-Eighth Quotes?, *Journal of Finance* **49**, 1813-1840.

[18] Cox, David R., 1975, Partial Likelihood, *Biometrika* **62**, 269-276.

[19] DeMarzo, Peter, Michael Fishman, and Kathleen Hagerty, 2005, Self-Regulation and Government Oversight, *Review of Economic Studies* **72**, 687-706.

[20] Dewatripont, Mathias, and Jean Tirole, 1999, Advocates, *Journal of Political Economy* **107**, 1-39.

[21] Diamond, Doug, 1984, Financial Intermediation and Delegated Monitoring, *Review of Financial Studies* **51**, 393-414.

[22] Duflo, Esther, and Emmanuel Saez, 2002, Participation and Investment Decisions in a Retirement Plan: The Influence of Colleagues' Choices, *Journal of Public Economics* **85**, 121-148.

[23] French, Kenneth, and James Poterba, 1991, Investor Diversification and International Equity Markets, *American Economic Review* **81**, 222-226.

[24] Geczy, Christopher and Jinghua Yan, 2006, Who Are the Beneficiaries When Insiders Trade? An Examination of Piggybacking in the Brokerage Industry, *Working Paper*, University of Pennsylvania.

[25] Glaeser, Edward L., Simon Johnson, and Andrei Shleifer, 2001, Coase Versus The Coasians, *Quarterly Journal of Economics* **116**, 853-899.

[26] Goetzmann, William N., and Alok Kumar, 2005, Why Do Individual Investors Hold Under-Diversified Portfolios?, *Working Paper*, Yale University.

[27] Greenstone, Michael, Paul Oyer, and Annette Vissing-Jorgensen, 2006, Manadated Disclosure, Stock Returns, and the 1964 Securities Acts Amendments, *Quarterly Journal of Economics* **121**, 399-460.

[28] Grinblatt, Mark, and Matti Keloharju, 2001, How Distance, Language, and Culture Influence Stockholdings and Trades, *Journal of Finance* **56**, 1053-1073.

[29] Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2006, Trusting the Stock Market, *Working Paper*, University of Chicago.

[30] Hazen, Thomas Lee, 2003, Broker-Dealer Regulation in a Nutshell, *West Group.*

[31] Hausman, Jerry, and Daniel McFadden, 1984, Specification Tests for the Multinomial Logit Model, *Econometrica* **52**, 1219-1240.

[32] Heinemann, David L., 1986, Arbitrability of Claims Arising Under the Securities Exchange Act of 1934, *Duke Law Journal* **1986**, 548-571.

[33] Hochberg, Yael V., Paola Sapienza, and Annette Vissing-Jorgensen, 2006, A Lobbying Approach to Evaluating the Sarbanes-Oxley Act of 2002, *Working Paper*, Northwestern University.

[34] Hong, Harrison, Jeffrey D. Kubik, and Jeremy C. Stein, 2004, Social Interaction and Stock Market Participation, *Journal of Finance* **59**, 137-163.

[35] Investment Company Institute and Securities Industry Association, 2005, Equity Ownership in America.

[36] Ivkovic, Zoran, and Scott Weisbenner, 2005, Local Does as Local Is: Information Content of the Geography of Individual Investors' Common Stock Investments, *Journal of Finance* **60**, 267-306.

[37] Kling, Jeffrey, 2006, Incarceration Length, Employment, and Earnings, *Forthcoming in American Economic Review.*

[38] Landes, William, and Richard Posner, 1975, The Private Enforcement of Law, *Journal of Legal Studies* **4**, 1-46.

[39] La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny, 1997, Legal Determinants of External Finance, *Journal of Finance* **52**, 1131-1150.

[40] La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny, 1998, Law and Finance, *Journal of Political Economy* **106**, 1113-1155.

[41] La Porta, Rafael, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny, 2002, Investor Protection and Corporate Valuation, *Journal of Finance* **57**, 1147-1170.

[42] La Porta, Rafael, Florencio Lopez-de-Silanes, and Andrei Shleifer, 2006, What Works in Securities Laws?, *Journal of Finance* **61**, 1-32.

[43] Levitt, Steven, and Chad Syverson, 2006, Market Distortions When Agents are Better Informed: The Value of Information in Real Estate Transactions, *Working Paper*, University of Chicago.

[44] Lin, Hsiou-wei, and Maureen F. McNichols, 1998, Underwriting Relationships, Analysts' Earnings Forecasts and Investment Recommendations, *Journal of Accounting and Economics* **25**, 101-127.

[45] Ljungqvist, Alexander, Christopher Malloy, and Felicia Marston, 2006, Rewriting History, *Working Paper*, New York University.

[46] McAfee, R. Preston, and John McMillan, 1989, Government Procurement and International Trade, *Journal of International Economics* **26**, 291-318.

[47] McCaffrey, David, and David Hart, 1998, Wall Street Polices Itself: How Securities Firms Manage the Legal Hazards of Competitive Pressures, *Oxford University Press*.

[48] McFadden, Daniel, 1974, Conditional Logit Analysis of Qualitative Choice Behavior, in *Frontiers in Econometrics*, Paul Zarembka, ed., Academic Press.

[49] Michaely, Roni, and Kent Womack, 1999, Conflict of Interest and Credibility of Underwriter Analyst Recommendations, *Review of Financial Studies* **12**, 653-686.

[50] Pirinsky, Christo, and Qinghai Wang, 2006, Does Corporate Headquarters Location Matter for Stock Returns?, *Journal of Finance* **61** 1991-2015.

[51] Priest, George L., and Benjamin Klein, 1984, The Selection of Disputes for Litigation, *Journal of Legal Studies*, **13**, 1-55.

[52] National Association of Securities Dealers, Inc., 1996, Securities Arbitration Reform: Report of the Arbitration Policy Task Force.

[53] National Association of Securities Dealers, Inc., 1998, Code of Arbitration Procedure.

[54] Odean, Terrance, 1999, Do Investors Trade Too Much?, *American Economic Review* **89**, 1279-1298.

[55] Perino, Michael A., 2002, Report to the Securities and Exchange Commission Regarding Arbitrator Conflict Disclosure Requirements in NASD and NYSE Securities Arbitrations.

[56] Pirrong, Stephen, 1995, The Self-Regulation of Commodity Exchanges: The Case of Market Manipulation, *Journal of Law and Economics* **38**, 141-206.

[57] Polinsky, A. Mitchell, 1980, Private versus Public Enforcement of Fines, *Journal of Legal Studies* **9**, 105-127.

[58] Povel, Paul and Rajdeep Singh, 2006, Takeover Contests with Asymmetric Bidders, *Review of Financial Studies* **19**, 1399-1431.

[59] Raheja, Charu G., 2005, Determinants of Board Size and Composition: A Theory of Corporate Boards, *Journal of Financial and Quantitative Analysis* **40**, 283-306.

[60] Solin, Daniel R., 2004, Does Your Broker Owe You Money?, *Silvercloud*.

[61] Stigler, George, 1971, The Theory of Economic Regulation, *Bell Journal of Economics and Management Science* **2**, 3-21.

[62] Tirole, Jean, 1996, A Theory of Collective Reputations with an Application to the Persistence of Corruption and Firm Quality, *Review of Economic Studies* **63**, 1-22.

[63] U.S. Securities and Exchange Commission, 1995, Report of the Committee on Compensation Practices.

[64] Vissing-Jorgensen, Annette, 1999, Limited Stock Market Participation and the Equity Premium Puzzle, *Working Paper*, Northwestern University.

[65] Vissing-Jorgensen, Annette, 2002, Towards an Explanation of Household Portfolio Choice Heterogeneity: Nonfinancial Income and Participation Cost Structures, *Working Paper*, Northwestern University.

[66] Zingales, Luigi, 2004, The Costs and Benefits of Financial Market Regulation, *Working Paper*, University of Chicago.

Table 1: Summary of Case Characteristics

This table reports descriptive statistics of case characteristics in the sample of cases that involve retail investors suing brokerage firms. $Claim_i$ is the monetary value of compensatory damages requested in the case and does not include amounts requested for interest or attorney fees (winsorized at the 95th percentile). $Decision_i$ is the award-to-claim ratio for compensatory damages. $Punitive_i$ is a dummy that equals 1 if punitive damages are requested. The dummy variable $PunitiveAwd_i$ equals 1 if any amount of punitive damages are awarded. $Employee_i$ is a dummy that equals 1 if a registered representative (individual broker) is included as a respondent in the case. The dummy variable $Expungement_i$ equals 1 if case i is erased from the registered representative's public CRD record. $Counterclaim_i$ equals 1 only if a counterclaim by the respondent includes a request for compensatory damages (rather than just attorney fees). The length of a case, $Length_i$, is defined as the period of time (in years) between the case's filing date and the decision date. The allegation dummies displayed in Panel B are described in Section 3.1.

				Distribution		
Case Characteristics:	N	Mean	SD	25^{th}	50^{th}	75^{th}
Panel A: Distribution of Case Characteristics						
$Claim_i$ (dollars)	15,306	239,864	402,840	15,000	73,702	250,000
$Decision_i$	13,913	0.284	0.367	0.000	0.047	0.524
$Settlement_i$	15,975	0.103	0.304			
$Punitive_i$	15,983	0.560	0.496			
$PunitiveAwd_i$	8,962	0.065	0.246			
$Dismissal_i$	15,975	0.368	0.482			
$Representation_i$	15,983	0.693	0.461			
$Employee_i$	15,983	0.746	0.435			
$Expungement_i$	11,924	0.200	0.400			
$Counterclaim_i$	15,983	0.064	0.245			
$ThirdParty_i$	15,983	0.037	0.190			
$Length_i$ (yrs)	15,983	1.304	0.722	0.849	1.159	1.567
Panel B: Distribution of Allegations Made in Disputes						
Direct Actions:						
$Churning_i$	15,983	0.135	0.342			
$Unauthorized_i$	15,983	0.230	0.421			
Indirect Actions:						
$Misrepresentation_i$	15,983	0.444	0.497			
$Omission_i$	15,983	0.350	0.477			
$Suitability_i$	15,983	0.436	0.496			
Other:						
$Instructions_i$	15,983	0.193	0.395			
$Supervision_i$	15,983	0.298	0.457			
$Negligence_i$	15,983	0.468	0.499			
$Fiduciary_i$	15,983	0.460	0.498			

Table 2: Case Outcomes Regression

This table reports coefficient estimates from regressions relating case outcomes to case characteristics. The listed characteristics are as defined in Table 1. The number of observations is lower than in Table 1 because information on hearing location (city) is not available for every case and city×years with less than observations are dropped. The variable $\ln Claim_i$ is winsorized at the 95th percentile. A brokerage firm is included in the Repeat Firms subsample if it is included as a respondent in at least 5 cases in the sample. The Punitive (Employee) subsample consists of all cases with $Punitive_i$ ($Employee_i$) equal to 1. Standard errors are clustered at the brokerage firm level.

Dependent Variable:	$Decision_i$		$Dismissal_i$	$PunitiveAwd_i$	$Expungement_i$
Subsample:	All	Repeat Firms	All	Punitive	Employee
$\ln Claim_i$	**-0.041*****	**-0.040*****	**-0.020*****	0.002	**0.015*****
	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)
$Employee_i$	**0.025*****	-0.007	-0.009	**0.020*****	
	(0.009)	(0.009)	(0.012)	(0.006)	
$Representation_i$	**0.074*****	**0.083*****	**-0.076*****	0.012	-0.014
	(0.009)	(0.009)	(0.011)	(0.010)	(0.009)
$Counterclaim_i$	**-0.021***	-0.012	0.012	-0.011	-0.012
	(0.012)	(0.013)	(0.017)	(0.011)	(0.012)
$ThirdParty_i$	**0.135*****	**0.099*****	**-0.079*****	**0.040****	**-0.050*****
	(0.021)	(0.025)	(0.020)	(0.016)	(0.017)
$Settlement_i$	**0.176*****	**0.136*****	**0.411*****	**-0.053*****	**0.625*****
	(0.028)	(0.035)	(0.020)	(0.006)	(0.016)
Allegation Dummies?	Y	Y	Y	Y	Y
City×Year FE?	Y	Y	Y	Y	Y
Brokerage Firm FE?	N	Y	N	N	N
No. of Firms	1,652	432	1,717	1,265	1,499
R^2	0.107	0.242	0.168	0.126	0.503
N	12,940	11,061	14,459	8,202	10,878

Table 3: Summary of Arbitrator Characteristics

This table reports descriptive statistics of arbitrator characteristics in the sample. The measures for pro-industry bias are: $Decision_j$, $ProInd_j$, and $ProInd_j^{FE}$. $Decision_j$ is arbitrator j's average $Decision_i$. $ProInd_j$ is j's average $ProInd_i \equiv Outcome_i$ - E[$Outcome_i$] where E[$Outcome_i$] is calculated using the estimates of the first column in Table 2 and winsorized at the 5th and 95th percentile. $ProInd_j^{FE}$ is similarly defined using the estimates from the second column of Table 2 to obtain $ProInd_i^{FE}$. All averages are claim-weighted. $Tenure_j$ is equal to the length of time (in years) between the filing date of the arbitrator's first case and the decision date of his last case. The dummy $Lawyer_j$ equals one if an Esq or JD suffix is attached to the arbitrator's name. $Industry_j$ is a dummy equal to one if the arbitrator is ever listed as an industry arbitrator. In Panel C, the dummy variable $RegRep_j$ is equal to 1 if an industry arbitrator can be identified as a registered representative (i.e., has a public CRD record). The dummy variable $Discipline_j$ is equal to one if the registered representative has potential disciplinary events listed in his CRD record. The p-values in Panel D give the significance of tests of the equality of means in the 1^{st} and 4^{th} selection quartiles. This test allows for different variances across groups and uses the Welch approximation for degrees of freedom.

				Distribution		
Arb. Characteristics:	N	Mean	SD	25^{th}	50^{th}	75^{th}
Panel A: All Arbitrators						
$Selections_j$	7,369	4.963	5.350	2	3	6
$Decision_j$	7,369	0.239	0.269	0.019	0.145	0.366
$ProInd_j$	7,369	0.004	0.250	-0.111	0.078	0.168
$ProInd_j^{FE}$	6,983	0.000	0.230	-0.098	0.055	0.139
$Tenure_j$	7,369	5.439	3.917	1.975	4.534	8.140
$Lawyer_j$	7,369	0.446	0.497			
$ChairExp_j$	7,369	0.415	0.493			
$Industry_j$	7,369	0.408	0.492			
Panel B: Public Arbitrators						
$Selections_j$	4,359	5.610	6.068	2	4	7
$Tenure_j$	4,359	5.550	3.897	2.159	4.658	8.148
$Lawyer_j$	4,359	0.607	0.489			
$ChairExp_j$	4,359	0.632	0.482			
Panel C: Industry Arbitrators						
$Selections_j$	3,010	4.027	3.907	1	3	5
$Tenure_j$	3,010	5.279	3.940	1.707	4.199	8.132
$Lawyer_j$	3,010	0.215	0.411			
$ChairExp_j$	3,010	0.100	0.300			
$RegRep_j$	3,010	0.410	0.492			
$Discipline_j$	1,233	0.150	0.357			

	Selection Quartiles				
Panel D: All Arbitrators	1^{st}	2^{nd}	3^{rd}	4^{th}	p-value
$Selections_j$	1.442	3.000	4.848	12.309	
$Decision_j$	0.268	0.249	0.222	0.201	<0.001
$ProInd_j$	-0.009	-0.006	0.012	0.023	<0.001
$ProInd_j^{FE}$	-0.004	-0.018	0.003	0.014	0.007
$Lawyer_j$	0.368	0.446	0.460	0.568	<0.001
$ChairExp_j$	0.202	0.382	0.501	0.709	<0.001

Table 4: Truncated Negative Binomial Regression on Number of Selections

This table reports coefficient estimates from zero-truncated negative binomial regressions relating the number of times an arbitrator is selected to panels to arbitrator characteristics. $\ln N_j$ is the natural log of the number of cases filed in j's home state during his tenure. Home city is defined as the city where j sits on the majority of his cases (only coded if this proportion is over 50%). All other variables are as defined in Table 3. The row $\Delta_{25,75}^{Bias}$ reports the percentage increase in the expected number of selections given a change in the continuous variable $Bias_j \in \{-Decision_j, ProInd_j, ProInd_j^{FE}\}$ from the 25th to 75th percentile holding all other variables at their means. Similarly, the row $\Delta_{0,1}^{Lawyer}$ gives the increase following a change in the dummy variable $Lawyer_j$ from 0 to 1. $\overline{Bias}$ indicates the estimated influence of selection on bias on the pro-industriness of a case (described in p.16 of Section 5.1). Pseudo-R^2 is reported using the relative gain convention (i.e., $1 - \mathcal{L}_{ur}/\mathcal{L}_0$) with the unconditional zero-truncated Poisson regression used as the baseline model. Standard errors are clustered at the state level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Selections_j$					
Subsample:	All				Public	Industry
$Decision_j$	**-0.209*****	**-0.219*****				
	(0.040)	(0.039)				
$ProInd_j$			**0.157*****			
			(0.034)			
$ProIndFE_j$				**0.072***	**0.095***	0.031
				(0.037)	(0.052)	(0.070)
$Lawyer_j$	**0.126*****	**0.133*****	**0.133*****	**0.134*****	**0.191*****	-0.009
	(0.021)	(0.020)	(0.020)	(0.021)	(0.024)	(0.026)
$RegRep_j$	**-0.143*****	**-0.144*****	**-0.144*****	**-0.149*****		**-0.182*****
	(0.033)	(0.034)	(0.034)	(0.033)		(0.031)
$Industry_j$	**-0.191*****	**-0.194*****	**-0.193*****	**-0.186*****		
	(0.037)	(0.038)	(0.038)	(0.038)		
$\ln N_j$	**1.383*****	1	1	1	1	1
	(0.085)	-	-	-	-	-
Home City FE?	Y	Y	Y	Y	Y	Y
Constraint on N_j?	N	Y	Y	Y	Y	Y
$\Delta_{25,75}^{Bias_j}$	7.5	7.8	4.5	1.7	2.2	0.7
$\Delta_{D}^{Lawyer_j}$	13.5	14.2	14.2	14.3	21.1	-0.9
$\overline{Bias}$	3.6	3.8	2.3	1.0	1.3	0.4
Pseudo-R^2	0.449	0.447	0.447	0.437	0.454	0.384
N	7,008	7,008	7,008	6,650	3,965	2,685

Table 5: Determinants of Arbitrator Selection: Is There Selection on Bias and Expertise?

This table reports coefficient estimates from logistic regressions with case fixed-effects that relate the selection of arbitrators in individual cases to arbitrator characteristics. $ProInd_j$, $ProInd_j^{FE}$, and $Lawyer_j$ are as defined in Table 3. $ChairExperience_{ij}$ is a dummy that equals 1 if arbitrator j has had experience as a chairperson prior to case i's filing. $CaseExperience_{ij}$ denotes the fraction of case i's allegations that have also been alleged in at least one of j's previous cases. $Length_j$ is the average length of time (in years) needed to resolve cases that j is selected to. $Tenure_{ij}$ equals the length of time (in years) between the arbitrator's first selection and the filing date of case i. $Panel_{ij}$ is a dummy variable that equals one if the arbitrator is sitting on another case on i's filing date. As in Table 4, $\Delta^z_{25,75}$ reports the percentage increase in the expected number of selections given a change in the variable z from the 25^{th} to the 75^{th} percentile (similar notation in the case of dummy variables). Standard errors are clustered at the home city×year level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Selected_{ij}$					
Subsample:	Public				Industry	
$ProInd_j$	**0.186*****	**0.185*****				
	(0.046)	(0.047)				
$ProInd_j^{FE}$			**0.130*****	**0.137*****	0.046	0.053
			(0.047)	(0.046)	(0.070)	(0.070)
$Lawyer_j$	**0.266*****		**0.264*****		0.028	
	(0.027)		(0.027)		(0.032)	
$ChairExperience_{ij}$		**0.381*****		**0.380*****		**0.347*****
		(0.035)		(0.035)		(0.061)
$CaseExperience_{ij}$	**0.524*****	**0.434*****	**0.514*****	**0.424*****	**0.423*****	**0.408*****
	(0.047)	(0.042)	(0.047)	(0.042)	(0.051)	(0.050)
$Length_j$	**-0.333*****	**-0.322*****	**-0.312*****	**-0.301*****	**-0.192*****	**-0.180*****
	(0.032)	(0.032)	(0.033)	(0.033)	(0.034)	(0.033)
$Tenure_{ij}$	**0.028*****	**0.014*****	**0.026*****	**0.012*****	**0.015****	**0.010***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)
$Panel_{ij}$	**0.242*****	**0.175*****	**0.250*****	**0.184*****	0.070	0.051
	(0.046)	(0.043)	(0.046)	(0.043)	(0.045)	(0.045)
$\Delta^{Bias}_{25,75}$	5.4	5.4	3.1	3.3	1.1	1.3
$\Delta^{Lawyer/ChairExperience}_{0,1}$	30.5	46.4	30.3	46.3	2.9	41.4
$\Delta^{CaseExperience}_{25,75}$	48.1	38.5	47.0	37.4	40.2	38.6
Pseudo-R^2	0.010	0.012	0.010	0.011	0.003	0.005
N_{obs}	1,110,283	1,110,283	1,086,115	1,086,115	401,900	401,900
N_{cases}	9,983	9,983	9,963	9,963	5,376	5,376

Table 6: The Determinants of Arbitrator Selection: How Do Selection on Bias and Expertise Vary Across Cases?

This table reports coefficient estimates from logistic regressions with case fixed-effects that relate the selection of arbitrators in individual cases to arbitrator characteristics across different levels of case importance and complexity. I_i^1 and I_i^2 denote interaction terms (which differ across columns). The interaction terms used are $LargeBrok_i$, $HiClaim_i$, $Supervision_i$, $Employee_i$, $ManyClaims_i$, and $MargLev_i$. $Supervision_i$ and $Employee_i$ are as defined in Table 3. $LargeBrok_i$ is a dummy that equals 1 if a brokerage firm is listed among the Top 10 employers of retail brokers in the SIA Yearbooks in over 80% of the years from 1990-91 to 2004-05 (see Footnote 31 on p.19). The dummy variable $HiClaim_i$ is set to 1 if $Claim_i$ is greater than or equal to its 75^{th} percentile value. $ManyClaims_i$ is a dummy that equals 1 if the number of allegations in a case is greater than 3. $MargLev_i$ is a dummy variable that equals 1 if a case involves transactions that include the use of margin or leverage. All other variables are as defined in Table 5. To conserve space, the coefficients on $Length_j$, $Tenure_{ij}$, and $Panel_{ij}$ are not displayed in the table (they are qualitatively identical to those in Table 5). As in the previous two tables, $\Delta^z_{25,75}$ reports the percentage increase in the expected number of selections given a change in the variable z from the 25^{th} to the 75^{th} percentile (similar notation in the case of dummy variables). These economic effects depend on the interaction term's value. Since all interactions are dummy variables, the economic effects for I_i^1 equal to 0 and 1 are both reported. Standard errors are clustered at the home city×year level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:		$Selected_{ij}$					
Subsample:		Public					
Sorting On:		Case Importance				Case Complexity	
Interactions:	$I_i^1 =$	$LargeBrok_i$	$HiClaim_i$	$Supervision_i$	$Employee_i$	$ManyClaims_i$	$MargLev_i$
	$I_i^2 =$	-	-	-	$LargeBrok_i$	-	-
$ProInd_j^{FE}$		0.054	0.077	**0.108****	-0.142	0.087	**0.097****
		(0.055)	(0.053)	(0.053)	(0.112)	(0.072)	(0.049)
$I_i^1 \times ProInd_j^{FE}$		**0.251****	**0.202***	0.099	**0.251****	0.083	**0.279****
		(0.100)	(0.103)	(0.099)	(0.124)	(0.088)	(0.128)
$I_i^2 \times ProInd_j^{FE}$					**0.535*****		
					(0.203)		
$I_i^1 \times I_i^2 \times ProInd_j^{FE}$					**-0.365***		
					(0.217)		
$ChairExperience_{ij}$		**0.425*****	**0.436*****	**0.461*****	**0.380*****	**0.562*****	**0.377*****
		(0.038)	(0.037)	(0.040)	(0.035)	(0.049)	(0.036)
$I_i^1 \times ChairExperience_j$		**-0.137*****	**-0.221*****	**-0.271*****		**-0.309*****	0.019
		(0.040)	(0.046)	(0.043)		(0.044)	(0.051)
$CaseExperience_{ij}$		**0.421*****	**0.440*****	**0.413*****	**0.424*****	**0.342*****	**0.407*****
		(0.051)	(0.049)	(0.045)	(0.042)	(0.051)	(0.043)
$I_i^1 \times CaseExperience_{ij}$		0.007	-0.004	0.051		**0.184*****	0.119
		(0.072)	(0.079)	(0.071)		(0.059)	(0.089)
If $I_i^1 = 0$:							
$\Delta^{Bias}_{25,75}$		1.3	1.8	2.6	-	2.1	2.3
$\Delta^{ChairExperience}_{0,1}$		53.0	54.7	58.6	-	75.3	45.8
$\Delta^{CaseExperience}_{25,75}$		37.1	39.0	36.3	-	29.3	35.7
If $I_i^1 = 1$:							
$\Delta^{Bias}_{25,75}$		7.4	6.8	5.0	-	4.1	9.2
$\Delta^{ChairExperience}_{0,1}$		33.5	24.0	21.0	-	28.8	48.7
$\Delta^{CaseExperience}_{25,75}$		37.8	38.6	41.7	-	48.4	48.3
Pseudo-R^2		0.012	0.012	0.012	0.011	0.012	0.011
N_{obs}		1,086,115	1,039,764	1,086,115	1,086,115	1,086,115	1,086,115
N_{cases}		9,963	9,963	9,963	9,963	9,963	9,963

Table 7: Case Outcomes Regression: Before and After the NLSS Rules Change

This table reports coefficient estimates from regressions relating case outcomes ($Decision_i$) to case characteristics. The listed characteristics are as defined in Table 1. The number of observations is lower than in Table 1 because information on hearing location is not available for every case. The variable $\ln Claim_i$ is winsorized at the 95th percentile. $PostNLSS_i$ is a dummy that equal to 1 after the change in selection procedures. A brokerage firm is included in the Repeat Firms subsample if it is included as a respondent in at least 5 cases in the sample.

Dependent Variable:	$Decision_i$			
Subsample:	All	Repeat Firms	All	
$PostNLSS_i$	**-0.058*****	**-0.017****		**-0.051****
	(0.013)	(0.008)		(0.021)
$Trend_i$			**0.006****	**0.010*****
			(0.003)	(0.003)
$Trend_i \times PostNLSS_i$			**-0.030*****	**-0.026*****
			(0.006)	(0.006)
$\ln Claim_i$	**-0.040*****	**-0.040*****	**-0.041*****	**-0.040*****
	(0.003)	(0.004)	(0.003)	(0.003)
$Employee_i$	**0.026*****	-0.008	**0.025*****	**0.024*****
	(0.009)	(0.008)	(0.010)	(0.009)
$Representation_i$	**0.076*****	**0.086*****	**0.076*****	**0.076*****
	(0.009)	(0.010)	(0.008)	(0.008)
$Counterclaim_i$	**-0.022***	-0.015	**-0.026****	**-0.025****
	(0.012)	(0.012)	(0.012)	(0.012)
$ThirdParty_i$	**0.136*****	**0.101*****	**0.136*****	**0.136*****
	(0.021)	(0.023)	(0.021)	(0.021)
$Settlement_i$	**0.178*****	**0.144*****	**0.178*****	**0.180*****
	(0.029)	(0.035)	(0.029)	(0.029)
Claim Type Dummies?	Y	Y	Y	Y
City FE?	Y	Y	Y	Y
Brokerage Firm FE?	N	Y	N	N
No. of Firms	1,652	432	1,652	1,652
R^2	0.066	0.208	0.069	0.070
N	12,940	11,061	12,940	12,940

Table 8: Arbitrator Selection Patterns Before and After the NLSS Rules Change: How Do Selection on Bias and Expertise Change?

This table reports coefficient estimates from logistic regressions with case fixed-effects that relate the selection of arbitrators in individual cases to arbitrator characteristics before and after the NLSS rules change. $PostNLSS_i$ is a dummy that equal to 1 after the change in selection procedures. All other variables are as defined in Table 5. To conserve space, the coefficients on $Length_j$, $Tenure_{ij}$, and $Panel_{ij}$ are not displayed in the table (they are qualitatively identical to those in Table 5). As in the previous three tables, $\Delta^{z}_{25,75}$ reports the percentage increase in the expected number of selections given a change in the variable z from the 25^{th} to the 75^{th} percentile (similar notation in the case of dummy variables). These economic effects are different in the pre- and post-NLSS periods and are reported separately. Standard errors are clustered at the home city×year level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Selected_{ij}$			
Subsample:	Public			
$ProInd_j$	**0.117***	**0.119***		
	(0.063)	(0.064)		
$PostNLSS_i \times ProInd_j$	**0.182***	**0.174***		
	(0.093)	(0.095)		
$ProInd_j^{FE}$			0.057	0.098
			(0.061)	(0.060)
$PostNLSS_i \times ProInd_j^{FE}$			**0.193****	0.145
			(0.094)	(0.094)
$Lawyer_j$	**0.369*****		**0.366*****	
	(0.036)		(0.036)	
$PostNLSS_i \times Lawyer_j$	**-0.237*****		**-0.232*****	
	(0.048)		(0.048)	
$ChairExperience_j$		**0.592*****		**0.591*****
		(0.033)		(0.033)
$PostNLSS_i \times ChairExperience_j$		**-0.563*****		**-0.564*****
		(0.048)		(0.047)
$CaseExperience_j$	**0.798*****	**0.607*****	**0.782*****	**0.592*****
	(0.058)	(0.053)	(0.058)	(0.053)
$PostNLSS_i \times CaseExperience_j$	**-0.789*****	**-0.533*****	**-0.774*****	**-0.518*****
	(0.084)	(0.075)	(0.084)	(0.075)
Pre-NLSS:				
$\Delta^{Bias}_{25,75}$	3.3	3.4	1.3	2.3
$\Delta^{Lawyer/ChairExperience}_{0,1}$	44.7	80.7	44.1	80.5
$\Delta^{CaseExperience}_{25,75}$	81.9	57.6	79.8	55.9
Post-NLSS:				
$\Delta^{Bias}_{25,75}$	8.8	8.6	6.0	5.9
$\Delta^{Lawyer/ChairExperience}_{0,1}$	14.2	3.0	14.3	2.7
$\Delta^{CaseExperience}_{25,75}$	0.7	5.6	0.6	5.7
Pseudo-R^2	0.012	0.015	0.012	0.015
N_{obs}	1,110,283	1,110,283	1,086,115	1,086,115
N_{cases}	9,983	9,983	9,983	9,983

Table 9: Determinants of Professional Representation: Do Investors Rely More on Lawyers After the NLSS Rules Change?

This table reports coefficient estimates from logistic regressions relating the use of professional representation by investors to case characteristics before and after the NLSS change. $Trend_i$ is a variable that measures the difference between the filing date of case i and the NLSS implementation date. It is negative for cases filed prior to the rules change and positive for those filed after the rules change. $ThreeMember_i$ is a dummy variable that equals one if an arbitration panel is composed of three arbitrators. All other variables are as defined in previous tables. Time fixed-effects are excluded because of collinearity with $PostNLSS_i$ and $Trend_i$. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Representation_i$					
Subsample:	All		3-Member		1-Member	
$PostNLSS_i$	**0.253*****		0.051		**0.385*****	
	0.052		0.069		0.083	
$Trend_i$		0.009		0.018		0.008
		0.011		0.015		0.018
$Trend_i \times PostNLSS_i$		**0.089*****		0.004		**0.119*****
		0.025		0.035		0.041
$\ln Claim_i$	**0.488*****	**0.476*****	**0.418*****	**0.407*****	**0.681*****	**0.678*****
	0.023	0.023	0.027	0.027	0.046	0.045
$Employee_i$	**0.221*****	**0.226*****	**0.214*****	**0.216*****	**0.165****	**0.172****
	0.053	0.053	0.074	0.074	0.081	0.081
$ThreeMember_i$	**0.878*****	**0.915*****				
	0.070	0.070				
Allegation Dummies?	Y	Y	Y	Y	Y	Y
City FE?	Y	Y	Y	Y	Y	Y
Pseudo-R^2	0.331	0.333	0.121	0.122	0.220	0.223
N	14,767	14,767	10,288	10,288	4,462	4,462

Table 10: Summary of Case Characteristics: Means Before and After the NLSS Rules Change

This table reports descriptive statistics of case characteristics before and after the NLSS rules change. For each subperiod, averages are computed for cases with 3-member arbitration panels. For $Claim_i$ the subsample is further restricted to cases with claim sizes exceeding the current threshold for moving from 1- to 3-member panels (50,000 dollars). The listed case characteristics are as defined in Table 1.

	Subperiod	
Case Characteristics:	Pre-NLSS	Post-NLSS
$Claim_i$ (dollars)	356,121	496,212
$LargeBrok_i$	0.339	0.454
$Employee_i$	0.709	0.701
$Churning_i$	0.168	0.150
$Unauthorized_i$	0.227	0.184
$Misrepresentation_i$	0.486	0.439
$Omission_i$	0.363	0.346
$Suitability_i$	0.437	0.449
$Mismanagement_i$	0.106	0.063
$Instructions_i$	0.188	0.121
$Supervision_i$	0.233	0.423
$Negligence_i$	0.407	0.617
$Fiduciary_i$	0.400	0.638

Table 11: Selection on Bias Across Cases Before and After the NLSS Rules Change: Does Time-Variation in Case Characteristics Explain the Increase in Selection on Bias?

This table reports coefficient estimates from logistic regressions with case fixed-effects that relate the selection of arbitrators in individual cases to arbitrator characteristics before and after the NLSS rules change across different case importance levels. $PreNLSS_i$ is a dummy variable that equals 1 before the NLSS switch and $PostNLSS_i$ is as defined in Table 7. $HiClaim_i$, $LargeBrok_i$, and $Supervision_i$ are as defined in Table 6. The dummy variables $LoClaim_i$, $SmallBrok_i$, and $NoSuper_i$ equal 1 if $HiClaim_i$, $LargeBrok_i$, and $Supervision_i$ are zero, respectively. In Model 1a (1b), the specification is identical to the third (fourth) column of Table 8 with the exception that the selection on bias coefficient is estimated for four groups: $PreNLSS_i \times LoClaim_i$, $PreNLSS_i \times HiClaim_i$, $PostNLSS_i \times LoClaim_i$, and $PostNLSS_i \times HiClaim_i$ (all interacted with $ProInd_j^{FE}$). Models 2a (2b) and 3a (3b) are identical to Model 1a (1b) except that they sort on case importance using broker size ($LargeBrok_i$) and the failure to supervise employees ($Supervision_i$), respectively. Only the four coefficients on selection on bias are reported from these regressions. Standard errors are clustered at the home city×year level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

	Model 1a		Model 2a		Model 3a	
Group:	$LoClaim_i$	$HiClaim_i$	$SmallBrok_i$	$LargeBrok_i$	$NoSuper_i$	$Supervision_i$
$PreNLSS_i$	0.010	**0.398*****	0.063	**0.318*****	0.103	0.178
	(0.074)	(0.108)	(0.068)	(0.112)	(0.070)	(0.120)
$PostNLSS_i$	0.009	**0.704*****	**0.185****	**0.409*****	**0.192****	**0.385*****
	(0.094)	(0.109)	(0.084)	(0.109)	(0.086)	(0.109)
$\Delta_{25,75}^{bias,pre}$	0.3	11.9	1.5	7.8	2.4	4.3
$\Delta_{25,75}^{bias,post}$	0.3	22.0	4.4	10.1	4.6	9.5
p-value	0.991	0.047	0.267	0.565	0.428	0.208
	Model 1b		Model 2b		Model 3b	
Group:	$LoClaim_i$	$HiClaim_i$	$SmallBrok_i$	$LargeBrok_i$	$NoSuper_i$	$Supervision_i$
$PreNLSS_i$	0.028	**0.182***	0.049	0.153	0.056	0.128
	(0.069)	(0.107)	(0.067)	(0.111)	(0.067)	(0.118)
$PostNLSS_i$	0.092	**0.440*****	0.123	**0.384*****	**0.199****	**0.269****
	(0.092)	(0.126)	(0.089)	(0.116)	(0.086)	(0.119)
$\Delta_{25,75}^{bias,pre}$	0.7	4.4	1.2	3.7	1.3	3.1
$\Delta_{25,75}^{bias,post}$	2.2	10.9	2.9	9.4	4.8	6.5
p-value	0.585	0.119	0.509	0.152	0.188	0.403

Table 12: Regression of Panel Bias on Pro-Industriness of Case Outcomes

This table reports coefficient estimates from least squares regressions relating the pro-industriness of case outcomes to measures of panel bias. For case i, panel bias measures, $ProIndPan_i$ and $ProIndPan_i^{FE}$, are obtained by computing arbitrator bias measures as in Table 3 but with case i omitted and then taking the average over arbitrators on the panel with $Selections_j$ greater than or equal to 2. Similarly, $ProIndPub_i$ ($ProIndPub_i^{FE}$) and $ProIndInd_i$ ($ProIndInd_i^{FE}$) take corresponding averages over the public and industry arbitrators only. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$ProInd_i$			$ProInd_i^{FE}$		
Subsample:	All Cases			All Cases		
$ProIndPan_i$	**0.136*****	**0.176*****				
	(0.022)	(0.030)				
$PostNLSS_i \times ProIndPan_i$		**-0.091***				
		(0.051)				
$ProIndPub_i$			**0.142*****			
			(0.024)			
$ProIndInd_i$			0.032			
			(0.021)			
$ProIndPan_i^{FE}$				**0.127*****	**0.144*****	
				(0.023)	(0.034)	
$PostNLSS_i \times ProIndPan_i^{FE}$					-0.037	
					(0.046)	
$ProIndPub_i^{FE}$						**0.103*****
						(0.023)
$ProIndInd_i^{FE}$						0.037
						(0.023)
R^2	0.004	0.004	0.005	0.003	0.003	0.003
N	12,401	12,401	7,462	10,588	10,588	6,201

Table 13: Regression of Panel Bias on Pro-Industriness of Case Outcomes for Repeated Cases

This table reports coefficient estimates from least squares regressions relating the pro-industriness of case outcomes to measures of panel bias as in Table 12 but only for the subsample of Grubman cases and repeated cases. A case is classified as a Grubman case if it involves an investor suing Citigroup and the analyst Jack Grubman for misrepresentations in Worldcom analyst reports. The repeated case subsample is as described on page 49. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$ProInd_i$	$ProInd_i^{FE}$	$ProInd_i$	$ProInd_i^{FE}$
Subsample:	Grubman Cases		All Repeated	
$ProIndPan_i$	**0.325****		**0.339****	
	(0.161)		(0.148)	
$ProIndPan_i^{FE}$		**0.313***		**0.332***
		(0.186)		(0.172)
R^2	0.031	0.022	0.150	0.102
N	131	128	186	183

Table 14: Regression of Pro-Industriness of Case Outcomes on Panel Bias and Bias of Dropped Arbitrators

This table reports coefficient estimates from least squares regressions relating the pro-industriness of case outcomes to measures of panel bias and the bias of a dropped arbitrator. For case i, panel bias measures, $ProIndPan_i$ and $ProIndPan_i^{FE}$, are obtained as in Table 12. $ProIndDrop_i$ and $ProIndDrop_i^{FE}$ are the bias measures for the dropped arbitrator. Regressions are run on the subsample of cases where an arbitrator drops from the original panel due to illness, accident or death and is identified by name in the award document. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$ProInd_i$	$ProInd_i^{FE}$
Subsample:	Dropped Arbitrator	Dropped Arbitrator
$ProIndPan_i$	0.313	
	(0.295)	
$ProIndDrop_i$	-0.140	
	(0.226)	
$ProIndPan_i^{FE}$		0.139
		(0.462)
$ProIndDrop_i^{FE}$		-0.124
		(0.186)
R^2	0.037	0.013
N	46	40

Table 15: Do the Bias Measures Predict Dissent Probabilities?

This table reports coefficient estimates from logistic regressions relating dissent to arbitration panel characteristics. The variable $DispProInd_i$ measures the difference in the bias of arbitrators within the panel and is defined as the difference between the lowest and highest $ProInd_j$ of arbitrators on the panel (with $Selections_j$ greater than or equal to 2). $DispProInd_i^{FE}$ is similarly defined. The variables $DispProIndLarge_i$ and $DispProIndLarge_i^{FE}$ are dummy variables equal to 1 if the values of $DispProInd_i$ and $DispProInd_i^{FE}$ are above the 90^{th} percentiles of their respective distributions. $ProInd_i^2$ and $(ProInd_i^{FE})^2$ measure how different case i's outcome is from the typical outcome of observationally similar cases. $AvgLawyer_i$ ($AvgChairExperience_i$) is equal to the fraction of arbitrators on case i's panel that are lawyers (have past experience as a chairperson). $AvgCaseExperience_i$ is defined similarly. All other variables are as defined in Table 1. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Dissent_i$			
Subsample:	All Cases			
$DispProInd_i$	**1.368*****			
	(0.462)			
$DispProIndLarge_i$		**0.583*****		
		(0.224)		
$DispProInd_i^{FE}$			**1.343****	
			(0.571)	
$DispProIndLarge_i^{FE}$				**0.677*****
				(0.229)
$ProInd_i^2$	**-0.854***	-0.682		
	(0.516)	(0.513)		
$(ProInd_i^{FE})^2$			-0.001	0.100
			(0.528)	(0.498)
$AvgLawyer_i$	**0.639****	**0.635****	**0.623****	**0.620****
	(0.279)	(0.275)	(0.282)	(0.280)
$AvgChairExperience_i$	0.005	0.004	-0.062	-0.073
	(0.280)	(0.280)	(0.296)	(0.291)
$AvgCaseExperience_i$	-0.161	-0.154	-0.079	-0.080
	(0.248)	(0.248)	(0.260)	(0.258)
$\ln Claim_i$	**0.200*****	**0.171*****	**0.203*****	**0.177*****
	(0.063)	(0.058)	(0.066)	(0.062)
$Employee_i$	**-0.376****	**-0.372****	**-0.420*****	**-0.418*****
	(0.152)	(0.151)	(0.157)	(0.156)
$Counterclaim_i$	-0.257	-0.251	-0.243	-0.240
	(0.263)	(0.263)	(0.283)	(0.280)
$ThirdParty_i$	0.209	0.212	0.354	0.337
	(0.331)	(0.331)	(0.340)	(0.335)
Allegation Dummies?	Y	Y	Y	Y
City and Year Dummies?	Y	Y	Y	Y
Pseudo-R^2	0.068	0.066	0.070	0.070
N	7,744	7,744	6,588	6,588

Table 16: Do the Expertise Measures Predict Selection as a Chairperson?

This table reports coefficient estimates from logistic regressions with case fixed-effects that relate selection as a chairperson in individual cases to arbitrator characteristics. The dependent variable, $Chairperson_{ij}$, equals one if arbitrator j is selected as the chairperson to case i. All other variables are as defined in previous tables. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Chairperson_{ij}$					
Subsample:	All Selected Arbitrators					
$Lawyer_j$	**1.827*****			**1.831*****		
	(0.073)			(0.072)		
$ChairExperience_{ij}$		**1.854*****			**1.857*****	
		(0.071)			(0.073)	
$CaseExperience_{ij}$			**0.570*****			**0.579*****
			(0.085)			(0.086)
$ProInd_j$	-0.026	-0.205	-0.077			
	(0.170)	(0.176)	(0.159)			
$ProInd_j^{FE}$				0.018	0.024	0.008
				(0.185)	(0.178)	(0.174)
$Tenure_{ij}$	**0.105*****	**0.026****	**0.077*****	**0.106*****	**0.025****	**0.076*****
	(0.013)	(0.012)	(0.013)	(0.014)	(0.012)	(0.013)
$Industry_j$	**-2.053*****	**-1.796*****	**-2.541*****	**-2.050*****	**-1.797*****	**-2.541*****
	(0.067)	(0.069)	(0.069)	(0.068)	(0.069)	(0.070)
$\Delta_{0,1}^{expert}$	521.3	538.6	57.7	524.1	540.5	58.9
Pseudo-R^2	0.441	0.452	0.316	0.442	0.452	0.316
N_{obs}	11,108	11,108	11,108	11,010	11,010	11,010
N_{cases}	4,424	4,424	4,424	4,394	4,394	4,394

Table 17: Do the Bias Measures Predict Settlement Probabilities?

This table reports coefficient estimates from logistic regressions relating observed settlement to arbitration panel characteristics. The panel bias measures, $AvgProInd_i$ and $AvgProInd_i^{FE}$, are as defined in Table 11. $AvgLawyer_i$ and $AvgChairExperience_{ij}$ are as defined in Table 13. All other variables are as defined in Table 1. Standard errors are clustered at the brokerage firm level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Settlement_i$			
Subsample:	All	3-Member	All	3-Member
$ProIndPan_i$	0.136	0.085		
	(0.210)	(0.343)		
$ProIndPan_i^{FE}$			0.047	-0.122
			(0.216)	(0.317)
$AvgLawyer_i$	-0.010	0.002	-0.012	0.003
	(0.139)	(0.162)	(0.139)	(0.161)
$AvgChairExperience_{ij}$	**-0.331*****	**-0.226****	**-0.328*****	**-0.225****
	(0.108)	(0.108)	(0.108)	(0.108)
$AvgCaseExperience_{ij}$	**-0.640*****	**-0.785*****	**-0.637*****	**-0.786*****
	(0.116)	(0.119)	(0.117)	(0.119)
$\ln Claim_i$	**0.179*****	-0.016	**0.181*****	-0.016
	(0.037)	(0.036)	(0.037)	(0.037)
$Employee_i$	**1.490*****	**1.507*****	**1.500*****	**1.508*****
	(0.234)	(0.222)	(0.233)	(0.220)
$Counterclaim_i$	0.088	0.131	0.087	0.128
	(0.167)	(0.175)	(0.168)	(0.175)
$ThirdParty_i$	-0.062	0.012	-0.063	0.012
	(0.231)	(0.244)	(0.231)	(0.244)
Allegation Dummies?	Y	Y	Y	Y
City and Year Dummies?	Y	Y	Y	Y
Pseudo-R^2	0.287	0.261	0.287	0.261
N	14,132	9,688	14,106	9,681

Table 18: The Determinants of Arbitrator Tenure

This table reports coefficient estimates from Cox proportional hazard regressions relating arbitrator tenure to other arbitrator characteristics. Arbitrators whose tenure windows end in the last year of the sample are classified as having right-censored tenures. The dependent variable, $Tenure_j$, is as defined in Table 3. $ProInd_j$, $ProInd_j^{FE}$, $Lawyer_j$ and $RegRep_j$ are also as defined in Table 3. $PostNLSS_i$ is as defined in Table 7. Standard errors are clustered at the state level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

Dependent Variable:	$Tenure_j$					
Subsample:	Public				Industry	
$ProInd_j$	**-0.318*****	**-0.327*****				
	(0.076)	(0.085)				
$PostNLSS_i \times ProInd_j$		0.086				
		(0.429)				
$ProInd_j^{FE}$			**-0.265****	**-0.284****	**-0.388*****	**-0.372*****
			(0.123)	(0.130)	(0.114)	(0.132)
$PostNLSS_i \times ProInd_j^{FE}$				0.163		-0.142
				(0.427)		(0.594)
$Lawyer_j$	**-0.342*****	**-0.342*****	**-0.338*****	**-0.338*****	**-0.176***	**-0.176***
	(0.083)	(0.083)	(0.079)	(0.079)	(0.071)	(0.071)
$RegRep_j$					**-0.254*****	**-0.254*****
					(0.056)	(0.056)
Home State FE?	Y	Y	Y	Y	Y	Y
Censoring?	Y	Y	Y	Y	Y	Y
Censored Obs	2,160	2,160	2,107	2,107	1,321	1,321
Log-likelihood	-17,009.97	-17,009.92	-16,252.04	-16,251.93	-11,205.36	-11,205.29
N	4,366	4,366	4,223	4,223	2,856	2,856

Table 19: Selection on Bias Before and After the NLSS Rules Change: Alternative Bias Measures

This table reproduces the conditional logit regressions from Table 8 using alternative measures for arbitrator bias. The equal- and $\ln Claim_i$-weighted bias measures are constructed using the same residuals as the earlier bias measures but with different weights as implied by their names. The semi-parametric bias measure is obtained using claim-size weights and residuals from a semi-parametric regression with respect to claim-size. The tobit bias measure is obtained using residuals from a tobit regression with the dependent variable censored below 0 and above 1. In panel A, the expertise measure used in the specifications is $Lawyer_j$. In panel B, the expertise measure used in the specifications is $ChairExperience_{ij}$. Only the coefficients needed to calculate selection on bias are reported. Standard errors are clustered at the home city×year level. Significance at the 10-percent, 5-percent, and 1-percent level are denoted by *, **, and ***, respectively.

	Panel A: $Lawyer_j$			
	$ProInd_j$		$ProInd_j^{FE}$	
Independent Variable:	$ProInd_j$	$PostNLSS_i \times ProInd_j$	$ProInd_j^{FE}$	$PostNLSS_i \times ProInd_j^{FE}$
Equal-weigthed	-0.032	**0.282*****	-0.022	**0.237*****
	(0.060)	(0.091)	(0.060)	(0.089)
$\ln Claim_i$-weighted	-0.019	**0.283*****	-0.016	**0.244*****
	(0.060)	(0.093)	(0.061)	(0.092)
Semi-parametric	0.076	**0.191****	0.035	**0.192****
	(0.063)	(0.093)	(0.061)	(0.094)
Tobit	0.026	**0.119*****	0.024	**0.105*****
	(0.027)	(0.039)	(0.028)	(0.039)

	Panel B: $ChairExperience_{ij}$			
	$ProInd_j$		$ProInd_j^{FE}$	
Independent Variable:	$ProInd_j$	$PostNLSS_i \times ProInd_j$	$ProInd_j^{FE}$	$PostNLSS_i \times ProInd_j^{FE}$
Equal-weigthed	-0.004	**0.243*****	0.017	**0.187****
	(0.060)	(0.092)	(0.058)	(0.088)
$\ln Claim_i$-weighted	0.006	**0.247*****	-0.023	**0.242*****
	(0.060)	(0.094)	(0.060)	(0.092)
Semi-parametric	0.083	**0.178***	0.079	0.141
	(0.064)	(0.095)	(0.060)	(0.094)
Tobit	0.030	**0.110*****	0.037	**0.085****
	(0.027)	(0.039)	(0.028)	(0.038)

Figure 1: Percentage of Investors with Professional Representation

This figure plots the fraction of cases with three- and one-member panels where the investor hires professional representation over various years (using filing dates from 1991 to 2003). The solid line represents three-member panels and the dashed line represents one-member panels.

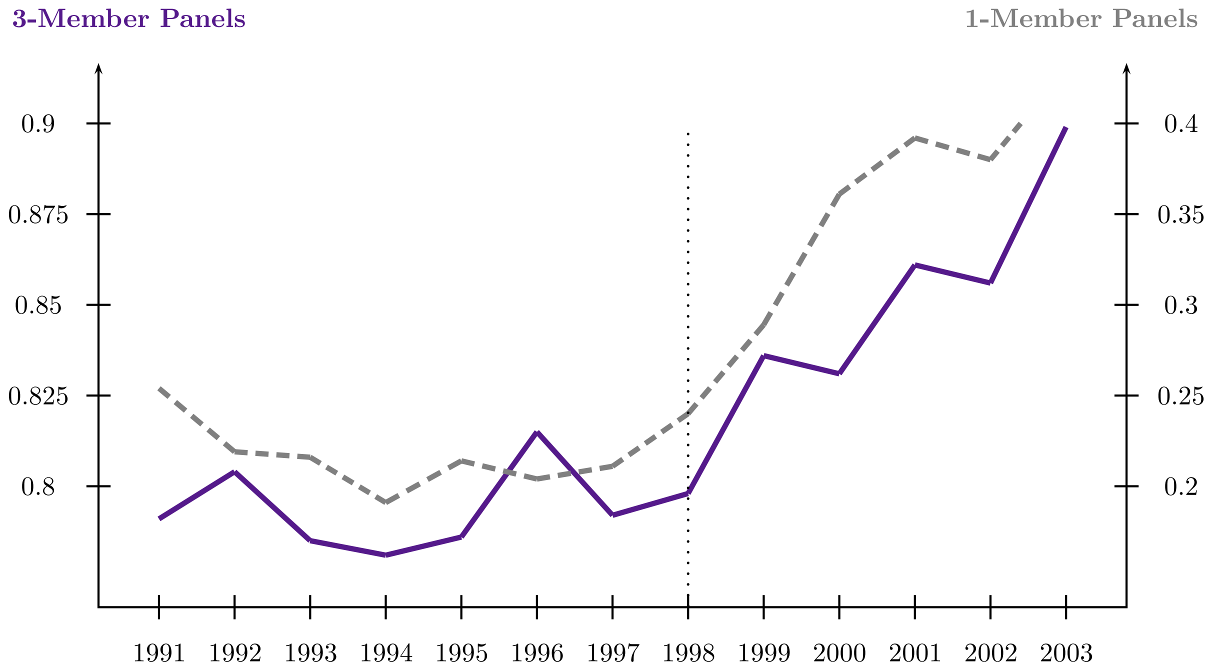

Figure 2: Mean and Median Claim Sizes

This figure plots the mean and median claim size of cases over various years (using filing dates from 1991 to 2003). The solid line represents mean claim sizes and the dashed line represents medians.

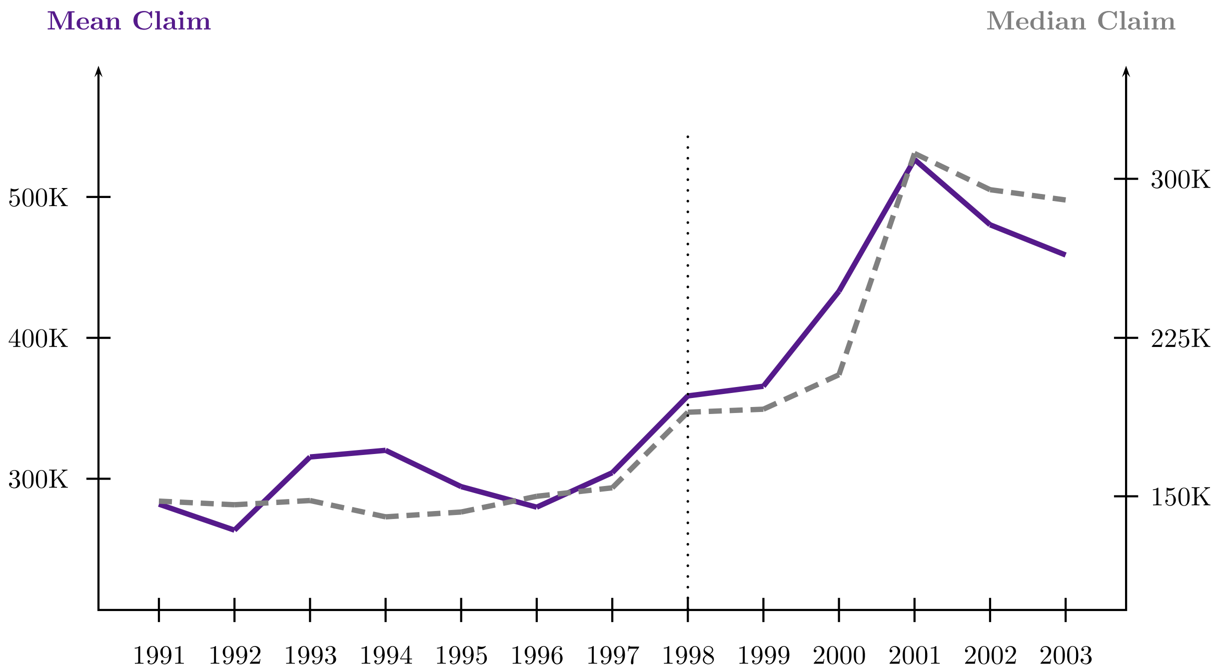

Figure 3: Coefficients on Bias Measures Before and After the NLSS Switch

This figure plots the coefficients on bias, $\beta_{t,t+1}$, from the fixed-effects logistic regressions as in Table 5 but only using selections data from year t to $t+1$ (relative to the NLSS switch date).

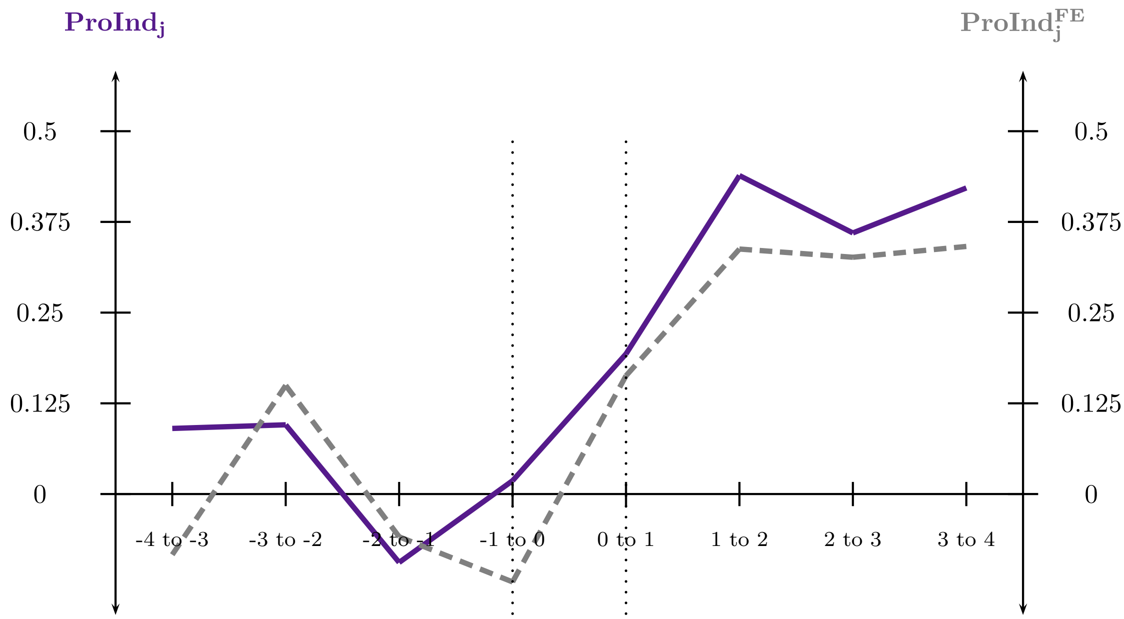

Figure 4: Coefficients on Expertise Measures Before and After the NLSS Switch

This figure plots the coefficients on expertise, $\beta_{t,t+1}$, from the fixed-effects logistic regressions as in Table 5 but only using selections data from year t to $t+1$ (relative to the NLSS switch date).