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Abstract

This paper studies the impact of the arbitrator selection process on consumer outcomes by examining roughly 9,000 consumer arbitration cases in the securities industry. Securities disputes present a good laboratory: arbitration is mandatory for all disputes, eliminating selection concerns; the parties choose arbitrators from a randomly generated list; and the selection mechanism is similar to other major arbitration forums. We establish several facts that suggest that firms hold an informational advantage over consumers in selecting arbitrators, resulting in industry-friendly arbitration outcomes. We then develop and calibrate a quantitative model of arbitrator selection in which firms hold an informational advantage in selecting arbitrators. Arbitrators, who are compensated only if chosen, compete with each other to be selected. The model allows us to decompose the firms' advantage into two components: the advantage of choosing pro-industry arbitrators from a given pool, and the equilibrium pro-industry tilt in the arbitration pool that arises because of arbitrator competition. Selecting arbitrators without the input of firms and consumers would increase consumer awards by $40,000 on average, relative to the current system. Forty percent of this effect arises because the pool of arbitrators skews pro-industry due to competition. Even an informed consumer cannot avoid this equilibrium effect. Counterfactuals suggest that redesigning the arbitrator selection mechanism for the benefit of consumers hinges on whether consumers are informed. Policies such as increasing arbitrator compensation or giving parties more choice, benefit informed consumers but hurt the uninformed.

Keywords: Arbitration, Financial Advisers, Brokers, Consumer Finance, Financial Misconduct, and Fraud

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I Introduction

Arbitration is a private dispute resolution mechanism. The parties present their case to an arbitrator who then issues a legally binding dispute resolution. When consumers purchase a product or service, the purchase often contains a pre-dispute arbitration provision. This provision prohibits the consumer from suing the seller in court and mandates resolving any dispute using arbitration. Such clauses have become increasingly common in the United States and are currently used by all brokerage firms; the largest insurance companies (AIG, Aetna, Inc., Blue Cross and Blue Shield, Travelers); the largest financial firms (American Express, Bank of America, Chase Bank, Citigroup); and the largest Fintech firms (PayPal, Venmo, Square). Arbitration clauses are also pervasive among non financial firms such as online retailers (Amazon, Ebay, Walmart.com); music service providers (Apple, Spotify); wireless providers (Verizon, AT&T, T-Mobile, Sprint); and sharing economy firms (Uber, Lyft, Airbnb), covering trillions of dollars of transactions. In short, a large share of potential disputes between consumers and firms in the United States, for purchases ranging from a toothbrush to a house, are settled through mandatory arbitration, rather than the court system.

A central feature of arbitration is the ability of both parties to explicitly exert control in the arbitrator selection process. For example, in securities arbitration, each party is presented with a randomly generated list of arbitrators and can influence the arbitrator selection process by striking a limited number of arbitrators from the list. This is a notable difference compared to judicial proceedings, where judges are assigned to cases. In addition, unlike judges, arbitrators are compensated only if they are selected. Practitioners strongly believe that choosing an arbitrator can significantly affect the case outcome: "The selection of an appropriate arbitrator or arbitration tribunal is nearly always the single most important choice confronting parties in arbitration" (Stipanowich et al., 2010). Despite the growing prevalence of arbitration in resolving consumer disputes, few empirical studies to date have examined the impact of the arbitrator selection process on consumer outcomes. This paper attempts to fill this gap.

This paper has two goals. The first is to establish several facts that suggest that firms hold an informational advantage over consumers in selecting arbitrators, resulting in industry-friendly arbitration outcomes. We then develop and calibrate a model of arbitrator selection in which firms hold an informational advantage in selecting arbitrators, who compete to be selected. The model allows us to decompose the firms' advantage into two components: the ability to choose pro-industry arbitrators from a given pool, and the equilibrium pro-industry tilt in the arbitration pool that arises because of arbitrator competition. The model reveals that accounting for this advantage is critical in assessing changes to arbitration design. Policies that would benefit consumers if they were informed hurt them instead, illustrating the importance of investor sophistication in consumer financial markets (e.g., Agarwal et al., 2015; Argyle et al., 2019; Anderson et

al., 2020). We also calibrate consumers' gains or losses when arbitrator selection rules are changed, such as those in recent policy proposals. This allows us to speak to market design in financial markets (e.g., Budish and Shim, 2015).

We study arbitration in the securities industry using a new data set of roughly 9,000 disputes between consumers and financial advisers. The securities industry lends itself to studying arbitration because of the data availability and the institutional setting. Our data on securities arbitration comes from the Financial Industry Regulatory Authority's (FINRA) Arbitration Awards Database, which we merge with FINRA's BrokerCheck data using unique case-level identifiers. The merged data allow us to observe detailed information on the claimant (consumer); respondent (firm); arbitrators; dispute details; and the awards. In addition, the institutional environment has several useful features. Pre-Dispute Arbitration Agreements (PDAA) are required in virtually all broker-dealer contracts, implying that there is no selection of firms or consumers into arbitration clauses. All disputes are resolved under the auspices of FINRA, which provides a uniform pool of arbitrators, as well as rules governing arbitration, so the choice of venue is also fixed. Nevertheless, the selection system used by FINRA is similar to those of the largest consumer arbitration forums such as the American Arbitration Association (AAA) and the Judicial Arbitration and Mediation Services, Inc. (JAMS). Most important for the research design, FINRA randomizes the list of potential arbitrators from which the parties select the arbitration tribunal, which we exploit in our research design.[1]

Arbitration in the brokerage industry is also interesting per se. Roughly 20 million U.S. households hold a brokerage account, amounting to $20 trillion of assets (2016 Survey of Consumer Finances). The cases involve significant amounts: mean and median damages requested are $785,000 and $175,000 respectively, providing substantial incentives for the parties in arbitration. The regulator, FINRA, established the Dispute Resolution Task Force to investigate concerns that the arbitration procedures lead to outcomes favoring the industry. More recently, the Consumer Financial Protection Bureau (CFPB) proposed a new rule regulating mandatory arbitration clauses in certain financial products (Arbitration Agreements, 12 C.F.R. § 1040 2017). Understanding arbitration design in the financial industry therefore has direct policy relevance.

We demonstrate in two steps that firms have an informational advantage in selecting arbitrators. First, we confirm practitioner intuition that some arbitrators are systematically more industry friendly and that others are more consumer friendly (Stipanowich et al., 2010). Controlling for the arbitrator overseeing the case explains an additional 24% of the variation in arbitration awards in excess of case characteristics. An arbitrator who is more industry friendly by one standard deviation awards 12 percentage points (pp) smaller damages relative to the damages requested. For a median case ($175,000), this translates to a $21,000 smaller award for the consumer. It is therefore not surprising that, anecdotally, brokerage firms maintain

[1] Ernst & Young has verified the randomization process. See https://www.finra.org/arbitration-mediation/arbitrator-selection.

proprietary internal arbitrator rankings, or arbitrator "strike lists," to guide their arbitrator selection process.

We provide suggestive evidence that arbitrators' industry friendliness is shaped by their own experience. Roughly 40% of arbitrators previously worked as financial advisers, and consequently, we observe their employment and misconduct history in FINRA's Brokercheck data. Of those arbitrators who previously worked as financial advisors, 8% previously lost in arbitration as a defendant and were required to pay an award to a consumer. As arbitrators, this group of former financial advisers grants awards that, on average, are over 4pp less consumer friendly. Conversely, those arbitrators who were previously fired from the financial advisory industry systematically grant higher awards to consumers. These results suggest there are systematic differences in how arbitrators grant awards and that their past personal experience shapes these decisions.

Second, we find that firms have an informational advantage over consumers in choosing favorable arbitrators. The pool from which the parties select arbitrators is randomly generated by FINRA. If both parties were equally well informed, they would strike arbitrators who favor the opposing side, and the median arbitrator from the list would be chosen. Because the list is random, being industry or consumer friendly would not increase selection chances. Instead, industry-friendly arbitrators are 40% more likely to be selected than their consumer-friendly counterparts.[2] The selection of industry-friendly arbitrators decreases award amounts by about 5pp or roughly $39,250, on average. We argue that the selection arises because firms are better at eliminating consumer-friendly arbitrators.

The main alternative hypothesis is that firms and consumers are equally sophisticated. Some arbitrators appear industry friendly in the data because they are systematically selected to cases with unobservable characteristics that merit lower awards. These arbitrators are selected more often—not eliminated by either consumers or firms—because their skills make them a good fit for future cases. If we controlled for these case characteristics, there would be no pro-industry bent. It is difficult to see why arbitrators who lost themselves in arbitration would have specific skills. Nevertheless, we address this alternative in several ways.

We exploit the 2007 change in FINRA rules governing arbitration, which reduced the number of arbitrators that each party could strike. If firms' advantages in arbitration are indeed driven by their ability to choose which arbitrators to eliminate, then restricting the number of arbitrators that each party can eliminate should reduce the impact of firms' informational advantages. We find that the effect of firms' informational advantages declines after the reform by more than half. We do not find evidence that the composition of cases overseen by consumer/industry-friendly arbitrators changes (which would be required to generate the results surrounding the 2007 rule change) if both parties were equally informed.

In fact, we find no evidence of sorting on observable case characteristics across arbitrators of different industry friendliness. This is despite the fact that the rich set of case characteristics we have are very relevant

[2]Kondo (2006) finds that pro-industry arbitrators were more frequently selected in NASD arbitrations.

to determining eventual damages. Using these case characteristics, including some that we construct using natural language processing of arbitration texts, we can explain approximately 70 percent of variation in case awards. We also perform Altonji et al. (2005) and Oster (2019) style tests and compute arbitrator fixed effects iteratively by conditioning on richer case characteristics. Despite large changes in R^2, we find little effect on the estimated arbitrator fixed effects.

We also provide some direct evidence that parties' sophistication in arbitration helps them choose more favorable arbitrators. The average firm in our data has been involved in 81 different arbitration cases. Such experience may improve firms' ability to eliminate arbitrators who are more likely to deliver unfavorable outcomes, and improve their "strike list" as anecdotal evidence suggests. We confirm that firms, which are more experienced in arbitration, select arbitrators who are relatively more industry friendly than less-experienced firms. Consumers might be less sophisticated than firms on average, but also differ in their sophistication (Barber and Odean, 2000). We find that consumers who are part of trusts select more consumer-friendly arbitrators. Fund trustees are frequently professionals with experience in legal and financial matters, and therefore likely to be more sophisticated. Consumers can compensate for their lack of experience by hiring an attorney. Indeed, consumers whose attorney specializes in arbitration select more consumer-friendly arbitrators. There is little evidence to suggest that unobservable case characteristics and the industry/consumer friendliness of arbitrators selected to these types of cases systematically sort in a way that is correlated with measured sophistication, especially given that we do not find selection on other observable case characteristics. It is much more plausible, and more consistent with the view of market participants, that the level of sophistication/experience plays a potentially critical role in the arbitrator selection process. These and additional tests make it very difficult to reconcile our results with the idea that consumers and firms are on equal footing when choosing arbitrators. Our explanation is simpler, more plausible, and consistent with the anecdotal evidence from practitioners and regulators.

In the second part of the paper, we present a quantitative model of arbitrator selection. The reduced form analysis in the first part of the paper only measures firms' informational advantage for a given pool of arbitrators. The model highlights a second advantage of the informed party: the whole pool of arbitrators becomes more industry-friendly as arbitrators compete to be selected in equilibrium. Because industry friendly arbitrators are more likely to be selected, all arbitrators are incentivized to act more industry friendly than other arbitrators. We use the calibrated model to decompose the equilibrium advantage of the informed party into these two components. We then use the model to show how accounting for the informational advantage is critical in assessing changes to arbitration design both qualitatively and qualitatively. If the informational advantage of firms is ignored, policy changes that aim to help consumers actually hurt them instead. We use the calibrated model to evaluate the magnitude of consumer gains and losses across different proposals.

The model mirrors the institutional setting: firms and consumers strike arbitrators from a randomly generated list. Arbitrators differ in their underlying beliefs of fair awards. They can depart from these beliefs, and choose how consumer or industry friendly they are, i.e., their "slant." This concept of slant is similar to the choice of political slant in the media industry (Gentzkow and Shapiro, 2006). Arbitrators are compensated only if they are selected to arbitrate a case. They compete with other arbitrators to be selected on the arbitration panel. In doing so, they trade-off their preferences for a fair award with monetary compensation from arbitration. Sophisticated firms observe arbitrators' slant; consumers, on the other hand, are uninformed.

A key result of the model is that because arbitrators compete to be selected, the whole pool of arbitrators becomes industry friendly, increasing the informational advantage of firms. Even though the underlying beliefs of arbitrators may be unbiased, competition among arbitrators drives all arbitrators to intentionally slant their case decisions in favor of firms. Intuitively, when consumers are uninformed, arbitrators compete to avoid being eliminated by firms. This competition between arbitrators exacerbates the informational advantage of firms in equilibrium and has several implications.

First, the reduced form analysis we perform in the first part of the paper cannot measure the extent of the competition effect. This is by design: to eliminate as much variation as possible, we compare how industry-friendly arbitrators are *relative to each other*. The analysis then measures the informational advantage for a given pool of arbitrators. The fixed effect analysis cannot detect the fact that the whole pool is industry friendly relative to arbitrators' beliefs of fair awards. We use the calibrated model to back out arbitrator beliefs, and decompose the informational advantage into its two components.

Second, the competition result stands in stark contrast to the situation in which both parties are informed. When both parties are informed, competition between arbitrators is desirable because it leads to less biased outcomes and statistical exchangeability of arbitrators. The idea behind statistical exchangeability is that "Since the parties play a role in the selection of the arbitrator who will decide their dispute, arbitrators who are known to favor one of the parties will be eliminated. This selection process created incentives for arbitrators to maintain characteristics that make them 'statistically exchangeable' with other arbitrators" (Ashenfelter et al., 1992, p. 1408). This argument is very powerful when both parties are informed about which arbitrators to eliminate, for example in the setting of employer/union arbitration. We show that the competitive forces that lead to statistical exchangeability when both parties are informed lead to biased outcomes when one party, such as a firm in the context of consumer arbitration, holds an informational advantage. This result has important consequences when considering policy changes to the arbitrator system.

We calibrate the model to quantify firms' informational advantage. We then decompose the informational advantage into the advantage of being better at striking arbitrators from a fixed arbitrator pool, and the equi-

librium effect on the pool itself. Calibrating the model, we obtain the underlying distribution of arbitrators' beliefs, i.e., the awards that arbitrators would have chosen absent incentives provided by the arbitration selection mechanism. Firms' informational advantage is substantial: selecting arbitrators without input of the parties would increase consumer awards by 5pp (relative to the amount requested), or $40,000 on average relative to the current system. Approximately 60% of that effect arises because firms are better than consumers at striking arbitrators from a given arbitrator pool. Competition between arbitrators accounts for the remaining 40%. The average arbitrator gives out an award that is 2pp lower than what she believes is fair, because doing so increases her probability of being selected for arbitration.

We use the calibrated model to investigate alternative arbitrator selection schemes, such as those proposed by FINRA in 2016. Policy proposals that aim to improve arbitration outcomes are frequently designed without considering the informational advantage of firms. The counterfactuals from the model suggest that several proposals, which would be consumer friendl" if consumers were informed, are instead industry friendly, once one accounts for firms' informational advantage. For example, increasing arbitrator compensation has been touted as potentially improving arbitration outcomes for consumers (FINRA Notice 14-49, 2014). Our estimates suggest that doubling arbitrator compensation would decrease awards by $31,000, on average, because increasing arbitrator compensation further incentivizes arbitrators to act industry friendly if firms hold an informational advantage. One implication of our model is that lower-powered incentives for arbitrators, potentially coupled with a flat wage, could decrease the pro-industry slant in arbitration. Similarly, increasing the number of strikes, which was also proposed to benefit consumers, would instead lower awards. Our empirical analysis and model helps provide insight into designing arbitration in the securities industry and potentially consumer arbitration more generally.

Related Literature: Our paper relates to the existing literature on arbitration. One strand of the literature provides empirical evidence that arbitrators are statistically exchangeable (Farber and Bazerman, 1986; Bloom, 1986; Ashenfelter et al., 1992). This result stands in contrast to the large differences among arbitrators we document. These studies mainly focus on arbitration in which both parties are equally informed, such as those arbitrations between unions and employers, or arbitration in an experimental setting. We study consumer arbitration, where, instead, potential differences in parties' information loom large.

The focus on the information gap in consumer arbitration also distinguishes our work from existing work on arbitrator selection. Our findings are consistent with Bloom and Cavanagh's (1986a), who find that arbitration parties tend to select arbitrators based on their preferences in arbitrations operated by the New Jersey Public Employment Relations Commission. Our works suggests that parties can only do so when informed. Kondo (2006) examines securities arbitration administered by the National Association of Securities Deal-

ers (NASD) over the period 1991-2004. Unlike standard arbitrator selection processes, the administrator actively participated in selecting arbitrators. Kondo (2006) finds that NASD selected industry-friendly arbitrators, and the effect is greater after a reform that reduced NASD's influence in arbitrator selection. Choi, Fisch, and Pritchard (2014) study 417 NASD arbitration awards over the period 1998-2000 and find that arbitrators with industry experience and those selected more frequently tend to give lower awards. Our work examines a longer panel of NASD arbitrations than Kondo (2006) and Choi et al. (2014), and also includes FINRA arbitrations. We study the effect of consumer sophistication on the arbitrator selection process and outcomes. We further contribute by developing and calibrating a quantitative model of arbitrator selection and use it to decompose the firms' advantage into two components: the advantage of choosing pro-industry arbitrators from a given pool, and the equilibrium pro-industry tilt in the arbitration pool that arises because of arbitrator competition. These components are then used to conduct policy counterfactuals.

Our paper is related to the theoretical literature on designing arbitration mechanisms. A large part of this literature has focused on the difference between conventional arbitration and final offer arbitration (Stevens, 1966).[3] De Clippel et al. (2014) develop a framework for understanding the selection of arbitrators from the perspective of implementation theory and test their theoretical framework in an experimental setting. The existing literature maintains the assumption that both parties are equally informed and have complete information when selecting arbitrators, which is reasonable in many settings (i.e., arbitration between firms, unions, or countries). We depart from the literature by finding evidence that one party holds an informational advantage and studying the associated consequences of arbitration design.

Our study is related to work in behavioral finance, which highlights the importance of trust and investor sophistication in consumer financial markets (Campbell, 2006; Guiso et al., 2008; Gennaioli et al., 2015; Agarwal et al., 2015; Argyle et al., 2019). Our conclusion that consumers fail to select friendly arbitrators is consistent with evidence that individual investors underperform in financial markets due to a lack of consumer sophistication (Barber and Odean, 2000; Egan, 2019).

More broadly, our paper links to the literature using quantitative models to study the effect of competition in financial markets (Benetton, 2018; Koijen and Yogo, 2016). We depart from this literature by focusing on competition between arbitrators in the area of dispute resolution, rather than in a setting in which consumers choose financial products. That competition can sometimes be "undesirable" is related to the larger literature of market design in financial markets (Budish and Shim, 2015; Budish, Lee, and Shim, 2019). This concept

[3]Crawford (1979, 1982) studies the effect of conventional and final-offer arbitration on negotiated settlements. Farber (1979, 1980) and Farber and Katz (1979) explore the case where the parties are uncertain about the arbitrator's preferences and find that the outcomes under conventional and final-offer arbitration generally differ. Brams and Merrill (1983, 1986) model arbitration as a zero-sum game of imperfect information. Gibbons (1988) analyzes strategic communication in equilibrium models of conventional and final-offer arbitration, and emphasizes the role of learning by the arbitrator from the parties' offers about the state of the employment relationship. Rosenthal (1978), Samuelson (1991), Farmer and Pecorino (1998, 2003), Deck and Farmer (2007), and Olszewski (2011) compare different arbitration procedures under incomplete, asymmetric information.

of slant and competition is similar to the choice of political slant in the media industry (Gentzkow and Shapiro, 2006). Providing a quantitative model also responds to the call by DellaVigna (2018) for more work in the area of structural behavioral economics (see Anderson et al., 2019).

Lastly, our paper also relates to the literature on misconduct among financial advisers including Dimmock et al. (2015); Qureshi and Sokobin (2015); Egan et al. (2017; 2019); and Charoenwong et al. (2019). Virtually all consumers in this market have signed a pre-dispute arbitration agreement with their advisers.

II Institutional Details: Consumer Arbitration

II.A Consumer Arbitration in the United States

Arbitration is a private dispute resolution alternative to civil courts. It differs from the civil court system along several important dimensions. First, arbitration is typically binding without appeals and courts have had limited ability to vacate or modify arbitration awards (*Hall Street Associate, LLC vs. Mattel, Inc.*, 552 US 576, 2008). Advocates of arbitration argue that this feature means arbitration is usually quicker and less expensive than litigation (U.S. Chamber of Commerce Institute for Legal Reform, 2005). Second, as described below, the parties involved in a given dispute exert significant control in selecting arbitrators, while courts select judges. Third, while judges are frequently paid a fixed salary, arbitrators are compensated only if they are selected for a case. Fourth, arbitration can either be voluntary or involuntary. When purchasing goods and services, consumers often agree to pre-dispute arbitration agreements, which mandate that any related disputes must be resolved through arbitration.

Consumer arbitration is ubiquitous in the United States. The CFPB's Arbitration Study (2015) estimates that 50% of credit card loans ($500 billion) and 44% of insured deposits ($3.1 trillion) are subject to mandatory arbitration. Arbitration is common in most consumer financial products, such as automobile loans, brokerage accounts, payday loans, etc., and in many other non financial products, such as cable TV, cellphone, internet, and car rental contracts (Silver-Greenberg and Gebeloff, 2015). Arbitration is also prominent in employment contracts. More than half (54%) of non-union private-sector employers have mandatory arbitration procedures, affecting an estimated 60 million American workers (Colvin, 2018).

Arbitration proceedings are governed by an administrator/forum who determines the procedural rules. Administrators often provide a list of potential arbitrators and govern the selection process. Our analysis focuses on securities arbitration between consumers and brokerage firms, which is exclusively administered by FINRA. The two other dominant forums for consumer arbitration are AAA and JAMS.[4]

[4]For example, AAA is listed as a potential forum in over 80% of credit card, checking account, prepaid card, and mobile wireless arbitration clauses studied by the CFPB (2015). The National Arbitration Forum previously administered consumer arbitrations but ceased administering consumer arbitration in 2009.

A central feature of arbitration is the parties' control over the arbitrator selection process. This selection process is based on the premise that arbitrators differ in terms of how favorable they might be to either party. Although the specifics vary across forums, the process typically involves ranking and striking potential arbitrators by consumer (claimant) and firm (respondent). For example, in FINRA and JAMS arbitration, the administrator sends a list of potential arbitrators to the consumer and firm. Each party can remove/strike a fixed number of arbitrators from the list, and then must rank the remaining arbitrators. Among arbitrators who were not struck, the one with the lowest joint rank is selected.

II.B FINRA (NASD) Arbitration

Here we briefly discuss the institutional details of the arbitration proceedings and the arbitrator selection process used by FINRA or, prior to 2007, the NASD.[5] We focus on consumer arbitration, in which consumers file a claim against a brokerage firm. Consumer arbitration mechanisms differ from mechanisms used to arbitrate union contracts, international business, or country treaties, which are not the focus of this paper. As we discuss in Appendix D, FINRA's arbitrator selection mechanism and arbitrator incentives are similar to other *consumer* arbitration settings.

Consumers initiate arbitration by filing a Statement of Claim with FINRA, in which they provide details of the dispute and the type of relief requested. Consumers can modify these claims until an arbitration panel is appointed; afterwards, consumers can only modify their claim if they are granted a formal motion to amend the claim (FINRA 12309).

Next, consumers select arbitrators. FINRA (formerly NASD) maintains a roster of more than 7,000 eligible arbitrators. Generally, arbitrators must have at least five years of any paid work experience and at least two years of college. FINRA describes the pool of arbitrators as ranging from "from freelancers to retirees to stay-at-home parents" ("Become an Arbitrator Frequently Asked Questions," 2018). As we document in Section III, arbitrators are often current or former financial advisers. Prior to hearing cases, an arbitrator must have completed FINRA's 12-hour Basic Arbitrator Training Program.

Since 1998, arbitrators are selected using the Neutral List Selection System (NLSS).[6] For each case, an automated process generates a list arbitrators on a rotational basis based on the geographic location of the hearing site (FINRA 10308(b)(4)(A)). If several arbitrators are needed, several lists are generated. Both parties read the generated list, as well as an Arbitrator Disclosure Report for each potential arbitrator. The Arbitrator Disclosure Report contains each potential arbitrator's education, employment history, skills, training, conflict information, and any publicly available arbitration awards the arbitrator granted ("Arbitrator

[5] Full details on the arbitration proceeding can be found on the FINRA website: https://www.finra.org/arbitration-and-mediation/code-arbitration-procedure.

[6] See FINRA code 10308 for full details.

Appointment FAQ," 2018).

Two aspects are critical to the process. First, to generate the list, NLSS randomly selects arbitrators. According to FINRA, "The randomized process has been verified by an Ernst & Young audit in a report that confirmed that a 'random pool management algorithm [is] used to ensure that each arbitrator in the pool has the same opportunity to appear on a list as all other arbitrators in that pool.'"[7] Second, each party then reviews and ranks the list of arbitrators according to the following rules. A party may strike one or more arbitrators from a list for any particular reason. The number of allowable strikes has changed over time; we describe this change in the arbitration selection process below. The number of strikes has ranged from four strikes by each side from a list of 10 potential arbitrators, to unlimited strikes. The struck arbitrators are immediately deemed ineligible to preside over the arbitration hearings. The parties then rank the remaining arbitrators. Arbitrators are then appointed based on their cumulative ranking, which is constructed by adding the rankings of both parties.

In general, an arbitration panel consists of one or three arbitrators. The composition of the arbitration panel depends on the claim amount. Under the current guidelines, claims under $50,000 generally have one public arbitrator, claims between $50,000-100,000 have one arbitrator but can have up to three arbitrators, and claims over $100,000 generally have three arbitrators.[8]

Arbitrators are compensated only for the cases they arbitrate. The minimal compensation for an arbitrator is $75 per hour, and can be substantially larger for shorter hearings.[9] In addition, arbitrators are entitled to reasonable local expenses. Their compensation is almost twice the median hourly compensation of $39.8 for financial analysts and financial advisers, who comprise a substantial amount of the arbitration pool, and is comparable to the average compensation of federal district judges ($79 per hour).[10],[11] Becoming an arbitrator also offers non-pecuniary benefits. FINRA advertises that arbitrators have the opportunity to "build networks," "gain professional experience," and "acquire knowledge of the securities industry" ("Become an Arbitrator Frequently Asked Questions," 2018). Given the sizable compensation, it is not surprising that FINRA has a large roster of potential arbitrators at its disposal. Critically, arbitrators are paid only if they are selected onto a panel; they do not receive benefits or other payments simply for being on the roster.

[7] https://www.finra.org/arbitration-mediation/arbitrator-selection.

[8] For cases with one arbitrator, the arbitrator is typically a public arbitrator. For panels with three arbitrators, the panel typically consists two public aribtrators and one non public arbitrator. Public arbitrators, have not worked in the financial industry in the past five years. Since 2015, this definition was expanded to exclude all individuals with any experience in the financial industry.

[9] Compensation comprises $300 per hearing (chairpersons earn an additional $125 per day), which can last at most four hours, with at most two hearings a day (hearings can be from the same case). The typical case lasts four days, which means arbitrators could expect to earn $1,200-2,900 on the average case, depending on whether the arbitrator serves as the chairperson and on the number of hearings per day.

[10] Adviser compensation data is from the BLS: https://www.bls.gov/oes/current/oes_nat.htm#13-0000. Average compensation of federal district judges is the annual salary as of 2006 divided by the number of annual work hours ($52 \times 40 = 2,080$). Judicial compensation data is from https://www.uscourts.gov/judges-judgeships/judicial-compensation.

[11] https://www.finra.org/sites/default/files/Education/p117487_0_0.pdf.

II.B.1 2007 Reform: Changing the Number of Strikes

The arbitrator selection process has undergone several changes but can be broadly captured into three periods: pre-1998, 1998-2007, and post-2007. Our main focus is the 2007 reform, which we exploit in Section IV.C.1. Pre-1998, a NASD arbitration committee was responsible for selecting arbitrators and members were permitted to use their discretion (Nichols, 1999). Concerns over whether the industry-sponsored arbitration was fair for consumers led to several investigations, including a congressional investigation in 1992.[12] In 1998, the NASD responded by adopting the NLSS, which we described above: parties obtain randomly generated lists of arbitrators and can strike arbitrators from the lists.

The arbitrator selection process was revised in 2007 as part of an overhaul to the system when FINRA succeeded NASD. The major change was to limit the number of arbitrators that parties can strike from the randomly generated subset of arbitrators. Prior to 2007, each party was able to strike any number of arbitrators from the list, while post-2007 each party could strike at most 4 out of 10 arbitrators. The concern with the arbitrator selection mechanism prior to 1998 and over the period 1998-2007 was that the industry had too much control over the arbitrator selection process. This is consistent with our idea that firms hold an informational advantage in arbitration. We exploit the 2007 rule change in Section VI.C.

III Data

III.A Data Construction

We construct a novel data set containing the details and awards of roughly 9,000 securities arbitration cases involving consumer disputes with financial advisers. The claimant is always a consumer and the respondent is always a financial adviser. We observe the details of each arbitration case, including the parties involved (claimant, respondent, and arbitrator); the nature of the allegations which are being arbitrated; detailed information on the respondent; and the outcome of the proceedings. We construct the data set primarily from two sources: FINRA's Arbitration Awards Online and FINRA's BrokerCheck website. We collect additional data, which we describe in the body of the paper.

FINRA's Arbitration Awards Online Data: FINRA's Arbitration Awards Online contains the details of FINRA and NASD arbitration hearings dating back to 1988. For each of the 53,062 arbitration cases, we collect the case/award documents and systematically parse each document for information regarding the consumer (claimant), financial adviser (respondent), and the arbitrator. The arbitration documents also provide accounts of the nature of the disputes, providing us with a detailed picture of which cases are similar to

[12] The 1996 NASD Arbitration Policy Task Force (The Ruder Report) determined that consumers were concerned that the arbitrator selection process "reflected staff bias and prejudgment" and that investors had "limited input on the choice of arbitrators."

each other and the cases themselves. These documents contain award details, such as whether damages are compensatory or punitive, as well as any fees assessed for arbitration. The data cover consumer arbitration, as well as employment arbitration and arbitration between securities firms. We match the arbitration awards data with FINRA's BrokerCheck data, which provides additional granular details on each consumer arbitration case. We also obtain detailed data on defendants' employment and misconduct history, and obtain the same information for any arbitrator who was employed in the financial industry.

FINRA's BrokerCheck Data: We use BrokerCheck to obtain additional data on the respondent, as well as case details, such as specific allegations that triggered the arbitration, requested damages, and arbitration award, all of which we discuss in more detail below. These data contain the employment, registration, and disclosure history for all individuals registered with FINRA. We collect the details of each financial adviser to construct a data set of the universe of financial advisers as described in Egan et al. (2019). Using these data, we construct employment histories of arbitrators who have been employed in the financial industry in the past, including whether these arbitrators were themselves subject to arbitration, the outcome of those proceedings, and whether these arbitrators were at any point terminated from a financial firm.

If a financial adviser is involved in an arbitration proceeding, the proceeding is reported on their disclosure record. Using unique case identifiers, we perfectly match the arbitration records reported in BrokerCheck to the arbitration case details reported in the Arbitration Awards Online database. We match 8,828 consumer arbitration disputes, which is the *universe* of arbitration disputes reported in BrokerCheck. The reason the matched data set (8,828 observations) is smaller than the FINRA Arbitration Awards Online data set (53,062 observations) is because we restrict our attention to consumer arbitration cases. These matched data represent our main data set. We describe the information we can observe in detail in the next section.

We observe the names of the arbitrators selected for 10,000 additional non-consumer arbitration cases, which are not reported BrokerCheck. Because non-consumer arbitration cases are not reported in BrokerCheck, we do not readily observe any information regarding them beyond the name of the arbitrator. These additional non-consumer arbitration cases allow us to examine an arbitrator's total case load in addition to the arbitrator's consumer arbitration case load.

III.B Summary Statistics: Cases and Arbitrators

Our primary unit of observation is at the case by arbitrator level. Our baseline data set consists of 8,828 consumer arbitration cases and 20,231 arbitrator-by-case observations.[13] These cases involve substantial monetary amounts: mean and median damages requested are $785,000 and $175,000, respectively. The

[13]Roughly 13% of consumer complaints in arbitration involve multiple financial advisers. Arbitrators separately assess damages across the financial advisers involved in the case.

median award granted is 32% of the requested amount, with large differences in arbitration outcomes: the standard deviation is 67% (Figure 1). The distribution is skewed to the right, with a mean award of 51% of damages, partially because awarded claims can exceed damages requested if punitive damages are awarded.

In Table 1, we report the six most commonly recorded allegations and financial products in arbitration.[14] Common allegations include the selling of unsuitable investments and misrepresentation. Fraud allegations comprise 24% of all claims. When alleged claims are directed at a specific financial product, we measure this as well. The most common allegations regard equity investments (9%), and insurance (5%). To measure how cases differ in complexity, we measure the total number of allegations and length of the arbitration case in counts of words and sentences. We also use natural language processing to further distill each case. Using a bag-of-words approach (Gentzkow, Kelly, and Taddy, 2019; Bodoh-Creed, Boehnke, and Hickman, 2018), we construct dummy variables for the 500 most common words in case documents, after stemming the words using Porter's Stemming Algorithm and dropping stop words as per the System for the Mechanical Analysis and Retrieval of Text (Lewis et al., 2004). While these 500 words account for 77% of the words appearing in case documents, there is substantial variation across the cases in which these words appear. Among the 500 most common words, 50% of the words appear in fewer than 20% of cases (Appendix E).

Our data set also contains detailed information on the employment, registration, and disclosure history of each respondent, i.e., the financial adviser named in the consumer dispute. Because the securities industry is highly regulated, financial advisers must be licensed in order to engage in certain business activities, such as providing advice, selling mutual funds, insurance, and other products. Advisers can hold up to 61 different types of licenses, which helps us control for potential differences across arbitration cases. The average adviser holds 3.9 qualifications. Most advisers in our sample hold a Series 7 license, which allows them to engage in a broad range of securities transactions. Roughly half of them hold investment adviser qualification licenses (Series 65 or 66), allowing them to provide financial advice rather than transaction services. Roughly half (48%) of the respondents in the sample have past histories of misconduct and are repeat offenders, which is eight times more than the average across all financial advisers (6%; Egan, Matvos, and Seru, 2019).

Parsing case text, we document whether a consumer was represented by an attorney, as well as the name of the attorney. Consumers use an attorney in roughly 88% of our observations. Seventeen percent of cases are represented by members of the Public Investors Arbitration Bar Association (PIABA), whose members specialize in securities arbitration.[15] These attorneys may be better informed about the arbitration proceedings, as well as about individual arbitrators. From the parsed text, we also determine that in 17% of the observations, the consumer is a part of a trust. We use this variation in consumer type and representation

[14]The allegation and product categories are not mutually exclusive (e.g., a case can allege both fraud and a breach of fiduciary duty).

[15]To determine PIABA membership, we manually match the attorneys representing consumers in our data to the roster of attorneys posted on the PIABA website by first and last name.

as proxies for sophistication in our analysis.

We observe 7,891 unique arbitrators and, central to our analysis, repeated observations for 3,917 arbitrators. The arbitration panel size typically consists of one to five arbitrators. The average arbitrator in our sample oversees 8.3 total arbitration cases (including non-consumer dispute arbitration cases) and 4.5 consumer dispute arbitration cases (Figures 2a and 2b). Figures 2a and 2b illustrate that even though many arbitrators have little case experience, the arbitrator who oversees an average case tends to have a lot of experience. Therefore, most cases are overseen by arbitrators with extensive arbitration experience. We match 40% of the arbitrators in our sample to financial advisers in the BrokerCheck database. These arbitrators have either been employed as financial advisers in the past, or currently work as financial advisers in the industry. Such arbitrators average three certifications and have been employed in the industry for nine years on average, which is in line with averages in the financial advisory industry (Egan, Matvos, and Seru, 2019). Among those arbitrators, 8% have themselves been defendants in securities arbitration that resulted in a customer award, which may make them more skeptical about the validity of consumer claims, all else equal. Conversely, 11% of these arbitrators have been previously fired from financial firms, which may make them less predisposed to rule in their favor. We confirm this intuition in Section IV.A.

IV Differences between Arbitrators, and Arbitrator Selection

The arbitrator selection process is based on the premise that arbitrators differ in how favorable they are to either party. These differences are why parties are allowed to eliminate arbitrators in the first place. We measure systematic differences between arbitrators in their awards. As our model in Section V highlights, differences between arbitrators arise in equilibrium as arbitrators compete to be selected. They therefore reflect both underlying differences in arbitrator beliefs, as well as arbitrators' choices.

IV.A Arbitrator Industry Friendliness

Ideally, we would observe two arbitrators ruling on identical cases, in the same location, and at the same point in time. An arbitrator, who grants the lower award would be more industry friendly, and the magnitude of the difference in awards would measure the extent of the difference in arbitrators' industry friendliness. We construct an empirical equivalent of this thought experiment to construct our measure of arbitrator industry friendliness. In this section, we construct our baseline estimates of arbitrator industry friendliness, which are easy to understand and transparent. We discuss estimates with richer, text-based data, as well as robustness in Section IV.C. We estimate a model of awards granted as a function of case characteristics, location and time of the arbitration, and, critically, the identity of the arbitrator:

$$Awarded_{ijklt} = \beta X_i + \mu_j + \mu_k + \mu_l + \mu_t + \epsilon_{ijklt}. \quad (1)$$

Observations are at the arbitrator-by-case level, where i indexes the arbitration case, j indexes the financial advisory firm involved in the case, k indexes the location, l indexes the arbitrator, and t indexes time. The dependent variable $Awarded_{ijkt}$ reflects the award granted divided by the award requested.[16]

We condition on case characteristics, X_i, to control for potential differences in the type of claim that is arbitrated and the merit of the claim. In the baseline specification we control for the 11 different allegations and six different financial products covered in the case. We condition for complexity of the case as measured by length of the case in sentences and words. In Section IV.C, we do extensive robustness testing using natural language processing to control for more detailed case specifics**.** To further control for case merits in our baseline specifications, we include the characteristics of the defendant, financial adviser, and the firm they work for. We control for the adviser's experience, the six most popular qualifications, the adviser's total number of qualifications, and any past record of misconduct. We also include adviser firm fixed effects, which accounts for possible heterogeneity in claims due to some firms specializing in activities that are more susceptible to arbitration.

We also include time and location fixed effects. In other words, we compare how industry friendly an arbitrator is relative to other arbitrators in the same location and at the same point in time. We include fixed effects for both the location of the arbitration hearing as well as county fixed effects corresponding to the office location of the offending adviser. These fixed effects help control for possible geographic differences in claims, and allow us to compare an arbitrator to the pool of other arbitrators who would be potentially assigned to the case. Time fixed effects help account for aggregate differences in claims.

The objects of interest are arbitrator fixed effects, μ_l, which measure whether an arbitrator, conditional on case characteristics, awards higher claims to consumers than other arbitrators at the same location at the same arbitration forum. An arbitrator l who is more industry friendly than arbitrator l' will have a lower associated fixed effect $\mu_l < \mu_{l'}$. This measure is relative. We do not measure whether arbitrators awarded too much or too little relative to some "correct" amount. We measure if arbitrators awarded more or less relative to other arbitrators. In Section VI.B, we use a model to estimate the arbitrators' beliefs on what the fair or correct award would have been.

Results in Table 2 show that the observable adviser and case characteristics explain 39% of the variation in awards without the knowledge of the arbitrator, i.e., without arbitrator fixed effects (column 3). For example, cases involving options have lower awards on average, while cases involving fee and commission-related allegations have higher awards. Arbitration involving advisers with prior misconduct generally have larger awards, consistent with the notion that past offenses are good predictors of future misconduct. The

[16]For robustness, we examine the log of awards granted through arbitration as function of case observable characteristics and the log of awards requested in the Appendix.

estimates in column (4) confirm that arbitrators differ in their degree of industry friendless. Including arbitrator fixed effects increases the R^2 from 39% to 63%. Inducing case characteristics from natural language processing increases the R^2 to 69% as we discuss further in Section IV.C. The differences among arbitrators are statistically significant: the F-test implies that they are jointly significant at 1%. In other words, the arbitrator plays a significant role in determining arbitration awards.

As we extensively discuss in Section IV.C.3, including different case controls (X_i) has little effect on the estimates of fixed effects. In other words, estimates of arbitrators' industry friendliness change little when we add different (or any) case controls, suggesting there is little matching between arbitrator industry friendliness and case characteristics. To evaluate the economic importance of arbitrator differences in determining arbitration awards, we have to consider the distribution of arbitrator industry/consumer friendliness. Because individual arbitrator fixed effects are estimated with noise, the estimated differences in industry/consumer friendliness among arbitrators will be larger than the true underlying differences between them. We account for noise by constructing empirical Bayes estimates of arbitrator industry/consumer friendliness, $\widehat{\mu_l^{EB}}$ (e.g., Chetty, Friedman, and Rockhoff, 2014).[17] In much of our proceeding analysis, the independent variable of interest is an arbitrator's slant, which we proxy for using our empirical Bayes estimated arbitrator fixed effects. The empirical Bayes estimator re-scales the distribution of OLS fixed effects by a constant factor. Therefore, using the empirical Bayes fixed effects rather than OLS estimated fixed effects as independent variables in our proceeding regressions will impact the estimated magnitude of our coefficient estimates, but will not impact inference.

We plot the distribution of estimated fixed effects in Figure 2c. We normalize the mean of fixed effects to match the average percent of awards granted in the data, 51%. Therefore, arbitrators with a fixed effect below 51% are on average more industry friendly than other arbitrators. Although the variation in the empirical Bayes estimated fixed effects is 31% of the variation in OLS estimated fixed effects, the results indicate substantial differences across arbitrators. If an arbitrator who is more industry friendly by one standard deviation is chosen to arbitrate the case, the damages awarded to the consumer will be 12pp smaller relative to the amount requested, holding other attributes of the case fixed. Given that the median damages requested are roughly $175,000, the consumer would be awarded $21,000 less. Overall, our results are consistent with the idea that the choice of arbitrator can have a meaningful impact on case outcomes.

[17] We re-scale the estimated distribution of arbitrator fixed effects from column (4) of Table 2 such that $\widehat{\mu_l^{EB}} = \alpha(\widehat{\mu_l} - \bar{\mu})$. $\bar{\mu}$ is the average OLS estimated fixed effect and $\alpha = \frac{F-1-\frac{2}{k-1}}{F}$, where F is the F-test statistic of a joint test of statistical significance of the fixed effects and k is the number of fixed effects under the assumption that the variance of the estimation error is homoskedastic (Cassella, 1992).

IV.A.1 Arbitrators' Past Experience in Financial Industry

In this section, we use additional data to help us understand the source of arbitrators being industry or consumer friendly. We focus on those 40% arbitrators who either currently or previously worked in the financial advisory industry. For these arbitrators, we observe their complete employment and misconduct history in the financial advisory industry. Using this data, we estimate an alternative version of eq. 1 where we control for the arbitrator's employment and misconduct history and report the corresponding estimates in column (4) of Table 2 . We find that those arbitrators with work experience in the financial advisory industry tend to be more industry friendly, awarding on average 1.51pp less relative to the amount requested.[18] We also look at two specific events during an arbitrator's career as a financial adviser that may have led them to be more or less industry friendly. Eight percent of former financial adviser arbitrators were themselves defendants in arbitration that resulted in consumer award. If they believe that in their experience as a defendant the consumer case was unjust, they may be more skeptical of the validity of a consumer's claims in general. Indeed, these arbitrators grant consumer awards that are 4.17pp lower relative to the amount requested on average. This evidence is consistent with the idea that convicted felons tend to have a pro-defense/anti-prosecution bias (Binall, 2014). In contrast, when the selected arbitrator was previously involved as a defendant in a customer dispute in which they prevailed (case was denied through arbitration or withdrawn), the arbitrator tends to grant a slightly higher award, although the point estimate is insignificant. Lastly, 11% of arbitrators who worked as financial advisers were previously fired from the financial advisory industry. These arbitrators' awards are 6.17pp higher relative to the amount requested on average, consistent with the idea that their experience with a specific financial firm made them less industry friendly. These results suggest there are systematic differences across arbitrators in how they grant awards and that their past personal experience shapes these decisions.

IV.B Arbitrator Selection and Industry Friendliness

The choice of arbitrator plays a significant role in arbitration outcomes and does so in a systematic way: some arbitrators are relatively more friendly to the firms, while others are more friendly to consumers. The idea behind striking and ranking is that parties can reduce favoritism in awards by eliminating arbitrators most favorable to the other party. As our model in Section V highlights, differences between arbitrators arise in equilibrium as arbitrators compete to be selected. They therefore reflect both underlying differences in arbitrators' beliefs and choices. While arbitrators who have the strongest pro-industry beliefs also choose to be most pro-industry in equilibrium, this distinction is not important to the parties in a given arbitration case. In other words, firms and consumers care if an arbitrator rules in their favor, not whether they did so

[18] This is consistent with Choi et al. (2014), who find that NASD arbitrators with past industry experience grant lower awards.

because of their beliefs or because of their strategic motives. However, the distinction becomes important when trying to interpret the magnitude of the informational advantage, and how different policies would affect the distribution of awards. We address these questions in Section V.

Here, we show that firms are better than consumers at choosing arbitrators because they eliminate those favoring the other side. As noted earlier, the list from which arbitrators are selected is randomly generated and audited for randomness by external auditors. If both sides were equally good at eliminating arbitrators, then neither side would have an advantage, and arbitrators' favoritism of a side would not help their selection. Alternatively, if firms are better than consumers at eliminating unfriendly arbitrators, then industry-friendly arbitrators would be chosen with a higher probability. Below we show that the latter is indeed the case, and that industry-friendly arbitrators are more likely to be selected. We begin with a simple cut of the data. Figure 3 displays a negative relationship between an arbitrator's estimated fixed effect (fixed effect obtained from column (4) of Table 2) and the number of times she is selected to arbitrate. Arbitrators who grant larger awards to consumers, given case characteristics, are less likely to be selected. This is despite their having equal chances of making it on the list, which is randomly generated. These results suggest that consumer friendly arbitrators face higher chances of elimination than industry-friendly arbitrators.[19]

More formally, we examine how an arbitrator's estimated fixed effect $\widehat{\mu_l^{EB}}$ impacts her probability of being selected in a given year using the following linear probability model:

$$Selected_{lkt} = \beta X_{lt} + \gamma \widehat{\mu_l^{EB}} + \delta_t + \delta_k + \eta_{lkt}. \tag{2}$$

Our observations are at the arbitrator-by-year level, where l indexes the arbitrator, k indexes the location, and t indexes time.[20] $Selected_{lkt}$ is a dummy variable that indicates whether arbitrator l was selected for a case in year t. The key independent variable of interest is the arbitrator's fixed effect $\widehat{\mu_l^{EB}}$. Note that because the empirical Bayes adjustment only re-scales OLS fixed effects, it aids in interpreting the magnitudes, but does not affect the regression estimates otherwise. The term X_{lt} is a vector of arbitrator controls that include the number of years she has been active in the industry. In the most saturated specification, we include fixed effects for the year and fixed effects for both the location of the arbitration hearing as well as county fixed effects corresponding to the office location of the offending adviser. Including fixed effects for the location of the arbitration accounts for the fact that some hearing locations may have a smaller pool of arbitrators to choose from relative to the case load, or that types of cases may differ across locations. We compute the location fixed effects based on the previous case the arbitrator oversaw.

[19] In the Appendix we examine an arbitrator's *first ruling in her career*, and see her future prospects of being selected for arbitration in Figure A2. The first award is likely the most salient ruling from which the parties update most on the arbitrator's type. We find that the higher the award to the consumer on the first ruling, the lower the chance of ever arbitrating again.

[20] An arbitrator enters our data as soon as she oversees her first case and remains in the data until 2015. We control for the number of years she's been active, and the number of cases in the data set she has overseen in order to adjust different attrition rates among arbitrators.

The estimates presented in Table 3 show that consumer-friendly arbitrators are less likely to be selected from a randomly generated panel of arbitrators. We observe a negative and significant relationship between an arbitrator's fixed effect $\widehat{\mu_l^{EB}}$ and the probability an arbitrator is selected across specifications. Recall that a greater fixed effect ($\widehat{\mu_l^{EB}}$) implies that the arbitrator was more consumer friendly and less industry friendly. We standardize the arbitrator fixed effects in units of standard deviation to ease interpretation in the corresponding regression table. The average probability that an arbitrator is selected to any arbitration case in a given year is 11%. Therefore, the estimates in column (1) indicate that a one standard deviation increase in an arbitrator's industry friendliness is associated with a roughly 12% (1.27pp) increase in the probability of being selected in a given year.

For an alternative interpretation of the results, suppose random arbitrators would be selected to cases instead. The average arbitrator fixed effect ($\widehat{\mu_l^{EB}}$) weighted by the probability of being selected in a given year is 5pp lower than the average fixed effect among the unconditional distribution of arbitrators. Given that the median (mean) award is 32% (51%), this represents an 16% (10%) decrease in awards to consumers. In dollar terms, this represents a $8,750 decrease for the median requested claim or a $39,250 decrease for the mean requested claim. These estimates suggest that industry-friendly arbitrator selection has a meaningful impact on eventual awards.

IV.C Alternative: Sophisticated Consumers and Sorting on Unobservables

Our central hypothesis is that firms are more sophisticated than consumers, which results in industry-friendly arbitrators being chosen more often. The main alternative hypothesis to ours is that firms and consumers are equally sophisticated. Arbitrators who grant lower awards are *not* industry friendly but are selected more often because their skill sets make them a good fit for future cases. We address this alternative in several ways. We first discuss how this selection would need to work in order to explain our main findings. We then present several pieces of evidence that are inconsistent with this alternative.

The alternative argument goes as follows: consumers and firms are equally sophisticated when choosing arbitrators, and arbitrators who grant lower awards are not biased towards the industry. Instead, some arbitrators appear industry friendly in the data because they are systematically selected to cases with unobservable characteristics that merit lower awards. If we conditioned on these case characteristics, there would be no pro-industry bent. These same arbitrators are selected (not eliminated by either consumers or firms) more often because their (unobserved) skill sets make them a good fit for future cases. This alternative has several direct implications. First, it implies that there are some omitted case characteristics that systematically bias our estimates of the arbitrator fixed effects from eq. (1). Notably, our rich and extensive controls of case characteristics already explain substantial variation in observable case awards (63%). Similarly, we

find that arbitrators' fixed effects are consistent with their past experience. First, it is not clear why arbitrators who *lost* a consumer case themselves (as a financial advisor) would be a better match for consumer cases. Second, it is also unclear why arbitrators who are systematically selected to cases that merit lower awards are also systematically selected to more cases. Third, the characteristics of the arbitrator selected to a case should not vary with the sophistication of either party because, under this alternative, both parties are sophisticated. While this alternative hypothesis could theoretically be consistent with our main results, we next show several pieces of evidence that are difficult to reconcile.

IV.C.1 Changing the Number of Strikes: 2007 Arbitration Rule Changes

We argue that industry-friendly arbitrators are selected more frequently because firms are better than consumers in eliminating arbitrators unfriendly to their side. As we describe in Section II.B.1, before 2007 firms and consumers were given a list of 10 arbitrators and both parties could strike/remove an unlimited number of arbitrators from the list. In 2007, the rules were updated such that the number of arbitrators whom each party could strike was limited to four. If firms' advantage in arbitration comes from the selection of industry-friendly arbitrators, the 2007 rule changes should have reduced this advantage. In fact, the 2007 change was enacted with the express purpose of making arbitration more favorable to consumers. If instead, under the alternative hypothesis, both parties are equally sophisticated at eliminating arbitrators, the reform should have resulted in wider variance in the types of arbitrators selected, not a shift towards consumer-friendly arbitrators. We formalize this point in the model in Section V.

If firms are better at eliminating consumer-friendly arbitrators, then industry-friendly arbitrators' chance of being selected should have declined after the reforms. We examine this hypothesis by re-estimating the arbitrator selection linear probability model (eq. 2), but allow the relationship between an arbitrator's fixed effect and selection probability to vary around the time period of the rule changes. Specifically, we estimate the following linear probability model:

$$Selected_{lkt} = \gamma \widehat{\mu_l^{EB}} + \gamma_{1999 \leq t < 2008} \widehat{\mu_l^{EB}} \times \mathcal{I}_{1999 \leq t < 2008} + \gamma_{t \geq 2008} \widehat{\mu_l^{EB}} \times \mathcal{I}_{t \geq 2008} + \beta X_{lt} + \delta_t + \delta_k + \eta_{lkt}. \quad (3)$$

Our observations are at the arbitrator-by-year level, where l indexes the arbitrator, k indexes the location, t indexes time, and $\mathcal{I}$ is an indicator variable designating a time period. We focus on the 1999-2007 per-period, because the prior change in rules was in 1998, which serves as the baseline period. The term $\gamma_{1999 \leq t < 2008}$ measures how the relationship between an arbitrator's fixed effect and her probability of being selected from 1999 to 2008. Similarly, the interaction term $\gamma_{t \geq 2008}$ measures how the relationship between an arbitrator's fixed effect and her probability of being selected changed after the 2007 rule change. As before, X_{lt} is a vector of arbitrator controls that includes the number of years she's been active in the industry. In the most saturated specification, we include year fixed effects (δ_t) and location fixed effects (δ_k) corresponding to the

location of the past case that the arbitrator worked on.

The estimates in Table 4 show that the rule change significantly decreased the probability that industry friendly arbitrators are selected. During the period 1999-2007, a one standard deviation increase in an arbitrator's consumer friendliness represented a 1.72pp (=4.57-2.85) decrease in the probability of being selected (column 1). During the post-reform period, the same increase in arbitrator's consumer friendliness represented a 0.2pp (=4.57-4.37) decrease in the probability of being selected. These results are consistent with the notion that firms possess substantial superior information about arbitrators relative to consumers, which lends them an advantage in the arbitration process. As the industry's control over the selection process diminished in 2007, the relationship between an arbitrator's past industry friendliness and probability of being selected diminished as well. These results are in line with the predictions of the arbitration model we present in Section V; our quantitative model indicates that changing the number of strikes will have a dramatic impact on which arbitrators are selected to a case (Section VI.C).

These estimates are difficult to explain if firms and consumers are equally sophisticated. If there is no change in sorting of cases following the 2007 reform, then the results are inconsistent with both parties being equally good at eliminating arbitrators: fewer strikes would lead to a wider variance in the types of arbitrators selected, not a shift towards consumer-friendly arbitrators. One might argue that this finding is not enough to rule out the alternative because composition of cases with unobservable characteristics that we discussed earlier changes in a way that coincides with the reforms. Figure A3 shows this is not the case. There was no difference in the types of cases assigned to consumer- versus industry friendly-arbitrators after the reform.

IV.C.2 Industry Friendliness Does Not Predict Sorting on Observable Case Characteristics

The alternative explanation to our results is based on the idea that our estimate of arbitrator type (granting high or low awards) is correlated with the unobserved type of cases they are assigned to. Here we show that arbitrator selection is not correlated with a rich set of measurable case characteristics. As we show, these characteristics are relevant, i.e., highly correlated with actual awards. In Figure 4a we show that the average characteristics of cases overseen by industry-friendly arbitrators (those with below average fixed effects) versus consumer-friendly arbitrators (those with above average fixed effects) are statistically indistinguishable. While uncorrelated with arbitrator type, Figure 4b illustrates that these observable case characteristics are highly correlated with awards. Similar to the results from Section 1, we find that cases involving fees, fraud, past-offenders, and unregistered securities tend to have higher awards, while cases involving mutual funds have lower awards.

We systematically examine the relationship between the arbitrator type (granting high or low awards)

and observable case characteristics in the following regression model:

$$\widehat{\mu_{il}} = \beta X_{jt} + \mu_l + \mu_t + \epsilon_{ijklt}. \tag{4}$$

Observations are at the arbitrator-by-case level; i indexes the arbitration case, j indexes the financial advisory firm involved in the case, k indexes location, l indexes the arbitrator, and t indexes time. The dependent variable $\widehat{\mu_{il}}$ measures the fixed effect of the arbitrator l selected for case i. We control for the observable case characteristics in X_{jt} as well as location fixed effects and time fixed effects.

We report the corresponding results in the Appendix E. There is no evidence that arbitrator types are selected (not eliminated by either consumers or firms) on observable case characteristics. We fail to reject the null hypothesis that our observable case characteristics in X_{jt} are uncorrelated with the fixed effect of the arbitrator selected for a case. This result is even more powerful when contrasted with our findings in the subsequent subsection (Section IV.C.4), where we find that consumer and firm sophistication predict arbitrator selection. For robustness, we construct an alternative backward-looking measure of arbitrator fixed effects based on the previous cases an arbitrator oversaw, and find similar results. We also re-estimate eq. (4), where we replace the dependent variable with the experience of the arbitrator and fail to find a statistically significant relationship. Overall, there is no evidence that the fixed effect or experience of an arbitrator selected to a case is correlated with an extensive set of case observable characteristics. These characteristics explain a large share of award variation and therefore are important case determinants.

IV.C.3 Alternative Measures of Arbitrator Industry Friendliness (Altonji et al., 2005; Oster, 2019)

In the Appendix, we construct several alternative measures of arbitrator industry friendliness–fixed effects. In the spirit of Altonji et al. (2005) and Oster (2019), we construct a measure of arbitrator fixed effects where we omit all controls. These control variables are highly correlated with case outcomes, increasing the R^2 by almost 50% (0.34 to 0.63). Despite their ability to predict awards, they have little effect on our estimated arbitrator fixed effects. We plot the correlation of the fixed effects across specifications in Appendix E. In other words, omitting first order case characteristics, which strongly predict awards, has little impact on how we estimate arbitrator industry friendliness. Therefore, it is less likely that unobservable case characteristics would play an important role in determining these fixed effects and drive our results.

We also construct several other alternative measures of arbitrator fixed effects and find no changes in our results. First, we use techniques in natural language processing to further control for case characteristics. We use a bag-of-words approach (Gentzkow, Kelly, and Taddy, 2019; Bodoh-Creed, Boehnke, and Hickman, 2018). We include dummy variables for the 500 most common words mentioned in the arbitration cases. With these additional controls we can explain roughly 70% of the variation in the award granted relative to the award requested and 80% of the variation in the log award requested. Second, we focus on cases

where adviser guilt is known and verifiable, so awards in such cases give a purer measure of arbitrator subjectivity/bias.[21] Third, we construct alternative measures of arbitrator fixed effects based on the log of the award granted. This alleviates concerns that normalizing awards granted by awards requested could have introduced additional noise into our measure of arbitrator fixed effects. Lastly, we also construct a backward-looking measure of arbitrator slant, using only information available up to the time of the case. In Appendix E, we show that our different measures of arbitrator fixed effects, using controls for very different observables, are all highly correlated and that our inferences on arbitrator section remain the same across each measure.

IV.C.4 Firm and Consumer Sophistication

Up to this point, we provided evidence that is consistent with the model in which firms are more sophisticated than consumers in choosing arbitrators, and that is difficult to reconcile with the alternative explanation of arbitrator sorting based on unobserved case characteristics. Here, we provide more direct evidence that parties' sophistication in arbitration helps them choose more favorable arbitrators. We exploit differences across firms' and consumers' sophistication. One broad idea is that firms are more sophisticated because they are more experienced in arbitration than the average consumer (see, Nichols, 1999; Gross, 2010; Barr, 2015; Silver-Greenberg and Gebeloff, 2015). Presumably, being involved in an arbitration case is informative about arbitrators, specifically, allowing the firms to design a better "strike list,"[22] but also about which information to acquire in future arbitrations, the importance of selecting arbitrators, and which attorneys to hire to help with selecting arbitrators.

In our data, the average firm is involved in 81 consumer dispute arbitrations, but there is substantial variation in firms' arbitration experience, making some firms more informed than others. We exploit these differences across firms. While we argue that firms are generally the better informed party, consumers also differ in their sophistication. For example, when a consumer is part of a trust, the trustee has a fiduciary duty to the beneficiary of the trust. Moreover, the trustee can be a professional, such as an attorney or accountant, who might be more sophisticated than the average consumer. After speaking with industry participants, we learned of a class of attorneys who specialize in securities arbitration (PIABA attorneys). We examine the fixed effect of the arbitrator l selected to case i as a function of firm and consumer sophistication:

$$\widehat{\mu_{il}} = \phi_0 + \phi_1 Attorney_i + \phi_2 PIABA_i + \phi_4 Trust_i + \phi_5 Firm\_Experience_i + \varepsilon_{il}. \quad (5)$$

Observations are at the arbitrator-by-case level, where i indexes the arbitration case and l indexes the arbitrator. The dependent variable $\widehat{\mu_{il}}$ measures the fixed effect of the arbitrator l selected for case i. The estimated

[21] In particular, these cases (about one-third of total cases) involve unauthorized trading, churning, or selling unregistered securities.
[22] Anecdotal evidence suggests that brokerage firms often maintain proprietary internal arbitrator rankings, or arbitrator "strike lists."

arbitrator fixed effects correspond to eq. (1), where a higher fixed effect implies that the arbitrator gives out higher awards on average. The independent variable $Attorney_i$ indicates whether the consumer used an attorney, $PIABA_i$ indicates whether the consumer used a PIABA attorney, and $Trust_i$ indicates whether the consumer is part of a trust. $Firm\_Experience_i$ indicates whether the firm has above median arbitration case experience in terms of the number of consumer arbitration cases a firm is involved in. The omitted category comprises consumers who ares self-represented/do not use an attorney, are not part of a trust, and face a less experienced firm. Table 5 displays the corresponding estimates.

We find that firms more experienced in arbitration also select arbitrators that are more industry friendly. The results in column (1) indicate that firms with above median experience select arbitrators that tend to give out 3pp lower awards relative to the amount requested. Consumers who use attorneys tend to select more consumer-friendly arbitrators. Arbitrators overseeing their cases grant 3.48pp higher awards on average (column 2). Hiring an attorney, especially one who specializes in arbitration, helps consumers compensate for their lack of sophistication. For example, consumers who use a PIABA attorney have their cases overseen by arbitrators whose awards are 10.23pp (=3.48+6.75) higher than in cases where consumers are self represented (column 2).[23] Moreover, our results also suggest that more sophisticated consumers choose more consumer-friendly arbitrators: the arbitrators chosen when consumers are a part of a trust grant 9.60pp higher awards on average (column 2). In other words, parties' expertise in arbitration allows them to select more favorable arbitrators. Selection on unobservable case characteristics could explain these results, but the explanation is not very plausible. One would have to believe that cases arbitrated against firms who are subject to frequent arbitration have unobservable characteristics that lend themselves to arbitrators who hand out low awards. Following the same logic, cases with specialized arbitration (PIABA) attorneys or those who were part of a trust, would have to be less likely to contain the same unobservable characteristic.

IV.C.5 Other Robustness: Awards Granted versus Awards Requested

We also explicitly explore whether consumers factor in arbitrator industry/consumer friendliness when *initially* requesting awards for a case. This is inconsistent with FINRA arbitration rules, which require that awards/claims must be formally requested before the arbitration panel has been appointed and can only be amended thereafter if the arbitration panel grants a formal motion to amend (FINRA Rule 12309). Nonetheless, we investigate the possibility in Appendix E. We find no relationship between *requested* awards and arbitrator industry/consumer friendliness in each specification.

[23] An interesting question that arises is why so many consumers choose non-PIABA attorneys. One could argue that knowing that there are attorneys who specialize in securities arbitration already requires a high level of information / sophistication from consumers. In other words, the reasons why these consumers do not choose a specialized attorney might be similar to ones that explain the need for a specialized attorney in the first place.

IV.C.6 Summary

In this section, we study the alternative explanation to our results. Under the alternative, both firms and consumers are equally sophisticated. Then arbitrators we measure as being industry friendly are not *industry friendly*. They are instead systematically selected to cases whose unobservable case characteristics merit lower awards and are systematically selected to more cases. We find no evidence of such selection on observed case characteristics. The 2007 reform evidence is difficult to reconcile with this alternative, unless it coincides with differential changes in the types of cases assigned to pro-industry or pro-consumer arbitrators. We find no evidence of a differential assignment of cases on observable characteristics post reform. We also find no evidence of such selection using Altonji et al. (2005) and Oster (2019) style tests. If such selection does exist, it seems very implausible. It implies that cases with unobservable case characteristics that merit lower awards have to be relatively more common than other cases. Arbitrators who have lost an arbitration case themselves (as financial advisers) are a good match for cases with these low merit (unobserved) characteristics, while arbitrators who have been fired by financial firms are a bad match for cases with these low merit (unobserved) characteristics. These cases with low merit unobservable characteristics would also have to be more common at firms that have many arbitration cases and less common in cases with specialized PIABA attorneys and cases involving consumer trusts. While it is possible that the unobserved case/arbitrator characteristic has the following (convoluted) correlation structure, our explanation is substantially simpler, more plausible, and consistent with the anecdotal evidence from practitioners and regulators.

V A Model of Arbitrator Competition

Our empirical analysis suggests that firms possess an informational advantage in choosing arbitrators from within a given pool of arbitrators. Arbitrators who are more industry friendly than other arbitrators are more likely to be selected to a case. Because arbitrators can choose how they rule on a case, our analysis suggests that arbitrators have an incentive to be more industry friendly than other arbitrators. In other words, arbitrators compete with other arbitrators in industry friendliness. Intuitively, this competition between arbitrators should change the overall industry and consumer friendliness of the arbitrator pool as a whole. The analysis we perform in the first part of the paper cannot measure the extent of the competition effect. This is by design: to eliminate as much variation as possible, we compare how industry-friendly arbitrators are *relative to each other*. Fixed effects sweep away the average level of pro-industry or pro-consumer tilt of the arbitration pool. To isolate this average, we next develop a stylized but quantitative model of consumer arbitration. The model is informed by our empirical findings and the institutional details laid out in Section II, but explicitly models how arbitrators compete on industry friendliness. Specifically, we model arbitrators'

endogenous choice of how to slant their decisions in order to increase their chances of being selected by the informed party. We use the model for several purposes.

First, the model allows us to recover the beliefs of arbitrators, i.e., the awards that arbitrators would have chosen absent incentives provided by the arbitration selection mechanism. This allows us to quantify the full extent of firms' informational advantage. More importantly, we can compute the pro-industry tilt in the arbitration pool *as a whole* that arises because of arbitrator competition. We can compare this change in the entire pool to the advantage of choosing pro-industry arbitrators from a given pool. This allows us to decompose firms' informational advantage into these two distinct components, i.e., advantage of striking from a fixed arbitrator pool, and the equilibrium effect on the pool itself.

Second, we use the calibrated model to show that these insights are critical in assessing changes to arbitration design both qualitatively and quantitatively. The model highlights how competition between arbitrators can be desirable if both parties are informed, but leads to more biased outcomes, if one party holds an informational advantage. Policy changes that aim to help consumers but ignore the informational advantage of firms end up hurting consumers instead. We use the calibrated model to compute gains or losses to consumers from alternative arbitrator selection schemes proposed by regulators, which have been touted to improve arbitration outcomes for consumers (FINRA Notice 14-49, 2014). Finally, while we apply the model to securities arbitration, its features are equally applicable to consumer arbitration proceedings more generally and to other arbitrator selection mechanisms (discussed in Appendix D).

V.A Set Up

The consumer (claimant) and firm (respondent) are arbitrating a claim that will be overseen by one of the available arbitrators who determines the award. The timing is as follows. First, arbitrators choose their slant, i.e., how industry or consumer friendly they are going to be. In choosing slant, arbitrators commit to how they will award a case to the participants. Second, following the institutional design for arbitrator selection, a list of arbitrators is randomly chosen from the pool of all available arbitrators. The consumer and firm can strike a limited number of arbitrators from the list. Among the remaining arbitrators, one is selected randomly. Lastly, the selected arbitrator is paid a fee for arbitrating the case and awards are paid to the parties. Next, we describe the incentives and information structure of the problem in more detail.

V.A.1 Consumers, Firms, and Arbitrators

Consumers and Firms: The award is the share of the requested damages $a_G \epsilon [\underline{a}, \bar{a}]$ that is granted to the consumer. Because the award is just a transfer from the firm to the consumer, it is a zero-sum game. We denote the payoff to the consumer as $U_C = a_G$ and the payoff to the firm as $U_R = -a_G$. For simplicity of exposition, we assume both parties are risk neutral. Risk aversion does not change the parties' strategies

for selecting arbitrators, or the resulting equilibrium. It would affect parties' preferences over alternative arbitration mechanisms, which we discuss in the Appendix C.

Arbitrators: Arbitrators trade-off monetary incentives of being selected on a case against the psychological costs of departing from their view of a "fair" award. This allows us to nest the extreme cases of arbitrators who are motivated purely by monetary incentives, as well as arbitrators who are motivated only by fairness concerns. As we discuss below, both features are important in order to capture arbitrator behavior in the data.

Conditional on the observable case characteristics, each arbitrator has an inherent belief $b_i \epsilon \left[\underline{b}, \overline{b}\right]$ regarding the fair award for the arbitration case.[24] We can think of these beliefs as innate characteristics that arbitrators bring to the case. These could be formed based on their prior work experience, education, upbringing, or personal interaction with the industry. As our results in Section IV.A.1 suggest, an arbitrator who has lost a consumer case herself may believe that investors frequently file baseless claims resulting in a low b_i. An arbitrator who was fired from a financial firm may believe that the financial industry frequently takes advantage of employees and consumers,and has a high b_i. The distribution of beliefs among arbitrators in the population is $F(\cdot)$; the density $f(\cdot) = F'(\cdot)$ is continuous and strictly positive everywhere. We can think of the distribution of inherent beliefs as the distribution of awards that would arise if arbitrators were selected to the cases randomly, with no input from the parties in the case.

Arbitrators earn a fee (fee) if they are selected to arbitrate a case. The probability that a given arbitrator i will be selected depends on the firm's and consumer's expectations of the award a_i that the arbitrator would grant were she selected, the arbitrator's "slant." For simplicity, we assume that arbitrators can pre-commit to what they would award for a case a_i before being selected on the panel. The idea is that, just as in the data, arbitrators can choose their slant, i.e., how industry friendly they want to be. Instead of modeling the reputation building process, which is not the focus of this paper, we assume that arbitrators choose their slant before arbitrating a case. To keep the notation simple, arbitrators' slant directly commits them to an award, rather than a noisy unbiased signal of the award, which would not alter the analysis.

Arbitrators can have a sense of fairness. When their decisions depart from their beliefs of fair awards, $a_i \neq b_i$, they suffer a disutility of $\theta |a_i - b_i|$. The parameter θ measures the weight that an arbitrator places on fairness relative to the monetary payoffs from arbitration. A lower θ implies that arbitrators care more about monetary payoffs. In the extreme case that arbitrators care only about monetary payoffs, $\theta = 0$. As $\theta \to \infty$ arbitrators are motivated only by their fairness beliefs, and do not respond to monetary incentives– i.e., $a_i = b_i$ so an arbitrator's slant represents just their underlying beliefs.

[24]The idea that arbitrators have an inherent notion of a "fair" outcome goes back to early models of arbitration (Crawford, 1979; Farber 1979, 1980; Farber and Katz, 1979; Ashenfelter and Bloom 1984; De Clippel et al., 2014)

Let $G(\cdot)$ be the equilibrium distribution of arbitrators' chosen slant, and denote the equilibrium probability that an arbitrator with slant a_i is chosen as $\Gamma(a_i, G(\cdot))$. As we show later, an arbitrator's probability of being chosen depends on her slant, as well as the slant of other arbitrators in the pool. An arbitrator's expected utility depends on her expected probability of being selected on the case, Γ, the fee she earns from arbitrating, fee, and the award she grants relative to her beliefs:

$$U(b_i, a_i) = \Gamma(a_i, G(\cdot))(fee - \theta|a_i - b_i|). \tag{6}$$

Consumer Sophistication:

In the empirical setting we study, firms are frequently large institutions that engage in arbitration repeatedly, while consumers typically engage only in arbitration once. Consistent with our empirical setting and analysis, we assume that firms are the informed party. They recognize arbitrators' slants and can therefore predict their awards when choosing among them. Consumers, however, are uninformed, and do not observe/anticipate how a given arbitrator will award a case.

V.A.2 Arbitration Selection Process and Uninformed Consumers

N risk neutral arbitrators are randomly drawn from the population of arbitrators $A = \{a_1, a_2, ..., a_n\}$ and the "list" is presented to the parties. Both the consumer and firm simultaneously submit k arbitrators to be struck from the list of available arbitrators, where $k < \frac{n}{2}$. Among the remaining arbitrators, one is chosen randomly. The chosen arbitrator j grants the award according to their chosen slant $a_G = a_j$. Firms observe the slant $a_1, a_2, ..., a_n$ of each arbitrator appearing on the randomly generated list. Consumers, being uninformed, do not observe the slant. Given the equilibrium distribution of slant $G(\cdot)$, we denote $\widetilde{G}(\cdot)$ the distribution of awards granted through arbitration, $a_G \sim \widetilde{G}(\cdot)$.

V.A.3 Equilibrium Definition

We study a pure monotone strategy symmetric Bayesian Nash equilibrium, which is characterized by the optimal behavior of consumers, firms, and arbitrators. Firms and consumers optimally strike arbitrators from the arbitration pool to maximize their utility given the set of arbitrator A and holding the strategy of the opposing party fixed. Arbitrators maximize their expected utility (eq. 6) by choosing their slant and taking the strategies of firms, consumers, and other arbitrators in the pool as given.

V.B Equilibrium: Arbitrator Selection and the Arbitrator Pool

Here, we illustrate two related advantages that informed parties hold over uninformed parties. First, given a population of arbitrators, consumers and firms influence the outcome by eliminating arbitrators from the pool. In other words, if firms are better informed than consumers, they can choose more favorable arbitrators. Second, arbitrators compete to be selected to the arbitration panel. We show how this competition

can be beneficial when both parties are equally informed, but that when only one party is informed, arbitrators have incentives to slant the awards they grant in the favor of the informed party. We highlight how competition among arbitrators exacerbates the pro-industry slant in arbitration outcomes.

V.B.1 Arbitrator Selection from a Fixed Pool

We first analyze which arbitrators are selected by consumers and firms, taking the arbitrator pool as given, i.e. given the equilibrium distribution of slant, $G(\cdot)$. Let $A = \{a_1, ..., a_n\}$ denote the list of arbitrators randomly drawn from the population. Without any loss in generality, arbitrators are indexed such that the most industry-friendly arbitrator who grants the lowest awards is indexed by 1 and the least industry-friendly arbitrator who grants the highest awards is indexed by n such that $a_1 < a_2 < ... < a_n$.

The incentives of firms and consumers are straightforward. The firm, being informed, will find it optimal to always strike the arbitrators with the k highest (most consumer-friendly) slant. By contrast, uninformed consumers randomly strike k arbitrators. An arbitrator is randomly selected from the pool of eligible (non stricken) arbitrators. Then the equilibrium probability that an arbitrator with slant a_i will be selected on the panel, given the distribution of other arbitrator slant in the population, is:

$$\Gamma(a, G(.)) = \frac{1}{n-k} P(a_i; 1, n-k, n). \tag{7}$$

where $P(a_i; l, m, n) = \sum_{j=l}^{m} \frac{(n-1)!}{(j-1)!(n-j)!} G(a_i)^{j-1}(1-G(a_i))^{n-j}$ denotes the probability that the arbitrator is between the $l'th$ and $m'th$ order statistics among a sample of n arbitrators.

This expression highlights the role of different information structures in the selection of arbitrators for a given arbitrator pool. Firms strike the k most consumer-friendly arbitrators with the highest slant. Thus, an arbitrator is selected only if her slant is one of the $n-k$ lowest order statistics among the set of n arbitrators. The probability an arbitrator is selected is then decreasing in her slant a, and arbitrators who are more industry friendly are more likely to be selected. Conversely, if consumers are informed then the arbitrators in either tail of the distribution face elimination, and the probability that an arbitrator is selected becomes:

$$\Gamma(a, G(.)) = \frac{1}{n-2k} P(a_i; k+1, n-k, n).$$

Informed consumers remove k arbitrators with the most pro-industry (lowest) slant, and firms remove the the k arbitrators with the highest slant. Thus, an arbitrator is selected only if she is one of the $k+1 : n-k$ middle order statistics of the distribution of slant among the set of N arbitrators appearing on the list. The striking mechanism helps eliminate extreme outcomes, and the closer an arbitrator's slant (a) is to the median, the higher the probability she is selected. This discussion illustrates that assuming that parties in arbitration are equally informed has important consequences on how we think about the design of the arbitration system and the corresponding arbitration outcomes.

V.B.2 Arbitrator Pool: Equilibrium Choice of Slant

Our discussion above holds the distribution of arbitrator slant fixed. In other words, it does not account for arbitrators' incentives to be selected on the panel. Arbitrators, however, can choose how they rule on cases, and can therefore choose how consumer or industry friendly they want to be. We show that competition among arbitrators can be desirable if both parties are equally informed. But in the presence of an information gap competition leads to the whole pool of arbitrators becoming industry friendly. Next, we characterize how the primitives of the model affect the severity of the equilibrium shift in the pool.

When arbitrators choose slant, they trade off two forces. On the one hand, they want to be selected on the arbitration panel (increase $\Gamma(a_i, G(\cdot)))$ to earn the arbitration fee fee. To do so, they want to choose a slant that will minimize their chance of being struck from the arbitrator panel by an informed firm or consumer. This probability is determined by their slant *relative* to other arbitrators. On the other hand, choosing awards that depart from their convictions, $a_i - b_i$, causes disutility. Arbitrator i with inherent belief b_i chooses slant a_i to maximize her expected utility given the choices of other arbitrators:

$$max_{a_i}\Gamma(a_i, G(\cdot))\left(fee - \theta\left|a_i - b_i\right|\right). \tag{8}$$

We look for a monotone equilibrium: arbitrators with more consumer-friendly beliefs choose a more consumer-friendly slant. For ease of intuition, assume that $\Gamma\left(a_i; G(\cdot)\right)$ is differentiable. The corresponding first order condition can be written as:

$$|a_i - b_i| = \frac{fee}{\theta} - sgn(a_i - b_i) \times \frac{\Gamma\left(a_i; G(\cdot)\right)}{\gamma\left(a_i; G(\cdot)\right)} \forall a_i \neq b_i. \tag{9}$$

where $\gamma(a_i; G(\cdot) = \frac{\partial\Gamma(a_i;G(\cdot))}{\partial a}$. An arbitrator's choice of slant relative to their underlying beliefs b_i depends on the trade-off between the costs and benefits of slant. Firms eliminate the k most consumer-friendly arbitrators from the pool. Therefore the probability an arbitrator is selected is decreasing in her slant a, $\gamma(a, G(\cdot)) < 0$. This implies that $a_i \leq b_i$. The choice in slant becomes:

$$a_i = \min\left\{b_i - \frac{fee}{\theta} - \frac{\Gamma\left(a_i; G(\cdot)\right)}{\gamma\left(a_i; G(\cdot)\right)}, b_i\right\}. \tag{10}$$

This expression shows the extent of an individual arbitrator's pro-industry slant. All arbitrators choose their slant to be more industry friendly than their underlying belief, $a_i < b_i$, as long as $\frac{fee}{\theta} + \frac{\Gamma(a_i;G(\cdot))}{\gamma(a_i;G(\cdot))} > 0$. The term $\frac{\Gamma(a_i;G(\cdot))}{\gamma(a_i;G(\cdot))}$ measures the inverse of the relative change in the probability of being selected for a marginal change in arbitrator's slant, holding other arbitrators' slant choices fixed. The term $\frac{fee}{\theta}$ is the fee that the arbitrator earns in utility terms if she is selected. Arbitrators who choose their slant equal to their beliefs ($a_i = b_i$) will award what they think is fair if the marginal benefit of slanting their award is less than the marginal cost when $a_i = b_i$ such that $\frac{fee}{\theta} + \frac{\Gamma(b_i;G(\cdot))}{\gamma(b_i;G(\cdot))} \leq 0$. In other words, arbitrators will find it optimal to skew pro-industry and grant lower awards relative to their true beliefs. We can express the distribution of

equilibrium probabilities as a function of the equilibrium distribution of slant:

$$a_i = \min \left\{ b_i - \frac{fee}{\theta} - \sum_{j=1}^{n-k} \binom{n-1}{j-1} \frac{(n-1)!}{(j-1)!(n-j)!} \frac{G(a_i)(1-G(a_i)}{g(a_i)\left(j-1-(n-1)G(a_i)\right)}, b_i \right\}$$

This equation is at the center of our estimation approach in Section VI. We also compute a closed form expression for the equilibrium distribution of arbitrator slant as a function of model primitives: the distribution of beliefs, the size of the list from which arbitrators are chosen, and the number of strikes from the list (see derivation in Appendix B):

$$a_i = \min \left\{ b_i - \frac{fee}{\theta} + \frac{\int_{b_i}^{\bar{b}} \Gamma(\tilde{b}, F(\cdot)) d\tilde{b}}{\Gamma(b, F(\cdot))}, b_i \right\}. \tag{11}$$

We use the closed form expression (11) when computing counterfactual equilibria, which link the model with actual policy proposals in Section VI.C.

V.B.3 Discussion: Sources of Informational Advantage and Statistical Exchangeability

A key result from the model is that there are two sources of benefits to the party (firms) that possess an informational advantage in selecting arbitrators. Together they result in substantially lower awards for uninformed party (consumers) relative to what arbitrators believe is fair. We illustrate these two sources of benefits in Figure 5, which displays the densities of awards granted $\widetilde{g}(\cdot)$, slant $g(\cdot)$, and beliefs $f(\cdot)$ that we estimate in the proceeding section. First, the informed party has the advantage of striking arbitrators from a given pool in eq. (7). The k most consumer-friendly arbitrators are struck from the randomly generated list. This striking effect shifts the distribution of awards granted $\widetilde{G}(\cdot)$ downwards (in terms of first order stochastic dominance) relative to the equilibrium distribution of arbitrator slant, $G(\cdot)$. Intuitively, this is the effect documented by our reduced form analysis in Section IV.B.

Second, the striking behavior of the informed party induces competition among arbitrators, leading to the whole pool of arbitrators becoming industry friendly. Formally, arbitrators compete to be selected to earn the fee. They do so by deviating from their beliefs in choosing a more pro-industry slant, $a_i \leq b_i$ (eq. 10 and 11). The distribution of arbitrator slant $G(\cdot)$ is industry friendly relative to the distribution of arbitrator beliefs on what a "fair" award should be, $F(\cdot)$, again, in the sense of first order stochastic dominance. Jointly, these two effects result in a distribution of awards for consumers that are lower than what arbitrators believe is fair: the distribution of awards granted $\widetilde{G}(\cdot)$ is stochastically dominated by the underlying distribution of arbitrator beliefs $F(\cdot)$, as illustrated in Figure 5 .

The competition effect can be very large. In the extreme example where arbitrators care only about monetary incentives ($\lim \theta \to 0$), the competition effect results in a race to the bottom: all arbitrators have the most industry-friendly arbitrator slant possible $a_i = \underline{a}$. To see why, imagine that the equilibrium

distribution of arbitrators, $G(\cdot)$, features different arbitrator slants. Then there is an arbitrator with the most pro-consumer slant, $\tilde{a}$. This arbitrator will certainly be eliminated by the informed firm, so she will never be selected on an arbitration panel. If she instead chooses a slant, that is more industry friendly than that of other arbitrators, then she will surely be selected if she is on the list, increasing her expected monetary payoff. Since she has no fairness concerns, there is no utility cost to changing her slant, so choosing the most industry-friendly slant is clearly a profitable deviation. We calibrate the model in Section VI to measure the magnitude of the competition effect and compare it to the advantage of striking from a fixed pool.

This example also highlights how our model links to a common fairness criterion in arbitration, "Statistical Exchangeability." Statistical Exchangeability of arbitrators implies that the identity of the arbitrator (their pro-industry or consumer slant) does not affect arbitration outcomes. Arbitrator exchangeability is therefore frequently seen as a sign of fairness, and arises through competition between arbitrators if both parties in arbitration are equally informed (Ashenfelter, 1987). As our example above illustrates, this is not the case in our model when consumers are uninformed. The competition effect results in Statistical Exchangeability, because all arbitrators reach the same decision $a_i = \underline{a}$. On the other hand, this decision is quite "unfair," since all arbitrators are as industry friendly as possible.

VI Informational Advantage Decomposition and Policy Analysis

In this section we calibrate the model to quantify the advantage of the informed party in the current system, and decompose the advantage into two components: the striking advantage from a fixed arbitrator pool, and the competition effect that shapes the pro-industry tilt of the arbitrator pool. In Section VI.C we use this model to study whether and to which extent different arbitrator selection schemes benefit the industry versus consumers. Rather than considering a complete re-design of the system, we examine changes to the features of the existing system of choosing and compensating arbitrators, quantitatively linking the model with current and past policy proposals.

VI.A Calibration

We calibrate the model using an approach that resembles the methodology developed in the auction literature by Guerre, Perrigne, and Vuong (2000). We use the observed distribution of arbitrator fixed effects to recover the underlying distribution of slant $G(\cdot)$ and the underlying distribution of arbitrator beliefs $F(\cdot)$. The idea is that an arbitrator's choice of slant in equilibrium is a best response to other arbitrators' choices of slant. From the data, we can measure other arbitrators' equilibrium choices of slant a_i, as we describe below. Given the other arbitrators' equilibrium choice of slant, we can infer every arbitrator's true beliefs b_i

from her own choice of slant a_i as follows:

$$b_i = \max\left\{ a_i + \frac{fee}{\theta} + \sum_{j=1}^{n-k} \binom{n-1}{j-1} \frac{(n-1)!}{(j-1)!(n-j)!} \frac{G(a_i)(1-G(a_i))}{g(a_i)\left(j-1-(n-1)G(a_i)\right)}, a_i \right\}. \tag{12}$$

In order to recover the true beliefs, b_i for an arbitrator with slant a_i, we need to observe the arbitrator fee, disutility from deviating from one's beliefs θ (which we have to calibrate) and the unconditional density and distribution of arbitrator slant $G(\cdot)$ and $g(\cdot)$. We parameterize and calibrate the model as follows.

We estimate the distribution $G(\cdot)$ and density $g(\cdot)$ of slant non parametrically in the data. We use the empirical Bayes estimates of arbitrator fixed effects to estimate the equilibrium distribution of slant. In the data, we observe the distribution of slant, conditional on arbitrators being chosen, $\tilde{G}(\cdot)$, rather than the distribution of arbitrators in the population $G(\cdot)$. Because k consumer-friendliest arbitrators are removed from the randomly generated list of n arbitrators, we observe the distribution of slant a_i conditional on a_i not being one of the k highest order statistics. Formally, the distribution $\widetilde{G}(\cdot)$ represents a weighted average of the $n-k$ first order statistics of $G(\cdot)$. To obtain the unconditional distribution of slant, $G(\cdot)$, we proceed in two steps. We first estimate $\widetilde{G}(\cdot)$ from the data non parametrically using the empirical distribution function. Then, we use the model to invert into the underlying distribution given firms' striking behavior,

$$\widetilde{G}(a_i) = \sum_{i=k}^{n-1} \left(\sum_{j=n-i}^{n} \frac{n!}{j!(n-j)!} G(a_i)^j (1-G(a_i))^{n-j} \right). \tag{13}$$

numerically solving for $G(\cdot)$. We also estimate the density of the slant distribution $g(\cdot)$. The density of arbitrator slant among selected arbitrators $\tilde{g}(a)$ is equal to the unconditional density $g(a)$ multiplied by the probability of being selected $n \times \Gamma(a, G(\cdot))$:$\tilde{g}(a) = g(a) \times n \times \Gamma(a, G(\cdot))$. We estimate $g(\cdot)$ non-parametrically using kernel density estimation where we weight each observation by our estimates of the inverse probability of being selected $\frac{1}{\Gamma(a,\widehat{G}(\cdot))}$.[25]

We next calibrate the parameters fee and θ, which measure the trade-off between monetary incentives and the cost of deviating from arbitrators' beliefs. Only their relative trade-off $\frac{fee}{\theta}$ matters in equilibrium (eq. 12). Arbitrators earn $\$300$ per hearing and the typical case lasts four days (FINRA Rule 12214), so we set the per-case fee equal to $fee = \$1,200$. We calibrate θ, which reflects the cost of deviating from an arbitrator's true beliefs using the 2007 rule change. As we describe in Section IV.C.1, the number of strikes available to firms and consumers decreased from nine to four. We examine how arbitrators responded to the rule change by re-estimating eq. (1) around the rule change. All else equal, with fewer strikes, there is a smaller chance that any given arbitrator is one of the k most consumer-friendly arbitrators who will be struck. Reducing the number of strikes curtails an arbitrator's incentive to slant their decisions in favor of the industry. Consistent with this intuition, our regression estimates indicate that after the 2007 rule

[25] Specifically, we use a Gaussian kernel and a smoothing parameter of 3%, which is in line with Silverman's Rule of Thumb (1986).

change, arbitrators increased awards by 3pp, on average. We calibrate the model to match this average change in awards when the number of strikes shifts from nine to four in the model. This calibration yields $\theta = 17,000$. This estimate implies that arbitrators are willing to deviate from their beliefs by 1pp for an extra \$170 increase in income. In other words, suppose the arbitrator believed that a fair award was to simply grant 100% of the amount requested. The arbitrator would be willing to grant an award of 90% in exchange for an extra \$1,700 increase in income. Because potential non-pecuniary benefits of being an arbitrator are difficult to measure (see Section II.B), we experiment with alternative calibrations, in which we scale $\frac{fee}{\theta}$ by 50% and 150% in the Appendix E. The alternative parameterizations yield similar inferences in Section VI.C.

Once we have obtained the magnitudes of disutility from deviating from one's beliefs θ, arbitrator compensation fee, and the unconditional density and distribution of arbitrator slant $\widehat{G(a)}$ and $\widehat{g(a)}$, we use eq. (12) to compute the density of arbitrators' beliefs of what a fair award would be, $(\widehat{f(b)})$.

VI.B The Cost of "Industry-Friendly" Arbitration for Consumers: A Decomposition

VI.B.1 The Cost of "Industry-Friendly" Arbitration for Consumers

We use our calibrated model to evaluate the cost to consumers because firms have an informational advantage. Figure 5 displays our non parametric estimation results, and Figure A4 in Appendix E displays our corresponding parametric estimates, where we assume that the underlying distribution of beliefs follows a gamma distribution.[26] The primary object of interest is the distribution of arbitrators' inherent beliefs of the appropriate arbitration awards, $f(b_i)$. We consider two ways to quantify firms' informational advantage.

The first is to compare arbitration outcomes under the current system to how consumers would fare if arbitrators were selected to the cases randomly, like judges in some courts, i.e., if parties in the case would have no input in the selection. Figure 5 shows how, relative to this scenario and under the current arbitration selection scheme, the density of arbitration awards in equilibrium $\widetilde{g}(\cdot)$ shifts to be more industry friendly relative to the distribution of arbitrators' inherent beliefs $f(\cdot)$. The average award in the data is 50% of the amount requested. If neither party had any input into the selection process, our estimates suggest that the mean award would be 55%. Given that the average award is on the order of \$800,000, the model estimates suggest that the current arbitrator selection scheme costs consumers roughly 5pp, or \$40,000 dollars. The shift in the distribution of awards affects the top half of the distribution more: the 10^{th} percentile award declines from 41% to 40%, while the 90^{th} percentile declines from 74% to 63%. In other words, the arbitration system especially decreases the propensity of large awards to consumers. The results show the extent to which the current arbitration scheme results in a biased distribution of arbitration awards

[26] We also estimate the parameterized distribution of beliefs via maximum likelihood to match the non-parametrically estimated distribution of beliefs. Both models estimate the same competition effect, but the parametric model estimates a larger effect of striking.

relative to the underlying distribution of beliefs of fair awards.

The second method to benchmark the effect of firms' informational advantage in arbitration outcomes is to estimate outcomes under the assumption that consumers are as informed as firms (Figure 6). In Appendix B, we show that when both parties are informed, the arbitrator selection mechanism results in a distribution of arbitration awards that is a median preserving contraction of arbitrators' underlying beliefs. The intuition for this result is straightforward, and is broadly the intuition used to rationalize the use of the arbitrator selection mechanism. Firms strike most pro-consumer arbitrators, and informed consumers strike the most pro-industry arbitrators, increasing the selection probability of arbitrators in the middle of the distribution. Because of striking, arbitrators are incentivized to choose a slant toward the median of the distribution. If both parties are informed, the arbitration selection mechanism results in a median preserving outcome, such that the median award equals the median belief. This is in sharp contrast to the scenario when only firms are informed, where the arbitration mechanism results in a lower mean and median award relative to the underlying distribution of arbitrator beliefs.

The current mechanism does reduce the variance of outcomes, which is an often touted benefit of the arbitration selection process. Even if only firms are informed, the variance of outcomes is reduced by 40% relative to the variance of beliefs. When consumers are informed as well, the selection mechanism reduces the variance of arbitration awards by 67% relative to the variance of beliefs. Therefore, ignoring the fact that consumers are uninformed also overstates the benefits from reducing the variance of outcomes in arbitration.

VI.B.2 Decomposition: Striking and Competition

The current arbitrator selection scheme costs consumers roughly 5pp of awards, or $40,000 dollars. Figure 5 decomposes that advantage into two components. The first is the advantage that firms derive in striking arbitrators from a given pool. This is measured as the shift between the awards granted $\widetilde{g}(\cdot)$ and the density of equilibrium slant $g(\cdot)$. Because firms strike the most consumer-friendly arbitrators, the mean award is roughly 3pp lower than the equilibrium density of slant $g(\cdot)$. The striking advantage of firms therefore accounts for approximately 60% of the total information that firms derive from striking.

In response to incentives provided by selection, arbitrators compete to be selected by choosing a pro-industry slant a that is biased relative to their beliefs b. Intuitively, we compare how individual arbitrators are ruling to how they would rule in absence of incentives provided by selection, i.e., their belief of a fair ruling. Formally, the magnitude of the competition effect is illustrated by comparing the distribution of slant $g(a)$ with the distribution of beliefs $f(b)$ (dashed line). The average arbitrator slant is roughly 2pp lower than their beliefs. In other words, the average arbitrator gives out an award that is 2pp lower than what she believes is fair because doing so increases her probability of being selected for arbitration. 40% of firms'

total informational advantage, therefore, comes from changes in the arbitrator pool as a whole. Recall that we cannot measure this aspect using the reduced form fixed effects approach in the first part of the paper.

Another interpretation of the competition effect is that it is the advantage that the industry holds even over *an individual consumer who is as informed* as the industry. Formally, consider a situation in which only a measure zero of consumers are informed—for example, because they purchase expertise by hiring PIABA attorneys as in Section IV.C.4 (see Appendix B for a formal treatment). This consumer would be as good as firms in striking arbitrators from a given pool. Nevertheless, she would be at a disadvantage. The whole pool would still have a pro-industry tilt because the ex ante chances of arbitrators being struck by an informed consumer are essentially zero. This result also implies that the aggregate consumer benefits from being informed as a group are larger than the sum of informed individuals. If all consumers are informed, then the competition effect is eliminated.

In other words, being informed generates positive externalities for other consumers because the presence of informed consumers incentivizes arbitrators to develop a reputation for being consumer friendly. Because individual consumers do not internalize the benefits of every consumer being informed, this externality opens the door for potential regulation. One example of such regulation is the prohibition on arbitration clauses, which rule out class action claims. For example, the CFPB proposed a rule preventing companies from using mandatory arbitration clauses, which was overturned by Congress ("New protections against mandatory arbitration," 2017).

VI.C Changing the Arbitrator Selection System

We use our model to quantitatively investigate different arbitrator selection schemes. Rather than considering a complete redesign of the system, we examine changes to the features of the existing system of choosing and compensating arbitrators. We study how changing the number of strikes (k), the size of the list/pool from which arbitrators are struck (n), and changing the fee (fee) would alter the award distribution and affect the slant in arbitration. One reason to study these counterfactuals is that FINRA has considered changing the arbitration system along these dimensions. More broadly, these policy changes were proposed with the idea that the arbitration process might lead to more "fair" outcomes for the consumer. We show that instead of achieving the intended objective, the outcomes are by and large more industry friendly once one considers the informational advantage that firms hold in the arbitration process.

To estimate the counterfactuals, we numerically solve for the updated slant strategies given the change in the arbitrator selection scheme and underlying arbitrator beliefs. In Appendix B, we formally solve for the optimal choice of arbitrators' slant for each counterfactual. For computational convenience, we use the parametrically estimated belief distribution that is displayed in Figure A4 in Appendix E rather than our

nonparametric estimates. As noted before, both models produce very similar estimates.

Changing the Number of Strikes: One dimension of arbitration selection that has been altered in the past, and which is actively being considered again is altering the number of arbitrators that each party can strike from the list. FINRA proposed increasing the number of strikes from four to six in 2016, allowing the parties more control over the process. We present the changes in awards as the number of strikes increases from one to seven in Figure 7a. As the number of strikes increases, the awards distribution becomes more favorable to the industry. Consider the concrete example of FINRA's proposed changes of increasing strikes from four to six. The average award we observe in the data when both parties are allowed four strikes, $k = 4$, is 50%. As the number of strikes increases to six, $k = 6$, our estimates suggest that the average award will decline by 4pp. This change partially occurs because firms are able to select more favorable arbitrators from the list, but also because arbitrators are incentivized to act more industry friendly. These results are consistent with our earlier reduced form findings from Section IV.C.1. When FINRA reduced the number of strikes available to firms and consumers, consumer-friendly arbitrators were more likely to be selected than previously. This counterfactual illustrates that increasing the control that the parties have over the process increases the slant in arbitration outcomes when consumers are uninformed. This result stands in stark contrast to consequences of this policy if consumers were informed. Then, increasing the number of strikes would indeed shrink the distribution of awards towards the more "fair" median outcome.

Increasing the Arbitration List Size: Another dimension that has been considered is allowing the parties to choose from a wider pool of arbitrators. To aid consumers, in 2016 FINRA proposed that rather than striking arbitrators among a list of 10 arbitrators, firms and consumers would choose from among a list of 15. Figure 7b illustrates that this change would benefit consumers. With the increased list size, arbitrators are less likely to be selected in general. All else equal, a given pro-consumer arbitrator is less likely to be one of the k most consumer-friendly arbitrators on the list, and thus is less likely eliminated. Figure 7b indicates that arbitrators would also be slightly less biased relative to their beliefs if they were chosen from a larger list. Holding the number of strikes fixed, increasing the number of arbitrators from 10 to 15 increases the average award by 1pp.

FINRA most recently proposed increasing the number of arbitrators on the list to 15, and simultaneously increasing the number of strikes to 6. Increasing the number of strikes increases the pro-industry slant, but increasing the list size decreases it. Figure 7c illustrates that the proposed policy change would further increase the pro-industry slant, but the effects are modest. The average award decreases by 0.5pp.

Changing Arbitrator Compensation: Another policy proposal that has been frequently considered is to increase the fees paid to arbitrators (FINRA Notice 14-49, 2014). The idea is that higher fees will provide

arbitrators with higher powered incentives to set aside their biases, and instead work towards reaching a fair outcome; i.e., that awards will be closer to the median. If consumers are as informed as firms, this is indeed the case. Figure 7d displays the counterfactual distribution of awards if FINRA doubled the fee paid to arbitrators. Figure 7d shows that doubling the fee paid to the arbitrator will cause the average award to decrease by 4pp. The intuition is simple: increasing the fee paid to the arbitrator further incentivizes the arbitrator to be selected. With higher powered incentives, arbitrators are more willing to be industry friendly in order to increase the probability of being selected. This counterfactual again illustrates that policies, which would potentially improve arbitration outcomes if consumers were informed, worsen the pro-industry slant in arbitration outcomes when consumers are uninformed. These results also suggest that lower powered incentives for arbitrators, coupled with a flat wage, could decrease the pro-industry slant in arbitration.

VII Conclusion

We argue that firms have an informational advantage over consumers in selecting arbitrators in consumer arbitration and document how the selection process impacts arbitration outcomes. We use securities disputes as a laboratory for our study. Securities disputes present a good laboratory for arbitration: arbitration is mandatory for all disputes so it eliminates selection concerns, the parties choose arbitrators from a randomly generated list, and this selection mechanism is similar to other major arbitration forums.

Here, we want to highlight some more speculative implications of our findings. The estimates from our model suggest a substantial pro-industry tilt in the arbitration pool that, because of arbitrator competition, accounts for 40% of the informational advantage. Individual consumers, even if they are fully informed, cannot avoid this equilibrium effect. Being informed generates positive externalities for other consumers because the presence of informed consumers incentivizes arbitrators to develop a reputation for being consumer friendly. Because individual consumers do not internalize the benefits of every consumer being informed, this externality opens the door for potential regulation. While analyzing such regulations is beyond the scope of the current paper, one example of such regulation is the prohibition on arbitration clauses, which rule out class action claims. For example, the CFPB proposed a rule preventing companies from using mandatory arbitration clauses, which was overturned by Congress in 2017.

Our counterfactuals suggest that re-designing incentive compensation and arbitrator selection design can ameliorate the pro-industry tilt, but only if the design accounts for uninformed consumers. We show examples of policies, such as increasing arbitrator compensation or giving parties more choice, benefit consumers if they are informed, but hurt them if they are uninformed. One avenue for future research is to examine the extent to which this result is generic. More broadly, our findings suggest that limiting the firm's and consumer's inputs over the arbitrator selection process could significantly improve outcomes for consumers.

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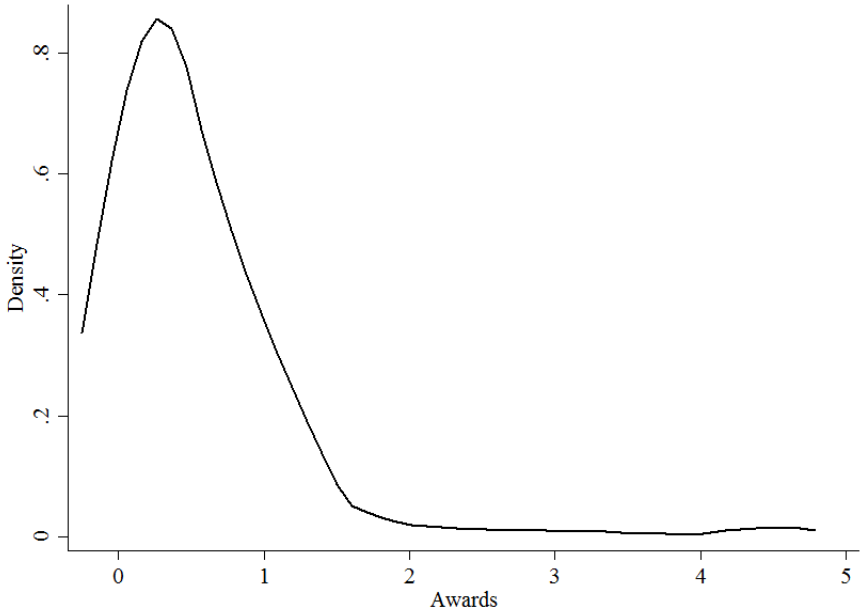
Figure 1: Award Distribution

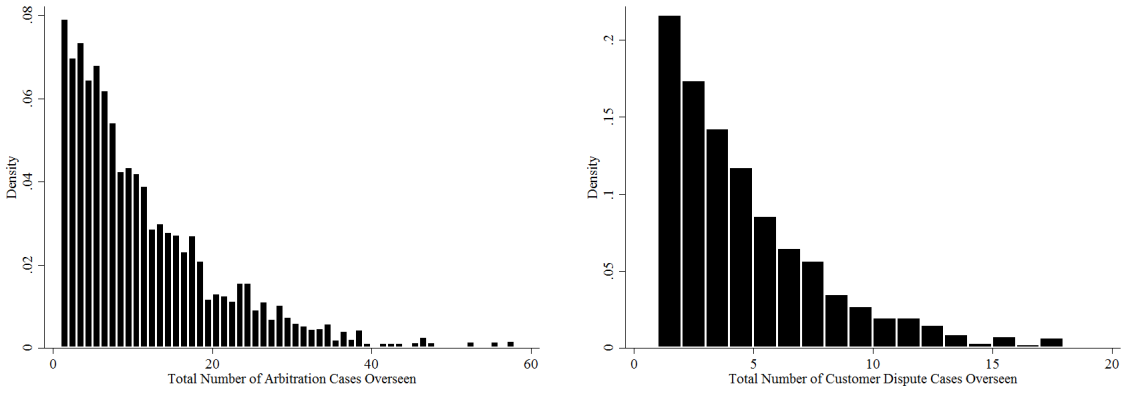


Note: Figure 1 displays the distribution of arbitration Awards. We calculate Awards as the percentage of awards granted through arbitration divided by the awards initially requested by the consumer. The arbitration panel can grant an award that is greater than the amount requested which is why Awards can be greater than 1. For example, the arbitration panel could award punitive damages. The distribution of Awards is winsorized at the 1% level. The sample consists of 8,828 different arbitration cases over the period 1988-2015.

Figure 2: Arbitrator Experience and Differences

(a) Arbitrator Experience

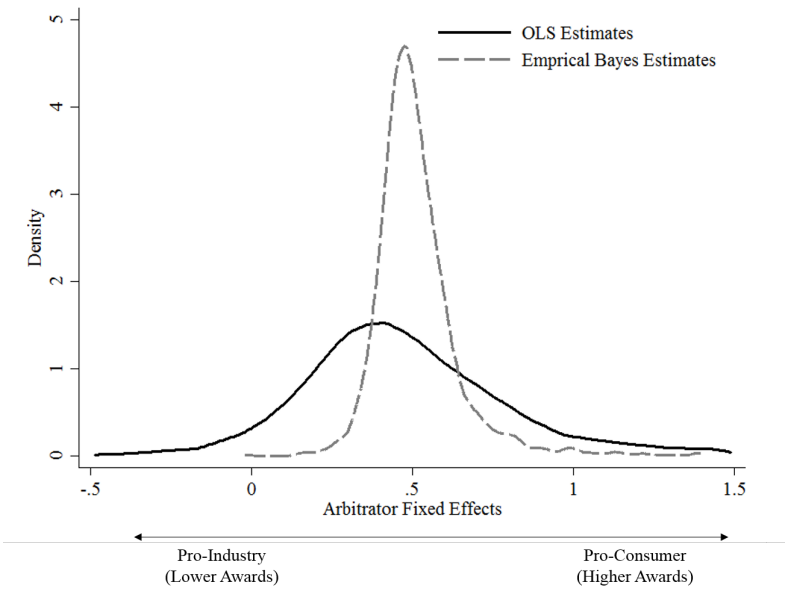
(b) Arbitrator Experience(Consumer Disputes Only)



Note: Figure 2a displays the lifetime experience of an arbitrator in terms of the number of cases she oversaw during her career. Observations are at the arbitrator-by-case level over the period 1988-2015 (35,353 observations). Figure 2b displays the lifetime experience of an arbitrator in terms of the number of consumer dispute cases she oversaw during her career. Observations are at the arbitrator by consumer-dispute case level over the period 1988-2015 (20,231 observations).

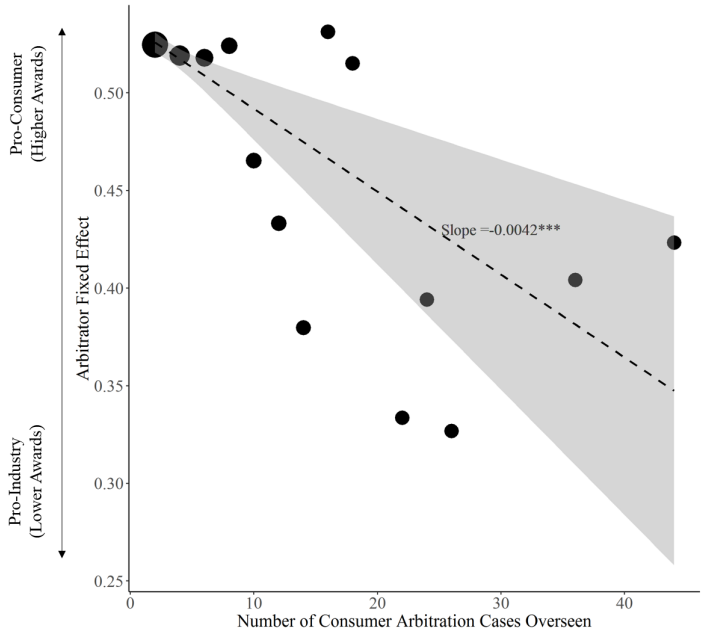
Figure 2: Arbitrator Differences

(c) Arbitrator Fixed Effects



Note: Figure 2c displays the estimated distribution of arbitrator fixed effects corresponding to eq. (1) and Table 2. The black empirical density reflects the distribution of OLS estimated arbitrator fixed effects estimated and the gray empirical density reflects the distribution of empirical Bayes estimated arbitrator fixed effects. We normalize the mean of the distribution of fixed effects to 51% which is the average Award in our sample.

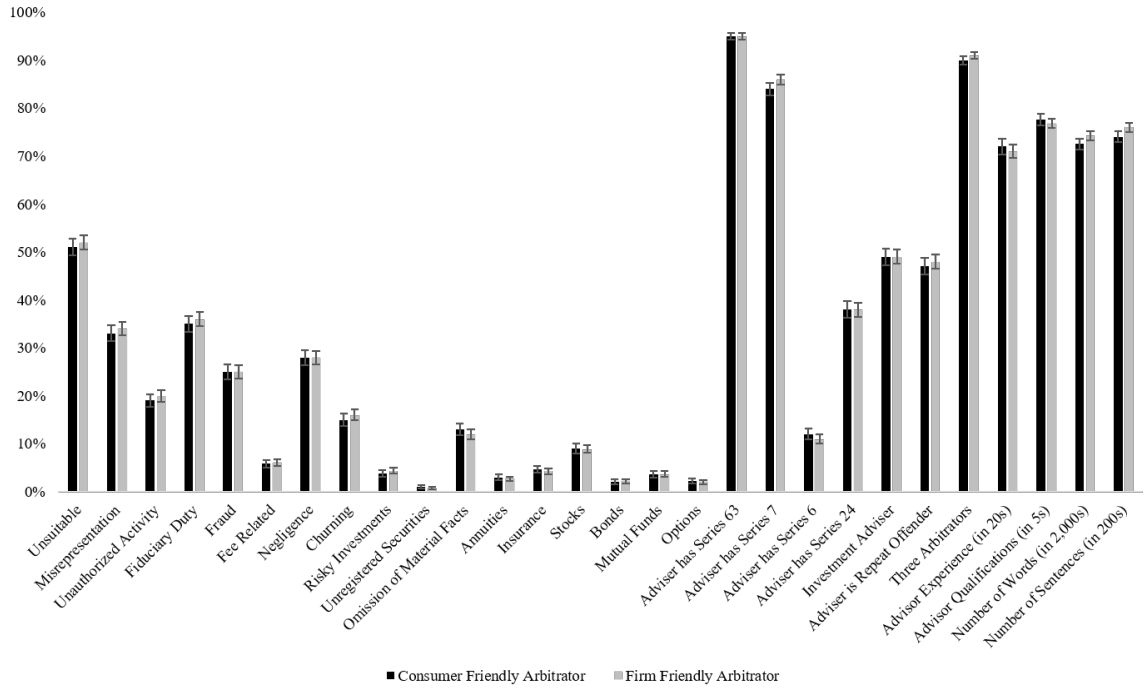
Figure 3: Are Industry Friendly Arbitrators Selected More Frequently?



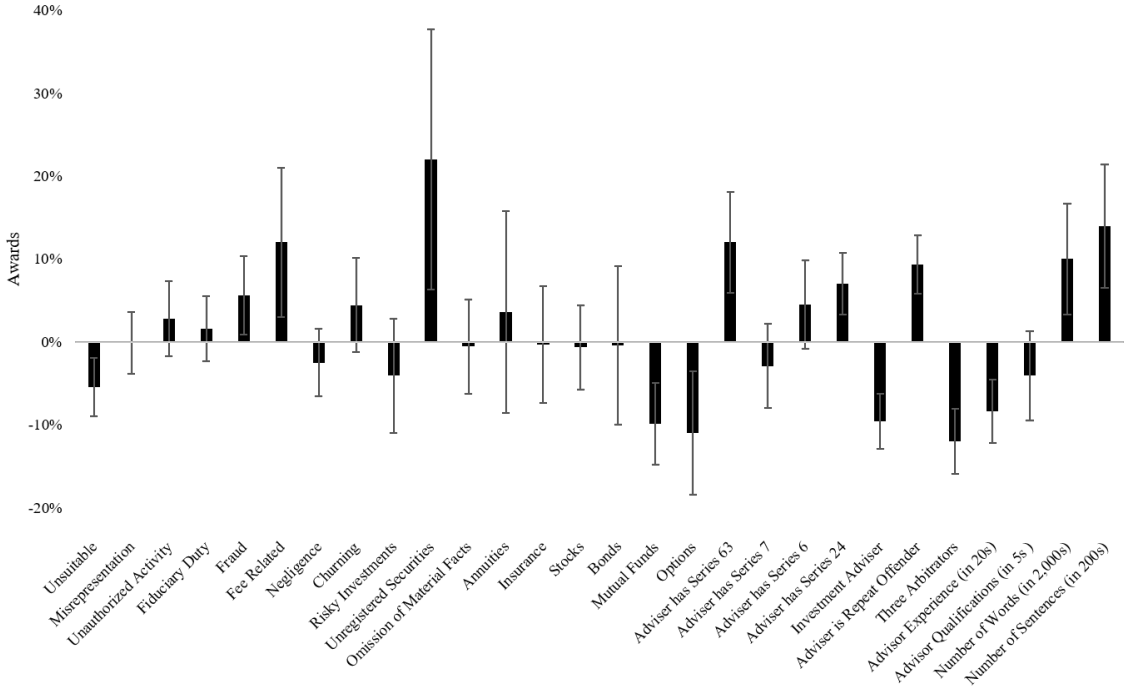
Note: Figure 3 displays a binned scatter plot of the arbitrator fixed effects versus the total number of cases the arbitrator oversaw in her career. Observations are at the arbitrator level over the period 1988-2015. The size of the bubble corresponds to the number of arbitrators in the bin. The gray shaded area reflects the 90% confidence interval for the corresponding weighted least squares regression of an arbitrator's fixed effect vs. the total number of cases the arbitrator oversaw in her career. We normalize the mean of the distribution of fixed effects to 51% which is the average Award in our sample. *** p<0.01, ** p<0.05, * p<0.10.

Figure 4: Observable Case Characteristics and Arbitrator Selection

(a) Observable Case Characteristics by Arbitrator Type

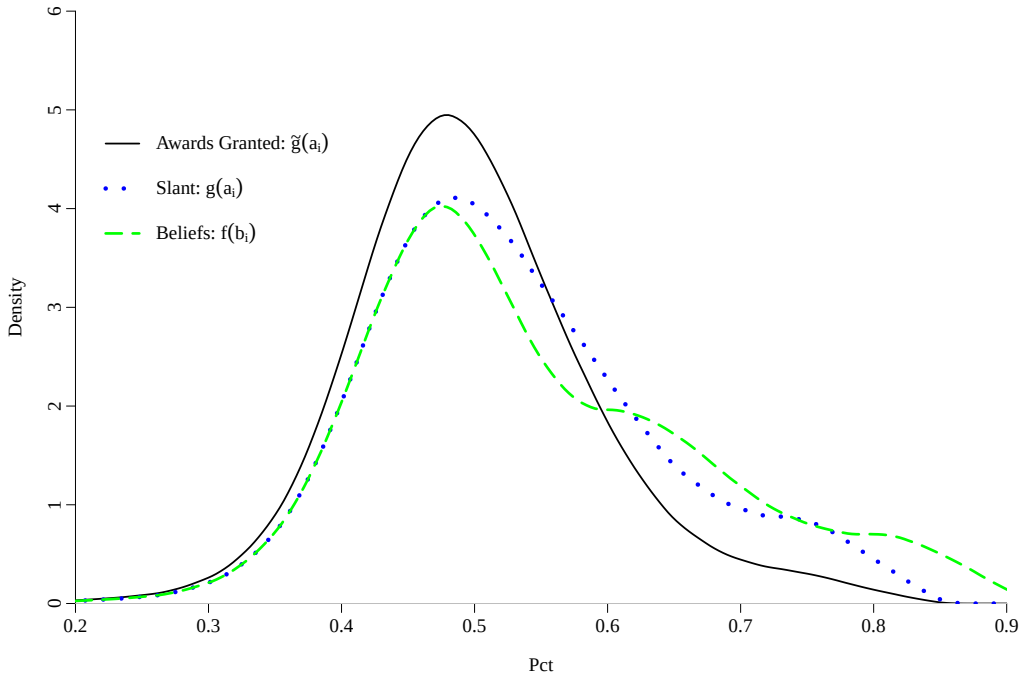


(b) Awards by Observable Case Characteristics



Note: Figure 4a displays the average of case characteristics by whether the arbitrator is consumer friendly (above mean fixed effect) or industry friendly (below mean fixed effect). Figure 4b displays the results corresponding to a univariate regression of case awards on observable case characteristics mirroring that of eq. (1). Each column corresponds to the estimated coefficient. The standard error bars in both panels (a) and (b) correspond to 95% confidence intervals. For scaling puproses, in both panels we report the non-dummy variables (adviser experience, adviser qualifications, etc.) in units such that the average case characteristic is in the interval 0-1.

Figure 5: Estimated Distribution of Arbitrator Beliefs, Slant, and Awards



Note: Figure 5 displays the estimated density of awards among the conditional distribution of selected arbitrators $\tilde{g}(a)$, the density of slant among the unconditional (entire) population of arbitrators $g(a)$, and the distribution of true beliefs among the unconditional (entire) population of arbitrators $f(b)$. The distributions correspond to our non-parametric estimates as described in Section VI.

Figure 6: Distribution of Arbitrator Beliefs, Slant, and Awards—Informed Consumers

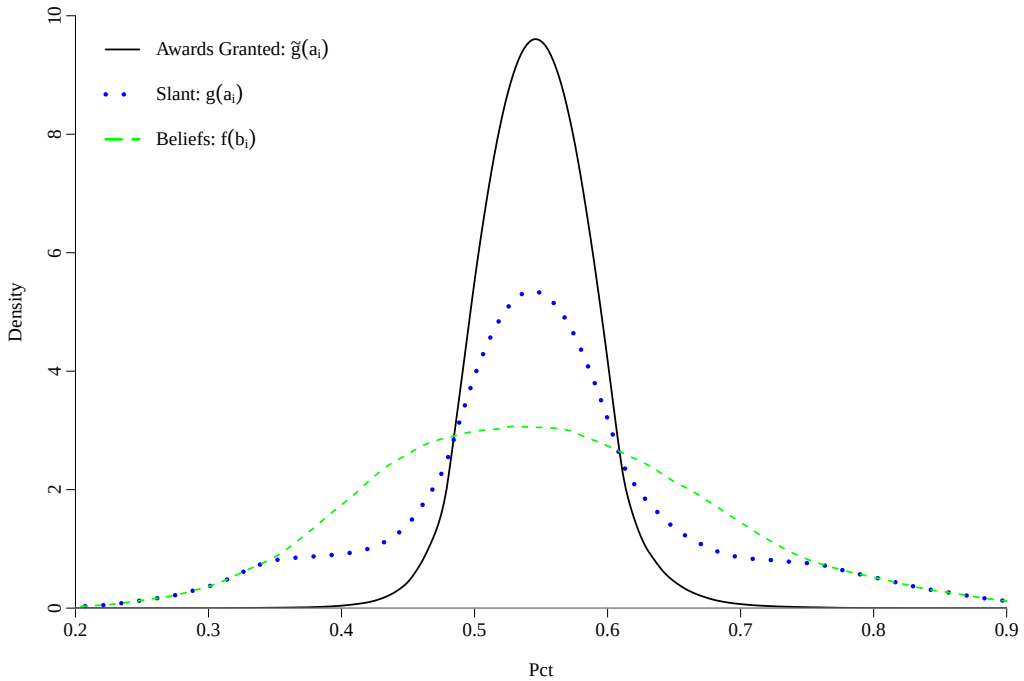
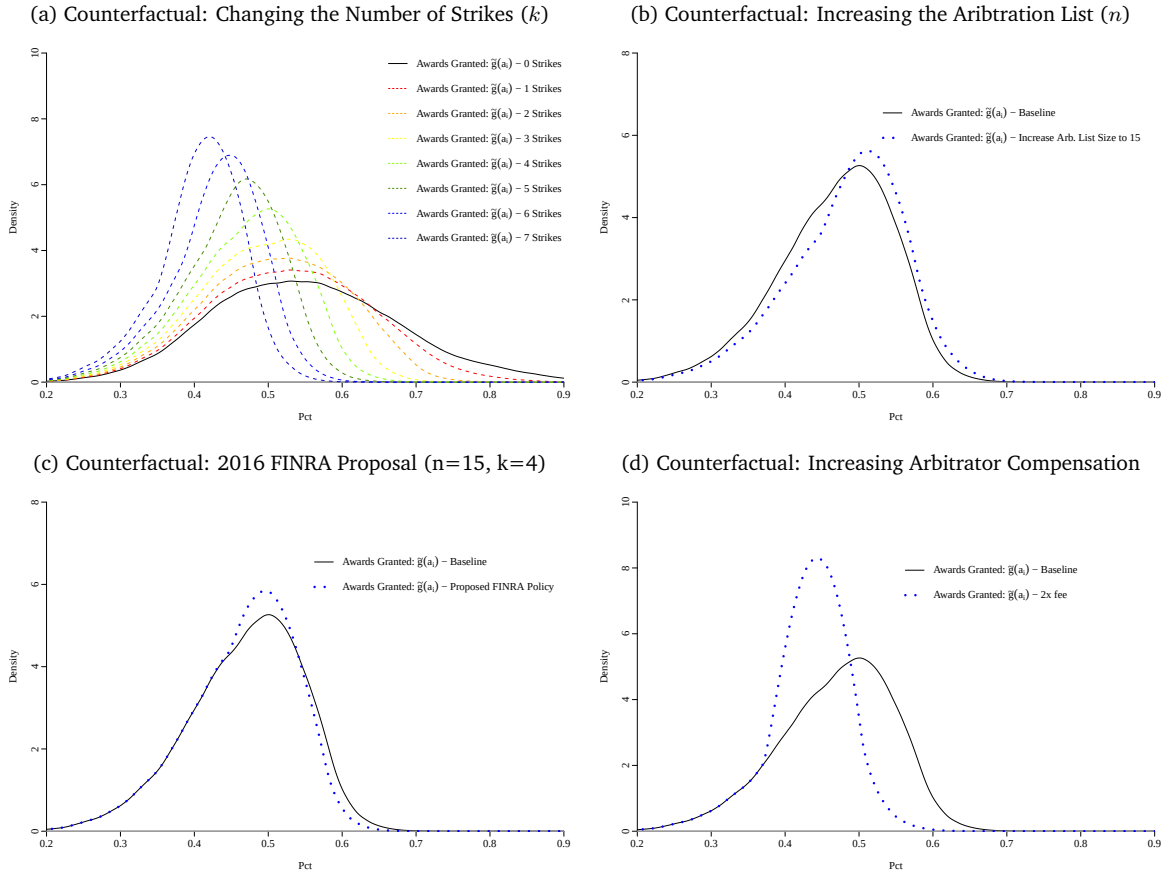


Figure 6 displays the model implied density of awards among the conditional distribution of selected arbitrators $\tilde{g}(a)$, the density of slant among the unconditional (entire) population of arbitrators $g(a)$, and the distribution of true beliefs among the unconditional (entire) population of arbitrators $f(b)$ if both parties are informed. The underlying distribution of arbitrator beliefs corresponds to our estimated parametric specification as described in Section VI.

Figure 7: Arbitration Awards Under Alternative Selection Mechanisms



Note: Panels (a)-(d) display the distribution of arbitration awards if regulators were to (a) change the number of strikes, (b) increase the number of arbitrators on the list from ten to fifteen, (c) increase the number of arbitrators on the list to fifteen and increase the number of strikes to six (FINRA's recent proposal) and (d) double the fee paid to arbitrators from \$1,200 to \$2,400. For each counterfactual we compute the counterfactual distributions of arbitrator slant and awards where we fix the distribution of beliefs and solve for the optimal choice for arbitrators' slant as we change parameters of the arbitrator selection process. We derive and characterize each arbitrator's optimal choice of slant in Appendix B. We obtain the distribution of beliefs from our parametric estimates as described in Section VI. Figure 7a displays the distribution of arbitration awards as a function of the number arbitrators firms are able to remove/strike from the arbitration pool. Each dashed line corresponds to a different number of strikes, and the dashed lines get darker as the number of strikes increases. Figure 7b displays the counterfactual distribution of arbitrator slant and awards if regulators were to increase the arbitration list size from ten to fifteen. The dotted (solid) line displays the counterfactual (baseline) distribution of arbitration awards if we were to increase the arbitration list from ten to fifteen. Figure 7c displays the distribution off arbitration awards following FINRA's most recent proposal which increases the size of the arbitration list from ten to fifteen and the number of strikes from four to six. The dotted (solid) line displays the counterfactual (baseline) distribution of arbitration awards corresponding to FINRA's most recent proposal. Figure 7d displays the counterfactual distribution of arbitrator slant and awards if regulators were to double the fee (fee) paid to arbitrators. The dotted (solid) line displays the counterfactual (baseline) distribution of arbitration awards if we were to increase the fee paid to arbitrators.

Table 1: Arbitration Summary Statistics

(a) Consumer Dispute Case Characteristics

Variable	Obs	Mean	Std. Dev.	Median
Requested Awards	20,196	785,025	4,867,927	175,000
Percent of Requested Awards Granted	20,231	51%	67%	32%
Allegations:				
Unsuitible	20,231	51%		
Fiduciairy	20,231	34%		
Misrepresentation	20,231	33%		
Negligence	20,231	27%		
Fraud	20,231	24%		
Unauthorized Activity	20,231	20%		
Products:				
Stocks	20,231	9%		
Insurance	20,231	5%		
Mutual Fund	20,231	3%		
Annuity	20,231	3%		
Bonds	20,231	2%		
Options	20,231	2%		
Complexity:				
Number of Allegations	20,231	2.3	1.7	2
Length of the Case Document: Words	19,451	1,430	649	1399
Length of the Case Document: Sentences	19,451	145	65	140
Offending Adviser Characteristics:				
Experience	20,231	14.5	9.2	13.0
No. Qualifications	20,231	3.9	1.6	4.0
Prior Record of Misconduct	20,231	48%		
Series 6	20,231	11%		
Series 7	20,231	85%		
Series 24	20,231	38%		
Series 65 or 66	20,231	49%		
Consumer Claimant Representation:				
Self-represented/No Attorney	20,231	11.7%		
Represented by an Attorney	20,231	88.3%		
Represented by a PIABA Attorney	20,231	17.0%		
Consumer is a Trust	20,231	7.3%		

Table 1: Arbitration Summary Statistics

(b) Arbitrator Characteristics

Variable	Obs	Mean	Std. Dev.	Median
Number of Arbitration Cases Overseen				
Total Number of Arbitration Cases	20,231	8.3	8.8	5.0
Total Number of Consumer Dispute Cases	20,231	4.5	4.3	3.0
Arbitrator Experience in the Financial Advisory Industry				
Former/Current Financial Adviser	20,231	40%		
Years Experience as an Adviser	8,147	8.9	12.1	8.0
Number of Regulatory Qualifications	8,147	3.3	1.6	3.0
Prev. Terminated for Cause	8,147	11%		
Regulatory Offense	8,147	20%		
Involved in Arbitration as a Respondent:				
Award was Granted	8,147	8%		
No Award Granted/Case Withdrawn	8,147	22%		

Note: Table 1 displays the summary statistics corresponding to our arbitration data set. Observations are at the arbitrator-by-case level, and correspond to 8,828 distinct consumer arbitration cases. The data set consists of the universe of consumer dispute arbitration cases reported in both FINRA's Arbitration Awards data and FINRA's BrokerCheck data over the period 1988-2015. Panel (a) corresponds to the consumer dispute arbitration case characteristics. The *Percent of Requested Awards Granted* is winsorized at the 1% level. The categories Allegations and Products are dummy variables indicating whether the specific product or allegation were mentioned in the arbitration case summary in BrokerCheck. The categories are not mutually exclusive and may sum up to more than 100%. We measure the complexity of each case based on the number of allegations in the case and based on the length of the associated FINRA arbitration award case document in terms of the number of words and sentences. We do not observe the case award document for 1,684 of our 8,828 cases. *Prior Record of Misconduct* indicates whether or not the adviser has a past record of misconduct in the financial advisory industry as defined in Egan, Matvos and Seru (2019). The variable *PIABA attorney* indicates whether the consumer used a attorney who specializes in arbitration and is a member of the Public Investors Arbitration Bar Association. *Trust* indicates that the consumer is part of a trust.

Panel (b) describes arbitrator characteristics for arbitrators involved in consumer dispute arbitration cases. The total number of arbitration cases includes all arbitration cases the arbitrator was involved in including non-consumer related arbitration. *Former/Current Financial Adviser* is a dummy variable indicating whether the arbitrator is a financial adviser. Years Experience as an Adviser measures the years an arbitrator was employed as a financial adviser. Number of Regulatory Qualifications measures the number of regulatory qualifications arbitrators had when they were registered as financial advisers. *Prev. Terminated for Cause* is a dummy variable indicating that the arbitrator was terminated for cause while he/she was employed as a financial adviser. *Regulatory Offense* is a dummy variable indicating that the arbitrator violated one or more investment related rules or regulations while working as a financial adviser. *Involved in Arbitration as a Respondent: Award was Granted* is a dummy variable indicating that the arbitrator was a respondent in a securities arbitration dispute that resulted in a customer award. *Involved in Arbitration as a Respondent: No Award Granted/Case Withdrawn* is a dummy variable indicating that the arbitrator was a respondent in a securities related customer dispute where the case was either withdrawn by the customer or no award was granted to the customer.

Table 2: Percent of Requested Awards Granted

	(1)	(2)	(3)	(4)	(5)
Allegations:					
Unsuitable	-3.16*	-2.67	-1.77	-1.43	-1.74
	(1.64)	(1.78)	(1.91)	(1.94)	(1.91)
Misrepresentation	-0.98	-1.84	-0.66	-0.92	-0.64
	(1.78)	(1.95)	(2.17)	(2.12)	(2.17)
Unauthorized Activity	-0.86	-0.21	0.88	0.92	0.80
	(2.17)	(2.32)	(2.41)	(2.50)	(2.40)
Omission of Key Facts	-1.18	0.10	-0.35	-0.13	-0.32
	(2.62)	(2.87)	(2.95)	(2.97)	(2.95)
Fee/Commission Related	11.2***	7.42*	9.76**	10.2**	9.70**
	(4.31)	(4.18)	(4.22)	(4.52)	(4.22)
Fraud	4.81*	4.56*	6.61**	5.67**	6.62**
	(2.53)	(2.74)	(3.02)	(2.83)	(3.02)
Fiduciary Duty	1.37	3.16	-0.37	0.95	-0.41
	(2.22)	(2.42)	(2.55)	(2.59)	(2.55)
Negligence	-4.43*	-5.62**	-6.23**	-6.96**	-6.18**
	(2.36)	(2.53)	(2.78)	(2.77)	(2.78)
Risky Investments	-0.82	0.55	1.79	-0.43	1.76
	(3.24)	(3.58)	(3.66)	(4.12)	(3.66)
Churning/ Excessive Trading	1.64	1.83	-1.05	-3.39	-1.02
	(2.63)	(2.80)	(2.93)	(2.93)	(2.93)
Unregistered Securities	17.8**	15.6	1.45	1.83	1.43
	(8.43)	(9.62)	(11.9)	(11.2)	(11.9)
Products:					
Insurance	4.69	4.08	7.08	1.00	7.25
	(4.16)	(3.86)	(4.57)	(4.04)	(4.57)
Annuity	7.59	6.70	12.8*	7.51	12.7*
	(6.16)	(6.08)	(7.40)	(7.52)	(7.40)
Stocks	1.22	0.45	0.41	2.96	0.40
	(2.51)	(2.72)	(3.02)	(3.04)	(3.01)
Mutual Funds	-9.59***	-7.45***	-10.1***	-5.72	-10.1***
	(2.67)	(2.80)	(3.45)	(4.11)	(3.44)
Bonds	-0.71	0.49	-7.33**	-5.51	-7.29**
	(3.96)	(4.33)	(3.49)	(4.29)	(3.49)
Options	-8.83**	-9.98**	-12.8***	-13.5***	-12.9***
	(3.64)	(3.96)	(4.32)	(5.09)	(4.32)
Adviser Characteristics:					
Prior Misconduct	6.54***	7.04***	6.94***	6.51***	6.94***
	(1.70)	(1.80)	(1.96)	(1.98)	(1.95)
Arbitrator Characteristics:					
Former/Current Financial Adviser					-1.51*
					(0.85)
Prev. Terminated for Cause					6.17**
					(2.49)
Regulatory Offense					1.38
					(1.85)
Involved in Arbitration as a Respondent					
Award Granted					-4.17*
					(2.43)
No Award Granted					1.88
					(1.88)
Year F.E.	X	X	X	X	X
Adviser County F.E.		X	X	X	X
Arbitration Location F.E.		X	X	X	X
Firm F.E.			X	X	X
Arbitrator F.E.				X	
Observations	19,451	18,630	18,505	15,168	18,505
R-squared	0.043	0.115	0.3853	0.626	0.3854

Note: Table 2 displays the regression results for a linear regression model (eq. 1). Observations are at the arbitrator-by-case level over the period 1988-2015. The dependent variable is Awards and is measured as awards granted through arbitration divided by awards requested. Prior Misconduct indicates whether or not the adviser has been previously reprimanded for misconduct. Arbitrator characteristics indicate whether the arbitrator has ever worked as a financial adviser, been terminated in the finanical advisory industry for cause, had any regulatory offenses, and been involved in arbitration as a respondent. The variables Award Granted and No Award Granted are dummy variables that indicate whether the arbitrator paid out an award when he/she was the respondent. We also control for the case size, the arbitration panel size, the case length in terms of the number of sentences and words, and other adviser controls. Other adviser controls include the corresponding adviser's experience and qualifications: Series 6, Series 7, Series 24, Series 63, Series 65/66, and number of other qualifications. In the full specification (column 4) we include arbitrator fixed effects. The F-test for whether arbitrator fixed effects are jointly significantly different from each other is significant at 1%. Standard errors are clustered at the case level. *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Are Industry Friendly Arbitrators Selected More Frequently?

	(1)	(2)	(3)
Arbitrator Fixed Effects	-1.27***	-1.06***	-1.15***
	(0.37)	(0.37)	(0.39)
Arbitrator Controls		X	X
Year F.E.		X	X
Location F.E.			X
Adviser County F.E.			X
Observations	61,559	61,559	60,981
R-squared	0.047	0.055	0.069

Note: Table 3 display the regression results corresponding to a linear probability model (eq. 2). Observations are at the arbitrator-by-year level over the period 1988-2015. The dependent variable is a dummy variable indicating whether an arbitrator was selected in a given year. Arbitrator Fixed Effects are empirical Bayes estimated arbitrator fixed effects as described in Section IV.A. A higher Arbitrator Fixed Effect indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. To ease interpretation of the regression results, we standardized the Arbitrator Fixed Effects such that they are in units of standard deviation. We control for the number of years the arbitrator has been active in the industry. We include year fixed effects, fixed effects for the location of the arbitration proceedings, and fixed effects for the county of the adviser's office location. The hearing location fixed effects and adviser office location county fixed effects correspond to the last consumer dispute case the arbitrator oversaw. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Industry Friendly Arbitrator Selection and the 2008 Rule Change

	(1)	(2)	(3)
Arbitrator F.E.	-4.57***	-4.53***	-4.11***
	(1.41)	(1.41)	(1.53)
Arbitrator F.E.$\times$(1999$\leq$Year$<$2008)	2.85*	3.07**	2.47
	(1.56)	(1.56)	(1.67)
Arbitrator F.E. $\times$(Year$\geq$2008)	4.37***	4.54***	3.96**
	(1.47)	(1.46)	(1.58)
Arbitrator Controls		X	X
Year F.E.		X	X
Location F.E.			X
Adviser County F.E.			X
Observations	61,559	61,559	60,981
R-squared	0.047	0.055	0.069

Note: Table 4 displays the regression results corresponding to a linear probability model (3). Observations are at the arbitrator-by-case level over the period 1988-2015. The dependent variable is a dummy variable indicating whether an arbitrator was selected in a given year. Arbitrator Fixed Effects are empirical Bayes estimated arbitrator fixed effects as described in Section IV.A. A higher Arbitrator Fixed Effect indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. To ease interpretation of the regression results, we standardized the Arbitrator Fixed Effects such that they are in units of standard deviation. We also control for the number of years the arbitrator has been active in the industry. We include year fixed effects, fixed effects for the location of the arbitration proceedings, and fixed effects for the county of the adviser's office location. The hearing location fixed effects and adviser office location county fixed effects correspond to the last consumer dispute case the arbitrator oversaw. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Industry Friendly Arbitrator Selection and Consumer Sophisitication

	(1)	(2)
Attorney	3.08***	3.48***
	(1.17)	(1.31)
PIABA Attorney	6.00***	6.75***
	(0.89)	(0.96)
Trust	10.2***	9.60***
	(1.29)	(1.39)
Firm Experience	-2.87***	-2.11***
	(0.67)	(0.76)
Other Controls	X	X
Year F.E.		X
Adviser County F.E.		X
Observations	15,155	15,155
R-squared	0.026	0.093

Note: Table 5 displays the regression results for a linear regression model (eq. 5). Observations are at the arbitrator-by-case level over the period 1988-2015 and come from our consumer dispute arbitration data set. The dependent variable is the selected Arbitrator's Fixed Effect as calculated in column (4) of Table 2. A higher Arbitrator Fixed Effect indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. Attorney is a dummy variable indicating whether the consumer used an Attorney. PIABA Attorney indicates whether the consumer used a attorney who specializes in arbitration and is a member of the of the Public Investors Arbitration Bar Association. Trust indicates whether the consumer claimant is a trust. Firm Experience is a dummy variable indicating whether the firm has above median experience in terms of the number of arbitration cases it has been involved in. The omitted category is consumers who are self-represented, not part of a trust, and are facing firms with below average experience. Coefficients are in percentage points. Other Controls include case size, the arbitration panel size, the case length in terms of the number of words, and other adviser characteristics. Other controls also include the corresponding adviser's qualifications: Series 6, Series 7, Series 24, Series 63, Series 65/66, and number of other qualifications. Standard errors are clustered at the case level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Appendices for Online Publication

Appendix A: Additional Robustness and Counterfactuals

A.1 Backward Looking Measure of Arbitrator Consumer Friendliness

Our previous results suggest that there are persistent and statistically significant differences in how individual arbitrators grant awards. In other words, our estimates suggest that the particular arbitrator who oversees a hearing has a substantial impact on the case outcome. Here we build on those findings to examine whether past judgments by an arbitrator are predictive of future judgments.

We construct a backwards looking measure of industry friendliness that firms could use to forecast the behavior of arbitrators. Using the residuals from the estimation results reported in column (2) of Table 2, we construct a measure of how friendly arbitrator l's decision regarding case i as:

$$\delta_{ijklt} = Pct\_Awarded_{ijklt} - \hat{\beta}X_i - \hat{\mu}_k - \hat{\mu}_t. \tag{14}$$

where i indexes the arbitration case, j indexes the financial advisory firm involved in the case, k indexes location, l indexes the arbitrator, and t indexes time. The terms $\hat{\beta}$, $\hat{\mu}_k$ and $\hat{\mu}_k$ correspond to the estimated coefficients and location and time fixed effects. We construct our measure of arbitrator past consumer/industry friendliness $\bar{\delta}_{lt}$, as the average of the residuals (δ_{ijklt}) from the cases arbitrator l previously oversaw. A higher $\bar{\delta}_{lt}$ implies that the arbitrator is less industry friendly and more consumer friendly.

We examine how an arbitrator's past decisions impact the probability she is selected as an arbitrator again in the future more formally in the following linear probability model.

$$Selected_{lt} = \beta X_{lt} + \gamma\bar{\delta}_{lt} + \eta_{lt} \tag{15}$$

Our observations are at the arbitrator by year level. Selected is a dummy variable that indicates whether or not arbitrator l was selected for a case in year t. The key independent variable of interest is the arbitrator's past slant $\bar{\delta}_{lt}$, which is computed as the average of the residuals (δ_{ijklt}) from the cases arbitrator l previously oversaw. The term X_{lt} is a vector of arbitrator controls that include the number of years she's been active in the industry, number of cases in the data set she has overseen, whether or not she worked as a financial adviser, and whether or not she has a record of misconduct as a financial adviser. We also include year fixed effects and fixed effects for the location of the past case the arbitrator worked on. Our sample represents an unbalanced panel of arbitrators over the period 1988-2015. An arbitrator enters the data set as soon as she oversees her first case and remains in the data set until 2015.

We report the corresponding estimates in Table A1. In columns (1)-(3) we examine the probability that an arbitrator is selected to a consumer arbitration case in a given year. In columns (4)-(6) we examine the probability that an arbitrator is selected to any arbitration case, including non-consumer arbitration cases. In each specification, we estimate a negative and significant relationship between an arbitrators past slant $\bar{\delta}_{lt}$ and the probability an arbitrator is selected. Recall that a greater past slant implies that the arbitrator was more consumer friendly and less industry friendly. The results suggest that those arbitrators that are industry friendly are more likely to be selected in the future. The results in column (1) of Table A1 indicate that a one standard deviation decrease in past slant (i.e. more industry friendly) is correlated with 0.23pp increase in the probability of being selected in a given year. To put this number in perspective, the average probability that an arbitrator is selected in a given year is 6%. Hence, this amounts to a roughly four percent increase in the probability of being selected. To the extent that our measure of past slant suffers from classical measurement error, our estimates understate the true effect.

A.2 Do Consumers Account for Arbitrator Slant when Requesting Awards?

The results from Section IV.B suggests that firms hold an informational advantage over consumers when selecting arbitrators. Why aren't investors using the same information to select arbitrators? One potential explanation is that consumers account for the potential slant of the arbitrator but do so when initially requesting/claiming awards though the timing of the proceedings suggests that this is highly unlikely. FINRA arbitration rules (Rule 12309) require that claims must be formally requested/stated before the arbitration panel has been appointed, and can only be amended thereafter if the arbitration panel grants a formal motion to amend. Here, we separately examine whether either the damages requested or the damages granted is correlated with the types of arbitrators that are selected for a case.

We first examine the damages requested by a client on the arbitrator's past slant and set of additional control variables.

$$\ln(Awards\_Requested)_{ijklt} = \alpha\bar{\delta}_{lt} + \beta X_i + \mu_j + \mu_k + \mu_t + \varepsilon_{ijklt} \tag{16}$$

Observations are at the arbitrator-by-case level; i indexes the arbitration case, j indexes the financial advisory firm involved in the case, k indexes location, l indexes the arbitrator, and t indexes time. The regression specification mirrors that of eq. (1), except that our dependent variable is now the awards requested, and we also control for the arbitrators past slant $\bar{\delta}_{lt}$ which is computed as defined above (eq. 14). The key independent variable is the arbitrator's past slant. We again control for case level characteristics and include time, county, and firm fixed effects (μ_t, μ_k, μ_j).

Table A2a displays the corresponding estimation results. We find essentially no relationship between the requested awards and the arbitrator slant in each specification. The corresponding estimates are relatively precise which suggests that this finding (or lack thereof) is not due to a lack of statistical power.

We also examine the relationship between awards granted and the past slant of an arbitrator.

$$\ln(Award\_Granted)_{ijklt} = \alpha\bar{\delta}_{lt} + \beta X_i + \mu_j + \mu_k + \mu_t + \varepsilon_{ijklt} \tag{17}$$

The regression specification corresponds to that of eq. (16) other than the dependent variable. We use the same set of controls as in eq. (16) and observations are at the arbitrator-by-case level.

Table A2b displays the corresponding estimation results. In each specification, we estimate a positive relationship between the awards granted and the arbitrator's past slant, and the estimates are statistically significant in each specification. The results in column (1) suggest that a one standard deviation increase in an arbitrator's past slant is associated with an 8% increase in the award amount.

A.3 Settling vs. Arbitrating Cases

Rather than going directly to arbitration, consumers and brokerage firms can try to negotiate a settlement. In BrokerCheck data we observe the resolution of all customer disputes, including settlements. Here, we examine the types of customer disputes that are arbitrated versus settled. Similar to our consumer arbitration data set, for each dispute we also observe the allegations, product involved, and the characteristics of the offending adviser as well as the settlement/award requested by the consumer and the agreed settlement/award. We examine which consumer disputes are arbitrated versus settled using the following linear probability model:

$$Arbitration_{ijkt} = \beta X_i + \mu_j + \mu_k + \mu_t + \epsilon_{ijkt} \tag{18}$$

Observations are at the consumer dispute level; i indexes the customer dispute, j indexes the firm involved in the dispute, k indexes the location of the dispute, and t indexes time. The dependent variable $Arbitration_{ijklt}$ is a dummy variable equal to one if case i was resolved through arbitration and is equal to zero if the case was settled. The vector X_i reflects a set of case level characteristics. As described previously, we control for the 11 different allegations and six different financial products covered in the case. We also control for the size of the case in terms of the award/settlement requested. In addition, we also control for the offending adviser's experience, the six most popular qualifications, the adviser's total number of qualifications, and any past record of misconduct. We include county fixed effects corresponding to the office location of the offending adviser to account for any geographic differences in arbitration. Similarly we include fixed effects for the firm employing the adviser because firms may have different propensities to settle versus arbitrate a case.

Table A5 presents the results corresponding to our linear probability model (eq. 18). One of the best predictors of whether a case gets arbitrated is size. Larger cases, in terms of the award/settlement requested, are more likely to be arbitrated rather than settled. The results in column (3) indicate that moving from the 10^{th} to the 90^{th} percentile in terms of the size of the case, increases the probability that the case is arbitrated by 4.9%. The result is intuitive. Given arbitration is costly for the parties, and involves fees paid to the arbitrators, the cost of legal representation, and opportunity cost of time, the parties involved may choose to settle smaller cases.

A.4 Purchasing Expertise: Spillovers from Uninformed Consumers

As we show in Section IV.C.4, some consumers hire PIABA attorneys, who specialize in arbitration. The presence of these attorneys diminishes the advantage that firms hold in selecting arbitrators. Here, we study the consequences if only a small subset of consumers is informed, either because they hired an expert or because they hired a PIABA attorney. Specifically, we show that the aggregate consumer benefits from being informed as a group are larger than the sum of informed individuals. In other words, being informed has externalities. To make the point most salient, imagine that this informed consumer was not anticipated by arbitrators. Formally, the mass of informed consumers is measure zero.

Given the list of arbitrators, the informed consumer will eliminate arbitrators who have the strongest pro-industry slant. On the other hand, because arbitrators assume almost all, except measure zero, consumers are uninformed, they will choose the same pro-industry slant. The informed consumer's choose arbitrators from the same pool $G(\cdot)$, but eliminate the k most pro-industry arbitrators. Our estimates suggest that a measure zero informed consumer's award is on average 6pp higher than that of an uninformed consumer (Figure A8.).

Second, this implies that the value of being informed for any individual consumer is smaller than the joint value of all consumers being informed. The estimates from our parametric model imply that the average gain for any individual consumer is 6pp, while the average gain, if all consumers are informed is 8pp.[27] The wedge arises because each individual consumer cannot change the distribution of arbitrators' slant. However, if consumers are informed as a group, then this changes arbitrators' incentives. Since individual consumers do not internalize the benefits of every consumer being informed, this externality opens the door for potential regulation. One example of such regulation is the prohibition on arbitration clauses, which rule out class action claims. For example, the CFPB proposed a rule preventing companies from using mandatory arbitration clauses, which was overturned by Congress ("New protections against mandatory arbitration," 2017).

[27] In the parameterized version of the model, the mean of the distribution of arbitrator beliefs $F(\cdot)$ is 8pp higher than the distribution of awards granted $\tilde{G}(\cdot)$ as displayed in Figure A4.

Appendix B: Model Solution

Arbitrators compete for cases by choosing their slant: how consumer or firm friendly they want to be. They trade off two forces. On the one hand, they want to be selected on the arbitration panel (increase $\Gamma(a_i, G(.)))$ to earn the arbitration fee. They want to slant an award which has a small chance of being rejected from an arbitration panel by an informed firm or consumer. This probability is determined by their type *relative* to other arbitrators. We solve for the optimal choice of slant as a function of the model primitives for two separate cases: first, when consumers are informed ($\mu_P = 1$); and second, when only consumers are uninformed ($\mu_P = 0$),

B.1 Informed consumers

We first present the benchmark model in which firms and consumers are fully informed ($\mu_P = 1$). This benchmark illustrates the potential benefits of the existing arbitrator selection mechanism. When both firms and consumers are equally informed, the outcome reached in expectation is fair, so the median arbitrator will be chosen. Moreover, the arbitrator selection process will result in awards closer to the fair outcome. More formally, the distribution of arbitration outcomes $\tilde{G}(\cdot)$,will be a median preserving contraction of the distribution of beliefs $F(\cdot)$.

We study a symmetric equilibrium in strictly increasing piece-wise differentiable strategies. If both parties are informed, then an arbitrator is selected if her type is the $k+1th$, $k+2th$,... $n-kth$ order statistic among the arbitrators in the pool. Given the selection mechanism, the probability an arbitrator is selected is increasing in a for a below the median ($\gamma(a, G(\cdot)) > 0, \forall a < G^{-1}(0.5)$) and is decreasing in a for a above the median ($\gamma(a, G(\cdot)) < 0, \forall a > G^{-1}(0.5)$). The first order condition (eq. 9) implies that arbitrators with below the median beliefs will slant their awards type upwards relative to their beliefs $a_i > b_i$, $\forall b_i < F^{-1}(0.5)$, arbitrators with above median beliefs will slant their awards downwards relative to their beliefs $a_i < b_i$, $\forall b_i > F^{-1}(0.5)$, and arbitrators with median beliefs will be unbiased $a_i = b_i \; \forall b_i = F^{-1}(0.5)$.

We begin by studying those arbitrators with beliefs above the median. These arbitrators will find it optimal to slant their awards downward relative to their beliefs such that $a_i < b_i$. We can write arbitrator's expected utility as a function of her beliefs b_i as

$$U(b_i) = max_{a_i} \Gamma(a^{-1}(a_i), F(\cdot)) \left(fee - \theta(b_i - a_i)\right) \tag{19}$$

From the envelope condition (Milgrom and Segal, 2002; Levin 2004), we have

$$\frac{\partial}{\partial b} U(b_i) = -\Gamma(b_i, F(\cdot))\theta \, \forall b_i > F^{-1}(0.5) \, and \, b_i \neq a_i \tag{20}$$

An arbitrator with median beliefs has no incentive to deviate and has the highest expected utility in equilibrium $\bar{U} = f\Gamma(F^{-1}(0.5), F(\cdot))$. Combining this initial condition and the differential equation from the envelope condition (eq. 20), we can write the utility of arbitrator with belief b_i as

$$U(b_i) = \bar{U} - \int_{F^{-1}(0.5)}^{b_i} \Gamma(\tilde{b}, F(\cdot))\theta d\tilde{b}, \, \forall b_i > F^{-1}(0.5) \, and \, b_i \neq a_i \tag{21}$$

Last, we can use equations (19) and (21) to solve for the optimal strategy.

$$a(b_i) = \min \left\{ b_i - \frac{fee}{\theta} + \frac{\left(\frac{\bar{U}}{\theta} - \int_{0.5}^{b_i} \Gamma(\tilde{b}, F(\cdot))d\tilde{b}\right)}{\Gamma(b_i, F(\cdot))}, b_i \right\}, \, \forall b_i > F^{-1}(0.5)$$

By symmetry we can write solve for the optimal strategy for arbitrators with below median beliefs as

$$a(b_i) = \max \left\{ b_i + \frac{fee}{\theta} - \frac{\left(\frac{\bar{U}}{\theta} - \int_{F^{-1}(0.5)}^{b_i} \Gamma(\tilde{b}, F(\cdot))d\tilde{b}\right)}{\Gamma(b_i, F(\cdot))}, b_i \right\}, \, \forall b_i < F^{-1}(0.5)$$

B.2 Uninformed Consumers

Here we analyze arbitration outcomes when the firm holds an informational advantage. Since firms are informed, they eliminate the most consumer friendly arbitrators from the pool. This shifts the distribution of awards granted $\tilde{G}(\cdot)$ to be more firm friendly than the pool of arbitrators $G(\cdot)$. Because arbitrators most friendly to the consumer are eliminated from the pool, arbitrators have the incentive to be more firm friendly than other arbitrators to avoid elimination.

If only the firm is informed, the probability an arbitrator is selected is equal to the probability she is one of $n - kth$ lowest order statistics. The probability an arbitrator is selected is therefore decreasing in her award a, $\gamma(a, G\cdot) < 0$. From the first order condition (9), we can see that $a \leq b$ such that an arbitrator's award is always slanted downwards relative to her beliefs. We can rewrite the arbitrator's problem as

$$U(b_i) = max_{a_i} \Gamma(a^{-1}(a_i), F(\cdot)) \left(fee - \theta(b_i - a_i)\right) \tag{22}$$

From the envelope condition, we have

$$\frac{\partial}{\partial b} U(b_i) = -\Gamma(b_i, F(\cdot))\theta \, \forall b_i \neq a_i \tag{23}$$

Note that an arbitrator with slant $\bar{b}$ will never be selected for arbitration; thus, $U(\bar{b}) = 0$. Combining (22) and (23) we solve for the equilibrium strategy

$$a(b_i) = \min \left\{ b_i - \frac{fee}{\theta} + \frac{\int_{b_i}^{\bar{b}} \Gamma(\tilde{b}, F(\cdot))d\tilde{b}}{\Gamma(b, F(\cdot))}, b_i \right\}$$

Appendix C: Risk Aversion

For convenience, our model assumes that consumers and/or firms are risk neutral. In practice, consumers and even firms are likely risk averse over arbitration outcomes. The arbitrator selection process is inherently stochastic, as the initial list/set of n arbitrators is randomly drawn from the pool of arbitrators. Here, we discuss two points related to risk aversion. First, risk aversion has no effect on outcomes *within* a given arbitrator selection mechanism. Second, risk aversion can alter parties preferences *across* arbitration mechanisms.

First, risk aversion does not alter the analysis in our model holding the arbitrator selection mechanism fixed. We do not need to specify the utility/risk preferences of consumers and firms. Regardless of their risk aversion, firms always remove the most consumer friendly arbitrators from the list; similarly, if consumers are informed, they always remove the least consumer friendly arbitrators from the list. In other words, risk aversion does not alter striking behavior, which is the source of arbitrator incentives.

Second, litigant risk aversion can alter preferences of the parties *across* different arbitration mechanisms. For example, risk averse consumers may prefer the current arbitrator selection mechanism to one in which arbitrators are selected randomly, in a similar manner to judges. As we discuss in Section VI.B, the current arbitration system has, on average, lower, more pro-industry awards relative to the random system. On the other hand, the distribution of awards in the current arbitration system has lower variance than under randomly assigned arbitrators. If consumers and firms are sufficiently risk averse, they may prefer the current system over the random assignment.

An advantage of our methodology is that we are able to recover the complete distribution of arbitration outcomes $\widetilde{G}(\cdot)$ given essentially any arbitrator selection mechanism $\Gamma(a, G(\cdot))$ as illustrated in Section VI.C. Thus, for any set of consumer and firm preferences, such as levels of risk aversion, one can compare outcomes across mechanisms, and choose the mechanism which has the preferred distribution of arbitration outcomes $\widetilde{G}(\cdot)$. This is the case if the criterion is overall welfare, consumer or firm welfare, or the welfare of a subset of certain consumers, such as those who are most vulnerable.

Appendix D: Consumer Arbitration Beyond the Securities Industry

Our empirical analysis and model focus on arbitration in the securities industry. This is primarily due to the availability of detailed and high quality data. In this section we suggest that the insights from our setting extend to consumer arbitration more generally. First, we discuss how the mechanism we illustrate in our model extends to other settings and other arbitrator selection systems. Second, with the limited data that is available, we provide suggestive evidence that the broad empirical facts we document in our analysis extend to two other large arbitration forums, the American Arbitration Association (AAA) and Judicial Arbitration and Mediation Services, Inc. (JAMS). These forums are used for consumer arbitration by over 8,000 firms ranging from banks (e.g., Wells Fargo, JPMorgan Chase, Citibank and Bank of America), credit card companies (e.g., American Express and Discovercard), as well as a wide variety of non-financial companies (e.g., AT&T, Blue Cross Blue Shield, Darden Restaurants, Macys Inc, United Health Group, Verizon Wireless, Apple, Uber and Spotify). As should be apparent, these forums moderate transactions totaling several billions of dollars.

Arbitrator Selection Mechanisms in Other Settings

The model in Section V highlights how arbitration outcomes change when one party holds an informational advantage in selecting arbitrators. In this section we discuss why this mechanism is not specific to the arbitrator selection system employed by FINRA, but extends to those of AAA and JAMS, and more generally to arbitrator selection systems in which one party holds an informational advantage. The intuition for this assertion is simple. One of the defining characteristics of arbitration is that parties participate in selecting arbitrators. If one party is better at selecting arbitrators, either because it is more sophisticated or better informed, then arbitrators favored by this party will be selected with a higher probability. Moreover, because arbitrators are compensated if selected, this will give arbitrators incentives to slant their decisions in favor of the informed/sophisticated party.

Two arbitrator selection mechanisms, which are sometimes used in conjunction, are broadly used in consumer arbitration: striking and ranking. In striking, which we model in Section V, both parties remove arbitrators from the proposed list, making them ineligible. In ranking, both parties rank arbitrators, and the arbitrator with the lowest/most preferred combined rank is appointed. These systems can be combined: each party first strikes a given number of arbitrators, and ranks the rest. The ranking is then used to select arbitrators who were not struck by either party. The standard process used by JAMS is strike and rank. A list of five arbitrators is presented to both parties, from which each party is allowed to strike 2 or 3.[28]

[28] [https://www.jamsadr.com/rules-comprehensive-arbitration/#Rule-15 accessed 6/5/2018]

AAA's Arbitrator Select List and Appointment system uses a ranking system of 5-15 arbitrators.[29] While these systems are similar to FINRA's, they are not identical.

Relative to the striking system, that we analyze, the ranking system (or strike and rank) allows the informed party more control over choosing arbitrators. In the striking system, the informed party can influence the selection by eliminating the least favorable arbitrators, for example, the 4 least favorable arbitrators from 10. In the ranking system, the party lists arbitrators from most to least desirable. The uninformed party either does not submit a ranking, or ranks randomly.[30] Then, the informed party can de facto eliminate 9 least favorable arbitrators from the list of 10, giving it an even larger advantage. In other words, the striking, ranking, and strike and rank arbitrator selection systems provide an advantage to the informed party.

This advantage provides incentives for arbitrators to choose a slant that favors the informed party in these systems. Arbitrators' choice of slant in eq. (8) depends on the probability of being selected onto the panel, $\Gamma(a_i, G(\cdot))$, which increases when they tilt their slant in favor of the informed party. In the ranking system, this incentive is exacerbated, since only the most favored arbitrator of the informed party is chosen. More broadly, the forces we identify in the model arise due to the defining characteristics of arbitration. Parties participate in selecting arbitrators giving the informed party more power over arbitrator selection. Arbitrators are paid when selected, and therefore have incentives to slant in favor of the informed party. Therefore insights from studying the mechanism in our model easily translates into the strike and rank (JAMS) or rank (AAA) systems.

Suggestive Evidence

In this section we present suggestive evidence that our empirical findings apply to arbitration more broadly. We examine whether arbitrators systematically differ, and whether more industry friendly arbitrators are more likely to be selected to arbitration cases in two main consumer arbitration forums outside of the securities industry, AAA and JAMS. The benefit of using these data is coverage across a wide range of industries and cases. The downside is that the cases are much less comparable, the data on each individual case is significantly more sparse, and firms can choose which arbitration forum they want to use. We construct two separate consumer arbitration data sets using the data posted online by the AAA and JAMS.[31] The JAMS data set consists of 391 arbitration cases overseen by 104 different arbitrators over the period 2002-2018. The AAA data set consists of 965 arbitration cases overseen by 265 different arbitrators over the period 2013-2018. We report the summary statistics in Table A6a. Figure A9 panels (a) and (b) display the types of

[29] [https://www.adr.org/sites/default/files/document_repository/AAA_Arbitrator_Select_2pg.pdf]

[30] When both parties are informed in the ranking system, they each rank the arbitrators honestly. Since all arbitrators have the same score, they are chosen randomly. Similarly, when both parties are informed in the strike and rank system, only the striking has an effect, and the ranking results in the remaining arbitrators to be chosen randomly.

[31] https://www.adr.org/consumer; https://www.jamsadr.com/consumercases/

arbitration cases administered by AAA and JAMS in our data set. Common types of cases range from financial services (non-brokerage related, e.g., credit/debit cards, banking and insurance) to telecom, healthcare and car sales.

The AAA and JAMS data contain less information relative to the FINRA data used in our main analysis. In the AAA data set we observe the arbitrator, industry and firm involved in the dispute, the award amount requested, and the award granted. In the JAMS data we observe the arbitrator, industry and firm involved in the dispute, and the award granted, but not the amount requested. In other words, from JAMS data we cannot compute our preferred outcome variable, award granted/award requested. Despite the sparse information, we use these additional data sources to provide some suggestive evidence that our main findings extend more broadly.

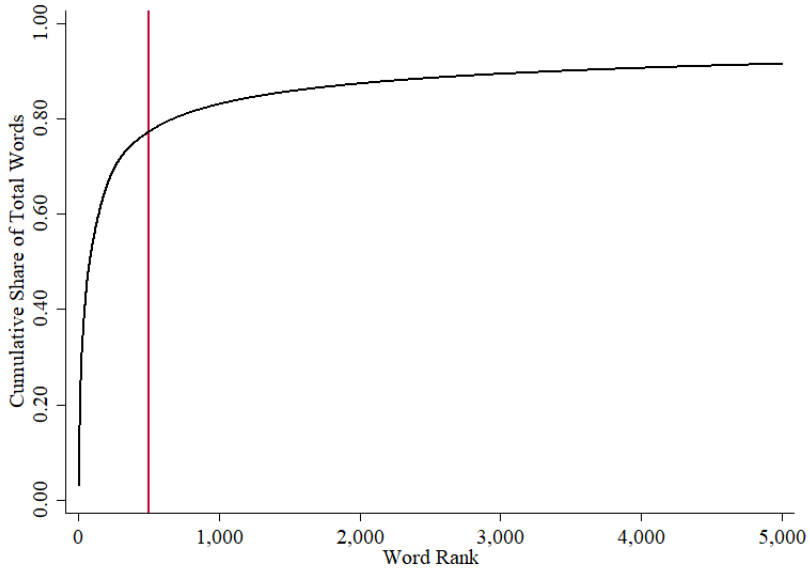
First, we show that arbitrators display a systematic industry/consumer friendliness in awarding claims. Some arbitrator slant more "industry friendly" than others. We employ eq. (1) and estimate differences in awards (either in dollars or percent awarded, depending on the data set) as a function of industry and arbitrator fixed effects (Table A6b). In both data sets, we find significant differences across arbitrators and reject the null hypothesis that arbitrator fixed effects are equal to each other at the 1% level. Arbitrator fixed effects explain 36% and 38% of the variation in awards in JAMS and AAA cases, respectively. Consistent with our set of results for securities arbitration, some arbitrators are consistently more consumer friendly while other arbitrators are consistently more industry friendly.

Second, we provide suggestive evidence that industry friendly arbitrators are selected to more cases. Figure A10 panels (a) and (b) display binned scatter plots between the estimated arbitrator fixed effects and the number of times an arbitrator is selected to a case. We find a negative and statistically significant relationship between the estimates of arbitrator fixed effects (consumer friendliness) and the number of cases the arbitrator oversees in JAMS data. In other words, arbitrators that give out lower awards are ultimately selected to more arbitration cases. We find similar evidence of a relationship between the arbitrator fixed effects and the number of cases the arbitrator oversees in AAA data. Even with substantially lower quality data, we find some suggestive evidence that more industry friendly arbitrators are chosen more often. These results are subject to the important caveat that the AAA and JAMS data sets are relatively sparse and span a wide range of industries and cases, resulting in larger measurement error. Nevertheless, the results in this section suggests that the mechanisms we identify in the securities industry apply to consumer arbitration more generally.

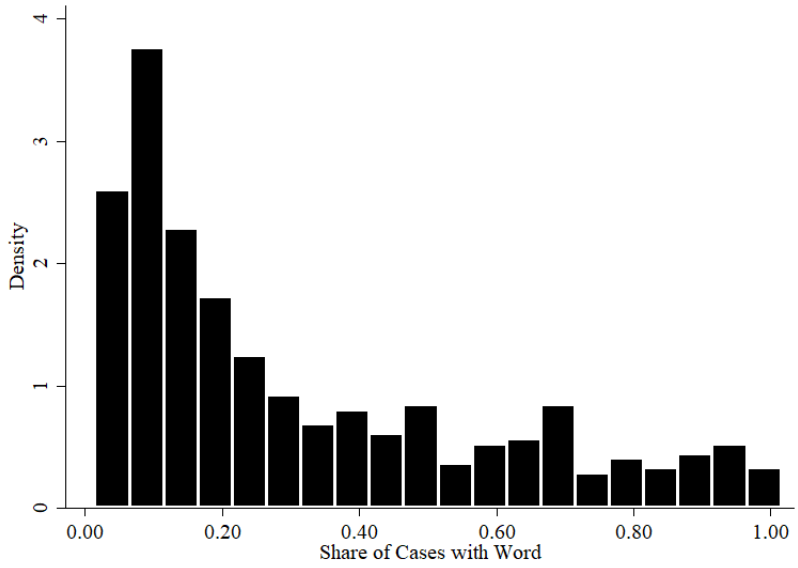
Appendix E: Additional Tables and Figures

Figure A1: Textual Analysis: Most Common Words Appearing in Arbitration Cases

(a) Word Rank vs Cumulative Share of Total Words

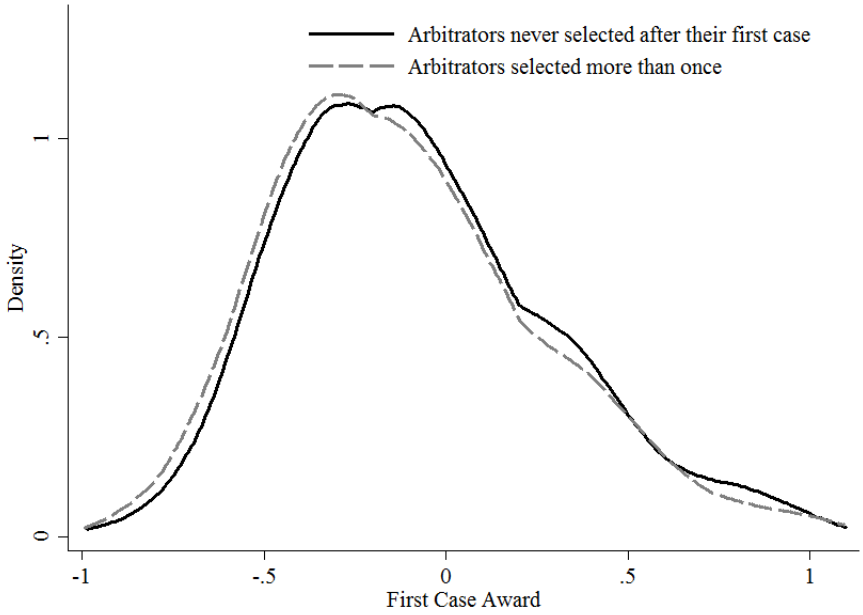


(b) Frequency of Each of the 500 Most Common Words: Share of Cases



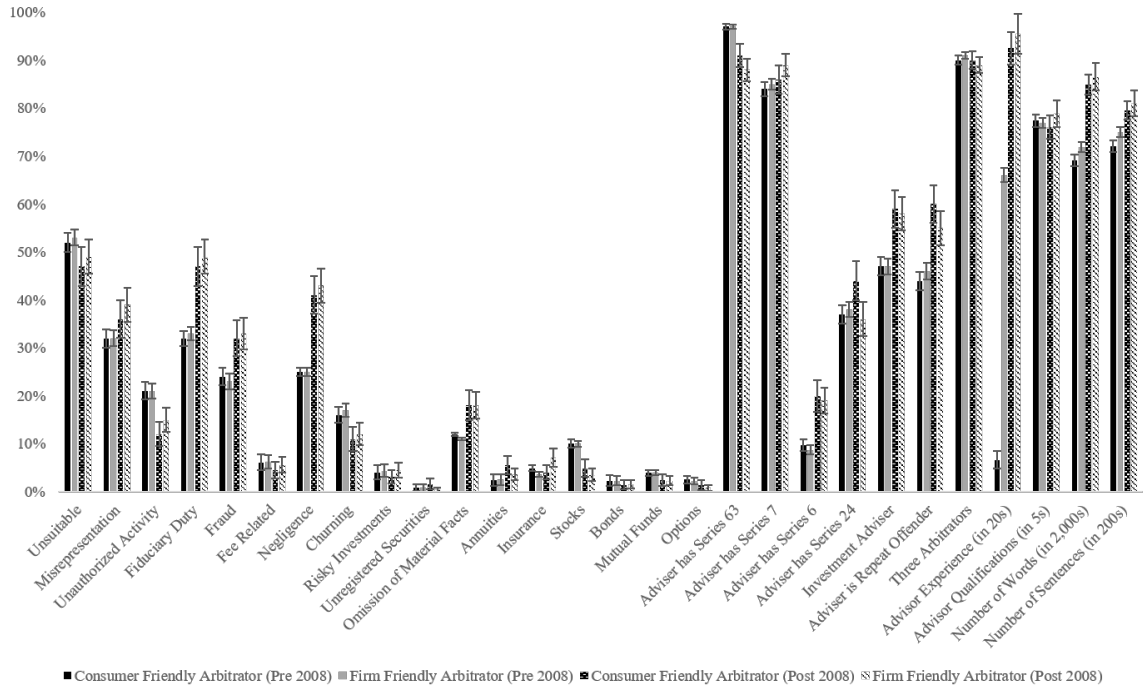
Note: Panel (a) plots the rank, in terms of frequency, of each word appearing in the arbitration case documents versus the cumulative share of total words of that rank or lower. Observations in panel (a) are the 5,000 most popular words in the case. The figure indicates that the top 500 words account for 77% of all words appearing in the case documents. Panel (b) displays the frequency of each of the 500 most common words in terms of the share of cases each word appears in. Observations are at the word level, where we restrict our attention to the 500 most common words. In both panels (a) and (b) we determine the top 500 words after stemming the words using Porter's Stemming Algorithm and dropping stopwords as per the System for the Mechanical Analysis and Retrieval of Text (Lewis et al. 2004).

Figure A2: Initial Case Awards and Future Arbitrator Selection



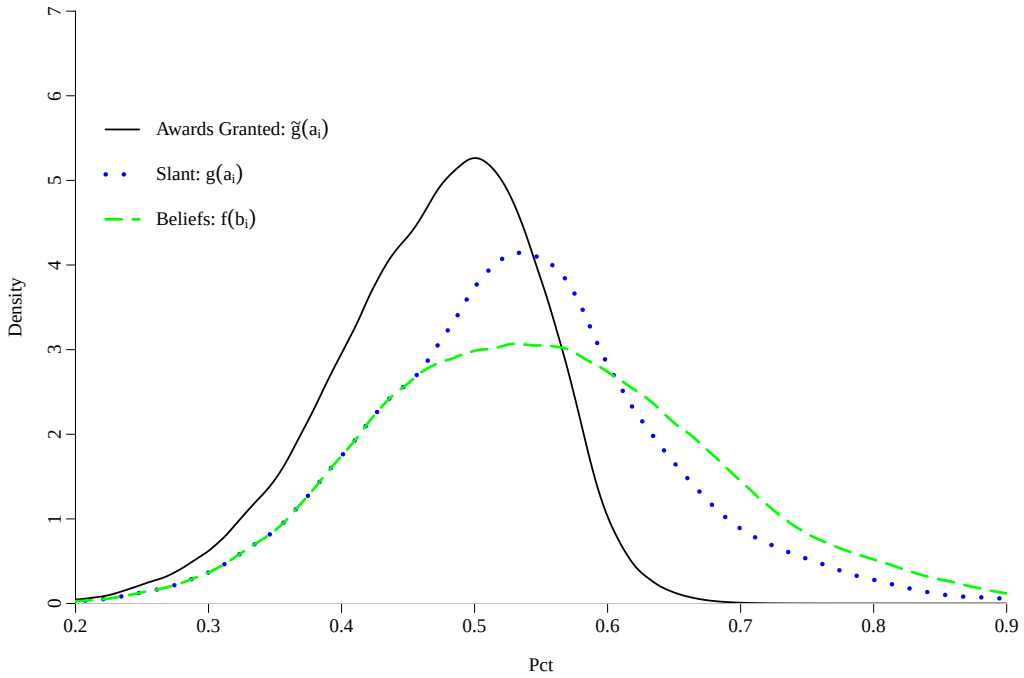
Note: Figure A2 displays the residualized distribution of initial arbitration Awards for arbitrators who were never selected to another consumer dispute case after their first case versus those arbitrators who were selected to multiple consumer dispute cases. We calculate Awards as the value of awards granted through arbitration divided by the awards initially requested by the consumer (we calculate residualized Awards as the residuals from the regression of case Awards on a vector of case characteristics, including time and location fixed effects, as reported in column (2) of Table 2). Using the Kolmogorov-Smirnov test, we reject the null hypothesis that the distributions of Awards are drawn from the same distribution at the 1% level.

Figure A3: Observable Case Characteristics by Arbitrator Type: Before and After the 2008 Rule Change



Note: Figure A3 displays the distribution of case characteristics by whether the arbitrator is consumer friendly (above mean fixed effect) or industry friendly (below mean fixed effect) before and after 2008. The standard error bars correspond to 95% confidence intervals. For scaling puproses, we report the non-dummy variables (adviser experience, adviser qualifications, etc.) in units such that the average case characteristic is in the interval 0-1.

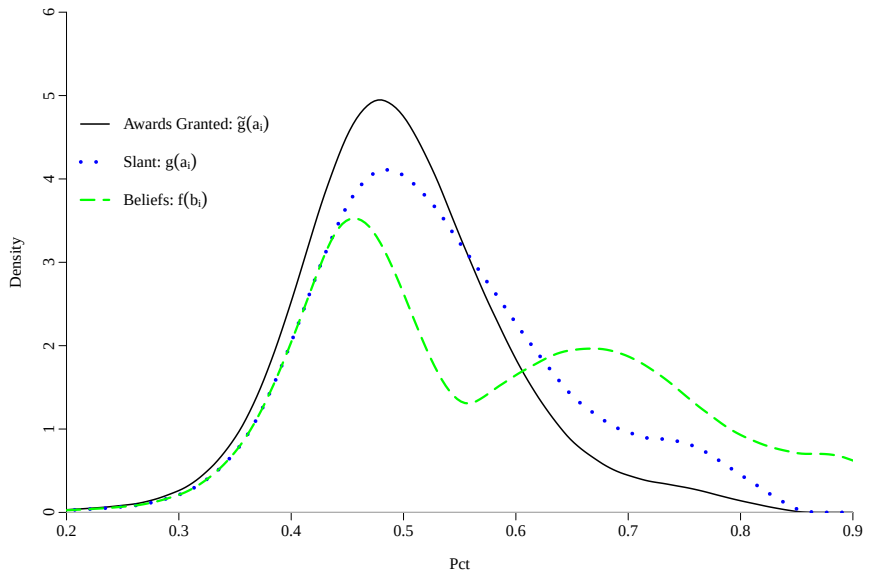
Figure A4: Distribution of Arbitrator Beliefs, Slant, and Awards (Parametric Model



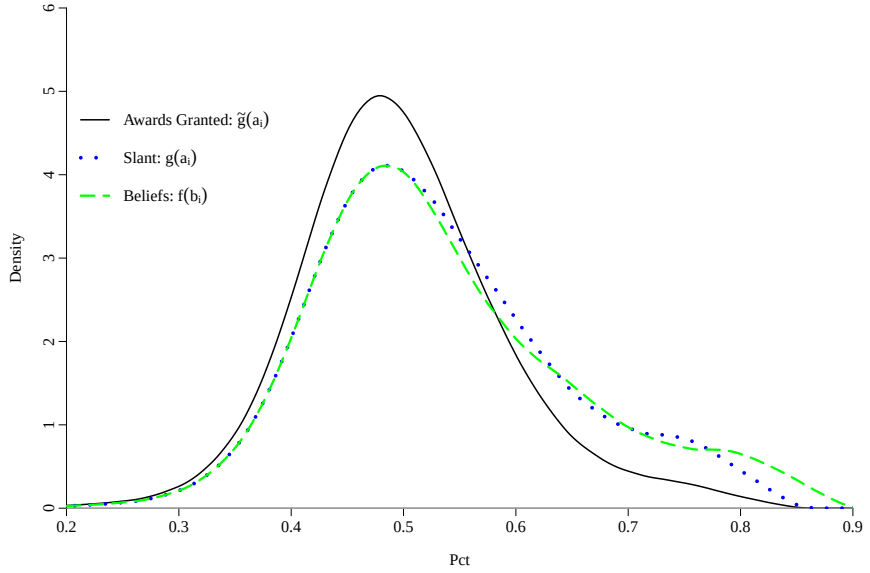
Note: Figure A4 displays the estimated density of awards among the conditional distribution of selected arbitrators $\tilde{g}(a)$, the density of slant among the unconditional (entire) population of arbitrators $g(a)$, and the distribution of true beliefs among the unconditional (entire) population of arbitrators $f(b)$. The distributions of awards, slant, and beliefs correspond to our parametric model. The underlying distribution of beliefs is estimated using MLE where we assume that the distribution of beliefs follows a gamma distribution as described in Section VI.

Figure A5: Distribution of Arbitrator Beliefs, Slant, and Awards Under Alternative Parameterizations

(a) $\frac{f}{\theta}$ scaled by 50%



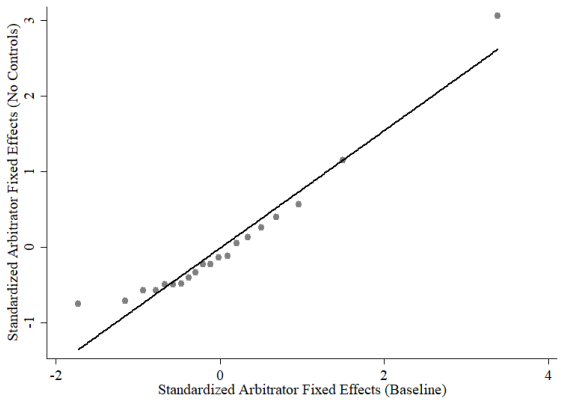
(b) $\frac{f}{\theta}$ scaled by 150%



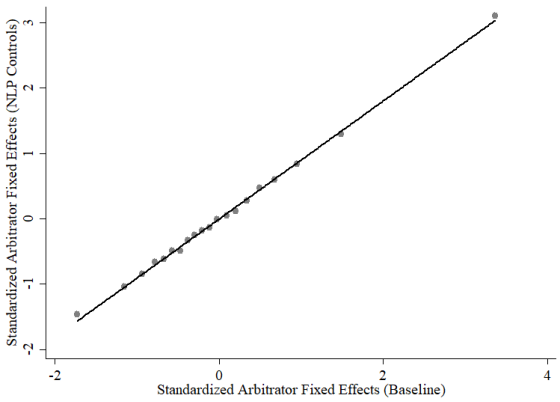
Note: Figures A5a and A5b display the estimated density of awards among the conditional distribution of selected arbitrators $\widehat{\tilde{g}(a)}$, the estimated density of slant among the unconditional (entire) population of arbitrators $\widehat{g(a)}$, and the estimated density of true beliefs among the unconditional (entire) population of arbitrators $\widehat{f(b)}$. The black line plots the distribution of realized awards/outcomes observed in the data. In panel (a) we calibrate the unconditional distributions of slant and beliefs by scaling the parameter $\frac{fee}{\theta}$ by 50% relative to our baseline calibration. In panel (b) we calibrate the unconditional distributions of slant and beliefs by scaling the parameter $\frac{fee}{\theta}$ by 150% relative to our baseline calibration. Both panels are estimated under the assumption that only firms are informed.

Figure A6: Alternative Arbitrator Fixed Effects Estimates

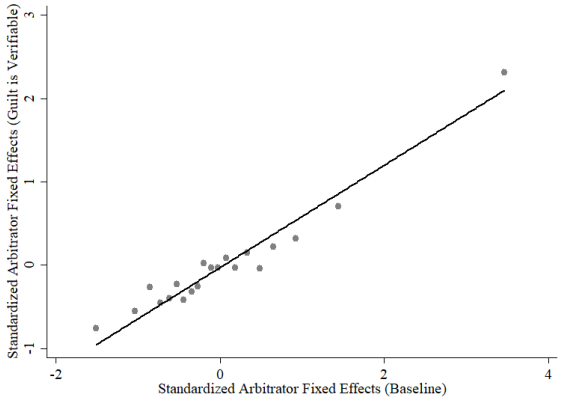
(a) Arb. F.E.: Baseline vs Est. without Controls



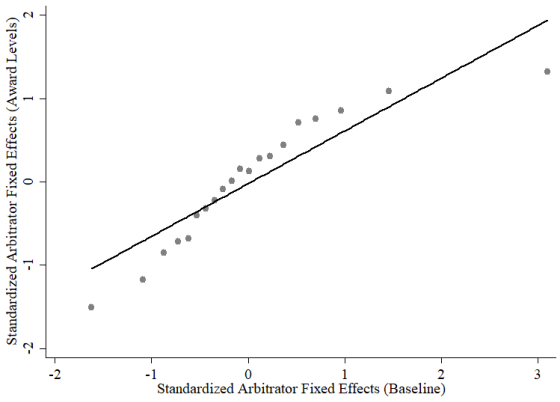
(b) Arb. F.E.: Baseline vs Est. with NLP Controls



(c) Arb. F.E.: Baseline vs Est. with Cases with Verifiable Guilt

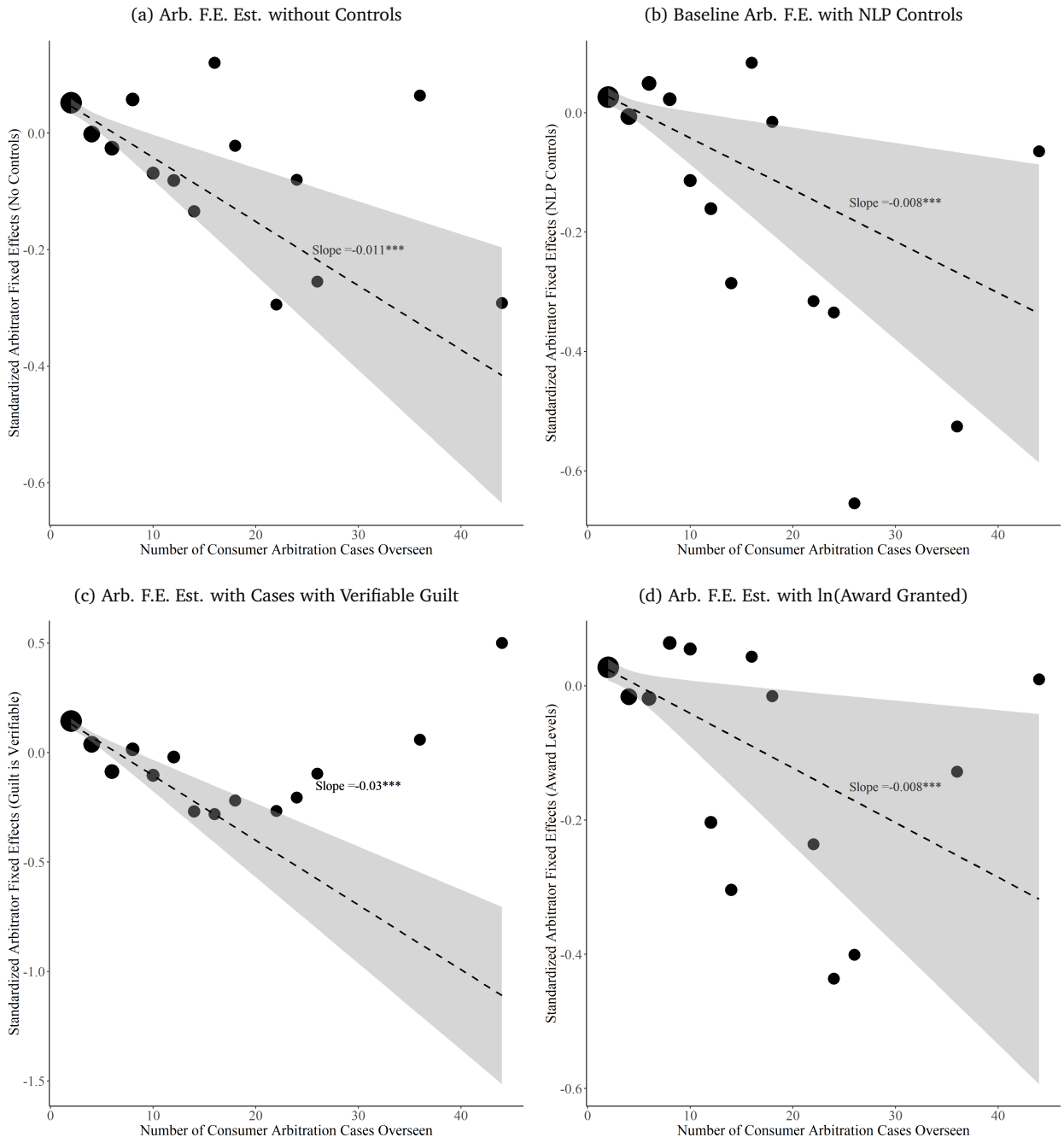


(d) Arb. F.E.: Baseline vs Est. with ln(Award Granted)



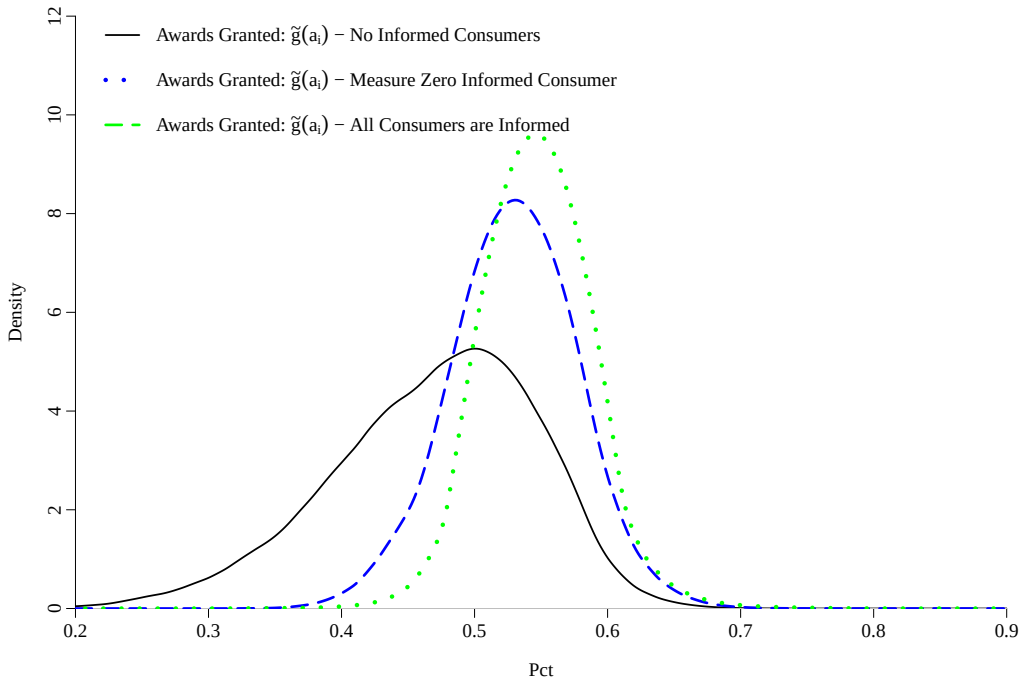
Note: Panels (a)-(c) display binned scatter plots of our alternative arbitrator fixed effects estimates versus our baseline fixed effects estimates. Observations are at the arbitrator level in each panel and the arbitrator fixed effects are all standardized. Our baseline fixed effects correspond to eq. (1) and the estimates reoprted in Table 2. In panel (a) we construct our arbitrator fixed effects by re-estimating eq. (1) without any control variables other than our set of arbitrator fixed effects. In panel (b) we construct our arbitrator fixed effects by re-estimating eq. (1) where augment our baseline specification by including dummy variables for the 500 words appearing in the case documents. In panel (c) we construct our arbitrator fixed effects by re-estimating eq. (1) where we restrict the data set to only those cases involving unauthorized trading, churning, or selling unregistered securities. In panel (d) we construct construct our arbitrator fixed effects by re-estimating eq. (1) where the dependent variable is in terms of the $\ln(AwardGranted)$ rather than $\frac{AwardGranted}{AwardRequested}$.

Figure A7: Are Industry Friendly Arbitrators Selected More Frequently? Alternative Arbitrator F.E. Estimates



Note: Panels (a)-(d) display binned scatter plots of our four different measures of arbitrator fixed effects versus the total number of cases an arbitrator oversaw. Observations are at the arbitrator level and the arbitrator fixed effects are all standardized in each panel. In panel (a) we construct our arbitrator fixed effects by re-estimating eq. (1) without any control variables other than our set of arbitrator fixed effects. In panel (b) we construct our arbitrator fixed effects by re-estimating eq. (1) where augment our baseline specification by including dummy variables for the 500 words appearing in the case documents. In panel (c) we construct our arbitrator fixed effects by re-estimating eq. (1) where we restrict the data set to only those cases involving unauthorized trading, churning, or selling unregistered securities. In panel (e) we construct construct our arbitrator fixed effects by re-estimating eq. (1) where the dependent variable is in terms of the $\ln(AwardGranted)$ rather than $\frac{AwardGranted}{AwardRequested}$. *** p<0.01, ** p<0.05, * p<0.10.

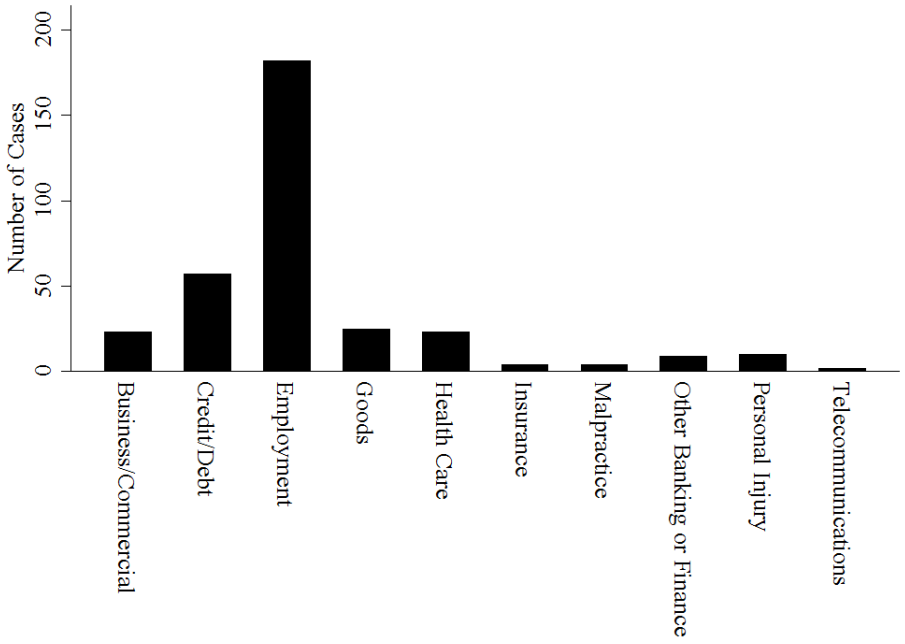
Figure A8: Distribution of Arbitrator Beliefs, Slant, and Awards—Measure Zero Informed Consumer



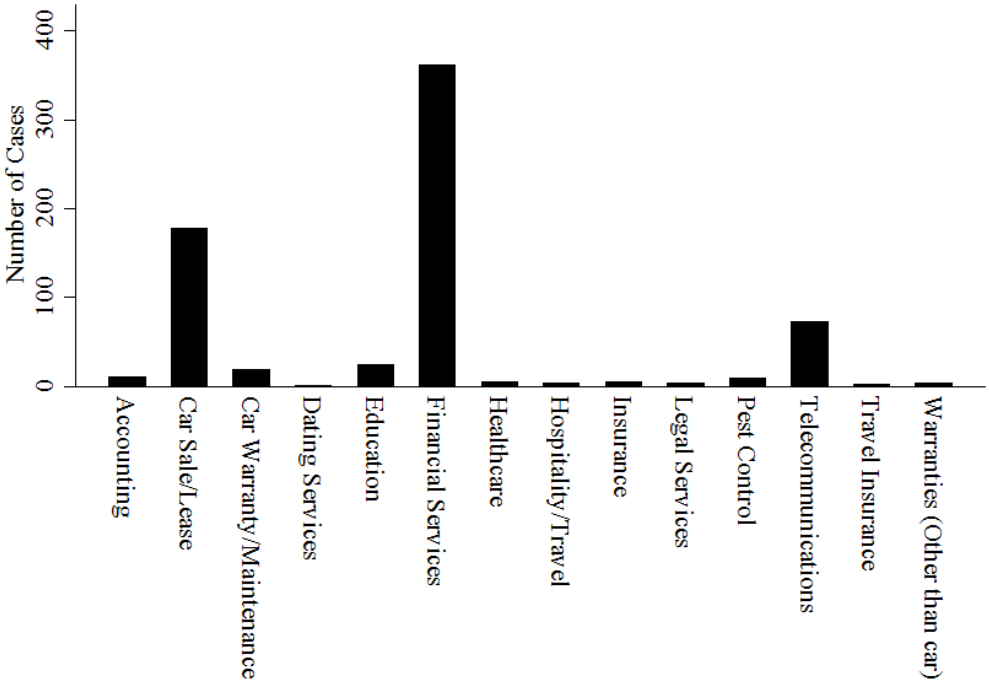
Note: Figure A8 displays the model implied density of awards if (i) all consumers are uninformed, (ii) a measure zero of consumers are informed, and (iii) all consumers are informed.

Figure A9: Types of Disputes at the American Arbitration Association (AAA) and JAMS

(a) JAMS



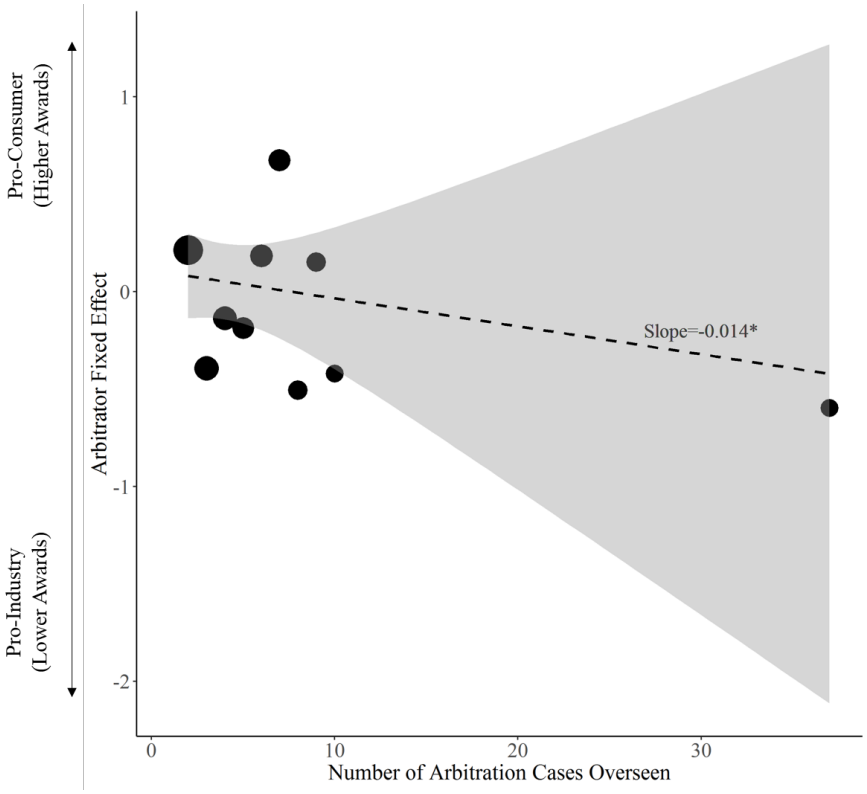
(b) AAA



Note: Figure A9 panels (a) and (b) display the types of arbitration/mediation overseen by the AAA and JAMS. Data are reported by the AAA and JAMS over the period 2013-2018. Panel (a) displays the frequency of all types of disputes in the JAMS data set. Panel (b) displays the frequency of all types of disputes in the AAA data set. The case types reported by JAMS do not directly correspond to the case types reported by AAA.

Figure A10: External Validity: Arbitrator Selection in AAA and JAMS

(a) Arbitrator Fixed Effects vs. Selection - JAMS



(b) Arbitrator Fixed Effects vs. Selection - AAA

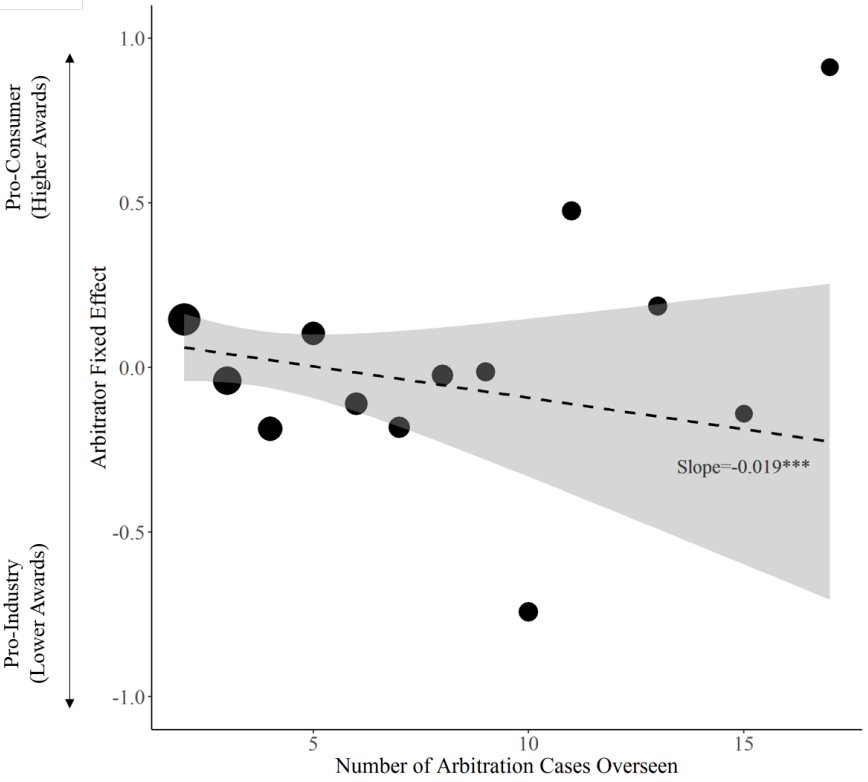


Figure A10: External Validity: Arbitrator Selection in AAA and JAMS

Note: Figure A10 panels (a) and (b) display the distribution between arbitrator case outcomes and the total number of times an arbitrator is selected. Figure A10a displays a binned scatter plot of the normalized arbitrator fixed effects versus the total number of cases the arbitrator oversaw in the JAMS data. Figure A10b displays a binned scatter plot of the standardized arbitrator fixed effects versus the total number of cases the arbitrator oversaw in the JAMS data. Observations in Figure A10 panels (a) and (b) are at the arbitrator level. A higher fixed effect indicates that the arbitrator gave out higher awards than expected given case observables. The size of the bubble corresponds to the number of arbitrators in the bin. The gray shaded area reflects the 90% confidence interval for the corresponding weighted least squares regression. The arbitrator fixed effects in panel (a) correspond to column (2) of Table A6b. The arbitrator fixed effects are computed from a regression of total awards granted in dollar terms on a vector of case controls and arbitrator fixed effects. The arbitrator fixed effects in panel (b) correspond to column (4) of Table A6b. The arbitrator fixed effects are computed from a regression of Awards, defined as awards granted divided by awards requested, on a vector of case controls and arbitrator fixed effects. We compute the arbitrator fixed effects for the JAMS cases based on the total awards granted in dollar terms rather than in percentage terms because we do not observe the awards requested in the JAMS cases. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1: Probability an Arbitrator is Selected - Past Consumer Friendliness

	(1)	(2)	(3)
Past Arbitrator Consumer Friendliness	-0.30***	-0.29***	-0.30***
	(0.072)	(0.072)	(0.073)
Arbitrator Controls		X	X
Year F.E.		X	X
Location F.E.			X
Adviser County F.E.			X
Observations	105,854	105,854	105,838
R-squared	0.028	0.031	0.045

Note: Table display the regression results corresponding to a linear probability model (eq. 15). Observations are at the arbitrator-by-year level over the period 1988-2015. The dependent variable is a dummy variable indicating whether an arbitrator was selected in a given year. The independent variable of interest is our measure of Past Arbitrator Consumer Friendliness. We measure Past Arbitrator Consumer Friendliness using a backward measure of friendliness as described in Appendix A (eq. 14). A higher Past Arbitrator Consumer Friendliness indicates that, all else equal, the arbitrator gave out higher awards in the past. Relative to Table 3, we have roughly 40k more observations because we are able to calculate our backawards looking measure of Arbitrator Consumer Friendliness for those arbitrators who only oversee one case in their career. Those arbitrators are dropped from our previous sample because we cannot compute Arbitrator Fixed Effects for those arbitrators. Arbitrator controls include the number of years the arbitrator has been active in the industry. We include year fixed effects, fixed effects for the location of the arbitration proceedings, and fixed effects for the county of the adviser's office location. The hearing location fixed effects and adviser office location county fixed effects correspond to the last consumer dispute case the arbitrator oversaw. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A2: Arbitrator Bias and Awards Requested

(a) Award Requested

	(1)	(2)	(3)	(4)
Past Arbitrator Consumer Friendliness	0.0176	0.0136	0.0130	0.032
	(0.0157)	(0.0119)	(0.0122)	(0.0114)
Arbitration Case Controls		X	X	X
Year F.E.		X	X	X
Adviser County F.E.			X	X
Location F.E.			X	X
Firm F.E.				X
Observations	11,768	11,092	10,758	10,530
R-squared	0.000	0.394	0.440	0.606

(b) Award Granted

	(1)	(2)	(3)	(4)
Past Arbitrator Consumer Friendliness	0.080***	0.077***	0.068***	0.055***
	(0.020)	(0.017)	(0.016)	(0.016)
Arbitration Case Controls		X	X	X
Year F.E.		X	X	X
Adviser County F.E.			X	X
Location F.E.			X	X
Firm F.E.				X
Observations	9,617	9,076	8,745	8,548
R-squared	0.002	0.282	0.347	0.529

Note: Table A2a and A2b displays the regression results for linear regression models (eq. 16 and 17). Observations are at the arbitrator-by-case level over the period 1988-2015 and come from our consumer dispute arbitration data set. The dependent variable in panel (a) is the log value of awards requested. The dependent variable in panel (b) is the log value of awards granted. The independent variable interest is Past Arbitrator Consumer Friendliness. We measure Past Arbitrator Consumer Friendliness using a backward measure of friendliness as described in Appendix A (eq. 14). A higher Past Arbitrator Consumer Friendliness indicates that, all else equal, the arbitrator gave out higher awards in the past. We also control for the arbitration panel size, the case length in terms of the number of words, and other adviser characteristics. Other adviser controls include the advisers qualifications: Series 6, Series 7, Series 24, Series 63, Series 65/66, and number of other qualifications. Standard errors are clustered at the case level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A3: ln(Award Granted)

	(1)	(2)	(3)	(4)
ln(Award Requested)	0.62***	0.62***	0.63***	0.64***
	(0.016)	(0.016)	(0.018)	(0.019)
Allegations:				
Unsuitable	0.029	0.044	0.049	0.056
	(0.034)	(0.037)	(0.041)	(0.042)
Misrepresentation	-0.026	-0.039	-0.019	-0.035
	(0.038)	(0.040)	(0.045)	(0.047)
Unauthorized Activity	-0.063	-0.0082	0.035	0.085
	(0.043)	(0.045)	(0.050)	(0.052)
Omission of Key Facts	-0.083	-0.087	-0.073	-0.061
	(0.059)	(0.063)	(0.072)	(0.072)
Fee/Commission Related	0.12**	0.11*	0.19***	0.17**
	(0.058)	(0.064)	(0.071)	(0.077)
Fraud	0.16***	0.13**	0.13**	0.058
	(0.055)	(0.059)	(0.066)	(0.067)
Fiduciary Duty	0.014	0.062	0.074	0.12**
	(0.052)	(0.054)	(0.060)	(0.061)
Negligence	0.022	-0.016	-0.025	-0.069
	(0.053)	(0.057)	(0.067)	(0.066)
Risky Investments	0.028	0.063	0.094	0.036
	(0.071)	(0.076)	(0.084)	(0.089)
Churning/ Excessive Trading	-0.020	0.0013	0.025	-0.030
	(0.053)	(0.055)	(0.060)	(0.064)
Unregistered Securities	0.88***	0.89***	0.73***	0.69***
	(0.15)	(0.16)	(0.19)	(0.19)
Products:				
Insurance	-0.012	0.0069	0.0056	-0.10
	(0.078)	(0.078)	(0.093)	(0.098)
Annuity	-0.068	-0.041	0.054	0.0054
	(0.096)	(0.11)	(0.12)	(0.12)
Stocks	0.047	0.028	0.049	0.12*
	(0.049)	(0.054)	(0.062)	(0.063)
Mutual Funds	-0.15**	-0.10	-0.14	-0.11
	(0.073)	(0.077)	(0.089)	(0.099)
Bonds	0.096	0.078	-0.013	0.015
	(0.077)	(0.084)	(0.10)	(0.11)
Options	0.0072	-0.019	-0.058	-0.20*
	(0.098)	(0.11)	(0.12)	(0.11)
Adviser Characteristics:				
Prior Misconduct	0.085**	0.097***	0.087**	0.089**
	(0.034)	(0.036)	(0.041)	(0.042)
Year F.E.	X	X	X	X
Adviser County F.E.		X	X	X
Arbitration Location F.E.		X	X	X
Firm F.E.			X	X
Arbitrator F.E.				X
Observations	15,220	14,460	14,347	11,044
R-squared	**0.487**	**0.540**	**0.658**	**0.802**

Note: Table A3 displays the regression results for a linear regression model (eq. 1). Observations are at the arbitrator-by-case level over the period 1988-2015 and come from our consumer dispute arbitration data set. The dependent variable is the log value of the award granted through arbitration. Prior Misconduct indicates whether or not the adviser has been previously reprimanded for misconduct. We also control for the arbitration panel size, the case length in terms of the number of sentences and words, and other adviser controls. Other adviser controls include the corresponding adviser's experience and qualifications: Series 6, Series 7, Series 24, Series 63, Series 65/66, and number of other qualifications. In columns (2)-(4) we include fixed effects for the location of the arbitration proceedings and a fixed effect for the adviser's office location. In the full specification (column 4) we include arbitrator fixed effects. The F-test for whether arbitrator fixed effects are jointly significantly different from each other is significant at 1%. Standard errors are clustered at the case level. *** p<0.01, ** p<0.05, * p<0.10.

Table A4: Selected Arbitrator Experience and Case Observables

Dep. Var.	Arb. F.E.	Arb. F.E. (Backward Looking)	Arb. Exp	Pct Granted
Allegations:				
Unsuitable	-0.0067	-0.044**	-0.017	-2.83
	(0.0088)	(0.022)	(0.042)	(1.76)
Misrepresentation	0.00067	-0.018	-0.031	-1.37
	(0.010)	(0.024)	(0.046)	(1.93)
Unauthorized Activity	0.000068	-0.030	-0.10**	-0.20
	(0.012)	(0.028)	(0.048)	(2.30)
Omission of Key Facts	0.011	0.025	-0.11*	-0.49
	(0.015)	(0.032)	(0.064)	(2.88)
Fee/Commission Related	-0.024	-0.017	0.17*	7.43*
	(0.017)	(0.047)	(0.098)	(4.19)
Fraud	0.012	0.042	-0.029	4.42
	(0.013)	(0.031)	(0.054)	(2.72)
Fiduciary Duty	-0.0062	0.0030	-0.041	3.36
	(0.012)	(0.029)	(0.055)	(2.40)
Negligence	0.0014	-0.015	-0.0011	-5.51**
	(0.013)	(0.033)	(0.055)	(2.53)
Risky Investments	0.0044	-0.0070	0.034	-0.42
	(0.018)	(0.044)	(0.11)	(3.57)
Churning/ Excessive Trading	0.033**	0.046	0.044	1.74
	(0.015)	(0.035)	(0.055)	(2.76)
Unregistered Securities	0.043	0.22	-0.0022	18.0*
	(0.052)	(0.16)	(0.22)	(9.90)
Products:				
Insurance	0.019	-0.00094	-0.0093	4.26
	(0.019)	(0.047)	(0.11)	(3.89)
Annuity	0.038	0.027	-0.021	6.61
	(0.026)	(0.068)	(0.15)	(6.03)
Stocks	-0.024*	-0.043	0.10	-0.22
	(0.013)	(0.035)	(0.071)	(2.69)
Mutual Funds	-0.013	0.047	0.070	-8.29***
	(0.016)	(0.053)	(0.13)	(2.78)
Bonds	-0.014	0.0094	-0.098	-0.44
	(0.023)	(0.096)	(0.12)	(4.28)
Options	0.0078	0.14	0.062	-9.17**
	(0.025)	(0.12)	(0.16)	(3.93)
Adviser Characteristics:				
Prior Misconduct	0.0023	0.026	-0.016	6.98***
	(0.0091)	(0.023)	(0.042)	(1.80)
Experience	-0.00098	-0.0025	0.0014	-0.45***
	(0.00079)	(0.0017)	(0.0030)	(0.14)
Year and Arb. Loc. F.E.	X	X		
Other Controls	X	X	X	X
Observations	15,168	11,477	18,618	18,632
R-squared	0.071	0.048	0.447	0.115
Joint Sign. of Case Controls (P-value)	0.40	0.56	0.19	0.00

Note: Table A4 displays the regression results corresponding to four linear regression models. Observations are at the arbitrator-by-case level over the period 1988-2015 and come from our consumer dispute arbitration data set. The dependent variable in column (1) reflects the fixed effect of the arbitrator selected for a case. The arbitrator fixed effects correspond to eq. (1) and the estimates reported in Table 2. The dependent variable in column (2) reflects the fixed effect of the arbitrator selected for a case where we compute each arbitrator's fixed effect based on the previous cases the arbitrator oversaw as described in Appendix A.1. The dependent variable in column (3) reflects the experience of the arbitrator selected for a case in terms of the number of consumer dispute arbitration cases the arbitrator previously oversaw. The dependent variable in column (4) is Awards and is measured as awards granted divided by awards requested. We also control for the arbitration panel size, the case length in terms of the number of words, and adviser qualifications (Series 6, 7, 24, 63, 65/66, total licenses). In column (3) we also control for the years since the arbitrator first entered the industry. We report the p-value corresponding to a test of the joint significance of the control variables (excluting the fixed effects). In columns (1)-(3) we fail to reject the null hypothesis that coefficent estimates corresponding to the control variables are equal to zero. Standard errors are clustered at the case level. *** p<0.01, ** p<0.05, * p<0.10.

Table A5: Cases Arbitrated vs Settled

	(1)	(2)	(3)
ln(Award Requested)	1.11***	1.10***	1.11***
	(0.26)	(0.25)	(0.25)
Allegations:			
Unsuitable	0.92**	1.03***	0.98**
	(0.39)	(0.36)	(0.37)
Misrepresentation	0.83*	0.52	0.48
	(0.47)	(0.42)	(0.40)
Unauthorized Activity	1.81***	0.54*	0.45
	(0.45)	(0.31)	(0.31)
Omission of Key Facts	3.88***	3.24***	3.05***
	(0.69)	(0.55)	(0.52)
Fee/Commission Related	-0.14	0.15	0.11
	(0.52)	(0.49)	(0.49)
Fraud	4.32**	3.86***	4.11***
	(1.75)	(1.32)	(1.30)
Fiduciary Duty	10.9***	9.43***	9.57***
	(1.16)	(0.93)	(0.95)
Negligence	5.33***	5.15***	5.33***
	(0.92)	(0.83)	(0.81)
Risky Investments	-1.40***	-0.75*	-0.76*
	(0.49)	(0.39)	(0.42)
Churning/ Excessive Trading	3.23***	1.50***	1.52***
	(0.70)	(0.55)	(0.54)
Unregistered Securities	-0.051	-0.75	-0.77
	(2.66)	(2.74)	(2.47)
Products:			
Insurance	-2.12***	-1.03**	-1.01**
	(0.56)	(0.39)	(0.41)
Annuity	-1.70***	-0.73***	-0.77***
	(0.39)	(0.25)	(0.26)
Stocks	-0.029	0.080	0.19
	(0.58)	(0.44)	(0.45)
Mutual Funds	-1.38***	-0.55	-0.49
	(0.34)	(0.37)	(0.39)
Bonds	-1.95***	-1.19**	-1.21**
	(0.51)	(0.46)	(0.49)
Options	-0.92	-0.100	-0.39
	(0.81)	(0.71)	(0.75)
Adviser Characteristics:			
Prior Misconduct	0.45	-0.58	-0.57
	(0.67)	(0.56)	(0.58)
Experience	-0.096***	-0.012	-0.013
	(0.032)	(0.024)	(0.023)
Year F.E.	X	X	X
Firm F.E.		X	X
County F.E.			X
Other Controls	X	X	X
Observations	100,279	99,045	96,075
R-squared	0.124	0.232	0.235

Note: The table displays the regression results corresponding to a linear probability model (eq. 18). Our data set consists of 100,279 customer disputes that are reported in BrokerCheck. Observations are at the customer dispute level. The dependent variable is an indicator variable that is equal to one if the customer dispute was decided in arbitration or zero if the case was settled outside of arbitration. ln(Award Requested) requested measures the award requested by the customer. Prior Misconduct indicates whether or not the adviser has been previously reprimanded for misconduct. Other controls include the corresponding offending adviser's qualifications (Series 6, 7, 24, 63, 65/66, total licenses). Standard errors are clustered at the year level and at the firm level. *** p<0.01, ** p<0.05, * p<0.10.

Table A6: AAA and JAMS Arbitration

(a) Summary Statistics

Data Set	JAMS			AAA		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Amount Awarded	408	109,619	352,311	965	6,656	78,676
Percent of Requested Awards Granted				965	20%	115%

(b) Awards

Dep. Var	$ Award Granted		$\frac{Award\_Granted}{Award\_Requested}$	
	JAMS		AAA	
	(1)	(2)	(3)	(4)
Dispute Type/Industry Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X
Arbitrator Fixed Effects		X		X
Observations	408	408	965	965
R-squared	0.038	0.386	0.206	0.427

Note: Tables A6a displays the summary statistics corresponding to our JAMS and AAA data sets. Observations are at the case-by-arbitrator level over the period 2013-2018. We estimate columns (1)-(2) using our JAMS arbitration data set and we estimate columns (3)-(4) using our AAA arbitration data set. Table A6b corresponds to a linear regression model (eq. 1). The dependent variable in columns (1)-(2) is the amount awarded to the consumer through JAMS arbitration. For the JAMS data set we only observe the award granted and do not observe the awards that were requested by the consumer. The dependent variable in columns (3)-(4) is the percentage of award granted relative to award requested. We include dispute type/industry fixed effects in each specification. The most popular dispute types in the JAMS data set are employment (n=184), debt collection (n=35), and credit (n=31). The most popular dispute types in the AAA data set are financial services related (n=435), car sale/lease (n=172), and telecommunications/wireless/cable/satellite (n=85).