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Mark L. Egan
Gregor Matvos
Amit Seru

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ABSTRACT

This paper studies the impact of the arbitrator selection process on consumer outcomes when firms hold an informational advantage in selecting arbitrators. Exploiting data on arbitration cases and randomly generated lists of potential arbitrators presented to both parties over the past two decades in the securities industry, we establish several motivating facts. These facts 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 $60,000 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 would benefit informed consumers but hurt the uninformed.

Mark L. Egan
Harvard Business School
Baker Library 365
Soldiers Field
Boston, MA 02163
and NBER
megan@hbs.edu

Gregor Matvos
Kellogg School of Management
Northwestern University
2211 Campus Drive
Global Hub 4361
Evanston IL, 60208
and NBER
gregor.matvos@kellogg.northwestern.edu

Amit Seru
Stanford Graduate School of Business
Stanford University
655 Knight Way
and NBER
aseru@stanford.edu

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 over the selection of the arbitrator. For example, in securities arbitration, each party is presented with a randomly generated list of arbitrators and can strike a limited number of arbitrators from the list. 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). The existing literature has mainly focused on arbitration in which both parties are equally informed, such as those arbitrations between unions and employers, or arbitration in an experimental setting.[1] This paper studies consumer arbitration, emphasizing the impact of the arbitrator selection process when firms hold an informational advantage in selecting arbitrators.

This paper has two goals. The first is to establish several motivating 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. 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 ability to choose pro-industry arbitrators from a given pool, and the equilibrium pro-industry tilt in the arbitration pool that arises because of competition between arbitrators. 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 can hurt them instead, illustrating the importance of investor sophistication in consumer financial markets (e.g., Agarwal, Chomsisengphet, Mahoney, and Stroebel, 2015; Argyle, Palmer, and Nadauld, 2019; Anderson, Campbell, Nielsen and Ramadorai, 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, by showing that increased competition is

[1](Farber and Bazerman, 1986; Bloom, 1986; Ashenfelter et al., 1992)

not always beneficial (e.g., Budish, Cramton, and Shim, 2015; Aquilina, Budish, and O'Neil 2020; Budish, Lee and Shim 2019).

We study arbitration in the securities industry using a new data set of roughly 5,000 disputes between consumers and financial advisers over the period 1998 to 2019. 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 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.[2] For a significant subset of cases, we observe the randomly generated list of potential arbitrators presented to both parties, in addition to the arbitrators who were selected to the case.

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 $760,000 and $240,000 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. We use these facts to motivate the assumptions behind our quantitative model. 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 36% of the variation in arbitration awards in excess of case characteristics. An arbitrator who is more industry friendly by one standard deviation awards 14 percentage points (pp) smaller damages relative to the damages requested. For a median case ($240,000), this translates to a $33,600 smaller award for the consumer. It is therefore

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

not surprising that, anecdotally, brokerage firms maintain proprietary internal arbitrator rankings, or arbitrator "strike lists," to guide their arbitrator selection process.

Second, we find that firms have an informational advantage over consumers in choosing arbitrators who are favorable to them. The list 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. Being industry or consumer friendly should not increase selection chances. Instead, we find that industry-friendly arbitrators are 50% more likely to be chosen from the list than their consumer-friendly counterparts. Several tests exploiting within geography $\times$ time variation among other things, suggest that these patterns suggest that these patterns are not due to unobserved case characteristic driving arbitrator selection. The strongest evidence comes from subset of cases for which we observe all arbitrators on the randomly generated list. Exploiting within case differences in arbitrators' industry friendliness on these cases, we show that arbitrator selection arises because firms are better at eliminating consumer-friendly arbitrators. Additional evidence reveals that firm's advantage is driven by their extensive experience with arbitration (see Appendix B).

In the second part of the paper, we present a quantitative model of arbitrator selection. The analysis in the first part of the paper shows that firms' have an informational advantage when choosing arbitrators from a given pool of arbitrators. The model highlights a second advantage of the informed party: because arbitrators compete to be selected by the informed party (the firm), the whole pool of arbitrators becomes more industry friendly in equilibrium. This competition effect can be large, because selection is determined by how industry friendly arbitrators are relative to 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, several 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. This effect can best be seen in the special case when arbitrators have no concerns about fairness and only want to maximize their monetary payoffs. In that situation, every arbitrator wants to be a bit more industry friendly than other arbitrators. This way, firms will not strike them from the list, increasing the arbitrators' chances of earning arbitration fees. Because all arbitrators want to do the same, this results in a "race to the bottom." The only equilibrium in that situation is for all arbitrators to be as industry friendly as possible. In our full model, the competition effect does not unravel to the same degree, because it is costly for arbitrators to deviate from what they believe is fair. The extent of this tradeoff is pinned down by the data. The fact that the industry friendliness of the overall pool of arbitrators changes in response to firms being informed has several implications.

The first regards the measurement of informed parties' informational advantage in arbitration. The model illustrates that the analysis we perform in the first part of the paper measures only one part of the advantage: it 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 whether 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. We show that this result has important consequences when considering policy changes to the arbitrator system in the counterfactuals.

We calibrate the model to quantify firms' informational advantage. 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. We use the estimates to decompose the informational advantage into the advantage of being better at striking arbitrators from a fixed arbitrator pool, and the equilibrium effect on the pool itself. Firms' informational advantage is substantial: selecting arbitrators without input of the parties would increase consumer awards by 8pp (relative to the amount requested), or $60,000 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 3.5pp 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 that have been proposed by the regulator (FINRA), to examine how different mechanisms impact both the mean and distribution of awards. 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 friendly" if consumers were as informed as firms, 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 $45,000, 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 substantially.

We also study several extensions of our model. One concern that is often voiced by the industry is that increasing the consumer friendliness of the arbitration system would result in an increased propensity for "frivolous" cases. Such cases have no merit; instead, consumers pay the legal costs of filing a case in the hope of winning a large award, for example, by drawing a consumer-friendly arbitrator. We compute the expected award for frivolous cases under different arbitration selection mechanisms, and find that the incentives to file a frivolous case under the current system are limited. The expected payoff of bringing a low-merit case is $2,150. Given the fixed costs of arbitration, our results suggest a limited upside to filing frivolous cases. Consistent with intuition, more consumer friendly mechanisms increase the payoffs to frivolous cases. Our results also suggest that mechanisms that reduce the variance of arbitration outcomes could be effective in decreasing the number of frivolous arbitration cases.

Overall, our model highlights the qualitative and quantitative importance of accounting for firms' informational advantage when designing arbitration in the securities industry and potentially consumer arbitration more generally. Policies, which would be consumer friendly if both parties were informed, can end up being industry friendly if only firms are informed. We hope to provide a workhorse quantitative model that allows for policy evaluation, which can be extended in several ways to account for risk aversion, frivolous cases, and different welfare criteria relevant to evaluating arbitration mechanisms.

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 work suggests that parties can only do so when informed. Kondo (2006) examines securities arbitration administered by the National Association of Securities Dealers (NASD) over the period 1991-2004. In contrast to the standard arbitrator selection process that we study, this period covers a time when the regulator actively participated in selecting arbitrators. Choi, Fisch, and Pritchard (2014) also 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. In contrast to these papers, our work focuses on assessing how consumer sophistication impacts the arbitrator selection process and outcomes in FINRA arbitration over the last two decades. During this time the arbitration process followed by FINRA in consumer securities disputes resembles the processes followed in consumer arbitration disputes more generally, making our findings potentially widely applicable (see Appendix D for some preliminary evidence). More importantly, we develop and calibrate a workhorse 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. We show that both these components are quantitatively important when evaluating various (some currently proposed) 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.

[3]Crawford (1979, 1982) studies the effect of conventional and final-offer arbitration on negotiated settlements. Farber (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.

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; Gomes, Haliassos, and Ramadorai, 2020). 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) and due to inattention (Giglio, Maggiori, Stroebel and Utkus, 2020).[4]

More broadly, our paper links to the literature using quantitative models to study the effect of competition in financial markets (Koijen and Yogo, 2016; Benetton, 2018; Crawford, Pavanini, and Schivardi 2018; Allen, Clark, and Houde 2019; Bhattacharya, Illanes, and Padi , 2019; Allen, Clark, Hickman, and Richert, 2020; Gilbukh and Goldsmith-Pinkham, 2020). We depart from much of this literature by focusing on competition between arbitrators in the area of dispute resolution, rather than on competition in the context of consumers choosing financial products. That competition can sometimes be "undesirable" is related to the larger literature of market design in financial markets (Budish, Cramton, and Shim, 2015; Aquilina, Budish, and O'Neil 2020; Budish, Lee, and Shim 2019). The concept of arbitrator slant and competition is similar to the choice of political slant in the media industry (Gentzkow and Shapiro, 2006).

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. 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.

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, cell-phone, 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 proce-

[4]Our paper also relates to the literature on misconduct among financial advisers including Dimmock et al. (2018); Qureshi and Sokobin (2015); Egan et al. (2017; 2019); and Charoenwong et al. (2019).

dural 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.[5]

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 the 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.[6] 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 and brokerage firms 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 "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. Arbitrators are classified as public, non-public, and public chairpersons. Public arbitrators are those that have not worked in the financial industry in the past five years.[7] Conversely, non-public arbitrators either currently work in the financial services industry or have done so within the past five years. Public arbitrators can qualify as chairpersons if they:

[5] 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.

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

[7] Since 2015, this definition was expanded to exclude all individuals with any experience in the financial industry (SR-FINRA-2014-028 eff. June 26, 2015).

(i) have served on at least three arbitration panels; or (ii) have served on at least one arbitration panel, have a law degree, and are members of the bar.

Arbitrators are selected using the Neutral List Selection System (NLSS). 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)). 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 chairperson arbitrator, claims between $50,000-100,000 have one chairperson arbitrator but can have up to three arbitrators, and claims over $100,000 generally have three arbitrators. Cases with three arbitrators have one chairperson, one public arbitrator and one non-public arbitrator.[8] For cases with one arbitrator, the NLSS randomly generates a list of 10 public arbitrators from the FINRA public chairperson roster. For cases with three arbitrators, the NLSS randomly generates three separate lists: a list of 10 arbitrators from the non-public arbitrator roster, a list of 10 arbitrators from the FINRA public arbitrator roster, and a list of 10 arbitrators from the FINRA public chairperson roster (FINRA 12400). [9]

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.'"[10] 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. Prior to a 2007 rule change, parties were allowed to strike an unlimited number of arbitrators from each list. Starting in 2007, the number of strikes was limited to four on each list. 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.

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.[11] 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 fed-

[8] Starting in 2011 FINRA allowed customers to choose an all public arbitration panel (FINRA Regulatory Notice - 11-05)

[9] In 2017 FINRA increased the size of the list of public arbitrators from 10 to 15 and allowed both parties to strike 6 arbitrators from the list (FINRA Regulatory Notice 16-44)

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

[11] 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.

eral district judges ($79 per hour).[12],[13] 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.

III Data

III.A Data Construction

We construct a novel data set containing the details and awards of roughly 5,000 securities arbitration cases involving consumer disputes with financial advisers occurring over the period 1998-2019. 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 universe of arbitration hearings. For each arbitration case, 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 contain detailed accounts of the nature of the disputes and awards, providing us with a detailed picture of the cases and the similarities between cases. The data cover consumer arbitration, as well as other arbitration disputes such as employment disputes between advisers and their respective employers. 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.

Random List of Arbitrators: For a subset of cases (536) studied in Honigsberg and Jacob (2020), we also observe the lists of arbitrators that were presented to the litigants. These lists are randomly generated through the NLSS system. The lists are de-identified, but have been merged with our estimates of individual arbitrator industry friendliness. For each case, FINRA typicallly generates three separate lists of 10 arbitrators and one arbitrator is selected from each list. In our empirical analysis we examine the probability an arbitrator is selected for a case conditional on the arbitrator appearing on the list. Thus, even though we only observe the list for a subset of our cases, the

[12]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.

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

effective sample size is quite large (5k+) because the appropriate unit of observation is at the case-by-list-by-arbitrator level. We also exploit exogenous variation in which arbitrators appear on the randomly generated list to estimate arbitrator fixed effects.

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 also construct employment histories of arbitrators who have been employed in the financial industry in the past.

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 4,699 consumer arbitration disputes, which is the *universe* of arbitration disputes reported in BrokerCheck. These matched data represent our main data set. We describe the information we can observe in detail in the next section.[14]

III.B Summary Statistics: Cases and Arbitrators

Our primary unit of observation is at the case-by-arbitrator level. Roughly 10% of consumer complaints in arbitration involve multiple advisers and claims, and arbitrators separately assess damages across advisers. Consequently, we define an arbitration case at at the case-by-adviser level, but account for the potential correlation when computing standard errors. Our baseline data set consists of 4,699 consumer arbitration cases and 11,756 arbitrator-by-case observations. These cases involve substantial monetary amounts: mean and median damages requested are $758,648 and $240,000, respectively. The median award granted is 35% of the requested amount, with large differences in arbitration outcomes: the standard deviation is 60% (Figure 1; Table 1). The distribution is skewed to the right, with a mean award of 53% of damages, partially because awarded claims can exceed damages requested if punitive damages are awarded.

We report the summary statistics for our main outcome variables of interest in Table 1 and report summary statistics for the remaining variables in the Appendix. As discussed in the Appendix, we observe detailed information on the nature of the financial products and advisers involved in the dispute and the specific allegations. In our baseline analysis, we classify allegations into 11 different categories across six different types of financial products. Common allegations include misrepresentation and fraud, and as discussed in the proceeding section, the specific allegations are highly correlated with arbitration awards. As a robustness check, we use bag-of-words, a common natural language processing method (Gentzkow, Kelly, and Taddy, 2019; Bodoh-Creed, Boehnke,

[14]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.

and Hickman, 2018), to further distill the case allegations where we construct dummy variables for the 500 most common words in case documents. 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 and 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.

We observe 4,992 unique arbitrators in our sample and we observe multiple observations (i.e. overseeing multiple cases) for 2,714 arbitrators. The arbitration panel size typically consists of one or three arbitrators. Even though many arbitrators have little case experience, an arbitrator who oversees an average case typically has a fair amount of experience. For the average case in our sample, the arbitrator has overseen 11 total arbitration cases (including non-consumer dispute arbitration cases) and 4 consumer dispute arbitration cases. Therefore, most cases in our sample are overseen by arbitrators with extensive arbitration experience.

IV Motivating Evidence: Arbitrator Heterogeneity

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' strategic response to competition. Here, we present simple evidence, which motivates the underlying assumptions in our structural model: (i) there are systematic differences in how arbitrators award cases (i.e. some arbitrators are more industry-friendly than others), and (ii) firms have an informational advantage in selecting favorable arbitrators.

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 the Appendix.

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:

$$Awarded_{ijlt} = \beta X_i + \mu_j + \mu_l + \mu_t + \epsilon_{ijlt}. \tag{1}$$

Observations are at the arbitrator-by-case level, where i indexes the arbitration case, j indexes the location, l indexes the arbitrator, and t indexes time. The dependent variable $Awarded_{ijlt}$ reflects the award granted divided by the award requested.[15]

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, and the size of the reward requested. In the Appendix, we do extensive robustness testing using natural language processing to control for more detailed case specifics. We control for the size of the case and the composition of the arbitration panel in terms of the number of arbitrators. To further control for case merits in our baseline specifications, we include the characteristics of the defendant financial adviser. We control for the adviser's experience, the six most popular adviser qualifications/licenses, the adviser's total number of qualifications, and any past record of misconduct.

We also include time (μ_t) and location (μ_j) 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. Location fixed effects help control for possible geographic differences in claims. Because arbitrators are drawn from a pool based on the hearing location, these fixed effects 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 and institutional changes in the arbitration proceedings. As discussed in the Appendix, we find that these observable adviser and case characteristics explain 19% of the variation in awards. The case characteristics also predict awards in meaningful and intuitive ways. For example, cases in which the adviser's guilt is verifiable such as those involving fraud, churning, and selling unregistered securities tend to have higher awards awards on average.

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 and arbitration forum. These fixed effects account for persistent differences in how arbitrators award cases. 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 over what the fair or correct award would have been.

We report the estimated distribution of arbitrator fixed effects in Figure 2. We normalize the mean of fixed effects to match the average percent of awards granted in the data, 53%. Therefore, arbitrators with a fixed effect below 53% are on average more industry friendly than other arbitrators. Including arbitrator fixed effects explains a substantial amount of the variation awards, increasing the R^2 from 19% to 55% in eq. (1). We also find that 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.

[15]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.

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), which scales the distribution of OLS fixed effects by a constant factor.[16] Although the variation in the empirical Bayes estimated fixed effects is 39% 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 14pp smaller relative to the amount requested, holding other attributes of the case fixed. Given that the median damages requested are roughly $240,000, the consumer would be awarded $33,600 less. Overall, our results are consistent with the idea that the choice of arbitrator can have a meaningful impact on case outcomes.

Accounting for Selection: One important consideration in estimating arbitrators' industry friendliness is the presence of unobserved case characteristics, which determine the extent of the award and simultaneously affect which arbitrators are chosen. Such selection could bias our estimates of arbitrator fixed effects. In the Appendix we re-estimate arbitrator fixed effects while accounting for selection. To do so, we require an instrument that is correlated with the probability an arbitrator is selected to a case but is otherwise uncorrelated with the unobserved merit of the case. We construct two instruments based on the arbitrators' physical distance to the case venue and exploit the fact that we observe the full random list of arbitrators for a subset of cases. The first instrument is the arbitrator's own distance to the case venue where we also control for both the home location of the arbitrator and location of the case. The second instrument is constructed based on the average distance of all other arbitrators appearing on the randomly generated list.[17]

The rationale behind the our first instrument is that, all else equal, distance decreases the probability an arbitrator accepts a case because of the travel involved. Similarly, the rationale behind our second instrument is that the further away the case is from all other arbitrators appearing on the list, the less likely those other arbitrators are to oversee the case. Both are likely to satisfy the exclusion restriction. It is unlikely that arbitrator friendliness to a case is related to how far they are from the case venue. Similarly, because the list of other arbitrators is randomly generated,

[16]We re-scale the estimated distribution of arbitrator fixed effects 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). 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

[17]As disucssed in Section III.A. we only observe the arbitration list for a subset of the cases in our data. We account for the missing lists as follows when constructing our second instrument $Z^{(2)}$. For cases where the list is not observed, we set the value of the instrument to zero, and we include a dummy variable indicating whether $Z^{(2)} > 0$, such that our instrumental variables strategy only exploits variation in the data where we observe the arbitration list. The fact that we do not observe the list for all cases does not invalidate or instrument, but it does reduce the statistical power of the instrument. See Appendix A for further details.

their distance from the case depends on pure chance. Notably, since we control for the location of the arbitrator, and the case venue, the instrument is not picking up the idea that some venues have different cases, or that arbitrators' locations are correlated with their quality. In the Appendix A, we show that arbitrators are more likely to preside over a case if they live closer to the case and if the other arbitrators on the randomly generated list live further away, which suggests our instruments are relevant. We find that accounting for potential selection, where arbitrators might be assigned to certain types of cases, has little effect on the estimated arbitrator fixed effects. This is not necessarily surprising given the way arbitrators are quasi-randomly assigned to cases.

IV.B Arbitrator Appointments and Industry Friendliness

The previous section documents that some arbitrators are relatively more friendly to 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. 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.

IV.B.1 How does Industry Friendliness Impact the Probability of Being Selected?

In this section, we show that industry-friendly arbitrators are more likely to be selected from a randomly generated list of arbitrators. For a subset of cases in our data, we observe the list of arbitrators generated by the NLSS from which the parties can strike arbitrators, rather than just observing the arbitrator that was appointed to the case. Observing the list allows us to exploit within case variation in arbitrators' industry friendliness, and therefore implicitly control for case characteristics.

We do so by estimating a multinomial logit regression model:

$$\Pr\left(D_{il}\right) = \frac{\exp\left(X_l\beta + \gamma\widehat{\mu_l^{EB}}\right)}{\sum_{n\in\mathcal{N}_i}\exp\left(X_n\beta + \gamma\widehat{\mu_n^{EB}}\right)}, \tag{2}$$

The probability arbitrator l is selected to case i, indicated by the dummy variable $D_{il} = 1$, is a function of her consumer friendliness $\widehat{\mu_l^{EB}}$ and her characteristics $X_l\beta$ relative to the characteristics and consumer friendliness of all other arbitrators on the list. We measure consumer friendliness using our estimated empirical Bayes fixed effects ($\widehat{\mu_l^{EB}}$) from the previous section (Figure 2), where a higher fixed effect indicates that the arbitrator grants higher awards conditional on observable case characteristics.[18] The set of arbitrators on the list for case i is denoted $\mathcal{N}_i$. The above probability

[18]The empirical Bayes adjustment only re-scales OLS fixed effects, it aids in interpreting the magnitudes, but does not

highlights the importance of relative differences in arbitrator consumer friendliness. What matters for selection is not how consumer friendly an arbitrator is in absolute terms $\widehat{\mu_l^{EB}}$, but how consumer friendly an arbitrator is relative to the other arbitrators on the list $\widehat{\mu_n^{EB}}$. The parameter of interest is γ which measures how consumer friendliness translates into the probability an arbitrator is selected. Under the hypothesis that firms are more informed than consumers, we would expect $\gamma < 0$ such that arbitrators who are more consumer friendly (higher $\widehat{\mu_n^{EB}}$) are less likely to be selected.

One caveat with our data is that for those cases involving multiple arbitrators and consequently multiple arbitration lists, we observe all arbitrator names that appear on the lists, but we do not observe whether they appeared on the chairperson list, public arbitrator list, or non-public arbitrator list. Consequently, in our empirical analysis, we define the set $\mathcal{N}_i$ based on all arbitrators that appear on the lists for case i, which introduces potential measurement error in the set $\mathcal{N}_i$. As a robustness check we re-estimate our multinomial logit model where we restrict our attention to those arbitrators l who have ever been chairpersons in our data—under the assumption that a chairperson is picked from such a list—which reduces measurement error in the set $\mathcal{N}_i$.

Table 2 displays the corresponding estimation results. The columns differ with respect to the sample. Column (1) presents our baseline estimates and in column (2), we restrict our attention to arbitrators who have ever been chairpersons. Arbitrator fixed effects are measured with noise, especially for arbitrators with few awards. To reduce the extent of measurement error, in columns (3)-(4), we restrict our analysis to those arbitrators who grant at least five awards in our baseline data and estimate the corresponding conditional likelihood (conditioning on the arbitrator observing at least five awards).

In each specification, we estimate a negative and significant relationship between how consumer friendly an arbitrator is and her probability of being selected. The results in column (4) indicate that, conditional on being on the list, a one standard deviation decrease in consumer friendliness (a 14pp decline in $\widehat{\mu_n^{EB}}$) is associated with a 33% (7pp) increase in the probability of being selected.[19] In other words, the results in column (4) indicate that an arbitrator who grants awards that are below average $\left(\widehat{\mu_n^{EB}} < \overline{\widehat{\mu_n^{EB}}}\right)$ is roughly 50% more likely to be selected than an arbitrator who grants above average awards $\left(\widehat{\mu_n^{EB}} > \overline{\widehat{\mu_n^{EB}}}\right)$.

IV.B.2 Extension to the full sample

We do not observe the list of arbitrators from which the parties can strike arbitrators for the full sample of cases. Nevertheless, we want to illustrate that the idea that industry-friendly arbitrators are selected more frequently is robust to the extended sample. We examine how an arbitrator's estimated fixed effect $\widehat{\mu_l^{EB}}$ impacts her probability of being selected in the following regression:

$$Number\,of\,Cases_l = \beta X_l + \gamma\widehat{\mu_l^{EB}} + \mu_g + \eta_l. \tag{3}$$

affect the regression estimates otherwise.

[19] We compute the marginal effect as $0.07 = -0.42 * \bar{p} * (1 - \bar{p})$ where $\bar{p} = 0.21$ is the average probability of being selected in the sample corresponding to column (3).

Our observations are at the arbitrator level, where l indexes the arbitrator.[20] $Number\,of\,Cases_l$ measures the number of cases an arbitrator oversees in her career. The key independent variable of interest is again the arbitrator's fixed effect $\widehat{\mu_l^{EB}}$. The term X_l is a vector of arbitrator controls and includes the number of years she has been active as an arbitrator in our sample.

We also include geographic region fixed effects (μ_g) to account for the fact that FINRA randomly selects a list of arbitrators based on the geographic region of the case. This fixed effect captures regional differences in case load and arbitrator fixed effects. FINRA randomly constructs its list of arbitrators based on the arbitrator's location and public arbitrator status. This determines the pool of arbitrators who compete to be selected. We already control for the location of the case when estimating arbitrator fixed effects. We also interact geographic region fixed effects with whether the arbitrator is a public arbitrator (i.e. non-industry affiliated), a non-public arbitrator, or eligible chairperson, which allows us to compare differences in how industry/consumer friendly arbitrator is within the pool of eligible arbitrators. We estimate eq. (3) using linear regression in our baseline specification, but we also estimate the model using Poisson and negative binomial regressions to account for the fact that the dependent variable is a count variable. The estimates presented in Table 3, column (3) indicate that a one standard deviation increase in an arbitrator's industry friendliness is associated with the arbitrator being chosen at a 8.5% higher rate.

Overall, we find that arbitrators who grant larger awards to consumers, given case characteristics, are less likely to be selected. This is despite the fact that they have an equal chance 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 and that firms have an informational advantage in striking arbitrators. In the appendix, we explore several robustness checks and find that the relationship between how industry friendly arbitrator is and the probability that an arbitrator is selected for a case is robust to alternative measures of industry friendliness, such as backward looking measures, measures constructed using machine learning, and accounting for selection (Appendices A and B).

IV.C Interpretation

Sophistication: Our central hypothesis is that firms are more sophisticated than consumers in striking arbitrators, which results in industry-friendly arbitrators being chosen more often. In Appendix B, we provide more direct evidence that parties' sophistication in arbitration helps them choose more favorable arbitrators. One source of firms' advantage is that they have more experience in arbitration than the average consumer (see, Nichols, 1999; Black and Gross, 2002; Barr, 2014; Silver-Greenberg and Gebeloff, 2015). Anecdotal evidence suggests that brokerage firms often maintain proprietary internal arbitrator rankings, or arbitrator "strike lists." Additional experience in arbitration then allows firms to design a better "strike list," and allows them to update on which information to acquire in future arbitrations, the importance of selecting arbitrators, and

[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.

which attorneys to hire to help with selecting arbitrators.

In our data, the average firm is involved in 36 consumer dispute arbitrations, but there is substantial variation in firms' arbitration experience, making some firms more informed than others. We find that more experienced firms select more industry friendly arbitrators, consistent with the idea that arbitration experience provides an advantage. While we argue that firms are generally the better informed party, consumers also differ in their sophistication. In our data we also observe details on the consumer claimant and their legal representation. 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 who might be more sophisticated than the average consumer, such as an attorney or accountant,. After speaking with industry participants, we learned of a class of attorneys who specialize in securities arbitration (PIABA attorneys). We find that those consumers who are part of a trust and use a PIABA attorney tend to select more consumer friendly arbitrators. Overall, these results suggest that more sophisticated consumers (firms) choose more consumer-friendly (industry-friendly) arbitrators (see Appendix B, Table A5).

Alternative Explanations: The main alternative hypothesis to our findings 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. As noted in Section IV.A, we exploit the randomly generated list of arbitrators, and the distance of arbitrators from the arbitrator case to account for such selection in computing arbitrator fixed effects. Moreover, in Section IV.B, we show that even holding the case fixed, i.e., using within case variation, the more industry friendly arbitrators from the list are chosen more frequently. Here, we discuss additional evidence that selection is not an issue, which we present in detail in Appendices A and B.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.

We also perform Altonji et al. (2005) and Oster (2019) style tests and compute arbitrator fixed effects iteratively by conditioning on richer case characteristics. Using these case characteristics, including some that we construct using natural language processing of arbitration texts, we can explain approximately 85% percent of variation in case awards. Despite large changes in R^2, we find little effect on the estimated arbitrator fixed effects (see Appendix B, Figure A2, Table A6).

Lastly, we find that an arbitrators' personal experience is correlated with his/her fixed effect. Roughly 40% of arbitrators either currently or previously work in the securities industry. We find some evidence suggesting that those arbitrators who have been involved in customer disputes tend to grant lower awards. Conversely, we find that those arbitrators who were fired from the industry (have an employment termination flag on their regulatory file) tend to grant higher awards. These results suggest that the estimated fixed effects are driven by inherent differences across arbitrators rather than by differences in case characteristics.

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 the tilt in the arbitration pool as a whole, 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 $a_G \epsilon \left[\underline{a}, \bar{a}\right]$ 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 (policies), which we incorporate in Section VI.D.2.

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 $b_i \epsilon \left[\underline{b}, \bar{b}\right]$ regarding the fair award for the arbitration case.[21] We can think of these beliefs as innate characteristics that arbitrators bring to the case, which determine how an arbitrator would rule on the case in the absence of monetary incentives. These could be formed based on their prior work experience, education, upbringing, or personal interaction with the industry. For example, in the Appendix, we show that arbitrators that were fired from the advisory industry tend to be more consumer friendly and grant higher awards. The distribution of beliefs among arbitrators in the population is $F\left(\cdot\right)$; the density $f\left(\cdot\right) = F'\left(\cdot\right)$ is continuous and strictly positive everywhere.

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

[21] 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)

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.

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:

$$U(b_i, a_i) = \Gamma(a_i, G(\cdot))\,(fee - \theta|a_i - b_i|)\,. \tag{4}$$

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. 4) 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:

$$\Gamma(a, G(.)) = \frac{1}{n-k} P(a_i; 1, n-k, n). \tag{5}$$

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. We illustrate the striking effect in Figure 3, which displays the densities of awards granted $\widetilde{g}(\cdot)$, slant $g(\cdot)$, and beliefs $f(\cdot)$ that we estimate in the proceeding section. The distribution of awards granted $\widetilde{G}(\cdot)$ is smaller (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.

It is useful to also provide the benchmark if both consumers and firms are informed about arbitrator slant, which is frequently used to guide policy discussion. If consumers are informed then the arbitrators in either tail of the distribution face elimination, and the probability that an arbitrator is selected becomes:

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

Informed consumers remove k arbitrators with the most pro-industry (lowest) slant, and firms remove 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 as 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:

$$max_{a_i} \Gamma(a_i, G(\cdot)) \left(fee - \theta \left|a_i - b_i\right|\right). \tag{6}$$

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:

$$|a_i - b_i| = \frac{fee}{\theta} - sgn(a_i - b_i) \times \frac{\Gamma\left(a_i; G(\cdot)\right)}{\gamma\left(a_i; G(\cdot)\right)} \forall a_i \neq b_i. \tag{7}$$

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:

$$a_i = \min\left\{b_i - \frac{fee}{\theta} - \frac{\Gamma\left(a_i; G\left(\cdot\right)\right)}{\gamma\left(a_i; G\left(\cdot\right)\right)}, b_i\right\}. \tag{8}$$

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 C):

$$a_i = \min\left\{b_i - \frac{fee}{\theta} + \frac{\int_{b_i}^{\bar{b}}\Gamma(\tilde{b}, F(\cdot))d\tilde{b}}{\Gamma(b, F(\cdot))}, b_i\right\}. \tag{9}$$

We use the closed form expression (9) when computing counterfactual equilibria, which link the model with actual policy proposals in Section VI.C. We illustrate the effect of competition among arbitrators in Figure 3, which corresponds to our model estimates (discussed in Section VI). The distribution of arbitrator slant $G(\cdot)$ is industry friendly relative to the distribution of arbitrator beliefs based on what a "fair" award should be, $F(\cdot)$ in the sense of first order stochastic dominance. Jointly, with the striking effect, the 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 $F(\cdot)$.

V.B.3 Special Case: Race to the Bottom and Statistical Exchangeability

Here we present a special case, where arbitrators care only about monetary incentives ($\lim \theta \to 0$). This case highlights why the competition effect can be very large. It also shows the link between our model, and the concept of "Statistical Exchangeability," a common fairness criterion in arbitration (Ashenfelter, 1987). If arbitrators only care about monetary incentives, the competition effect results in a race to the bottom: all arbitrators choose the most industry-friendly arbitrator slant possible $a_i = \underline{a}$. To see why, imagine that in equilibrium some arbitrators are more industry-friendly than others–$G(\cdot)$ 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 be selected with a positive probability, increasing her expected monetary payoff. She has no fairness concerns, so there is no utility cost to changing her slant. Choosing the most industry-friendly slant is therefore clearly a profitable deviation. Because every arbitrator wants to be the most industry friendly, there is a "race to the bottom." The only equilibrium is one in which all arbitrators, regardless of their beliefs of what is fair, choose a slant that is as industry friendly as possible, $a_i = \underline{a}$.

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 a benefit of competition between arbitrators (Ashenfelter, 1987). As our example above illustrates, with purely monetary incentives, the competition effect results in Statistical Exchangeability whether consumers are informed or not: all arbitrators reach the same decision. Therefore, statistical exchangeability may be a good criterion for fairness when all parties are informed. On the other hand, if only one party is informed, then the resulting decision can be quite "unfair," since all arbitrators are as industry friendly as possible. In other words, statistical exchangeability could be a necessary, but not sufficient condition for fairness.

VI Informational Advantage 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 redesign 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:

$$b_i = \max\left\{ a_i + \frac{fee}{\theta} + \sum_{j=1}^{n-k} \binom{n-1}{j-1} \frac{(n-1)!}{(j-1)!(n-j)!} \frac{G(a_i)(1-G(a_i))}{g(a_i)\,(j-1-(n-1)G(a_i))}, a_i \right\}. \qquad (10)$$

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, where we restrict the data to the post-2007 reform when FINRA limited the number of strikes to 4. In the data, we observe the distribution of slant, conditional on arbitrators being chosen, $\widetilde{G}(\cdot)$, 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 the underlying distribution given firms' striking behavior,

$$\widetilde{G}(a_i) = \sum_{i=k}^{n-1} \left(\sum_{j=n-i}^{n} \frac{n!}{j!(n-j)!} G(a_i)^j (1 - G(a_i))^{n-j} \right). \tag{11}$$

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 $\frac{1}{\Gamma(\widehat{a, G(\cdot)})}$.[22]

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. 10). 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 II, 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 change, arbitrators increased awards by roughly 5pp, 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 $\theta = 10,000$. This estimate implies that arbitrators are willing to deviate from their beliefs by 1pp for an extra \$100 increase in income. In other words, suppose the arbitrator

[22] Specifically, we use a Gaussian kernel and a smoothing parameter of 5%, which is in line with Silverman's Rule of Thumb (1986).

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 $1,000 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 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. (10) 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

VI.B.1 Overall 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 4 displays our non parametric estimation results. We also present a parametric model in which the underlying distribution of beliefs follows a gamma distribution, which is computationally tractable for computing counterfactual equilibria. To construct the parametric model, we parameterize the distribution of beliefs via maximum likelihood to match the non-parametrically estimated distribution of beliefs. Figure 3, displayed in the previous section, displays the corresponding parametric estimates. The primary object of interest is the distribution of arbitrators' inherent beliefs of the appropriate arbitration awards, $f(b_i)$, and we are interested in how the distribution of beliefs compares with the distribution of awards granted. 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 parties in the case had no input in the selection of arbitrators. Then arbitrators would be selected to the cases randomly, like judges in some courts. If arbitrators were randomly assigned to cases, the distribution of awards granted would simply reflect the distribution of arbitrator beliefs, as arbitrators no longer have any incentive to slant awards. Figure 4 shows that the density of arbitrator beliefs $f(\cdot)$ is shifted to the right of the density of arbitration awards $\widetilde{g}(\cdot)$. Thus, if arbitrators were randomly assigned, the distribution of awards would shift from $\widetilde{g}(\cdot)$ to $f(\cdot)$ and become more consumer friendly. The average award under the current system is 53% of the amount requested. If neither party had any input into the selection process, our estimates suggest that the mean award would be 61%. Given that the average award is on the order of $750,000, the model estimates suggest that the current arbitrator selection scheme costs consumers roughly 8pp, or $60,000 dollars. The shift in the distribution of awards affects the top half of the distribution more: the 10^{th} percentile award increases from 32% to 33%, while the 90^{th} percentile increases from 73% to 89%. In other words, the current arbitration system especially decreases the propensity of large awards to consumers relative to the system in which arbitrators are randomly chosen. The current mechanism does reduce the variance of outcomes, which is an often touted benefit of the arbitration selection process. The standard deviation of outcomes is reduced by 24% relative to a system in which parties have no input into arbitrator selection. These results show the extent to which the current arbitration scheme results in a biased distribution of arbitration awards 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 5). 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 same as 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. Therefore, if both parties are informed, our model estimates suggest that the average award would be roughly 60%. Importantly, with firms and consumers both informed, the selection mechanism reduces the variance of arbitration awards by 68% relative to the variance of beliefs. Thus, if both parties were informed, the current arbitration selection mechanism would be more effective in reducing the variance of arbitration awards and would be fair in the sense that the median award reflects the median belief.

VI.B.2 Decomposition: Striking and Competition

The current arbitrator selection scheme costs consumers roughly 8pp of awards, or $60,000 dollars. Figure 4 decomposes firm's 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 4.5pp lower than the equilibrium density of slant $g(\cdot)$. The striking advantage of firms therefore accounts for approximately 60% of firms' informational advantage.

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, which generates the competition effect. 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 3.5pp lower than their beliefs. In other words, the average arbitrator gives out an award that is 3.5pp 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 (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 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 C, 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 3 rather than our non-parametric estimates. As noted before, both models produce very similar estimates.

***Changing the Number of Strikes and Arbitration List Size*:** In 2017 FINRA proposed increasing the number of arbitrators on the list to 15, and simultaneously increasing the number of strikes to 6. We first separately study how each of these dimensions changes the distribution of awards, and then examine the plan as a whole. We first present the changes in awards as the number of strikes increases from one to seven in Figure 6a. 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 53%. As the number of strikes increases to six, $k = 6$, our estimates suggest that the average award will decline by 6pp. 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. 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.

We next study how changing the size of the list from which arbitrators are chosen would benefit consumers in Figure 6b. 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 6b 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 2pp.

Overall, FINRA's proposal therefore introduced two changes, which work in opposite directions. Increasing the number of strikes increases the pro-industry slant, but increasing the list size decreases it. Figure 6c illustrates that the proposed policy change would further increase the pro-industry slant, but the effects are modest. The average award decreases by 0.6pp.

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 would indeed be the case. Figure 6d displays the counterfactual distribution of awards if FINRA doubled the fee paid to arbitrators. Doubling the fee paid to the arbitrator will cause the average award to decrease by 6pp. The intuition is simple: increasing the fee paid makes the arbitrator more responsive to the informed party's preferences. 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.

VI.D Model Extensions

VI.D.1 Frivolous Cases

One concern in designing the arbitration system is the presence of "frivolous" cases. Because awards to consumers cannot be negative (awarded to the firm), the consumer may be willing to pay the legal costs of filing a merit-less case in hope of winning a large award, for example, by drawing a consumer friendly arbitrator. Making the arbitration system more consumer friendly could increase the probability a consumer files a frivolous case. Here, we formally examine a consumer's incentives of filing a frivolous case. We define a frivolous case as one where the average arbitrator's belief over a fair award, b_i, is zero. Even though the the average arbitrator believes the fair award is zero, a consumer may still bring a frivolous case to arbitration because there is a chance that consumer-friendly arbitrator is appointed to the case. We compute the expected award for frivolous cases under the different arbitrator selection mechanisms examined in the previous section.

We report the probability of winning and expected payoff of arbitrating a frivolous case in Table 4. The expected payoff of a frivolous case from the consumer's perspective under the current regime (k=4, n=10) is small. There is a 17% chance the consumer receives a positive award and the expected award is 0.89%. Relative to the median case size ($240k), the expected payoff of bringing a frivolous case is $2,150. Given the fixed costs of arbitration, our results suggest that there is a limited benefit to filing frivolous cases under the current regime.

Of course, different arbitrator selection mechanisms may have different implications for frivolous cases relative to the current regime. Consistent with intuition, more industry friendly mechanism decrease the payoffs to filing frivolous claims. Increasing the number of strikes to 6 (or doubling the fee paid to arbitrators) virtually eliminates the expected payoff to filing frivolous claims ($E[Award] = 0.07$) and the chances of finding a favorable arbitrator decrease to 2%. The decline arises through two forces. First, the average award decreases. Second, the variance of arbitrator slant also decreases, decreasing the chances of finding the consumer-friendly outlier. Increasing the strike list to 15 arbitrators increases the payoff to frivolous lawsuits by about 50%, to 1.31pp. Our results suggest that the incentives to file frivolous claims under the current system are present, but are not enormous. The results suggest that, if one did want to reduce number of frivolous arbitration cases further, introducing some industry-friendly bias into the arbitration proceedings or reducing the variance of arbitration outcomes could be effective. Of course, after considering the associated legal costs of filing a case, the payoff to filing a well justified, but low award case would also decline.

VI.D.2 Party 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, allowing consumers and/or firms to be risk averse does not alter the equilibrium distribution of awards or arbitrator slant. 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. Because arbitrator incentives are unaffected, they choose the same type. Thus, we do not need to specify the utility/risk preferences of consumers and firms to understand the distribution of awards for a given mechanism.[23]

Second, litigant risk aversion can alter preferences of the parties *across* different arbitration mechanisms. Risk averse consumers may prefer a system that is more pro-industry on average, but has a lower variance of awards. 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.

We illustrate the effect of risk aversion by assuming that consumers are risk averse and firms are risk neutral. Consumer *i* has constant absolute risk aversion preferences over awards (a_i) with a coefficient of risk aversion $\rho : U_C\left(a_i\right) = \frac{1-e^{\rho a_i}}{\rho}$. To evaluate consumer preferences across different mechanisms, we set $\rho = 3$ in our baseline analysis using the estimates form Egan et al. (2020). Also, for a point of comparison, we also evaluate the mechanisms under the assumption that consumers are quite risk averse ($\rho = 10$).[24] Since most reforms revolve around improving consumer outcomes across mechanisms, we compute consumers' expected utility relative to the current mechanism (expressed relative to the award requested) and display the estimates in Table 5.[25] The results indicate that if the number of strikes were limited to 2, the expected award would increase by 6pp from 52.6% to 58.6%, which would benefit consumers, but the standard deviation of the award would increase from 12.6% to 16.2%, which would make consumers worse off. Overall consumer's expected utility would increase by 4.4pp relative to the amount requested. In other words, awards would need to increase by 4.4pp in the baseline scenario in order for consumers to achieve the same expected utility as in the counterfactual with two strikes.

Overall, the results show that the rank ordering of consumer preferred mechanism does not change with modest risk aversion of 3. Mechanisms that increase expected awards are preferred by consumers. A large reason for that is that the standard deviation of awards does not change much across mechanisms ranging from 8.8% pp to 16.2%. These small changes in variance do not have a big impact on a consumer's utility unless the consumer is very risk averse (i.e. $\rho = 10$).

[23]Note that if arbitrators were risk averse, rather than risk-neutral, arbitrators would further slant awards in favor of the industry (assuming consumers are uninformed) in order to increase their probability of being selected.

[24]In terms of certainty equivalent, the parameter $\rho = 3$ ($\rho = 10$) implies that consumers are indifferent between the current mechanism which offers an expected award of 53% with a standard deviation of 12.6% and a mechanism that offers 50% (41%) award with certainty.

[25]Because awards are zero sum between firms and consumers, overall welfare under CARA preferences is mechanically a function of the variance of awards only. Welfare is therefore highest in mechanisms with lowest variance of awards.

As this example illustrates, our methodology is flexible enough to accommodate risk aversion and different welfare criteria to evaluate arbitration mechanisms.

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 those in 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. We hope that future work can extend our workhorse quantitative model to evaluate such alternative arbitration selection mechanisms.

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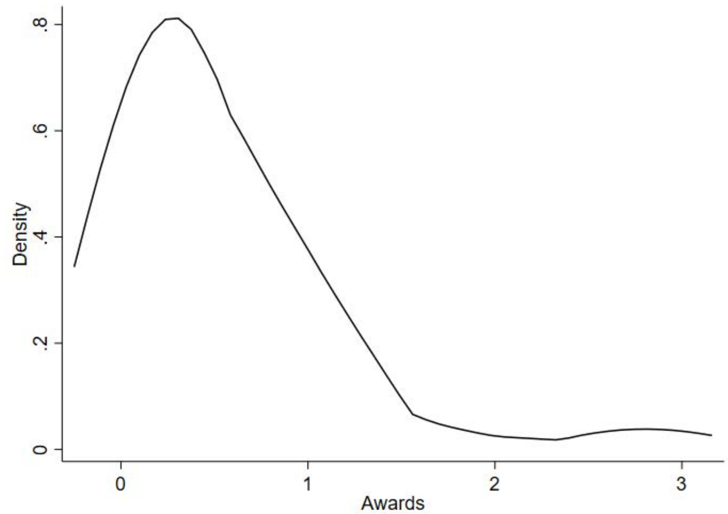
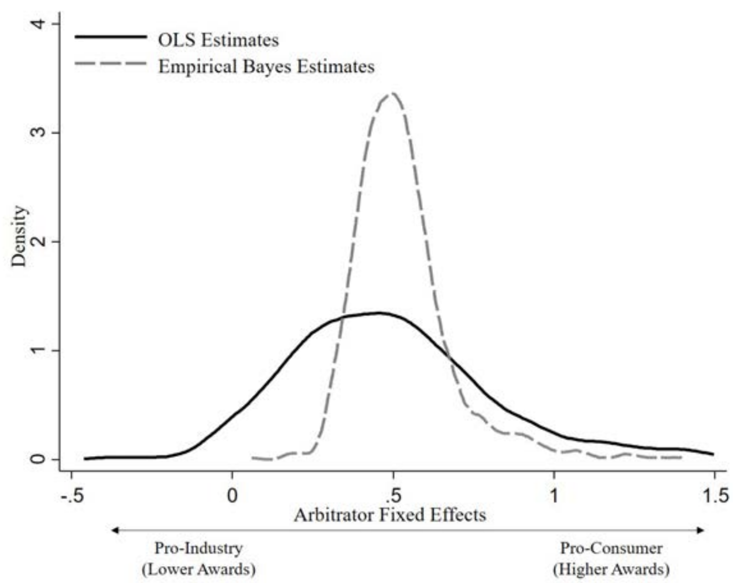
Figure 1: Award Distribution



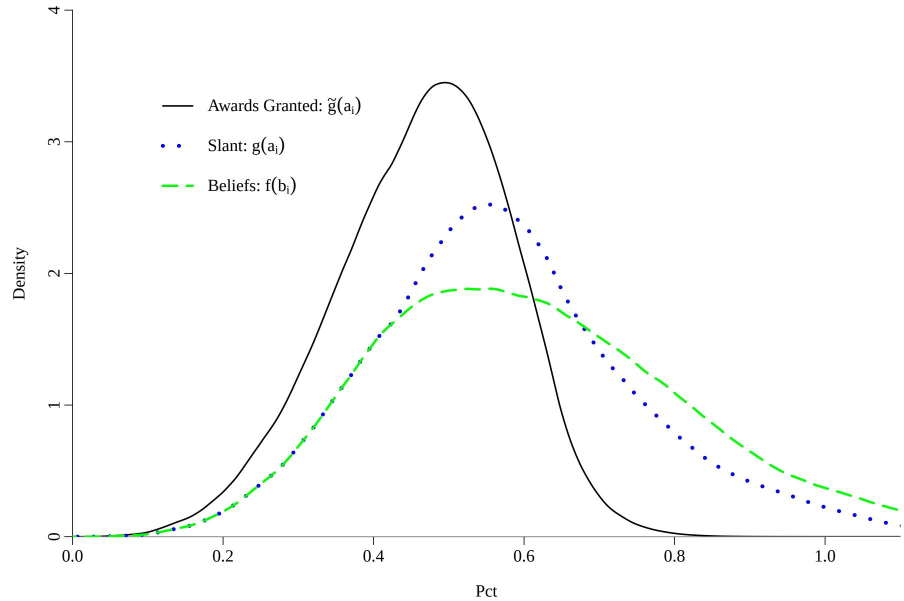
Note: the figure displays the distribution of arbitration Awards. We calculate Awards as the percentage of awards granted through arbitration divided by the awards initially requested by the consumer. The distribution of Awards is winsorized at the 1% level.

Figure 2: Arbitrator Fixed Effects



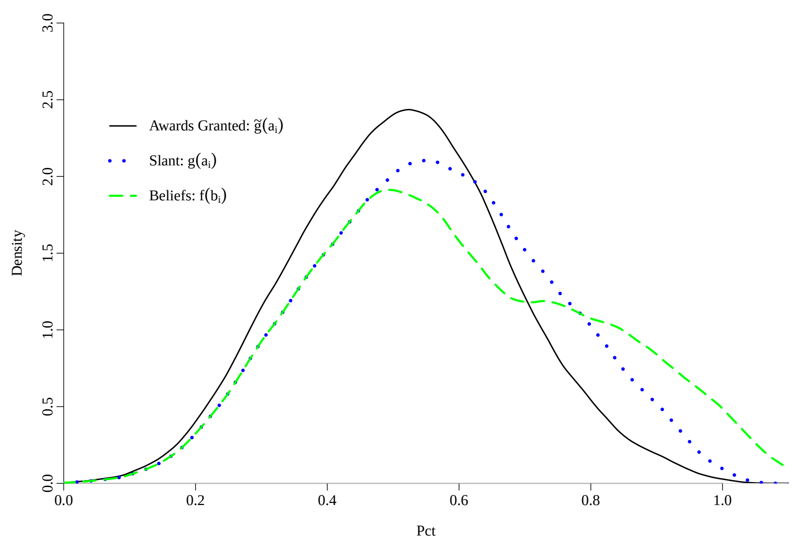
Note: the figure displays the estimated distribution of arbitrator fixed effects corresponding to eq. (1). The black empirical density reflects the distribution of OLS estimated arbitrator fixed effects estimated and the gray empirical density reflects the distribution of empirical Bayes estimated arbitrator fixed effects. We normalize the mean of the distribution of fixed effects to 53% which is the average Award in our sample.

Figure 3: Estimated Distribution of Arbitrator Beliefs, Slant, and Awards



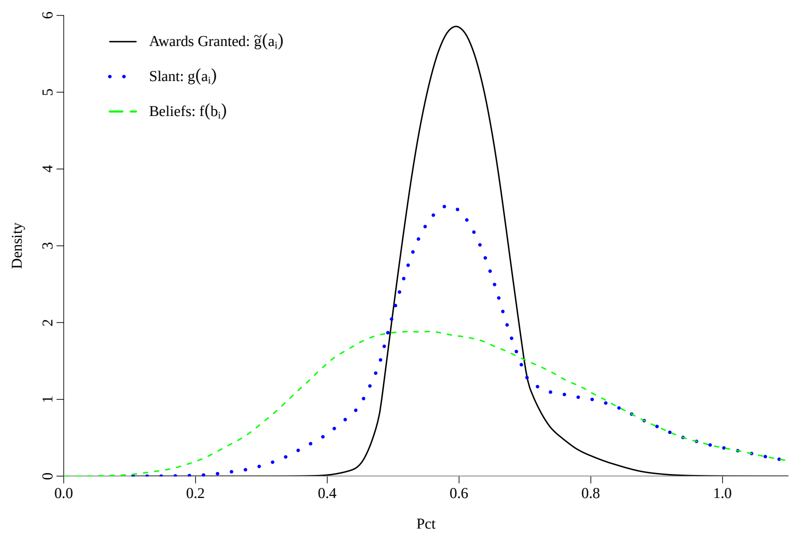
Note: the figure displays the estimated density of awards among the conditional distribution of selected arbitrators $\tilde{g}(a)$, the density of slant among the unconditional (entire) population of arbitrators $g(a)$, and the distribution of true beliefs among the unconditional (entire) population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

Figure 4: Estimated Distribution of Arbitrator Beliefs, Slant, and Awards



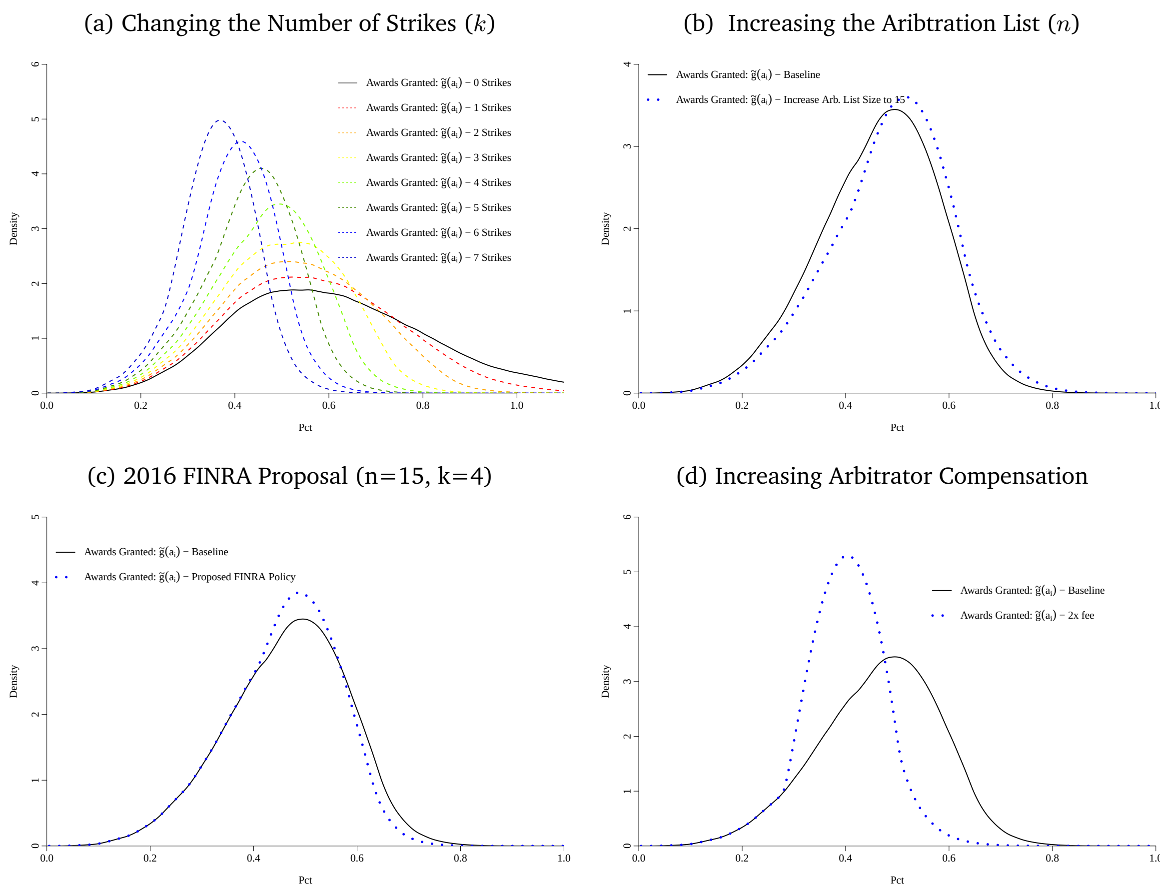
Note: the figure displays the estimated density of awards among the conditional distribution of selected arbitrators $\tilde{g}(a)$, the density of slant among the unconditional (entire) population of arbitrators $g(a)$, and the distribution of true beliefs among the unconditional (entire) population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

Figure 5: Distribution of Arbitrator Beliefs, Slant, and Awards—Informed Consumers



Note: the figure 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.

Figure 6: 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.

Table 1: Arbitration Summary Statistics

(a) Consumer Dispute Case Characteristics

Variable	Obs	Mean	Std. Dev.	Median
Requested Awards	11,756	758,648	1,826,864	240,000
Percent of Requested Awards Granted	11,756	53%	60%	35%
Arbitrator Characteristics				
Total Number of Arbitration Cases	11,756	11.29	13.02	7.00
Total Number of Consumer Dispute Cases	11,756	3.64	3.40	3.00
Former/Current Financial Adviser	11,756	41%		

Note: Panel (a) corresponds to the consumer dispute arbitration case characteristics. Observations are at the consumer arbitration case-by-arbitrator.

Table 2: Arbitrator Selection From the List

	(1)	(2)	(3)	(4)
Consumer Friendliness	-0.17*	-0.32***	-0.30*	-0.42**
	(0.097)	(0.12)	(0.17)	(0.20)
Arbitrator Controls	X	X	X	X
Sample				
Chairperson Sample		X		X
Greater than 5 or more Obs.			X	X
Observations	7,059	4,281	2,534	1,920

Note: the table displays the results corresponding to a multinomial logit model (eq. 2). Observations are at the case by eligible arbitrator level. Consumer Friendliness is measured using the empirical Bayes estimated arbitrator fixed effects as described in Section IV.A, which are standardized to ease in interpretation and winsorized at the 1% level to account for outliers. More consumer friendly (a higher arbitrator fixed effect) indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. Arbitrator controls include whether the arbitrator is a public arbitrator and eligible to serve as a chairperson. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 3: Are Consumer Friendly Arbitrators Selected Less Often?

	(1)	(2)	(3)	(4)	(5)	(6)
Consumer Friendliness	-0.26***	-0.20***	-0.085***	-0.071***	-0.088***	-0.072***
	(0.077)	(0.073)	(0.026)	(0.025)	(0.027)	(0.025)
Arbitrator Controls		X		X		X
Geographic $\times$ Arb. Type F.E.		X		X		X
Model						
OLS	X	X				
Poisson			X	X		
Negative Binomial					X	X
Observations	2,714	2,714	2,714	2,714	2,714	2,714
R-squared	0.002	0.142				

Note: the table displays the results corresponding to a regression model (eq. 3). Observations are at the arbitrator level. The dependent variable is the total number of consumer arbitration cases an arbitrator has overseen over the period 1998-2019. Consumer Friendliness is measured using the empirical Bayes estimated arbitrator fixed effects as described in Section IV.A, which are standardized to ease in interpretation and winsorized at the 1% level to account for outliers. A higher Arbitrator Fixed Effect indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. Arbitrator controls include the number of years the arbitrator has been active as an arbitrator. We control for geographic region fixed effects and geographic region fixed effects interacted with the type of arbitrator (public, non-public, chairperson eligible). Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 4: Expected Award in Low Merit Cases

	Prob. Award>0	E[Award]
Mechanism		
Baseline Mechanism(k=4,n=10)	17%	0.89%
Increase the Number of Strikes (k=6)	2%	0.07%
Increase the Size of the List(n=15)	22%	1.31%
2016 FINRA Proposal (k=6, n=15)	14%	0.56%
Increasing arbitrator compensation (2x)	1%	0.05%

Note: the table displays the probability a consumer wins a positive award and the expected award of bringing a low merit case when consumers are uninformed. We define a low merit case as one where the average arbitrator believes the fair award is zero.

Table 5: Expected Utility Under Different Selection Mechanisms

Counterfactual	E[a]	σ	Δ Expected Utility ($\rho = 3$)	($\rho = 10$)
Baseline	52.6%	12.6%	0.0%	0.0%
Change the Number of Strikes				
2 Strikes	58.6%	16.2%	4.4%	0.8%
6 Strikes	46.6%	10.0%	-5.1%	-3.1%
Increase the List Size to 15	54.7%	12.9%	2.0%	1.7%
FINRA Proposal	52.0%	11.9%	-0.3%	0.3%
Increase Arbitrator Compensation (2x)	46.9%	8.8%	-4.5%	-1.6%

Note: the table displays the mean and standard deviation of the awards distribution under different policy counterfactuals.The third column displays the change in expected consumer utility in terms of compensating variation relative to the baseline scenario. Compensating variation is in terms of awards. We calculate compensating variation under the assumption that consumers have constant absolute risk-aversion with: (i) risk aversion parameter 3, which corresponds to the average estimated risk aversion parameter in Egan, MacKay, and Yang (2020); and risk aversion parameter 10.

Online Appendix for Arbitration with Uninformed Consumers

Mark Egan, Gregor Matvos, and Amit Seru

Appendix A: Accounting for Selection

A.1 Arbitration Awards and Selection

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:

$$Awarded_{ijlt} = \beta X_i + \mu_j + \mu_l + \mu_t + \epsilon_{ijlt}. \tag{A-1}$$

Observations are at the arbitrator-by-case level, where i indexes the arbitration case, j indexes the location, l indexes the arbitrator, and t indexes time. The dependent variable $Awarded_{ijlt}$ reflects the award granted divided by the award requested. The key independent variables of interests are the arbitrator fixed effects μ_l, which measure persistent differences in arbitrator consumer friendliness in awards. All else equal, an arbitrator with a higher fixed effect grants higher awards to specific case. We also control for case characteristics in X_i and time and location fixed effects. The term ϵ_{ijlt} reflects case-specific unobservable characteristics.

One important consideration in estimating eq. (A-1) is whether the unobservable error term ϵ_{ijlt} is observable to the arbitration participants at the time when arbitrators are selected, or in other words, does the model suffer from selection. We account for selection following Heckman (1976; 1979). We model the arbitrator selection process using a Roy Model. Let q_{ijlt} denote the quality or fit of arbitrator l for case i which can be written as

$$q_{ijlt} = \Psi X_l + \gamma Z_{il} + \iota_j + \iota_{j'(l)} + \iota_t + \eta_{ijlt} \tag{A-2}$$

where X_l is a vector of arbitrator characteristics, ι_j are case hearing location fixed effects, $\iota_{j'(l)}$ are arbitrator home-location fixed effects, and ι_t are time fixed effects. The term η_{ijlt} reflects case and arbitrator unobservable (to the econometrician) characteristics. The term Z_{il} is an arbitrator-by-case exclusion restriction. As discussed below, the variables in Z_{ij} impact the probability an arbitrator is selected to a case but are otherwise uncorrelated with the outcome of the case. Without any loss in generality an arbitrator is selected to the case if $q_{ijlt} > 0$.

We account for selection using a control function approach. Following Heckman (1976; 1979) we control for the conditional expectation of the error term $E[\epsilon_{ijlt}|\Psi X_l + \gamma Z_{il} + \iota_j + \iota_{j'(l)} + \iota_t > -\eta_{ijlt}]$ when estimating eq. (A-1).

A.2 Estimation and Results

We account for selection by explicitly modeling the first stage selection decision. We first estimate the probability an arbitrator is selected to a case following eq. (A-2). We then use the first stage estimates to form the control function and estimate the second stage equation (eq. A-1). We estimate eq. (A-2) using a probit model. Observations are at the arbitrator opportunity by potential arbitrator level. Specifically for each arbitration opportunity, we examine the probability that each arbitrator in the entire sample is selected to the case. Our arbitrator characteristics include whether the arbitrator is public, a chairperson, and/or has work experience in securities industry. We include fixed effects for both the home location of the arbitrator $\iota_{j'(l)}$ and the case hearing location ι_j. We also include time fixed effects.

In order to account for selection semi-parametrically, we need an exclusion restriction/instrument Z_{ij} that is correlated with the probability an arbitrator is selected to a case but is otherwise uncorrelated with arbitration outcomes. We construct two instruments. We construct our first instrument $Z^{(1)}$ as the logged distance from the arbitrator's home to the case as our exclusion restriction.[1] The instrument is likely relevant because the closer an arbitrator is to a case, the more likely she will be part of the pool, and consequently, the more likely she will be selected to a case. For our exclusion restriction to be exogenous, we need an arbitrator's distance to the case to be uncorrelated with case awards. We control for both the location of the arbitrator and the location of the case, which captures geographic differences in awards. For the exogeneity condition to be violated it would have to be that cases where the arbitrator lives further away either systematically have either more/less merit conditional on the location of the arbitrator and case.

We construct our second instrument $Z^{(2)}$ based on the average distance of all other arbitrators appearing on the randomly generated list.[2]The rationale behind the our second instrument is that the further the away the case is from all other arbitrators appearing on the list, the less likely those other arbitrators are to oversee the case. Furthermore, because the list of other arbitrators is randomly generated, their distance from the case depends on pure chance. Therefore our instrument is likely exogenous.

Table A3 displays the results corresponding to our first-stage regression. We include control for adviser's distance $(Z^{(1)})$ in column (1), competitors' distance $(Z^{(2)})$ in column (2), and we include both instruments in column (3). Importantly, we find a strong negative and significant relationship between the probability an arbitrator is selected to a case and distance and a significant positive relationship between the probability an arbitrator is selected to a case and competitors' distance.

We use our estimates from the first-stage to form the control function to estimate the second stage equation. The predicted values from the first-stage, $\widehat{q}$, are used to calculate the conditional

[1] We determine the home location of the arbitrator based on the location of the first case he/she oversaw.

[2] As discussed in Section III.A of the paper, we observe the arbitration list for a subset of the cases in the data. For those cases where the list is available, we calculate the instrument as the average distance of all other arbitrators appearing on the list. For cases where the list is not observed, we set the value of our instrument to zero. In our first-stage specification we include the instrument $Z^{(2)}$ as well as a dummy variable indicating whether $Z^{(2)} > 0$, such that our instrument only exploits variation in the data where we observe the arbitration list. The fact that we do not observe the list for all cases does not invalidate or instrument, but it does reduce the statistical power of the instrument.

expectation $E[\epsilon_{ijlt}|\Psi X_l + \gamma Z_{il} + \iota_j + \iota_{j'(l)} + \iota_t > -\eta_{ijt}]$. Under the assumption that η and ϵ are distributed jointly normal, the conditional expectation can be written as $E[\epsilon_{ijlt}|\Psi X_l + \gamma Z_{il} + \iota_j + \iota_{j'(l)} + \iota_t > -\eta_{ijt}] = \rho\lambda(\widehat{q}_{ijlt})$ where $\lambda(\cdot)$ is the inverse mills ratio and ρ is the correlation between η and ε. We then estimate the second-stage equation as

$$Awarded_{ijlt} = \beta X_i + \mu_j + \mu_k + \mu_t + \lambda(\widehat{q}_{ijkt}) + \nu_{ijkt}. \tag{A-3}$$

Eq. (A-3) mirrors that of our baseline analysis, except that we now include the control function $\lambda(\cdot)$. Observations are at the arbitrator-by-case level and we use the same set of controls as in our baseline analysis.

We report the corresponding estimates in Table A4. The coefficient estimate corresponding to $\lambda(\cdot)$ is statistically insignificant and close to zero, suggesting that there is little selection on unobservables. The object of interest from our analysis is the set of arbitrator fixed effects. Figure A1a illustrates that there is a strong positive relationship between our baseline estimated arbitrator fixed effects and our selection-corrected estimated arbitrator fixed effects. Furthermore, the inference corresponding to our previous analysis in Section 4 remains unchanged if we use our selection-corrected estimated arbitrator fixed effects (Table A6).

Appendix B: Additional Robustness and Counterfactuals

B.1 Firm and Consumer Sophistication

In Section 4 we provide evidence 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 examine the fixed effect of the arbitrator l selected to case i as a function of firm and consumer sophistication:

$$\widehat{\mu_{il}} = \phi_0 + \phi_1 Attorney_i + \phi_2 PIABA_i + \phi_4 Trust_i + \phi_5 Firm\_Experience_i + \varepsilon_{il}. \tag{A-4}$$

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 arbitrator fixed effects correspond to eq. (A-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 (a class of attorneys who specialize in arbitration), 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 A5 displays the corresponding estimates.

We find that consumers who use attorneys who specialize in arbitration, PIABA attorney, have their cases overseen by arbitrators whose awards are 8.14pp higher than in cases where consumers are self represented (column 1).[3] 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 5.83pp higher awards on average (column 1). In other words, parties' expertise in arbitration allows them to select more favorable arbitrators. We also find some evidence that firms with more experience in arbitration select more industry friendly 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.

B.2 Alt. Measures of Industry Friendliness (Altonji et al., 2005; Oster, 2019)

We construct several alternative measures of arbitrator industry friendliness–fixed effects and report the distribution of fixed effects in Figure A1. 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 25% (0.44 to 0.5). 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 Figure A1. 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 85% of awards granted. 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.[4] 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. Figure A1 illustrates 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

[3] 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.

[4] In particular, these cases (about one-third of total cases) involve unauthorized trading, churning, or selling unregistered securities.

across each measure.

We replicate our baseline results, examining the relationship between arbitrator consumer friendliness and the probability she is selected to a case (eq. 3), and report the corresponding results in Table A6. The results indicate that, regardless of the specific measure of arbitrator consumer friendliness, we find that those arbitrators that grant higher awards are selected to fewer cases.

B.3 Backward Looking Measure of Arbitrator Consumer Friendliness

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 A2, we construct a measure of how friendly arbitrator l's decision regarding case i as:

$$\delta_{ijlt} = Pct\_Awarded_{ijlt} - \hat{\beta} X_i - \hat{\mu}_j - \hat{\mu}_t. \tag{A-5}$$

where i indexes the arbitration case, j 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_{ijlt}) 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.

$$Selected_{lt} = \beta X_{lt} + \gamma \bar{\delta}_{lt} + \eta_{lt} \tag{A-6}$$

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 1998-2019. An arbitrator enters the data set as soon as she oversees her first case and remains in the data set until 2019.

We report the corresponding estimates in Table A7. 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 A7 indicate that a one standard deviation decrease in past slant (i.e. more industry friendly) is correlated with a 0.17pp increase in the probability of being selected in a given year. To the extent that our measure of past slant suffers from classical measurement error, our estimates understate the true effect.

B.4 2008 Rule Change

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 2, 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 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 examining the probability an arbitrator is selected in a given year as a function of her past bias and her past bias interacted with the rule change. Specifically, we estimate the following linear probability model:

$$Selected_{lkt} = \gamma_{1999\leq t<2008}\widehat{\mu_l^{EB}} \times \mathcal{I}_{1999\leq t<2008} + \gamma_{t\geq 2008}\widehat{\mu_l^{EB}} \times \mathcal{I}_{t\geq 2008} + \beta X_{lt} + \delta_t + \delta_j + \eta_{ljt}. \tag{A-7}$$

Our observations are at the arbitrator-by-year level, where l indexes the arbitrator, j indexes the location, t indexes time, and $\mathcal{I}$ is an indicator variable designating a time 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 1998 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. The term 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_j) corresponding to the location of the past case that the arbitrator worked on.

The estimates in Table A8 show that the rule change significantly decreased the probability that industry friendly arbitrators are selected. During the period 1998-2007, a one standard deviation increase in an arbitrator's consumer friendliness represented a 1.24pp decrease in the probability of being selected (column 1). During the post-reform period, the same increase in arbitrator's consumer friendliness represented a 0.64 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 5; our quantitative model indicates that changing the number of strikes will have a dramatic impact on which arbitrators are selected to a case (Section 6).

B.5 Do Consumers Account for Arbitrator Slant when Requesting Awards?

The results from Section 4 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.

$$\ln(Awards\_Requested)_{ijlt} = \alpha\bar{\delta}_{lt} + \beta X_i + \mu_j + \mu_t + \varepsilon_{ijlt} \tag{A-8}$$

Observations are at the arbitrator-by-case level; i indexes the arbitration case, j indexes location, l indexes the arbitrator, and t indexes time. The regression specification mirrors that of eq. (A-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. A-5). The key independent variable is the arbitrator's past slant. We again control for case level characteristics and include time and county fixed effects (μ_t, μ_j).

Table A9a 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.

$$\ln(Award\_Granted)_{ijlt} = \alpha\bar{\delta}_{lt} + \beta X_i + \mu_j + \mu_t + \varepsilon_{ijlt} \tag{A-9}$$

The regression specification corresponds to that of eq. (A-8) other than the dependent variable. We use the same set of controls as in eq. (A-8) and observations are at the arbitrator-by-case level.

Table A9b 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 11% increase in the award amount.

B6 Purchasing Expertise: Spillovers from Uninformed Consumers

As we show in Appendix B.1, 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 8pp higher than that of an uninformed consumer (Figure A3.).

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 8pp, while the average gain, if all consumers are informed is 13pp.[5] 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).

Appendix C: 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 ($\mu_P = 0$),

C.1 Informed consumers

We first present the benchmark model in which firms and consumers are fully informed. 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 $F(\cdot)$.

[5] In the parameterized version of the model, the mean of the distribution of arbitrator beliefs $F(\cdot)$ is 13pp higher than the distribution of awards granted $\tilde{G}(\cdot)$

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. 7) 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

$$U(b_i) = max_{a_i}\Gamma(a^{-1}(a_i), F(\cdot))\,(fee - \theta(b_i - a_i)) \qquad \text{(A-10)}$$

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

$$\frac{\partial}{\partial b}U(b_i) = -\Gamma(b_i, F(\cdot))\theta\,\forall b_i > F^{-1}(0.5)\,and\,b_i \neq a_i \qquad \text{(A-11)}$$

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. A-11), we can write the utility of arbitrator with belief b_i as

$$U(b_i) = \bar{U} - \int_{F^{-1}(0.5)}^{b_i}\Gamma(\tilde{b}, F(\cdot))\theta d\tilde{b},\,\forall b_i > F^{-1}(0.5)\,and\,b_i \neq a_i \qquad \text{(A-12)}$$

Last, we can use equations (A-10) and (A-12) 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

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

C.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 (eq. 7), 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

$$U(b_i) = max_{a_i} \Gamma(a^{-1}(a_i), F(\cdot)) \left(fee - \theta(b_i - a_i) \right) \tag{A-13}$$

From the envelope condition, we have

$$\frac{\partial}{\partial b} U(b_i) = -\Gamma(b_i, F(\cdot))\theta \, \forall b_i \neq a_i \tag{A-14}$$

Note that an arbitrator with slant $\bar{b}$ will never be selected for arbitration; thus, $U(\bar{b}) = 0$. Combining (A-13) and (A-14) 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 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 of the paper 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 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 5, 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.[6] AAA's Arbitrator Select List and Appointment system uses a ranking system of 5-15 arbitrators.[7] 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.[8] 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 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.[9] The JAMS data set consists of 391 arbitration cases overseen by 104 different arbitrators over the period 2002-2018. The AAA data set

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

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

[8] 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.

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

consists of 965 arbitration cases overseen by 265 different arbitrators over the period 2013-2018. We report the summary statistics in Table A10a. Figure A4 panels (a) and (b) display the types of 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.

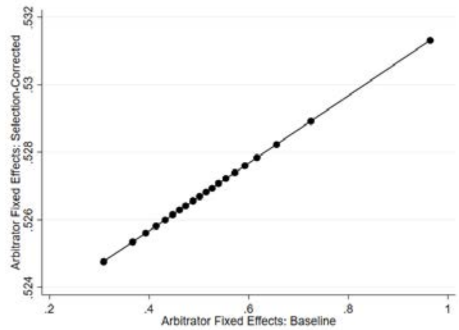
First, we show that arbitrators display a systematic industry/consumer friendliness in awarding claims. Some arbitrator slant more "industry friendly" than others. We employ eq. (A-1) and estimate differences in awards (either in dollars or percent awarded, depending on the data set) as a function of industry and arbitrator fixed effects (Table A10b). 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 A5 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.

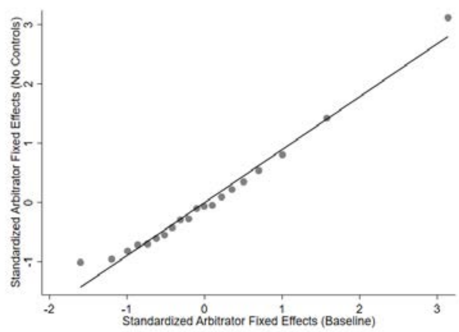
Appendix E: Additional Tables and Figures

Figure A1: Alternative Arbitrator Fixed Effects Estimates

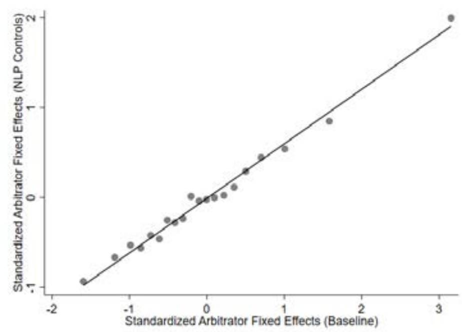
(a) Arb. F.E.: Selection Corrected



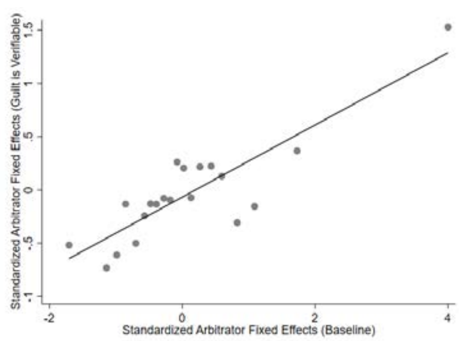
(b) Arb. F.E.: No Controls



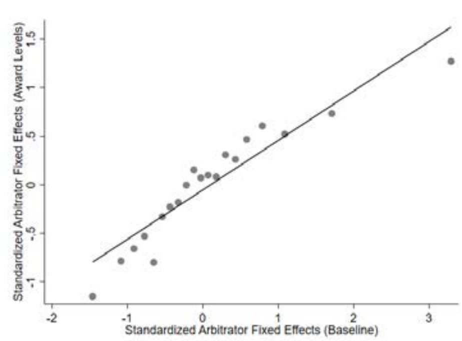
(c) Arb. F.E.: NLP Controls



(d) Arb. F.E.: Cases with Verifiable Guilt



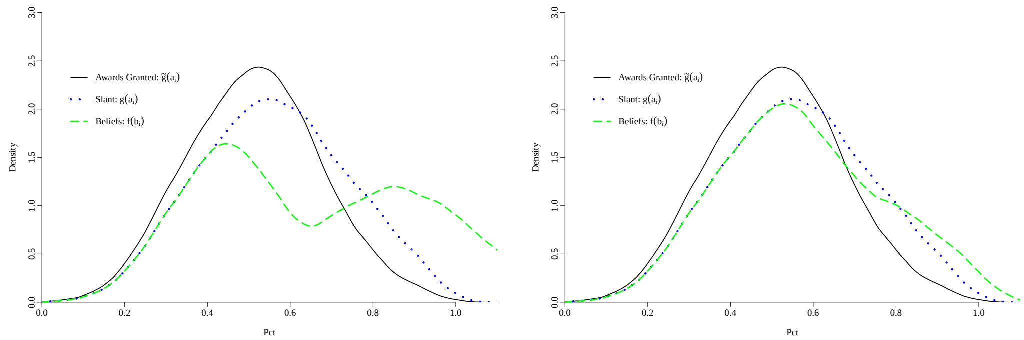
(e) Arb. F.E.: ln(Award Granted)



Note: Panels (a)-(e) 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 A2. Panel (a) displays our selection-correction estimated arbitrator fixed effects as discussed in Appendix A and reported in Table A4. In panel (b) 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 (c) 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 (d) 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 $\frac{AwardGranted}{AwardRequested}$.

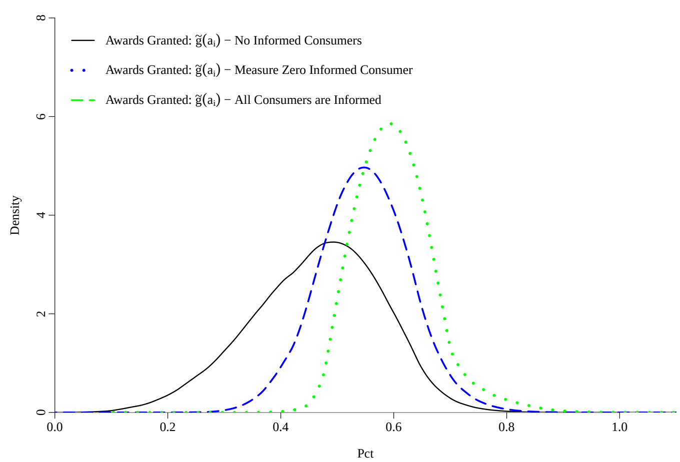
Figure A2: Distribution of Arbitrator Beliefs, Slant, and Awards Under Alt. Parameterizations

(a) $\frac{f}{\theta}$ scaled by 50% (b) $\frac{f}{\theta}$ scaled by 150%



Note: Figures A2a and A2b 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.

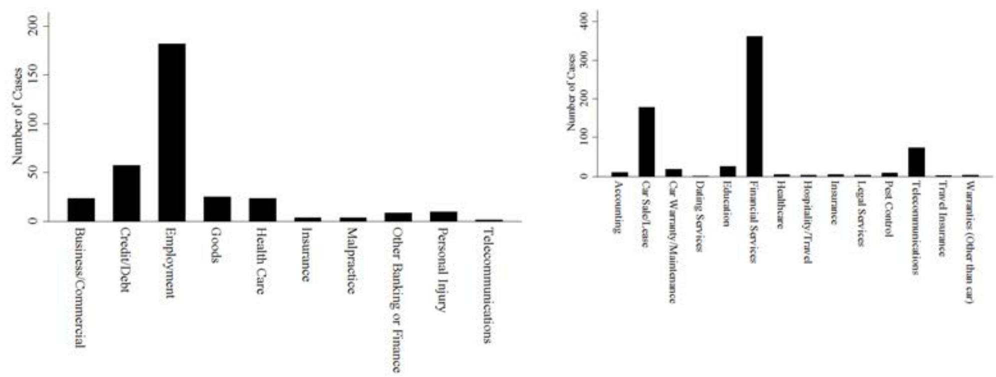
Figure A3: Distribution of Arbitrator Beliefs, Slant, and Awards—Measure Zero Informed Consumer



Note: Figure A3 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 A4: Types of Disputes at the American Arbitration Association (AAA) and JAMS

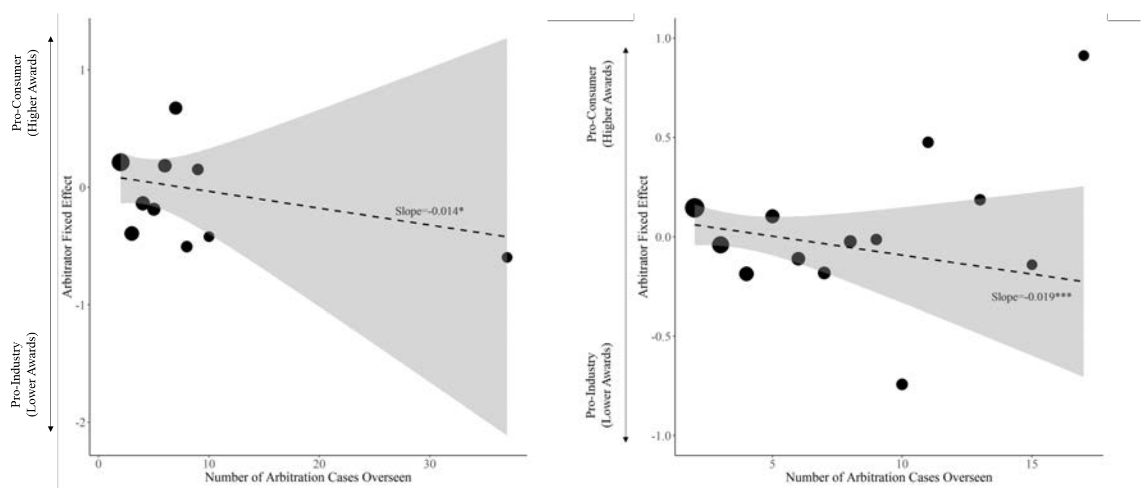
(a) JAMS (b) AAA



Note: Figure A4 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 A5: External Validity: Arbitrator Selection in AAA and JAMS

(a) Arbitrator Fixed Effects vs. Selection - JAMS (b) Arbitrator Fixed Effects vs. Selection - AAA



Note: Figure A5 panels (a) and (b) display the distribution between arbitrator case outcomes and the total number of times an arbitrator is selected. Figure A5a 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 A5b 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 A5 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 A10b. 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 A10b. 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: Additional Summary Statistics

Variable	Obs	Mean	Std. Dev.	Median
Allegations:				
Unsuitible	11,756	53%		
Fiduciairy	11,756	42%		
Misrepresentation	11,756	34%		
Negligence	11,756	34%		
Fraud	11,756	30%		
Unauthorized Activity	11,756	20%		
Products:				
Stocks	11,756	7%		
Insurance	11,756	4%		
Mutual Fund	11,756	3%		
Annuity	11,756	3%		
Bonds	11,756	2%		
Options	11,756	2%		
Complexity:				
Number of Allegations	11,756	2.6	1.8	2
Length of the Case Document: Words	11,756	884	402	823
Length of the Case Document: Sentences	11,756	222	90	212
Offending Adviser Characteristics:				
Experience	11,756	15.34	9.71	14.00
No. Qualifications	11,756	15.34	9.71	14.00
Prior Record of Misconduct	11,756	51%		
Series 6	11,756	13%		
Series 7	11,756	87%		
Series 24	11,7561	36%		
Series 63	11,7561	95%		
Series 65 or 66	11,756	49%		
Consumer Claimant Representation:				
Self-represented/No Attorney	11,756	5.6%		
Represented by an Attorney	11,756	94.2%		
Represented by a PIABA Attorney	11,756	20.4%		
Consumer is a Trust	11,756	9.5%		

Note: Table A1 displays the summary statistics corresponding to our arbitration data set. Observations are at the arbitrator-by-case level, and correspond to 4,699 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 1998-2019. 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. *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.

Table A2: Percent of Requested Awards Granted

	(1)	(2)	(3)	(4)
Allegations:				
Unsuitable	-2.22	-1.62	-2.33	-1.62
	(1.81)	(1.82)	(1.81)	(1.82)
Misrepresentation	1.16	0.30	-1.38	0.32
	(2.05)	(2.05)	(2.08)	(2.05)
Unauthorized Activity	-0.91	-0.38	-1.21	-0.42
	(2.41)	(2.45)	(2.43)	(2.45)
Fiduciary Duty	-1.12	-0.97	-1.16	-0.96
	(2.31)	(2.32)	(2.39)	(2.32)
Fraud	4.34*	4.10	0.30	4.10
	(2.48)	(2.50)	(2.60)	(2.51)
Fee/Commission Related	13.1***	13.4***	15.8***	13.3***
	(4.76)	(4.75)	(5.18)	(4.74)
Negligence	-7.98***	-7.50***	-6.73**	-7.53***
	(2.47)	(2.49)	(2.63)	(2.49)
Churning/ Excessive Trading	8.95***	8.16***	2.47	8.17***
	(2.83)	(2.87)	(2.83)	(2.87)
Risky Investments	3.84	4.54	4.20	4.57
	(4.03)	(4.00)	(4.12)	(4.00)
Unregistered Securities	25.0**	25.3**	28.3***	25.4**
	(12.5)	(11.9)	(10.8)	(11.9)
Omission of Key Facts	1.97	2.29	0.77	2.27
	(2.86)	(2.87)	(2.96)	(2.87)
Arbitrator Characteristics:				
Former/Current Financial Adviser				0.20
				(1.05)
Prev. Terminated for Cause				5.15*
				(2.74)
Involved in Customer Dispute				-3.69
				(2.95)
Year F.E.	X	X	X	X
Arbitration Location F.E.		X	X	X
Arbitrator F.E.			X	
Observations	11,756	11,698	9,427	11,698
R-squared	0.163	0.187	0.549	0.187

Note: Table A2 displays the regression results for a linear regression model (eq. A-1). Observations are at the arbitrator-by-case level over the period 1998-2019. 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, the financial product involved (i.e. dummy variable stocks, bonds, etc.), 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 3) 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 A3: Selection Correction - First Stage

	(1)	(2)	(3)
$\ln(Distance)$	-0.18***		-0.18***
	(0.0012)		(0.0012)
$\ln(\overline{Distance})$		0.049***	0.0088*
		(0.0060)	(0.0048)
List Available		0.27***	0.052*
		(0.036)	(0.030)
Other Controls	X	X	X
Arbitration Case Location F.E.	X	X	X
Arbitrator Location F.E.	X	X	X
Time Fixed Effects	X	X	X
Observations	65,597,466	65,613,864	65,597,466

Note: the table displays the results corresponding to a probit model (eq. A-2). Observations are at the arbitrator-by-case-opportunity level over the period 1998-2019. The dependent variable is dummy variable indicating that the arbitrator was selected to the case. The independent variable $\ln(Distance)$ measures how far away the the arbitrator is located from the case in logs. The independent variable $\ln(\overline{Distance})$ is the log of the average distance of the other arbitrators appearing on the arbitration list for the case. We only observe the arbitration list for a subset of the cases in the data. For those cases where the list is available, we calculate $\ln(\overline{Distance})$ as the log of the average distance of all other arbitrators appearing on the list. For cases where the list is not observed, we set the value of $\ln(\overline{Distance})$ to zero and include a dummy variable (List Available) indicating whether the list is available such that $\ln(\overline{Distance})$ only exploits variation in the data where we observe the arbitration list. Other controls include whether the arbitrator is a public arbitrator, eligible chairperson, or has worked(s) as a financial adviser. We also control for the past case experience of the arbitrator. Robust standard errors are in parenthesis*** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A4: Selection Corrected - Second Stage - Percent of Requested Awards Granted

	(1)
Inverse Mills Ratio	-0.017
	(0.016)
Allegations:	
Unsuitable	-0.024
	(0.018)
Misrepresentation	-0.013
	(0.021)
Unauthorized Activity	-0.011
	(0.024)
Fiduciary Duty	-0.012
	(0.024)
Fraud	0.0036
	(0.026)
Fee/Commission Related	0.16***
	(0.052)
Negligence	-0.069***
	(0.026)
Churning/ Excessive Trading	0.025
	(0.028)
Risky Investments	0.041
	(0.041)
Unregistered Securities	0.28***
	(0.11)
Omission of Key Facts	0.0078
	(0.030)
Adviser Characteristics:	
Prior Misconduct	0.070***
	(0.018)
Year F.E.	X
Arbitration Location F.E.	X
Arbitrator F.E.	X
Observations	9,380
R-squared	0.549

Note: Table A4 displays the regression results for a linear regression model (eq. A-1). Observations are at the arbitrator-by-case level over the period 1998-2019. The dependent variable is Awards and is measured as awards granted through arbitration divided by awards requested. The Inverse Mills Ratio Term accounts selection and corresponds to the estimates displayed in column (3) of Table A3. 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 financial product involved (i.e. dummy variable stocks, bonds, etc.), 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 3) 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 A5: Industry Friendly Arbitrator Selection and Consumer Sophisitication

	(1)	(2)	(3)
Attorney	-1.51	0.23	-0.12
	(1.77)	(1.87)	(2.01)
PIABA Attorney	8.05***	8.14***	7.94***
	(1.09)	(1.09)	(1.21)
Trust	4.60***	5.83***	6.21***
	(1.36)	(1.39)	(1.58)
Firm Experience	-1.77**	-0.28	-0.43
	(0.77)	(0.85)	(0.96)
Other Controls		X	X
Year F.E.			X
County F.E.			X
Observations	9,427	9,427	9,294
R-squared	0.010	0.030	0.112

Note: Table A5 displays the regression results for a linear regression model (eq. A-4). Observations are at the arbitrator-by-case level over the period 1998-2019 and come from our consumer dispute arbitration data set. The dependent variable is the selected Arbitrator's Fixed Effect as calculated in column (3) of Table A2. 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. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A6: Are Consumer Friendly Arbitrators Selected Less Often? - Alt. Measures of Consumer Friendliness

	(1)	(2)	(3)	(4)	(5)	(6)
Consumer Friendliness	-0.071***	-0.064***	-0.051**	-0.14**	-0.021	-0.065***
	(0.025)	(0.024)	(0.025)	(0.060)	(0.025)	(0.024)
Consumer Friendlieness Measure/Arbitrator Fixed Effects:						
Baseline	X					
No Controls		X				
NLP Controls			X			
Verifiable Guilt				X		
Award Levels					X	
Selection Corrected						X
Arbitrator Controls	X	X	X	X	X	X
Geographic $\times$ Arbitrator Type F.E.	X	X	X	X	X	X
Observations	2,712	2,712	2,644	625	2,392	2,699

Note: The table displays the results corresponding to a Poisson model. Observations are at the arbitrator level. The dependent variable is the total number of consumer arbitration cases an arbitrator has overseen over the period 1998-2019. We use five different estimates of arbitrator consumer friendliness/arbitrator fixed effects, each of which is standardized to ease in interpretation and winsorized at the 1% level to account for outliers. The arbitrator fixed effects in column (1) ("Baseline") correspond to the empirical Bayes estimated arbitrator fixed effects as described in Section 4. In column (2) we construct our arbitrator fixed effects ("No Controls") by re-estimating eq. (1) without any control variables other than our set of arbitrator fixed effects. In column (3) we construct our arbitrator fixed effects ("NLP Controls") by re-estimating eq. (1) where augment our baseline specification by including dummy variables for the 500 words appearing in the case documents. In column (4) we construct our arbitrator fixed effects ("Verifiable Guilt") by re-estimating eq. (A-1) where we restrict the data set to only those cases involving unauthorized trading, churning, or selling unregistered securities. In column (5) we construct construct our arbitrator fixed effects ("Award Levels") by re-estimating eq. (1) where the dependent variable is in terms of the $\ln(AwardGranted)$ rather than $\frac{AwardGranted}{AwardRequested}$. In column (6) we construct construct our arbitrator fixed effects ("Selection Corrected") by accounting for selection as per Heckman (1976) as discussed in Appendix A. A higher Arbitrator Fixed Effect indicates that, all else equal, the arbitrator gives out higher awards and is therefore more consumer friendly. Arbitrator controls include the number of years the arbitrator has been active as an arbitrator. We control for geographic region fixed effects and geographic region fixed effects interacted with the type of arbitrator (public, non-public, chairperson eligible). Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A7: Probability an Arbitrator is Selected - Past Consumer Friendliness

	(1)	(2)	(3)
Past Arbitrator Consumer Friendliness	-0.17***	-0.18***	-0.19***
	(0.065)	(0.065)	(0.066)
Arbitrator Controls		X	X
Year F.E.		X	X
Location F.E.			X
Observations	90,038	90,038	90,038
R-squared	0.045	0.045	0.049

Note: Table display the regression results corresponding to a linear probability model (eq. A-6). Observations are at the arbitrator-by-year level over the period 1998-2019. 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. A-5). A higher Past Arbitrator Consumer Friendliness indicates that, all else equal, the arbitrator gave out higher awards in the past. Arbitrator controls include the number of years the arbitrator has been active in the industry. We include year fixed effects and fixed effects for the location of the arbitration proceedings. The hearing location 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 A8: Industry Friendly Arbitrator Selection and the 2008 Rule Change

	(1)	(2)	(3)
Arbitrator F.E.$\times$(Year$<$2008)	-1.24**	-1.17*	-1.13*
	(0.60)	(0.60)	(0.61)
Arbitrator F.E. $\times$(Year$\geq$2008)	-0.64**	-0.59**	-0.64**
	(0.26)	(0.26)	(0.29)
Arbitrator Controls		X	X
Year F.E.		X	X
Location F.E.			X
Observations	53,901	53,901	53,837
R-squared	0.070	0.074	0.090

Note: Table A8 displays the regression results corresponding to a linear probability model (A-7). Observations are at the arbitrator-by-case level over the period 1998-2019. 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 4. 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 and fixed effects for the location of the arbitration proceedings. The hearing location 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 A9: Arbitrator Bias and Awards Requested

(a) Award Requested

	(1)	(2)
Past Arbitrator Consumer Friendliness	1.81	1.30
	(1.56)	(1.58)
Arbitration Case Controls	X	X
Year F.E.		X
Location F.E.		X
Observations	6,681	6,664
R-squared	0.393	0.425

(b) Award Granted

	(1)	(2)
Past Arbitrator Consumer Friendliness	0.11***	0.096***
	(0.026)	(0.025)
Arbitration Case Controls	X	X
Year F.E.		X
Location F.E.		X
Observations	5,613	5,598
R-squared	0.268	0.302

Note: Table A9a and A9b displays the regression results for linear regression models. Observations are at the arbitrator-by-case level over the period 1998-2019 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 B (eq. A-5). 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 A10: 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 A10a 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 A10b corresponds to a linear regression model (eq. A-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).