Privatized Justice: An Empirical Analysis of Mandatory Employment Arbitration in the Post-*Concepcion* Period

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Abstract

Commentators and scholars alike have long worried that mandatory arbitration's rise as a common method of employment dispute resolution has significantly hampered employees' ability to vindicate their statutory rights and seek justice. These concerns have only grown in the wake of the Supreme Court's *Concepcion* ruling (2011),[1] which paved the way for companies to ban the use of class-action in their contracts of employment, in addition to requiring that all employee claims be resolved through arbitration. This thesis examines the results employees have achieved in arbitration in the wake of the ruling. Specifically, it analyzes the outcomes of all mandatory employment cases filed with the nation's largest arbitration provider—The American Arbitration Association (AAA)—between 2012 and 2018. The goal of the study is three-fold: To determine how employee outcomes in arbitration compare with those achieved by employees in litigation post-*Concepcion*; To determine whether these outcomes have changed in the wake of the ruling; And finally, to assess whether arbitration is uniquely vulnerable as an institution to anti-employee bias. Leveraging the statutorily-mandated reportings of the AAA, I conduct both bivariate and multivariate regression analysis on case outcomes, and calculate summary statistics for the time-period in question. I find that, in the wake of the *Concepcion* ruling, the rate at which employees prevail has not changed significantly; it is still lower than estimates of comparable litigation. But arbitrators are increasingly using summary dismissals to dispose of employee cases prior to adjudication, raising questions of whether estimates of employee performance must be revised downwards. In addition, arbitration in the post-*Concepcion* period appears to significantly advantage the largest, most-experienced corporations in their disputes with employees. Unlike prior research, however, I find no evidence to suggest that this advantage is unique to the arbitral forum.

[1] *AT&T Mobility v. Concepcion, 563 U.S. 333*

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Introduction

Over the past 35 years, the United States has experienced a profound transformation in its system of labor relations. Whereas, before, employee rights were vindicated through a mix of collective bargaining and litigation, this task now largely falls on the para-judicial system of privatized arbitration. Non-union private-sector employers have increasingly included arbitration agreements as mandatory terms in their contracts of employment as the Supreme Court, through a series of rulings, has steadily expanded their enforceability. These clauses preempt employees from bringing civil suit related to contractual and statutory rights violations, and force them instead to adjudicate all claims with a private third-party neutral known as an arbitrator (Stone and Colvin 2015). While virtually unheard of before the 1980s, recent studies have estimated that over half of all non-union private sector employees are now covered by one in the terms of their contract of employment (Colvin 2017).

Given the scope of this shift, it is unsurprising that commentators and scholars alike have vociferously debated whether mandatory arbitration is a just, ethical, and socially-beneficial substitute for the method of employment dispute resolution it has largely superseded: litigation. In the past, proponents have argued that the new venue offers a more efficient process, and lower legal costs—changes over litigation that benefit all parties—while granting employees no less ability to vindicate their rights or to seek redress (*See e.g.* Hill 2003; Sherwyn Estreicher, and Heise 2005). Opponents, alternatively—while ceding that arbitration may be less costly in terms of both time and money—have, in most cases, argued that the arbitral system significantly advantages corporations, leading to lower win rates and smaller awards for employees (*See e.g.* Stone and Colvin 2015; Estlund 2018).

The issues surrounding mandatory employment arbitration were further complicated in 2011 by a Supreme Court decision that asserted the enforceability, not just of mandatory arbitration clauses, but of class-action bans as well. Class-action lawsuits have traditionally been what allow plaintiffs to seek redress when the monetary amount they are seeking is less than the legal fees or opportunity cost they would face bringing the case to trial or to arbitration. The legal mechanism allows a group of plaintiffs all seeking similar redress from the same defendant to file their cases together, splitting the cost of adjudication and whatever award is received. Many commentators argue that class-action suits are an essential part of ensuring that employees have the ability to hold businesses accountable regardless of their resources or the amount they claim (Stone and Colvin 2015). Nonetheless, in 2011, the Supreme Court held, in *AT&T Mobility v. Concepcion, 563 U.S. 333*, that a company could ban the use of class-action in the terms of their employment contract, and that the ban would be legally enforceable in the event that a dispute arose. Since 2011, many corporations have responded to the ruling by adopting a duel mandatory arbitration/class-action ban as a term in their contracts of employment, causing many who already worried as to the fairness of employment arbitration, to fear the ability of employees to protect their statutory and contractual rights is being further eroded (Horton and Chandrasekher 2016).

While a number of studies have assessed employee outcomes and experiences in mandatory arbitration, few have analyzed a set of cases far enough out from the Supreme Court's *Concepcion* ruling to assess how the landscape has changed. In this B.A. thesis, I go beyond prior research by analyzing the outcomes of mandatory employment arbitration cases conducted by the nation's largest arbitration provider, The American Arbitration Association, between 2012 and 2018. I determine key statistics on case outcomes, like employee win rate and average award

size, that can be compared to the results of studies on similar litigation. I also conduct both bivariate and multivariate regression analysis on case outcomes to understand how various factors—including the background of the arbitrator, and the frequency with which an employer has participated in arbitration—impact an employee's chance of success and whether arbitration is uniquely vulnerable to anti-employee bias. While other studies have attempted to do this by analyzing three measures of employee outcome—the employee win rate, expected award amount, and the rate at which cases settle pretrial—I go beyond prior studies by additionally analyzing what factors influence the rate at which employees' cases are summarily dismissed.

Background

What is Arbitration?

Arbitration is a dispute resolution alternative to traditional litigation used to resolve contractual or statutory disagreements. Whereas traditional litigation is decided in a court of law by a jury, or less frequently by a judge or panel of judges, arbitration cases are decided by a private extrajudicial third-party neutral often referred to as an arbitrator. Arbitrators do not need to possess a legal background to act in the capacity, though, in practice, they often do: Many are retired judges or practicing or retired lawyers (Stone and Colvin 2015). While the regulations governing arbitration differ from country to country, in the United States, the use of arbitration as a dispute resolution method is not limited to one particular area of statutory or contract law: employment, business-to-business, and consumer disputes are all regularly settled using the method, and arbitration can be used to settle disputes in both a union and non-union context (Colvin and Gough 2015). Additionally, the practice can either be initiated on a voluntary basis,

or as a mandatory term of a signed contract (Stone and Colvin 2015). This thesis deals with a specific subset of arbitration in the United States: mandatory non-union employment arbitration.

What is Mandatory Arbitration?

Mandatory arbitration is arbitration conducted under a binding contractual mechanism that forces those party to the contract to take their dispute to arbitration rather than to civil court. These contractual mechanisms are known as *mandatory arbitration clauses*, and while their use in business-to-business and business-to-union contracts has been commonplace since the early 20th century, their use in non-union employment and consumer terms-of-use contracts is a relatively new invention. Until the 1980s, the federal legislation authorising the use of these clauses—the 1925 *Federal Arbitration Act* (FAA)—was generally interpreted by the courts as applying only to commercial contractual disputes involving federal law. However, in a long series of decisions over the past four decades, the Supreme Court has continuously broadened the legislation's scope, allowing for the use and enforceability of mandatory arbitration clauses in almost all contractual contexts. In the case of employment disputes, mandatory arbitration is often referred to as *employer-promulgated* as it is the employer who creates the terms of the contract. Given the focus of my thesis, I use the terms interchangeably.

Legal Basis for Mandatory Arbitration in Non-Union Employment Contracts

While the FAA contains language that many legal scholars have argued removes non-union employment contracts from its purview (Ware 1996), in *Gilmer v. Interstate/Johnson Lane Corp.*, 500 U.S. 20 (1991), the Supreme Court held that an employee was required to bring his age-discrimination case to arbitration under the FAA because his employment contract included a mandatory arbitration clause. The case suggested that the high court favored a broad

reading of the FAA in such circumstances. However, because the dispute involved a trade agency the employee was required to join, rather than the employer itself, it did not establish a firm precedent (Stone and Colvin 2015). This interpretive ambiguity, however, did not last for long. In the 2001 case, *Circuit City Stores v. Adams*, 532 U.S. 105, the Supreme Court clarified its thinking on the matter, ruling that an employee, who was subject to a mandatory arbitration clause, was required to arbitrate rather than litigate his case. Since *Adams*, the courts have held that almost all employment cases involving a mandatory arbitration clause must be resolved through arbitration. Later rulings have, almost unwaveringly, made mandatory arbitration clauses more binding. For instance, in the 2006 case, *Buckeye Check Cashing, Inc. v. Cardegna, 546 U.S. 440*, the Supreme Court held that a mandatory arbitration clause requires parties to arbitrate, even when the underlying contract is determined to be illegal.

Proliferation of Mandatory Employment Arbitration

As the supreme court has expanded the enforceability of the clauses in employment contracts, evidence suggests that more businesses have made use of the tactic as a way of avoiding the relatively high cost of litigation and the possibility of large jury settlements (Colvin 2008). Yet knowing just how prolific mandatory arbitration clauses have become in employment contracts is difficult because businesses are not required to disclose their use of them. Despite this, many studies in the past couple decades have attempted to estimate their prevalency. The results seem to suggest that usage of mandatory arbitration clauses has risen exponentially since the Supreme Court began expanding the practice's enforceability. A 1992 study of conflict resolution procedures in non-union settings, conducted directly after the Supreme Court's first employment-arbitration ruling, found that only 2.5 percent of non-union employers included mandatory arbitration clauses in their contracts (Feuille and Chachere 1995). Three years later,

the US Government Accountability Office (GAO) found that the number had grown to 7.6 percent (GAO 1995). In 2003, Alexander Colvin, a leading researcher of dispute resolution, conducted a survey of the telecommunications industry (Colvin 2008). The survey found that while only 14.1 percent of industry corporations included mandatory arbitration clauses in their employment contracts, 22.7 percent of industry employees were, in fact, party to one as they were more likely to be used by larger employers. As the Supreme Court continued to expand the ways in which mandatory arbitration clauses could be used through out the aughts and early teens, more anecdotal evidence seemed to suggest that their prevalency had increased as well (Stone and Colvin 2015). This was confirmed in 2017, when Colvin conducted a national survey of private-sector non-union workplaces. The results of the survey indicated that the percentage of nonunion businesses now using the clauses had increased to a whopping 50.4 percent. Adjusting for workforce size of the companies surveyed, the study found that 56.2 percent of all United States non-union private sector employees were now subject to a mandatory arbitration clause—approximately 60 million Americans (Colvin 2017).

Why is Non-Union Employment Arbitration Controversial?

The critiques that commentators and legal scholars have made about mandatory employment arbitration generally fall under one of two categories: those that deal with the ethics of the institution (*See, e.g.* Ware 1996; Farmer 2011), and those that deal with its outcomes (*See e.g.* Colvin 2011; Colvin and Gough 2015). In both cases, criticism is mainly targeted at a specific subset of cases: Those in which the arbitration clause at hand is *employer-promulgated ,* rather than *individually negotiated*. Whereas the latter type of case typically involves employment contracts of high level-executives, substantively negotiated between both parties, and often containing many due process protections, the former most often applies to

non-executives. These employer-promulgated clauses are often included in boiler plate contracts and—contrary to individually negotiated clauses—they are generally presented as "take-it-or-leave-it" propositions (Colvin 2011). In other words, if the employee does not agree to the terms initially presented to them, they will simply not receive the job. When I refer to mandatory arbitration broadly in this paper these are the cases to which I am referring.

Criticism falling under the ethics category has generally decried the coercive nature of these cases: Low-level employees have no real ability to negotiate if they wish to secure employment, yet they are forced to forgo their right to judicial recourse anyways. Some legal scholars have even suggested that these contracts ought to be considered illegal under the common-law doctrine that contracts are only valid and enforceable if made at will (*See e.g.* Ware 1996; Stone and Colvin 2015).

The ethical arguments notwithstanding, the largest set of criticisms made since the Supreme Court's *Gilmer* (1991) decision have been those addressing the purportedly inferior outcomes employees achieve. These critiques have generally held that arbitration is significantly more vulnerable to anti-employee bias and, as a consequence, employees win less often and win smaller monetary awards when they do win (*See e.g.* Bingham 1997, Colvin 2011). As this is the area of study to which my thesis aims to contribute—assessing whether employer-promulgated mandatory employment arbitration truly does lead to inferior outcomes for employees—I review the existing literature on the subject in the following section.

Literature Review

Empirical scholarship into whether employees fare materially worse in mandatory arbitration than they do in litigation is, on the whole, inconclusive. Many studies have found that employees seem to win less often in arbitration, and win smaller awards when they do, and that arbitration

appears to give a comparative advantage to the largest, most experienced employers (*See e.g.* Bingham 1997; Colvin 2008, Colvin 2011; Colvin and Gough 2015; Horton and Chandrasekhar 2016). However, a number of other studies have concluded that, while arbitration has its flaws, it ultimately offers employees a number of advantages over other forms of employment dispute resolution (*See e.g.* Sherwyn *et al* 2005, Eigen and Sherwyn 2016). These studies have found that civil litigation often leads to similar outcomes, and that arbitration is often significantly less costly and offers a higher speed of case resolution. In this section, I first discuss why the literature is so divided, and then examine the current state of research on the various aspects of arbitration that must be understood to assess its relative merits.

Methodological Challenges

The bifurcated nature of the literature between studies that reach favorable conclusions and those that do not is, at least in part, due to some of the inherent difficulties in comparing the results of arbitration with those of similar employment litigation. No clearing house currently exists for employment arbitration outcomes, and no federal statute dictates that the results of employment arbitration be made public. As a consequence, it is difficult to make systematic comparisons between the two institutions, and most of the existing literature has focused either on litigation (*See e.g.* Delikat and Kleiner 2003; Clermont and Schwab 2004), or else arbitration (*See e.g.* Bingham 1997; Colvin 2011; Colvin and Gough 2015; Horton and Chandrasekher 2016) with rudimentary comparisons between the two.

Certain studies have questioned whether a straight comparison of win rate—the percentage of cases in which the employee prevails—and award size—the amount of money an employee is awarded if they win their case—between litigation and arbitration is appropriate (*See e.g.* Schwartz 2009; Sherwyn *et al* 2005). These studies have argued that there may be

significant differences between the types of cases that are brought to litigation and those brought to arbitration: That given the generally higher cost of litigation when compared to arbitration, litigation cases with a lower probability of success may be weeded out before they ever make it to trial, thereby biasing the comparison (Schwartz 2009). Additionally, these studies have questioned whether summary judgments—the practice of judges ruling on cases pre-trial at the motion of either party if there is no substantive disagreement over facts—has led to artificially inflated win rates and award sizes for employees in litigation (Sherwyn *et al* 2005). Recent studies, however, have called these objections into question, finding that summary judgments are not significantly less common in the arbitral forum (Stone and Colvin 2015), and that there is little evidence of significant differences in the quality of cases being brought (Gough 2014).

In the absence of more comprehensive sources of case statistics, however, questions over the comparability of analyses of arbitration and litigation are likely to remain. Despite this, in this author's view, raw comparisons still have public-policy value as a first approximation of differences between the two forums. Without alternatives, they remain a vital part of determining whether mandatory arbitration, particularly in the employment sphere, amounts to a simple exchange of venue or an actual example of justice deferred.

Win Rates

Before the 2000s, little publicly available information on arbitration outcomes existed for researchers to study, and what did exist was provided at the discretion of arbitration providers themselves. Consequently, early studies of arbitration win rates tended to show exceedingly high estimates for employees, often between 65 and 75 percent (*See e.g.* Bingham 1997; Maltby 1998). Additionally, these early studies did not differentiate between employment arbitration arising under employer-promulgated arbitration clauses and employment arbitration arising

under individually-negotiated contracts. As previously discussed, individualized employment contracts are most often used in the case of high-level, high-earning executives, and more recent studies have demonstrated that plaintiffs in arbitration cases that arise under them can expect to see substantially better results (Hill 2003).

Even so, studies released in the years directly following the earliest research still tended to find win rates for employer-promulgated mandatory arbitration that were similar to comparable litigation (Hill 2003; Delikat and Kleiner 2003; Schwab 2004). For instance, Hill (2003) found a 34 percent win rate when analyzing a sample of 200 cases from one arbitration provider, while Delikat and Kleiner (2003), in a study of 3000 federal employment litigation cases, found a 33.6 percent win rate. Similarly, Clermont and Schwab (2004) found that employees won 38 percent of jury trials when they examined all 235,356 federal employment discrimination cases that had been filed between 1988 and 2000—results very similar to what Hill and others had found for comparable arbitration. Like with the earliest studies, however, we should be cautious about putting too much stock in the findings of Hill (2003) and other research from the early 2000s. Later work has suggested that the use of convenience samples, and small overall sample size may have biased the estimated win rate upwords (Colvin 2008; Colvin 2011). And indeed much of the literature that has been released since Hill's study has shown far lower win rates for employees in mandatory arbitration (Colvin and Gough 2015; Horton and Chandrasekher 2016).

Part of this trend has been access to better data. In 2002, California amended its code of civil procedure to include a disclosure requirement for some types of arbitration. Specifically, the amendment required that all arbitration providers operating in the state disclose certain summary statistics, like award size and prevailing party, for all consumer and employment mandatory

arbitration cases conducted by the institutions nationally. This new cache of data has, at least partially, lifted the veil on the often secretive world of private arbitration. And many recent studies have used it to calculate more reliable estimates of case outcomes than were possible with the smaller convenience samples that earlier studies examined. These recent studies have, in particular, tended to focus on the California-mandated reportings of one specific arbitration provider: the American Arbitration Association (AAA)—the nation's largest.

Just as earlier theoretical work had predicted, empirical studies utilizing the new AAA dataset have found far lower win rates for employee plaintiffs. In 2011, Alexander Colvin consecrated the era of comprehensive AAA-focused arbitration analysis by examining 3,945 employment cases filed and closed between 2003 and 2007. He found, contrary to earlier studies, that employee plaintiffs won only 21.4 percent of the time when their cases made it to a final arbitrator decision. In more recent years, other AAA-focused studies of employment arbitration have found even lower plaintiff win rates. In 2015, Colvin and Gough analyzed all employment cases filed and closed between 2003 and 2013, and found an employee win rate of only 19.1 percent. In 2016, Horton and Chandrasekher analyzed the AAA casefile for cases between 2009 and 2013, and found a win rate of only 18 percent. These newer findings would be significant on their own, but they are further validated by recent research on employment arbitration that has ventured outside of the AAA.

As part of his 2015 PhD dissertation, Mark Gough surveyed 1256 employment plaintiff attorneys on the outcome of their most recent civil litigation and arbitration discrimination cases (Gough 2020). Although the arbitration win rate he found of 46 percent was significantly higher than the win rates found in AAA studies—potentially due to the fact that Gough focuses only on

discrimination claims—it was significantly lower than the 64 percent win rate he found for employment discrimination litigation.

Award Sizes

Although plaintiffs often give an award amount request when they file their case, juries, judges, and arbitrators have substantial discretion to determine the ultimate size of the award if one is given (Colvin 2011). Thus, award size functions as another mechanism—alongside win rate—for assessing the fairness of arbitration when compared to litigation. As with win rates, the earliest studies of the issue tended to find only negligible differences with the results of similar litigation. For instance, Eisenberg and Hill's 2003 analysis of 70 arbitration cases conducted by one provider found a median award amount of $64,700 and a mean award amount of $144,496—results that compare only somewhat negatively with their own $200,000 estimate of average award size in similar state-level litigation (Eisenberg and Hill 2003). In another early study of arbitration award sizes, Delikat and Kleiner (2003) compare the outcomes from a sample of financial-sector employment arbitration to outcomes from a contemporary sample of federal employment-discrimination litigation. They find that arbitration award sizes compare favorably to those in litigation with an arbitration median and mean of $100,000 and $236,292, and a litigation median and mean of $95,000 and $377,030.

The results from these early studies, however, have to be questioned because of certain methodological issues they possess. Colvin (2011), for instance, suggests that Eisenberg and Hill (2003) most likely overestimate mandatory arbitration award sizes because their sample also includes cases brought under individually-negotiated agreements. Similarly, Delikat and Kleiner's (2003) results may also be suspect as their litigation sample includes cases with

plaintiffs from all industries, while their arbitration sample includes only plaintiffs employed in the notoriously well-compensated financial sector.

The enactment of the California statutory disclosure requirement—as it did with win rates—marked a turning point in the literature on award sizes. In its wake, a number of studies have used the more representative picture that the AAA disclosures provide to better assess arbitration award amounts across all types of employment cases. In Colvin's 2011 study of AAA employment arbitration, he finds a median employee award amount of just $36,500 and a mean award of just $109,858. These amounts are far smaller than those found in earlier studies for comparable litigation even before adjusting for inflation. Likewise, in Colvin and Gough's 2015 study of all AAA employment cases filed and closed between 2003 and 2013, they find similar results even after expanding the temporal scope of cases under analysis, with a median award amount of $48,670 and a mean award amount of $135,316. In 2016, Horton and Chandrasekher released another AAA-focused study, this time analyzing employment cases that had been filed between July 2009, and the end of 2013. Because the AAA only adds cases to their disclosure after they have been closed, Horton and Chandrasekher's sample is materially different from the one used by Colvin and Gough even though the timeframe of the former is encompassed by the latter. Regardless, Horton and Chandrasekher find comparable results when adjusted for inflation, with a median award of $52,129 and a mean award of $203,362.

In recent years, only Gough's 2015 survey of employment plaintiff lawyers has looked at employment arbitration award sizes outside the context of the AAA (Gough 2020). Gough's study does find significantly higher arbitration award amounts. But, because he collects information on both the lawyers' most recent arbitration results and their most recent litigation

results, he is able to directly compare the two, and still finds a large negative discrepancy between the lawyers' results in arbitration and their results in litigation.

Repeat Player Effect

Beyond win rates and award amounts, one area that has been a particular focus of much of the existing literature on employment arbitration is the concept of the *repeat-player effect*. In his groundbreaking 1974 study, Galanter argues that litigation systematically advantages those who participate multiple times—"repeat players"—over those who do not participate regularly—"one shotters" (Galanter 1974). Galanter offers multiple reasons for why this may be the case: For one, repeat players tend to be larger, wealthier firms, and they may simply have more resources to expend on winning cases. For the same reasons, they may also be more sophisticated in their use of the courts, knowing which cases would be advantageous to settle pretrial and which would be better to argue to resolution. These hypotheses for the cause of the repeat-player effect are the most innocuous. They suggest that the advantage arises out of fundamental qualities of the repeat player, ones that are external to the courts themselves. But Galanter proposes another too: Repeat players may also achieve an informational advantage from their intensive use of the litigation system. As they participate more often, the theory goes, they learn best-practices for optimising outcomes. Such a theory is more concerning because it suggests that the system itself is playing a role in entrenching the advantage, calling into question its impartiality.

Galanter proposed his theory specifically as one of the judicial forum, but as the use of arbitration, and particularly mandatory-employment arbitration, has risen in the U.S., many scholars have argued that the same dynamics hold true for it as well (*See e.g.* Bingham 1998; Hill 2003; Colvin 2008). And its existence in this context is of no small concern because, in the case

of employment arbitration, the repeat player—if one exists—is almost always the business (Colvin 2011).

Since research began to be conducted, empirical assessments have generally agreed that repeat players do receive a similar advantage in arbitration. Even studies that have reached a favorable conclusion of arbitration over all have generally found a statistically significant correlation between the experience of a disputing party and case outcome (*See e.g.* Hill 2003; Sherwyn *et al* 2005). Disagreement, however, has arisen over which theory of the repeat-player effect is most plausible. Some studies like Bingham (1998), Colvin (2011), and Colvin and Gough (2015) have argued that the advantage associated with repeat players most likely arises from some combination of all the causal theories initially proposed by Galanter—that repeat-playing businesses are wealthier, more sophisticated, *and* have an informational advantage. Other studies like Hill (2003) and Sherwyn *et al* (2005), have argued that the repeat-player effect in arbitration is most likely exclusively the result of size, wealth, and sophistication. This distinction is not without consequence as, in many ways, it forms the basis for whether one views the repeat-player effect as pernicious or innocuous.

In more recent years some studies have moved beyond simply looking for a binary repeat player effect—whether an advantage develops from participating more than once—to looking for a marginal effect—whether participating more often leads to better outcomes proportional to the amount of participation (Colvin and Gough 2015; Horton and Chandrasekher 2016). Theoretically this would make sense: There is certainly a substantive difference between a small business that arbitrates two cases and an international conglomerate that arbitrates hundreds. Empirically, the results so far have been mixed: Colvin and Gough (2015) which analyzed AAA employment cases filed and closed between 2003 and 2013 found a significant negative

correlation between the number of cases in which the employer was named as a defendant and employee win rate. They did not, however, find a significant correlation when analyzing award amount. Likewise, Horton and Chandrasekher (2016) found significance with win rates but not with award amounts when analyzing AAA employment cases filed between July 2009 and December 2013.

Repeat Pair Effect

More concerning than its simple existence, some studies have suggested that the repeat-player effect may, in fact, be larger in arbitration than it is in litigation (*See e.g.* Bingham 1998, Colvin 2011, Colvin and Gough 2015). Arbitration is unique from other forms of conflict resolution in that the disputing parties have the ability to directly influence which individual hears the case. In mandatory employment arbitration, the arbitrator may either be designated directly in the contract (*i.e.* chosen by the business), or selected off a list presented by the arbitration provider with both parties striking names until one remains (Colvin and Gough 2015). Regardless of which particular procedure is used, concern arises that the arbitrator-selection process may give repeat-playing businesses an even greater ability to systematically bias outcomes against employees than they have when participating in litigation.

Two reasons have been proposed for why this may be the case: The first suggests that this dynamic arises because arbitrators *themselves* are influenced in their decision making—that the incentives they possess to please parties like large corporations that have the ability to provide more business in the future may lead to preferential treatment. Less insidiously, a second school of thought holds that this additional bias may not arise out of borderline arbitrator corruption, but rather through more-experienced parties better understanding the characteristics that make an arbitrator likely to rule in their favor (Colvin and Gough 2015). This is important as it means

that unethical behavior on the part of the arbitrator is not necessarily a prerequisite for additional bias in arbitration.

Regardless of which theory is correct, some studies have suggested that evidence for this additional repeat-player advantage can be found by analysing whether a corporation using the same arbitrator multiple times provides its own advantage above and beyond simply participating in arbitration more often (*See e.g.* Bingham 1998; Colvin and Gough 2015; Horton and Chandrasekher 2015). The term of art for this is the *repeat-pair effect*, and like the theory that underlies it, its existence is far more controversial in the literature than that of the standard repeat-player effect. An early empirical analysis of employment arbitration outcomes, Bingham (1998), found that employees had a significantly lower win rate when facing a repeat business-arbitrator pair, but a number of studies released in the following years argued that the finding was the result of a small, selective sample among other methodological deficiencies (Hill 2003; Sherwyn et al. 2005). In more recent years, however, evidence for the existence of a repeat-pair effect has been bolstered by a group of studies that have taken advantage of larger, more representative data sets and used more robust controls in their analysis (Colvin 2011; Colvin and Gough 2015; Horton and Chandrasekher 2016). These studies have found that the presence of a repeat business-arbitrator pairing is predictive of lower employee win rates and smaller award amounts, suggesting that arbitration may truly possess biases above and beyond what can be found in litigation.

Plaintiff Lawyers as Antidote to Repeat-Player and Repeat-Pair Effect

Considering only the employer and the employee, the employer is almost always the repeat-player in mandatory employment arbitration; Employees almost never participate in more than one case, while a large national corporation could easily be involved in hundreds over a

relatively short amount of time (Stone and Colvin 2015). Employees and employers, however, are not the only relevant parties involved: Both are usually represented by counsel that have their own history within the system. And recent research has found that, increasingly, arbitration in the U.S. involves not just repeat-playing businesses, but repeat-playing plaintiff lawyers as well. That is to say, the attorneys plaintiffs hire as council are increasingly arbitration specialists—lawyers who have already participated in large numbers of arbitration cases (Searle Report 2009). Given the concern over the repeat-player advantage, this begs the question whether repeat-playing plaintiff lawyers could help parties with less arbitration experience overcome the disadvantages they may face when going up against more experienced businesses.

To date, only a small number of studies have attempted to empirically test this proposition. As part of a larger analysis of consumer arbitration outcomes, Horton and Chandrasekher (2015) estimate multivariate regression models for both consumer win rate and expected award amount, including both a repeat-playing business variable and a repeat-playing plaintiff-lawyer variable. Surprisingly, while they find no significant effect for most categories of plaintiff lawyers, they find that the presence of a high-level repeat-playing plaintiff lawyer is, in fact, negatively correlated with consumer win rate, while the presence of a mid-level repeat-playing plaintiff-lawyer is negatively correlated with award amount—the opposite of what one might expect, given the theory that repeated participation benefits the participant.

In a paper released the next year, Horton and Chandrasekher (2016) bring the same analysis to bear, but this time on employment arbitration. In addition to analyzing the effect of plaintiff lawyer experience, their second study also analyzes the impact of repeated plaintiff lawyer-arbitrator pairings—the frequency with which the plaintiff lawyer and arbitrator have worked together. Unlike their original study, Horton and Chandrasekher here find a significant

positive correlation between repeat-playing plaintiff lawyers and case outcome, suggesting that, at least for employment arbitration, experienced plaintiff lawyers may be helpful in improving the results of non-corporate parties. They do not, however, find a significant correlation with case outcome when examining plaintiff lawyer-arbitrator pairings.

Former-Judge Arbitrators

Recall that a primary concern for employment arbitration is that the ability the involved parties have to decide who hears the case may potentially create biases towards experienced participants *above and beyond* what is found in litigation. If this is the case, and the additional advantage arises out of experienced participants better knowing which arbitrators are likely to rule in their favor—not arbitrator corruption—one of the easiest traits on which a repeat player could differentiate potential arbitrators would be their professional background. While most arbitrators were originally lawyers, a growing number are former judges who started arbitration practices after retiring from the bench (Colvin and Gough 2015). Thus, a question emerges as to whether experience as a judge may influence arbitrator decision-making. Clermont and Schwab (2004) found that employment litigation decided by a jury was more likely to be decided in favor of the employee plaintiff than cases decided by a judge. However, only one study to date—Colvin and Gough (2015)—has looked specifically at whether arbitrators who were formerly judges exhibit systematic differences in adjudication behavior between themselves and their non-former judge counterparts. After controlling for confounders, the study finds, that there is no significant correlation between the presence of a former-judge arbitrator and an employee's win probability, but that there is a significant positive correlation with the award amount an employee can expect to receive.

Arbitrator Gender

Another potential characteristic on which a repeat-playing business could select an arbitrator is the arbitrator's gender. While the question of gender's impact on decision making remains highly controversial, scholars in a wide array of disciplines including economics and psychology have theorised that, when comparing population averages, there may be material differences in how men and women make difficult choices (*See e.g.* Bampton and Maclagan 2009; Friesdorf, Conway, and Gawronski 2015). The corresponding empirical literature on the matter is, on the whole, inconclusive, but a number of studies have suggested that on average women may be more sensitive to the context in which a decision is made. In essence, they may be more likely to take into account extenuating circumstances whereas men may be more likely to adhere to a rigid conception of right and wrong (Friesdorf *et al.* 2015). In the context of dispute resolution, the practical implications of this posited effect are theoretically ambiguous: It is not clear, if a gender effect does exist, whether it would cause a male (or female) judge or arbitrator to be more or less likely to rule in favor of a plaintiff.

Since the 1980s, a large amount of scholarly attention has been devoted to answering this question for judges and justices. Some scholars have found that a significant relationships exist, but at least as many have not. Additionally because these studies tend to focus on small subsets of cases and specific judicial venues, it is hard to know just how broadly applicable their results actually are. For example, Gryski Main & Dixon (1986) find a marginally significant gender effect when annalyzing a specific subset of state High Court gender-discrimination cases decided between 1971 and 1981—in essence, suggesting that a panel of judges is more likely to rule in favor of a plaintiff if it includes at least one female jurist. Kulik, Perry, and Pepper (2003), however, find no significant correlation for jurist gender and case outcome when they examine a

sample of 143 federal civil bench trials pertaining to gender discrimination and occuring between 1981 and 1996.

In the context of employment arbitration, few studies have been conducted that attempt to assess the relationship between gender and case outcome; Those that have have likewise reached mixed conclusions. Lamare and Lipsky (2014), in an analysis of employment arbitration administered by the Financial Industry Regulatory Authority, found a significant positive correlation between the use of a female arbitrator and the size of the award distributed when using bivariate regression, but found no significance when other control variables were added to the model. Colvin and Gough's 2015 analysis of AAA employment cases filed and closed between 2003 and 2013 found that male arbitrators were significantly more likely to preside over cases that ended up settling pretrial, but that gender had no significant effect on the outcome of adjudication.

Settlement Rate and Dismissal Rate

While the vast majority of empirical arbitration studies have focused on analyzing employee win rate and expected award amount, the majority of arbitration cases do not actually end with an arbitrator award-determination. This is because arbitration, like litigation, is far more likely to end with a pretrial settlement than it is to reach a formal arbitrator-decided conclusion. Recent studies focusing on the pre-*Concepcion* period of employment arbitration like Colvin and Gough (2015) and Horton and Chandrasekher (2016) have estimated that just under 64 percent of mandatory employment cases are settled pre-trial while only 18 percent make it to the final phase of adjudication. This implies that the majority of employees who receive remuneration from arbitration are doing so in the form of settlements rather than arbitrator-decided awards.

While it is impossible to analyze the content of employment arbitration settlements as this information remains non-public, understanding how various factors, including the repeat-player and repeat-pair effects, influence settlement behavior is crucial to understanding whether bias exists in arbitration, and if so, how it functions. A number of studies have come to a favorable conclusion as to the fairness of employment arbitration, despite finding evidence of a repeat-player or repeat-pair effect, in part because they assume these dynamics are the result of differences in settlement behavior between experienced and non-experienced parties (Schwartz 2009). The logic here is certainly debatable—a bias attributable to settlement behavior may still mean the system overall is unfair—but in any case, it underscores how settlement behavior constitutes an important factor in assessing arbitration's merits on the whole. The issues importance notwithstanding, only one study to date—Covlin and Gough (2015)—has sought to empirically determine whether differential settlement behavior actually exists. Looking at all AAA mandatory employment cases filed and closed between 2003 and 2013, the study finds no evidence that settlement behavior is influenced by either the repeat-player or the repeat-pair effect, among other tested factors. These results suggest that the more benign explanations for anti-employee arbitration bias—that it is largely a selection effect—must be ruled out.

In addition to pre-trial settlements, cases can also be shunted off from final adjudication through summary dismissal. This is when an arbitrator, on the motion of the defendant, throws out a case pre-trial for being overly-frivolous, or otherwise unworthy of a full hearing. It is an inherently subjective decision, one with a large impact on the overall performance of employees in the arbitral system, and evidence suggests that its usage is becoming increasingly common. Horton and Chandrasekher (2016), for instance, find that only .22 percent of AAA employment cases filed between 2009 and 2013 and closed before July of 2014 were summarily dismissed.

For cases filed between 2012 and 2018, however, I find that this number has increased a shocking 81 fold to 18 percent of all cases filed—a concerning trend given that all dismissed cases are ones where the employee, by definition, receives no recompense, monetary or otherwise. Despite its shocking rise in prevalency, to the best of this author's knowledge, no study yet published has attempted to understand what factors, if any, influence its occurrence.

Effect of Class Action Bans on Employment Arbitration

In April 2011, the Supreme Court ruled in *AT&T Mobility LLC v. Concepcion* that bans on class-filings used in conjunction with mandatory arbitration clauses are legal and enforceable in consumer and employment contracts. This means that, since that time, corporations have had the ability to prevent claims from multiple parties being brought against them in class-action, and can, instead, force each individual claimant to arbitrate their case separately. As was expected in the wake of the ruling, many companies that had not previously used class-action bans adopted one as a mandatory term in their contracts of employment alongside existing arbitration clauses (Horton and Chandrasekher 2016). Many commentators have argued that the rise of the duel class-action ban/arbitration clause has further eroded employees' ability to vindicate their statutory rights in the event that they are violated. They suggest that, in the absence of class-action, employees will often forgo seeking legal redress as either the monetary cost or the opportunity cost of individually filing a case will likely be greater than the small claim they are making (*See e.g.* Stone and Colvin 2015; Horton and Chandrasekher 2016).

To date, there has been little empirical research into whether the Supreme Court's 2011 decision has truly made arbitration materially worse for employees. Horton and Chandrasekher (2016), as part of a larger study of AAA employment arbitration outcomes, did look for signs that the arbitral system had changed post-ruling, finding none. But, the study suffers from many

methodological problems that hinder it from accurately addressing this question. For one, the AAA data set from July 2014 that was used in the study only includes cases that were *closed* by that time. Given that the study's timeframe includes cases filed up until the end of 2013, it quite likely misses a significant number of cases that were then working their way through the system, biasing the data towards quicker cases which may have had a material impact on the findings. In addition, the studies time range, more generally, only includes the first two years after the *Concepcion* ruling. To this author's mind, the assumption that large corporations would add class-action bans to their employment contracts in large numbers so soon after the ruling seems questionable. It seems far more likely that companies would shift gradually towards the practice as it evolved into a corporate norm over a number of years. None of this is to say that the results of Horton and Chandrasekher (2016) are necessarily wrong. Only that their results can neither be considered conclusive nor representative of the long-term impacts of *Concepcion* on employee performance in mandatory arbitration.

……..…….

My study aims to fill this gap in the literature, examining employee-plaintiff performance in the decade directly following the Supreme Court's 2011 ruling. Building off the work of Colvin and Gough, Horton and Chandrasekher, and other dispute-resolution scholars, I attempt to determine the relative performance employees achieve in arbitration post-*Concepcion*, understand what has changed since the pre-*Concepcion* studies were conducted, and assess what factors are currently influencing case outcomes. In addition, I also go beyond prior scholarship by examining not just settlement rates, win rates, and expected award amounts, but looking also at the rate at which employee's cases are summarily dismissed. Thus my study, both improves

our understanding of post-*Concepcion* arbitration, and provides insight into a fundamental aspect of employment arbitration that prior scholarly work has neglected.

Methods and Data

Data

In this empirical analysis of mandatory employment arbitration in the post-*Concepcion* period, I analyze data from the American Arbitration Association (AAA)—the nation's largest arbitration provider—to assess the outcomes employees are currently achieving within the arbitral system. Specifically, I analyze the results of all mandatory employment cases filed with the AAA between 2012 and 2018, and closed before October of 2020, to understand how the arbitral landscape has changed since earlier pre-*Concepcion* studies were conducted. The state of California has mandated since the early 2000s that all arbitration providers operating within the state maintain a publicly-available record of case outcomes and participant demographics for all consumer and employment arbitrations they conduct nationally. Like many prior arbitration studies, the data I analyse was released under the auspices of this California mandated-reporting requirement.

The AAA's statutorily mandated database is updated once a quarter, and only includes cases that have been closed in the last five years. As a consequence, using only the most recent version of the casefile may be insufficient for assessing the state of employment arbitration outcomes as it does not include cases that were closed over five years ago, and those that, while already filed, have yet to reach a conclusion. Thus, to increase the robustness of my findings, I merge the Q3 2020 AAA report, with the Q1 2017 report, and drop duplicated cases that were created as a result of merging the two files. In addition, I pick a time range of cases to

analyze—those filed between 2012 and 2018—that I feel give the most accurate, unbiased picture of post-*Concepcion* employment arbitration currently available. To my mind, cases filed prior to 2012 occurred too close to the court decision to exhibit any of the subsequent impact on the industry—should it exist—that would have occurred as a result, and the set of cases filed after 2018 would be irredeemably biased towards those that closed very quickly after filing. Beyond these measures, I aimed to ensure that I was only including cases where the employee would be the plaintiff if the case had been brought to litigation—an important distinction given that these are the cases this study is interested in analyzing. For this reason, I follow the procedures pioneered by Horton and Chandrasekher (2015) of removing cases that were not initiated by the employee, and those where the award amount the business was claiming was larger than the award amount claimed by the employee. As a result of these decisions I was left with a case file of 13,245 cases with which to conduct my analysis.

At the outset, I will acknowledge the limitations of using AAA data to draw larger conclusions about the state of employment arbitration more broadly. The AAA may be the leading and largest provider of arbitration services in the country—by some estimates conducting 50 percent of all arbitrations nationally each year (Estlund 2018)—but a significant amount of arbitration is conducted beyond its purview. Furthermore, the AAA has industry leading ethics standards for its arbitrators that clearly indicate procedures and arbitrator responsibilities in a way that many other provider's standards do not (Colvin and Gough 2015). When taken together, these factors suggest that AAA outcomes may be significantly more favorable to employees than the actual outcomes they experience more broadly.

These issues notwithstanding, given that no clearing-house exists for arbitration data industry-wide, and that arbitration providers who do not operate in the state of California are not

required to disclose their results publically, the AAA casefile remains the best of a number of imperfect options for assessing arbitration in America. This is for four main reasons: First, while California state law since 2003 has mandated that *all* arbitration service providers operating within the state provide data on the employment arbitration cases they oversee nationwide, not all of these reports are created equally. Of the eight arbitration-providing institutions currently operating in the state, the AAA reports by far the most complete set of information, with the fewest omissions of both participant demographics and outcomes. Second, the AAA is not only the largest arbitration service provider in California, but in the nation as a whole. This means that its data inherently represents a larger portion of the industry than that of anyone else. Third, because the AAA uses industry-leading ethics standards, studying its data as a proxy for the larger industry amounts to a conservative test of the industry's treatment of employees generally. That is to say, that if problems show up in the AAA dataset, we can be reasonably sure that they are problems that would appear for most arbitration providers. Fourth and finally, if for nothing else, the majority of recent arbitration studies have also utilized AAA data. Thus, by following in their footsteps my study becomes more comparable, and better tracks changes across time in the arbitral landscape. Taken together, these four points offer a compelling logic for using AAA data as the proxy, and therefore, it is what I choose to do for this study.

Methods

To begin my analysis, I start by calculating summary statistics on employment arbitration outcomes. Here, I focus specifically on determining the employee win rate, average award size, and the breakdown of how cases were disposed (*e.g.* settled, summarily dismissed, awarded). Thereafter, I conduct simple bivariate analysis of the effect of participation-frequency and arbitrator-characteristic variables on case outcomes. Finally, I estimate multivariate

regression models with the independent variables used in the binary analysis to assess concurrent effects and control for confounders. In the case of both analyses, my aim is to assess the validity of a number of theories proposed in the literature for how arbitration may be biased against employees and how it may, in fact, be more biased than comparable litigation. To understand what each independent variable is measuring, and what issues it is meant to test for, I offer the following list:

Repeat Business Score—This is a count that measures the number of cases within the broader AAA casefile that the business has participated in. If the score is three, it means that the business has participated in three cases within the dataset. It is meant to assess the existence of the *repeat-player effect*—the notion that more experienced participants will be advantaged in any form of dispute resolution. In my bivariate analysis, I treat this variable as a binary, looking at the differences in outcomes between cases involving a business with a score of 1 *(not a repeat business)* and cases involving a business with a score of greater than 1 *(repeat business)*. In my multivariate analysis I treat this variable as continuous to look for the marginal effect of participation.

Repeat Business-Arbitrator Pair Score—This is a count that measures the number of cases within the broader AAA casefile where the business and arbitrator involved in the case have worked together. If the score is three, it means that the business and arbitrator have worked together on three cases within the dataset. It is meant to assess the existence of the *repeat-pair effect*—the notion that because arbitration offers participants a unique ability to influence who hears the case, more experienced participants will receive an advantage *above and beyond* what they do in other forms of dispute resolution like litigation. In my bivariate analysis, I treat this variable as a binary, looking at the differences in outcomes between cases involving a business-arbitrator pair with a score of 1 *(not a repeat pair)* and cases involving a business with a score of greater than 1 *(repeat*

pair). In my multivariate analysis I treat this variable as continuous to look for the marginal effect of repeat business-arbitrator pairings.

Repeat Plaintiff Lawyer Score—This is a count that measures the number of cases within the broader AAA casefile on which the plaintiff lawyer involved in a case has worked. If the score is three, it means that the plaintiff lawyer has participated in three cases within the dataset. It is meant to assess whether more experienced lawyers can help employees overcome the repeat-player advantage. In my bivariate analysis, I treat this variable as a binary, looking at the differences in outcomes between cases involving a plaintiff lawyer with a score of 1 *(not a repeat plaintiff lawyer)* and cases involving a business with a score of greater than 1 *(repeat plaintiff lawyer)*. In my multivariate analysis I treat this variable as continuous to look for the marginal effect of the lawyer's arbitration experience level.

Repeat Plaintiff Lawyer-Arbitrator Pair Score—This is a count that measures the number of cases within the broader AAA casefile on which the plaintiff lawyer and arbitrator have worked together. If the score is three, it means that the plaintiff lawyer and arbitrator have worked together on three cases within the dataset. It is meant to assess whether a higher level of familiarity between the plaintiff lawyer and arbitrator can help employees overcome a potential *repeat-pair* advantage that a business may possess. In my bivariate analysis, I treat this variable as a binary, looking at the differences in outcomes between cases involving a plaintiff lawyer and arbitrator with a score of 1 *(not a repeat plaintiff lawyer-arbitrator pair)* and cases involving a plaintiff lawyer and arbitrator with a score of greater than 1 *(repeat plaintiff lawyer-arbitrator pair)*. In my multivariate analysis I treat this variable as continuous to look for the marginal effect of the lawyer's familiarity with the given arbitrator.

Self-Represented Employee—This is a dummy variable that notes whether the employee involved in the case is self-represented—a fairly common occurrence given arbitration's notably informal style. It is meant to test whether employees who represent themselves fare materially worse in arbitration than those who hire professional representation. In both my bivariate and multivariate analysis this variable is binary.

Former-Judge Arbitrator—This is a dummy variable that notes whether the arbitrator was formerly a judge. I am able to determine this because—as prior arbitration studies have noted (Colvin and Gough 2015)—the professional norm for former-judges is to maintain the use of "Honorable" in their formal title. If the arbitrator is listed in the casefile with "Honorable" or "Hon." before their name, this variable is coded as 1; If not it is coded as 0. It is meant to test whether this aspect of the arbitrators background has a significant impact on the outcomes of the cases they decide. While between one quarter and one third of the cases in the casefile are missing the arbitrator name, and therefore automatically get coded as 0, I feel this test is still appropriate as the missing names make the test more conservative. That is, the missing data makes it *harder* to detect a difference between former-judge and non-former-judge arbitrators.

Male Arbitrator—This is a dummy variable that notes whether the arbitrator is male. I determine whether this is the case by systematically going through all the arbitrators whose names are listed in the casefile and coding male names as 1, and female names as 0. If the name was ambiguous, I looked up the arbitrator to determine their gender. It is meant to test whether the gender of the arbitrator has a significant impact on case outcome. As mentioned for the former-judge arbitrator variable, between one quarter and one third of the cases in the casefile are missing the arbitrator's name. For my bivariate analysis, I exclude these cases when looking for a gender effect. While there are arguments for and against this treatment, I feel this was the best option, given that I could not be certain whether the gender composition of the cases without names was the same as

the gender composition of those where a name had been listed. For my multivariate analysis, I exclude this variable from regressions dealing with the overall casefile as including it would significantly reduce the statistical power. For regressions dealing only with the cases that make it to the final adjudicatory phase, I do include this variable as the number of missing names for this subset of cases is much smaller.

As prior studies have done for earlier periods, in my regression analysis, I analyze how my independent variables affect three measures of employee case outcome: For the set of cases that make it to the final adjudicatory phase, I look at the employee win rate, and expected employee award amount; For all cases in the case file, I look at the rate at which cases settle prior to a final hearing. In addition to these analyses, I also go beyond prior research by looking at a fourth outcome factor: the rate at which cases are summarily dismissed. As win rate, settlement rate, and dismissal rate are measures of binary outcomes, I estimate logit models in these cases for my multivariate analysis, while I use an ordinary least squares regression (OLS) when dealing with expected award amount.

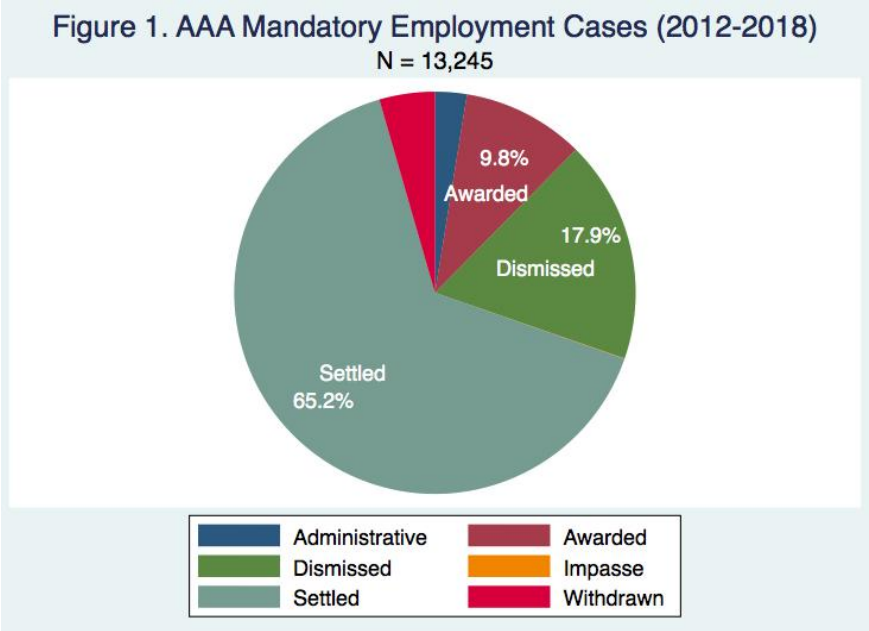
Findings

This section proceeds in two parts. I begin by discussing the results of my simple bivariate analysis, analyzing summary statistics and the impact that individual, binary participation-frequency and arbitrator-characteristic variables have on case outcome. I then move beyond the first-order analysis, and analyse the results of the multivariate regression models I estimate in an attempt to understand concurrent effects and to control for potential confounders. Unlike with my bivariate analysis, I use continuous, rather than binary, participation-frequency variables in order to estimate the marginal impact of participation frequency in my multivariate

regression models. In both parts, I focus on four main aspects of case outcome: the probability a case is settled pre-adjudication; the probability a case is dismissed pre-adjudication; the probability that an employee wins if their case does make it to adjudication; and finally, the monetary value of award an employee can expect to receive should their case make it to adjudication.

Bivariate Analysis and Summary Statistics

Figure 1 illustrates the breakdown of case dispositions (*e.g.* settled, dismissed), while Table 1 reports the summary statistics for the casefile, and shows the results of simple bivariate regression analyses of participant-frequency and arbitrator-characteristic effects on case outcomes. Of the 13,245 mandatory employment cases filed with the AAA between 2012 and 2018, and closed prior to October 2021, 8,635 (65%) were settled pre-trial, 2,371 (18%) were summarily dismissed, and only 1,303 (10%) actually made it to the final adjudicatory phase. Of these, employees won just over 22 percent of the time, achieving an expected award value of just over $69,641 in 2020 dollars. Given that employees lost well over three quarters of the cases that



made it to adjudication, award amounts were inevitably right skewed with a median award amount of zero dollars. Looking only at cases where the employee received a positive monetary sum, award amounts still skewed rightward, with a small number of very large awards driving the overall expected value upwards. For only cases where a positive sum was awarded, the mean award amount is $284,404 and the median award amount is $62,414 in 2020 dollars.

The win rate I find is similar to those found by other recent arbitration studies looking at earlier sets of cases, while the expected award amount appears to have risen somewhat (Colvin and Gough 2015; Horton and Chandrasekher 2016). Both, however, are still significantly lower than what has been estimated for comparable litigation after adjusting for inflation (Delikat and Kleiner 2003; Clermont and Schwab 2004). More interestingly, the breakdown of case dispositions appears to have changed significantly in the post-*Conception* period. Relative to the results of earlier studies, the percent of cases being summarily dismissed seems to have grown exponentially, while the percent of cases making it to the final adjudicatory stage has fallen by about half (Horton and Chandrasekher 2016). This is significant because every case that is dismissed is effectively one where the employee receives no recompense, monetary or otherwise. If cases are being dismissed unjustifiably, this could mean that the picture the overall employee win rate and average award amount are painting is altogether too rosy.

First-order bivariate analysis of the impact of participant-frequency measures and arbitrator characteristics on case outcomes yielded a number of notable findings. Analyzing settlement rates, I find that cases involving either a repeat-playing business or a repeat business-arbitrator pair are significantly less likely to settle ($P<.01$), while cases involving either a repeat plaintiff lawyer or a repeat plaintiff lawyer-arbitrator pair were significantly *more* likely

Table 1

Simple Binary Bivariate Analysis: Win Rates, Award Amounts, & Case Dispoitions

		Case File Composition				Award Amount ($2020)
	Total	Settled	Dismissed	Awarded	Employee Win Rate	Mean
Category	N(%)	N(%)	N(%)	N(%)	%	[SD]
N	13,245	8,635	2, 371	1,303	22.4%	69,641.46 [641,126]
Repeat Employer (Repeat Player > 1)	10,753 (81.2%)	6,856 (79.4%)**	2,205 (93.0%)**	971 (74.5%)**	20.0%**	62,119.78 [637,288]
One-Shot Employer (Repeat Player = 1)	2,492 (18.8%)	1,779 (20.6%)	166 (7.0%)	332 (25.5%)	29.5%	91,640.10 [652,697]
Repeat Employer-Arbitrator Pair (Repeat Pair > 1)	3,052 (23.0%)	1,930 (22.4%)**	731 (30.8%)**	264 (20.3%)*	19.3%	31,488.86 [172,190]
One-Shot Employer-Arbitrator Pair (Repeat Pair = 1)	10,193 (77.0%)	6,705 (77.6%)	1,640 (69.2%)	1,039 (79.7%)	23.3%	77,675.28 [711,769]
Repeat Plaintiff Lawyer (Repeat Lawyer > 1)	9,732 (73.5%)	6,641 (76.9%)**	1,956 (82.5%)**	574 (44.1%)**	27.5%**	54,562.69 [364,253]
One-Shot Plantiff Lawyer (Repeat Lawyer = 1)	3,513 (26.5%)	1994 (23.1%)	415 (17.5%)	729 (55.9%)	18.3%	81,514.18 [793,966]
Repeat Plaintiff Lawyer-Arbitrator Pair (Lawyer Pair > 1)	2,888 (21.8%)	2,146 (24.9%)**	493 (20.8%)	158 (12.1%)**	31%**	28,587.40 [91,214]
One-Shot Plaintiff Lawyer-Arbitrator Pair (Lawyer Pair = 1)	10,357 (78.2%)	6,489 (75.1%)	1,878 (79.2%)	1,145 (87.9%)	21.2%	73,727.02 [681,786]
Self Represented Employee	1,010 (7.6%)	334 (3.9%)**	186 (7.8%)	286 (21.9%)**	6.9%**	5,155.88* [37,474]
Professionally Represented Employee	12,235 (92.4%)	8301 (96.1%)	2185 (92.2%)	1017 (78.1%)	26.7%	87,776.04 [724,469]
Former-Judge Arbitrator	1,918 (14.5%)	1,276 (14.8%)	310 (13.1%)*	260 (20.0%)**	23.5%	149,704.9* [1,204,027]
Non-Judge Arbitrator	11,327 (85.52%)	7,359 (85.2%)	2,061 (86.9%)	1,043 (80.0%)	22.0%	47,828.83 [384409]
Female Arbitrator	3,662 (37.2%)	2485 (36.8%)	512 (38.6%)	481 (37.1%)	23.2%	83,638.62 [820,811]
Male Arbitrator	6,180 (62.8%)	4261 (63.2%)	815 (61.4%)	815 (62.8%	21.8%	59,499.72 [504,783]

Bivariate significance: †P<.10, *P<.05, **P<.01

Note: Dataset contains missing values for Arbitrator Gender ; N for female and male judge dichotomous variable does not sum to total N

to settle (P<.01). This is a change from prior arbitration studies that found no correlation between participation-frequency variables and rate of settlement (Colvin and Gough 2015). It is important to note, however, that for all four of these variables, the simple bivariate correlations are quite small in magnitude. For instance, while repeat businesses are involved in 81.2 percent of all cases under study, they are involved in 79.4 percent of settled cases, a decline in probability of just 2 percent. By contrast, the correlation between settlement probability and a case involving a self-represented employee is much larger. While 7.6 percent of all cases involved a self-represented employee, these cases made up only 3.9 percent of those that ended in settlement (p<.01), a notable finding given that 65 percent of all cases were settled pre-adjudication.

Turning to dismissal rates, I find much larger, significant repeat-business and repeat business-arbitrator pair associations than those found for settlement rate. While cases involving the first were 81.2 percent of the total case file, and cases involving the second were 23 percent, they made up 93 percent and 30.8 percent of summarily dismissed cases respectively (p<.01). Surprisingly, cases involving a repeat plaintiff lawyer also made up a significantly *larger* portion of dismissed cases, 82.5 percent, than they did total cases, 73.5 percent. Unlike with settlements, the presence of a self-represented employee had no significant association with the probability of dismissal in bivariate analysis, while the use of an arbitrator who was formerly a judge had a significant positive correlation (p<.05). For both settlements and dismissals, the gender of the arbitrator showed no significance.

Continuing on to the employee win rate for cases that made it to the final adjudicatory phase, I again find evidence of a strong repeat-business effect in bivariate analysis: employees going up against a repeat business won only 20 percent of the cases they brought, compared with

29.5 percent when going up against a one-shot business—an almost 50 percent rise in probability and one with a significance level of $p<.01$. As one would expect under the prevailing theory, I also find that cases involving either a repeat plaintiff lawyer or a repeat plaintiff lawyer-arbitrator pair are both significantly correlated with a relatively large increase in employee win rate ($p<.01$). Interestingly and contrary to earlier studies, however, I find no significant relationship between the presence of a repeat employer-arbitrator pair and the probability of an employee win. Beyond these findings, the presence of a self-represented employee is significantly—and negatively—correlated with an employee winning their case, while the presence of an arbitrator who was formerly a judge and the arbitrator's gender possessed no significant correlation.

For employee award amount, the only variables with significant correlations were self-represented employee (negative; $p<.05$) and former-judge arbitrator (positive; $p<.05$). This is probably in part due to the fact that there were so few non-zero award amounts in the casefile. Indeed, of the 1,303 cases that made it to the final adjudicatory phase, employees received positive sums in only 292 of them, significantly decreasing the statistical power of the dataset.

Multivariate Regression Analysis

To understand the concurrent effects that my independent variables have on case outcomes, and to control for confounders, I go beyond my bivariate analysis and estimate multivariate regression models for predicted settlement rate, predicted dismissal rate, predicted win rate, and expected award amount. As the first three models attempt to predict the probability of binary outcomes, I use a logit model in these cases while I use an ordinary least squares (OLS) regression model for expected award amount. Table 2 presents the results of my logit model for settlement rates. In a logit model, the value of an independent variable's coefficient (β) entails that for each one-unit increase in the independent variable, the odds of the tested outcome are

multiplied by e^{β}. In this model, as well as those for dismissal rate and win rate, I present my results both as coefficients and as the more intuitive corresponding odds ratios for the convenience of the reader.

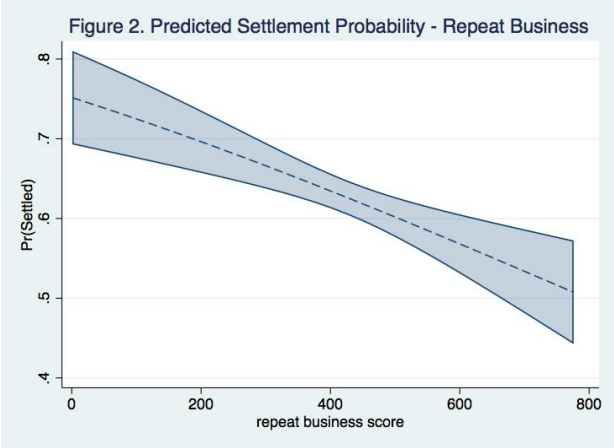
Table 2.
Arbitrator Demographic, and Participation Frequency Effects on Employee Settlement Rate *(Logit Model)* N = 13,245

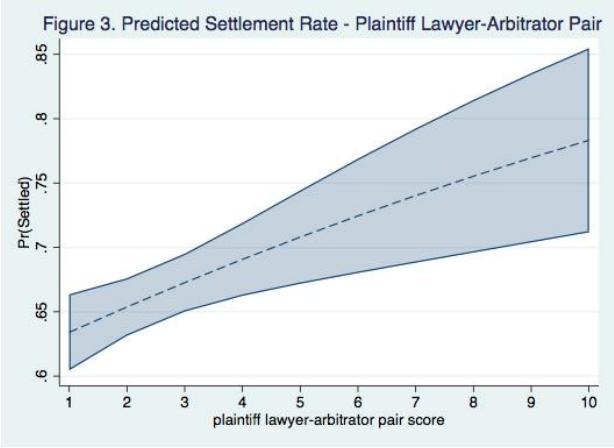
Covariates	Coefficient (Odds Ratio)	Robust Std. Err.	95% Confidence Interval
Repeat Business Score	-0.00154 (0.99846)***	0.00040	-0.00231; -0.00076
Repeat Business-Arbitrator Pair Score	0.01147 (1.01154)	0.02016	-0.02805; 0.05101
Repeat Plaintiff Lawyer Score	0.00110 (1.00110)	0.00338	-0.00552; 0.00772
Repeat Plaintiff Lawyer-Arbitrator Pair Score	0.11823 (1.12550)**	0.04117	0.03753; 0.19892
Self-Represented Employee	-1.62912 (0.19610)***	0.16993	-1.96218; -1.29606
Former-Judge Arbitrator	-0.12590 (0.88170)†	0.07103	-0.26512; 0.01332

Robust standard errors clustered around the business defendant
Regression includes fixed effects for filing year
†P<.10, *P<.05, **P<.01, ***P<.001

Whereas other recent arbitration studies that dealt with earlier sets of cases did not find significant participation frequency effects for settlement rates (Colvin and Gough 2015), I find that the repeat-business score—the number of times the business has participated in AAA arbitration—has a significant ($p<.001$) negative impact on the odds that the case is settled pre-adjudication after controlling for confounders. Likewise, I find that the repeat plaintiff lawyer-arbitrator pair score—the number of times that the plaintiff lawyer and the arbitrator have participated together in AAA arbitration—also has a significant ($p<.01$) effect, but this time in a positive direction. Interestingly, I find no significance for either the repeat business-arbitrator score or the repeat-plaintiff lawyer score. To further demonstrate the impact of the continuous variables that exhibited significant associations with settlementment probability, Figure 2 visualizes the marginal effects of the repeat business score, holding all other covariates constant

at their population means. Figure 3 does the same, but for the repeat plaintiff lawyer-arbitrator pair score.





In addition to these results, I also find that self-represented employees, even when controlling for the effects of other covariates, were significantly less likely to settle. This self-representation effect—as it was in most of my regressions—was the largest in magnitude for any variable exhibiting significance. Indeed, self-represented employees, after controlling for confounders, were over 80 percent less likely to reach a settlement than their professionally represented counterparts.

Table 3 lists the results of the logit model I estimate for case dismissal rates. As we would expect to see, going off the reasoning that repeat employers will achieve better outcomes for themselves, a higher repeat business score was significantly ($p<.001$) correlated with a higher probability of an employee's case against them being summarily dismissed. Interestingly, while the repeat business-arbitrator pair score also exhibited significance ($p<.05$), the effect was in the opposite direction: the higher the score, the lower the probability of an employee's case being dismissed when controlling for confounders. For both the repeat-business score, and the repeat business-arbitrator pair score, I display the marginal effects by charting predicted dismissal

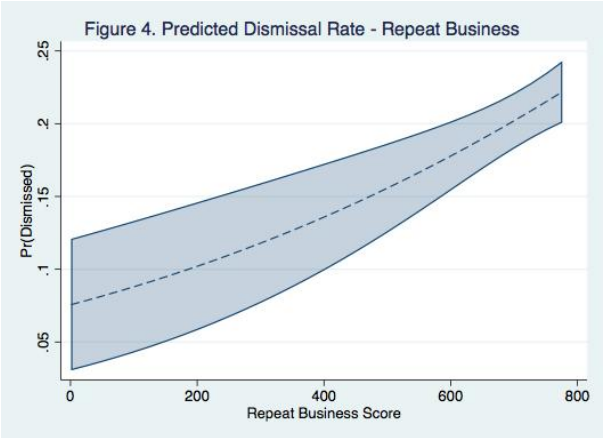
Table 3.

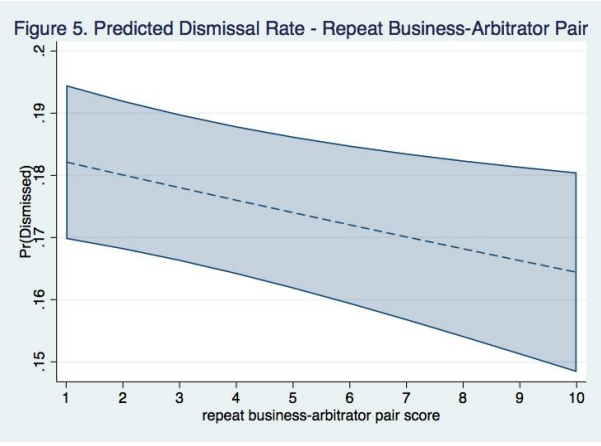
Arbitrator Demographic & Participation Frequency Effects on Employee Dismissal Rate *(Logit Model)* N = 13,245

Covariates	Coefficient (Odds Ratio)	Robust Std. Err.	95% Confidence Interval
Repeat Business Score	0.00174		
	(1.00175)***	0.00031	0.00114; 0.00235
Repeat Business-Arbitrator Pair Score	-0.02948		
	(0.97095)*	0.01388	-0.05668; -0.00228
Repeat Plaintiff Lawyer Score	0.00293		
	(1.00294)	0.00305	-0.00304; 0.00891
Repeat Plaintiff Lawyer-Arbitrator Pair Score	-0.10573		
	(0.89967)†	0.06106	-0.22540; 0.01395
Self-Represented Employee	0.79779		
	(2.22063)*	0.38304	0.04705; 1.54853
Former-Judge Arbitrator	0.30140		
	(1.35175)**	0.09691	0.11147; 0.49133

Robust standard errors clustered around the business defendant
Regression includes fixed effects for filing year
†P<.10, *P<.05, **P<.01, ***P<.001

probability for a given level of the variable with other covariates held constant at their population means in Figures 4 and Figure 5. Other findings from the logit model of dismissal rates include a significant (p<.05), and positive, self-representation effect—as in, the inclusion of a self-represented employee predicts a higher dismissal rate—and, interestingly, a significant (p<.01), and positive, effect for the presence of a former-judge arbitrator.





The next two regression models deal with the set of cases that make it to the final adjudicatory stage. Table 4 gives the results of the logit model of employee win probability while

Table 5 gives the results of an OLS model of expected award amounts. In the case of the former, I find a significant negative relationship for the repeat-business score, and, surprisingly, a marginally significant positive relationship for the repeat business-arbitrator pair score after controlling for confounders. Turning to the plaintiff lawyer frequency measures, I find no significant correlation between win rate and either the repeat plaintiff lawyer score or the repeat plaintiff lawyer-arbitrator pair score. Beyond the participant frequencies, the only other variable

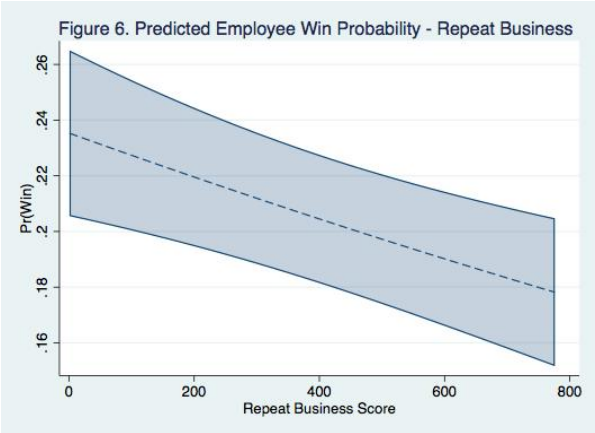
Table 4.

Arbitrator Demographic & Participation Frequency Effects on Employee Win Rate *(Logit Model)* N = 1,291

Covariates	Coefficient (Odds Ratio)	Robust Std. Err.	95% Confidence Interval
Repeat Business Score	-0.00044 (0.99956)**	0.00013	0.99929; 0.99982
Repeat Business-Arbitrator Pair Score	0.05070 (1.05201)†	0.03043	0.99402; 1.11337
Repeat Plaintiff Lawyer Score	0.00099 (1.00099)	0 .00201	0.99707; 1.00493
Repeat Plaintiff Lawyer-Arbitrator Pair Score	-0.00225 (0.99775)	0.03611	0.92943; 1.07110
Self-Represented Employee	-1.53128 (0.21626)***	0.05381	0.13279; 0.35220
Male Arbitrator	-0.09882 (0.90591)	0.13805	0.69679; 1.35481
Former-Judge Arbitrator	-0.02880 (0.97160)	0.16481	0.67200; 1.22123

Robust standard errors clustered around the business defendant
Regression includes fixed effects for filing year
†P<.10, *P<.05, **P<.01, ***P<.001

that exhibited significance in the model was self-represented employee. As with the other regressions, this was the largest in magnitude and significantly ($p<.001$) negative, suggesting again that self-represented employees face a significant disadvantage in the arbitral system. In Figures 6 and 7, I present the marginal effects that the repeat-business score and the repeat business-arbitrator pair score have on employee win probability by charting the relationship, holding all other covariates constant at their population means. Figure 6 displays this for repeat business score while figure 7 does the same for the repeat business-arbitrator pair score.



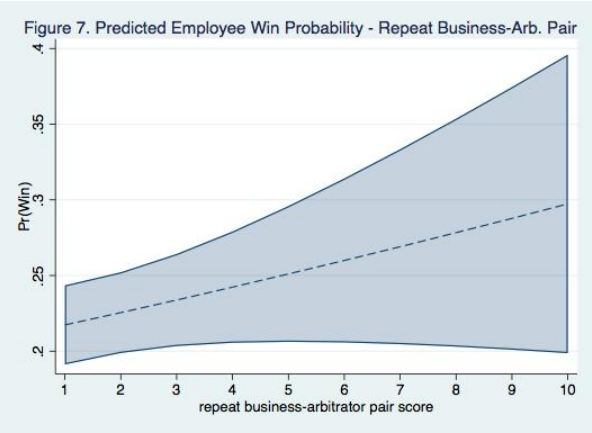


Table 5 displays the results from the OLS model I estimate for expected employee award amount. For the same reasons I discuss in the bivariate analysis section, I find very few significant relationships with my independent variables here. In fact, the only variable that displays any significance at all is 'self-represented employee' which is significantly ($p<.05$), and negatively correlated with the size of the award an employee can expect to receive. Given how few cases there are in the casefile where an employee receives a non-zero award amount, this probably says more about how poorly self-represented employees fare, than it does about the effects of other covariates.

Table 5.
Arbitrator Demographic, and Participation Frequency Effects on Employee Award Amounts in $2020 *(OLS)* N = 1,291

Covariates	Coefficient	Robust Std. Err.	95% Confidence Interval
Repeat Business Score	-$50.31	$46.14	-140.90; 40.27
Repeat Business-Arbitrator Pair Score	-$1,777.37	$1,255.61	-4242.27; 687.53
Repeat Plaintiff Lawyer Score	$1,093.60	$1,453.49	-1759.77; 3946.98
Repeat Plaintiff Lawyer-Arbitrator Pair Score	-$8,568.55	$7,429.93	-23154.35; 6017.25
Self-Represented Employee	-$53549.42*	$24,366.33	-101383.3; -5715.51
Male Arbitrator	-$21,396.59	$36,658.87	-93362.18; 50569.00
Former-Judge Arbitrator	$81,841.44	$65,908.13	-47543.81; 211226.70

Robust standard errors clustered around the business defendant
Regression includes fixed effects for filing year
†P<.10, *P<.05, **P<.01, ***P<.001

To summarize my findings: My study offers strong evidence of a repeat-business effect in employment arbitration across most aspects of case outcome, even after controlling for confounders. However, it provides no evidence of a similar repeat business-arbitrator pair effect, and only limited evidence of an effect derived from the experience of the plaintiff lawyer. In addition, the study also demonstrates clearly that the cases at the largest disadvantage across all measures of outcomes are those brought by self-represented employees. And, finally, while arbitrators who were formerly judges display no systematic differences between themselves and their non-judge counterparts in adjudication behavior, they are, in fact, significantly more likely to summarily dismiss the cases they oversee. In the next section I discuss the ramifications of my findings in detail.

Discussion

My study contributes to the arbitration literature both by uncovering how employees have fared in the wake of the Supreme Court's *Concepcion* ruling, and by examining a new metric of employee outcome, one that had been neglected by prior studies—the rate of summary case dismissal. The results I find suggest that for cases that make it to the final adjudicatory phase, where the arbitrator determines a winner and any compensatory damages, the overall results that employees achieve have not changed significantly in the wake of the ruling (Colvin and Gough 2015; Horton and Chandrasekher 2016). Indeed, they are still inferior to estimates of the results employee's achieve in similar litigation (Delikat and Kleiner 2003; Clermont and Schwab 2004). Unlike prior studies, however, I find no evidence that these results are the product of an anti-employee bias that is uniquely associated with arbitration. After controlling for confounders, I find that the repeat-business score—the number of times the business has participated in

arbitration—is significantly and negatively correlated with employee win rate. But I find no comparable dynamic for the repeat business-arbitrator pair score, which measures the number of times that the business has worked with the arbitrator deciding the case in question. These results suggest that, while arbitration suffers from the same bias towards larger, more-sophisticated participants that has also been documented in employment litigation—the repeat player effect—it does not, at least in the post-*Concepcion* period, suffer from additional anti-employee bias arising out of the unique ability for parties in arbitration to influence who hears the case.

In addition, the results of my study also suggest, that the use of more experienced, more sophisticated plaintiff lawyers, is not an efficacious way to reduce anti-employee bias in arbitration, at least in the final adjudicatory phase. After controlling for confounders, I find that neither the repeat plaintiff-lawyer score—which measures the number of arbitration cases in which the plaintiff lawyer has been involved—nor the repeat plaintiff lawyer-arbitrator pair score—which measures the number of times the lawyer in question has worked with the arbitrator hearing the case—has a significant effect on either the employee win rate or the employee expected award amount. In other words, if one seriously wanted to improve employee outcomes in either litigation or arbitration, more fundamental changes would need to be made than simply furnishing employees with better representation.

This conclusion, however, does not hold true for the set of employee plaintiffs who represent themselves. Like prior studies of earlier periods of employment arbitration (*See e.g.* Colvin and Gough 2015, Horton and Chandrasekher 2016), I find that the presence of a self-represented employee is one of the strongest predictors of inferior case outcome. After controlling for confounders, the results of my study suggest that self-represented employees are almost 80 percent less likely to win their case, and have an expected award amount that is over

$50,000 dollars lower than their professionally-represented counterparts. As previous studies have noted, this dynamic may not be entirely causal (Colvin and Gough 2015). It may be the case, that self-represented employees end up representing themselves precisely because they are bringing the weakest cases. That is to say, potential plaintiff lawyers refuse to take these cases as they fear the probability of success is too low to make it worth their while. However, this is not the only plausible interpretation. Both opponents and proponents of mandatory employment arbitration recognize that the arbitral forum is significantly more informal than that of litigation (*See e.g.* Eisenberg and Hill 2003; Colvin 2011). And this reality may, in all likelihood, encourage some number of employees to forgo hiring professional representation that could have helped them achieve a more favorable outcome had their decision been different. While my study was not designed to conclusively determine the relative merits of these dueling explanations, I suspect that the truth is probably somewhere in the middle. Cases brought by self-represented employees are probably somewhat weaker on average than those brought with professional representation. But to the extent that arbitration encourages more plaintiffs to *choose* to represent themselves, it does lead to measurably worse outcomes for employees than comparable litigation.

Moving beyond the results employees' achieve when their case reaches final adjudication, my study also finds that employees are less likely to reach a settlement pre-adjudication, the higher the repeat-business score of the employer they are bringing a case against. This finding is important for two reasons: To start, cases that end in settlements, simply put, comprise the vast majority of cases in which an employee actually receives remuneration. If repeat employers—who as a group represent the vast majority of all employers participating in arbitration—are significantly less likely to settle, this means that the settlement dynamic has a

significant negative impact on overall employee success. Beyond this however, my finding of differential settlement rates for repeat-employers also calls into question one of the prevailing theories for why the repeat-player effect may not actually be detrimental to employees. This theory suggests that the repeat-player effect, in both arbitration and litigation, is largely the result of more-experienced employers better understanding which cases would be advantageous to settle pre-adjudication. It implies that any repeat-player effect found in the portion of cases that make it to adjudication is largely the result of a selection effect that leads to stronger cases being removed from the pool through mutually-agreed upon settlements. The findings of my study, however, cast serious doubt on the plausibility of this theory: Far from finding that repeat-employers are more likely to settle, as one would expect to see under this paradigm, I find that they are, in fact, *less* likely to settle. This suggests that the repeat-player effect in adjudication is truly a product of a system that advantages experienced employers rather than the result of some other confounding factor. It does not, however, suggest the repeat-player effect is any worse in arbitration than it is in litigation.

Results from my multivariate analysis of settlement behavior also yield evidence of a significant positive association between the plaintiff lawyer's level of familiarity with the arbitrator—as measured by the repeat plaintiff lawyer-arbitrator pair score—and the probability of a case settling pretrial. This finding suggests that more experienced plaintiff lawyers may have some positive impact on employee outcomes, at least in the pre-adjudication phase. They may better know, whether the arbitrator in question will view the case their client is bringing favorably, and therefore can better advise their client on whether it would be advantageous to settle. However, this conclusion is called into question by the fact that I find no corresponding repeat plaintiff lawyer-arbitrator pair advantage when analyzing employee win rates for the cases

that do make it to final adjudication. If more experienced plaintiffs lawyers were truly better advising their clients on settlements, than we might expect cases of theirs that do make it to adjudication to fare better than those of less experienced plaintiff lawyers. As I do not find such a dynamic, I am inclined to believe that the repeat plaintiff lawyer-arbitrator pair effect on settlement rates may be more noise than signal, or at least due to some other as yet unidentified confounding factor.

As with win rates, my study suggests that the subgroup of cases that fare worst in terms of achieving a pre-adjudication settlement, are those that involve a self-represented employee. The disadvantage self-represented employees face in achieving a mutually agreed upon settlement is almost exactly the same in magnitude as the disadvantage they face in winning their cases when they do move to final adjudication: Compared with cases involving professionally-represented employees, they are almost 80 percent less likely to reach a pre-adjudication settlement. This seems to suggest that self-representation is significantly harmful to an employee's prospect of achieving a favorable outcome in arbitration. The finding, like those for win rates, however, must be caveated with the acknowledgment that self-representation comes with significant confounders, and may not be entirely causal in nature. Businesses may be significantly less likely to agree to pre-adjudication settlements with self-represented employees, as their cases may be systematically weaker than those brought by their professionally-represented counterparts. As with the self-representation effect on win rates, my study was not designed to conclusively determine which interpretation—causal or non-causal—is most accurate. However, I suspect that, again, the truth is probably somewhere in the middle: the relative weakness of self-represented cases may discourage businesses, to a certain extent, from agreeing to settlements. But self-represented employees, most likely also

face an additional disadvantage *because* they are self-represented. It seems likely that an employee inexperienced in the art of legal negotiation would be less successful in reaching a settlement agreement with their employer, regardless of the relative merits of their case.

In addition to examining settlement rates, my study moves beyond prior arbitration research by examining also the rate at which cases are summarily dismissed. Recall that a summary dismissal is when the arbitrator throws out a case pre-adjudication for being overly-frivolous or otherwise unworthy of a full hearing. My study finds that these dismissals are becoming increasingly common in mandatory employment arbitration. Whereas earlier studies focusing primarily on the pre-*Concepcion* period found that around .2 percent of cases were disposed of in this way (Horton and Chandrasekher 2016), my study of the post-*Concepcion* period finds that that number has risen to 18 percent of all mandatory employment arbitrations. This rise would be notable in and of itself, but the trend is rendered even more striking by the fact that the sharp increase in dismissals is not just cutting into the number of cases being disposed of through other administrative channels like when a case is withdrawn. Instead, as dismissals have risen exponentially between the time period of my study and those of the most recent pre-*Concepcion* studies, the number of cases making it to final adjudication has been cut in half. As dismissed cases are inherently ones in which the employee receives no remuneration, their rise has had a significant impact on employee-performance in the arbitral forum overall.

While it could be that employees have simply filed a larger number of frivolous cases during the time period under study, and thus the heavy increase in dismissals is justified, the results of my analysis call this more benign hypothesis into question. Under such a paradigm, we would expect a case's probability of dismissal to be unaffected by factors like the repeat-business score of the employer defendant; There is no reason *a priori* to believe that cases filed against

repeat-playing businesses are more likely to be frivolous than those filed against one-shot businesses. Yet my study finds the opposite: A higher repeat-business score is, in fact, significantly associated with a higher probability of case dismissal. The rapid rise in dismissals, then, may not represent increased litigiousness on the part of employees, but rather a new way in which repeat-playing businesses are managing to tilt the odds of arbitration in their favor. Adding to the concern, neither the repeat plaintiff-lawyer score nor the repeat plaintiff lawyer-arbitrator pair score displays a similar effect on a case's probability of dismissal. This is important because if we saw that these two factors measuring plaintiff-lawyer experience had some countervailing impact, we might be less concerned as to the overall effect on employees' access to justice. As this is not the case, however, the rise in case dismissals seems simply to represent an unmitigated decline in the quality of employee outcomes post-*Concepcion*.

It is important to acknowledge here that my study finds no evidence these case-dismissal dynamics are necessarily unique to arbitration. If this were true, we might expect the repeat-business arbitrator pair score—which attempts to assess the unique advantage accrued by repeat-players in arbitration through their ability to influence who hears the case—to have a positive association with case dismissals as well. However, far from finding such an effect, my study actually identifies a significant negative association between the repeat business-arbitrator pair score and the case-dismissal rate after controlling for confounders. That is to say, the more times the business has worked with the arbitrator in question, the *less* likely it is for the case to be dismissed, all else equal. While my study does not seek to determine the rate at which employees cases are summarily dismissed in similar litigation—and therefore cannot say anything conclusive on the matter—these findings offer some suggestion that repeat players may

be advantaged in securing summary dismissals across the broader civil-justice system, rather than in arbitration alone.

Further evidence suggesting this can be found in the fact that arbitrators who were formerly judges—an increasingly common occurrence as more judges use arbitration as a part-time retirement job (Colvin and Gough 2015)—are significantly more likely to summarily dismiss an employee's case than their non-former judge counterparts. If a repeat-player advantage in securing dismissals was truly a singular aspect of arbitration, then it might be expected that those arbitrators who have had experience overseeing similar litigation would be *less* likely to summarily dismiss a case, or at least to exhibit no differences in dismissal behavior. Of course, my study finds the opposite, further suggesting that the repeat-player advantage for dismissals may not solely be a product of arbitration alone.

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Workplace dispute-resolution research is often dry, focusing on an arcane area of our legal framework that few worry will come to impact them personally. Who begins a job considering how their employer will exploit them: whether their wages will at some point be unfairly garnished, or whether they will be terminated capriciously or in service of discriminatory ends? Yet, this is exactly what makes the work so important. Employees have little incentive to turn down a job over the process by which a hypothetical dispute will be handled years down the road, yet collectively that process can significantly impact the overall level of protection employees receive. Understanding how these systems of workplace justice operate is a necessary first step in ensuring that employees in the United States maintain substantive access to justice even as the venue in which their claims are heard has shifted away from litigation and towards privatized arbitration.

On the whole, the results of this study suggest that employment arbitration is not irredeemable: In the post-*Concepcion* period, it does not appear that arbitration is uniquely vulnerable to anti-employee bias. But this does not mean employment arbitration as an institution is absent of shortcomings. This study also shows that arbitrators are increasingly dismissing cases prior to adjudication and that employees who choose to represent themselves—which arbitration may well encourage—are at a significant disadvantage. In the following section, I explicate these issues and provide policy recommendations for rectifying them going forward.

Policy Recommendations

While some opponents of mandatory arbitration have argued that the Federal Arbitration Act (FAA)—the federal statute that governs arbitration in the United States—ought to be amended to explicitly ban the use of mandatory arbitration clauses in non-union contracts of employment (Stone and Colvin 2015), the results of my study, and those of prior arbitration analyses, lead me to believe this is neither warranted nor necessary. Indeed, while not the focus of my own work, many arbitration scholars have noted a number of advantages arbitration offers employees over traditional litigation, namely much shorter trial lengths and significantly lower legal fees (*See e.g.* Hill 2003; Sherwyn *et al* 2005). On balance, these facets of the arbitral forum mitigate some of its drawbacks like the lower average award amounts and employee win rates relative to litigation, and complicate the calculus of assessing its relative merits. Furthermore, the results of my study suggest that the bias the arbitral system possesses towards wealthier, more sophisticated parties who participate more often, may not be unique to arbitration alone, but instead a quality of the broader civil justice system. Given this, it does not seem likely that simply switching back to an employment dispute resolution system where litigation is the norm would materially improve employee outcomes.

My study does, however, identify a number of concerning trends in arbitration that have emerged after, or continued into, the post-*Concepcion* period that ought to be addressed. Summary dismissals are increasingly being used to dispose of employee's cases in ways that do not inspire confidence that they exclude only the most frivolous, least meritorious cases from adjudication as they are intended to do so. And self-represented employees continue to face significant disadvantages when compared to their professionally-represented counterparts. Neither of these problems require the wholesale banning of mandatory employment arbitration to solve, and in fact both may be more effectively tackled through arbitration-specific regulation.

Unjustified Case Dismissals

The results of my study show that the rate of summary case dismissal in mandatory employment arbitration has risen exponentially in the years following *Concepcion*, that this rise in dismissals has largely come at the expense of cases making it to adjudication, and that dismissals are significantly impacted by the same forces that potentially bias the outcomes of final rulings. Taken together, these trends suggest that summary dismissals have significantly curtailed employee's access to justice over the last decade, and that their rise cannot simply be attributed to an increase in frivolous case filings alone. A large driver of the problem may be that the FAA, the federal statute governing arbitration in the United States, does not explicitly outline what responsibility and/or authority arbitrators have when ruling on dispositive motions. As a consequence, arbitration providers have largely been left to decide these questions for themselves, and many have offered only cursory or vague guidance which clearly leaves room for capricious rulings. Take for instance, the relevant clause in the AAA's "Rules of Employment Procedures": "The arbitrator may allow the filing of a dispositive motion if the arbitrator determines that the moving party has shown substantial cause that the motion is likely to succeed

and dispose of or narrow the issues in the case (AAA "Employment Procedures" 2009)." The AAA's procedural code is considered to be the most comprehensive of any arbitration provider in the U.S. (Estlund 2018), and yet, the section pertaining to this matter offers only a single sentence of guidance. Furthermore, the clause contends only with the question of when filing a motion for dismissal should be allowed, but says nothing of what considerations should guide the arbitrator's ultimate judgment should the motion be heard. This means that arbitrators are *de facto* left to make these determinations on a case by case basis—a situation which is clearly inconsistent with evenhanded justice.

To rectify this problem, Congress should amend the FAA so that it delineates clearly what standards should be used by arbitrators to decide whether a summary dismissal is in order. It should make clear that such motions should only be granted in exceptional cases when the accusations of the plaintiff lack almost any merit. And it should legally require that arbitrators operate with the base-line assumption that all available evidence should be considered, regardless of the arbitrator's initial reaction to the case in question. The informality of the arbitral process may offer a number of benefits over litigation, but for such informality to avoid corrupting the integrity of the system, it needs to be restrained by a strong partiality towards letting cases run their course before a determination is made.

Self-Representation

In all aspects of case outcome that I examine—settlement rate, dismissal rate, win rate, and expected award amount—the subgroup of cases that fares the worst are those that include a self-represented employee. Part of this dynamic, may be attributable to self-represented employees bringing systematically inferior cases, and therefore having a difficult time attracting legal counsel. But to the extent that employees are *choosing* to represent themselves, which the

informality of arbitration may well encourage, employees face a significant disadvantage in arbitration relative to comparable litigation.

Currently, no federal statute, requires arbitration providers to assist claimants in securing representation, or even to advise them that they would materially benefit from seeking council. Some providers including the AAA do, at least as official policy, recommend to employee plaintiffs that they find professional representation, offering to refer them to local Bar Association for further assistance (AAA "Representing Yourself"). However, there is no way of knowing to what extent these policies are followed nor how effectively they are being implemented. And given that one of the primary selling-points of arbitration is the speed at which trials are conducted, even firms that discourage self-representation have an economic incentive not to dwell on the issue for long should it hinder process efficiency.

Given this, and the significant disadvantage that self-represented employees face in the arbitral system, Congress should further amend the FAA, creating a legally binding responsibility for arbitration providers to verbally inform plaintiffs prior to adjudication of the risks they are taking by proceeding without counsel. This requirement could function so if not followed, the arbitral decision is voided and the plaintiff is free to bring the case to court to pursue civil litigation. In addition, the amendment should also include a requirement for arbitration providers, to make plaintiffs aware of all the options they have available to pursue legal counsel. This should go beyond simply giving the plaintiff the number of the State Bar Association, and should include actively assisting plaintiffs in reaching out to nonprofits and local law firms should they request it. While providers like the AAA claim that assistance beyond simply giving the number of the local Bar Association would be inappropriate given their status as a neutral in deciding the case (AAA "Representing Yourself"), it seems far more likely that *effective* neutrality would be

compromised by having the party who is already most likely to be at a significant resource and informational disadvantage, precede self-represented when there are other options available. Employees obviously have the right to argue their cases themselves as they do in a court of law, and no person necessarily has a right to counsel in bringing civil complaints. But given the ways that the informality of arbitration may encourage self-representation, which is demonstrably harmful to employee outcomes, policy must be put in place to discourage plaintiffs whose cases possess sufficient merit to attract counsel from attempting to argue their cases themselves.

Conclusion

The ascension of mandatory arbitration as the primary means of non-union employment dispute resolution in the Untied States constitutes a sea change in America's civil justice system. Whereas before the 1990s the vast majority of employment disputes were adjudicated through litigation, the majority are now decided through arbitration. The scope of this change has given rise to acrimonious public debate over whether arbitration is a fair and beneficial replacement, or whether it simply represents a means for corporations to limit employees' access to justice, and vindication of their statutory rights. Furthermore, concerns have been raised that the Supreme Court's *Concepcion* (2011) decision, has made the system even more hostile to employee claimants by rendering enforceable class-action bans used in conjunction with mandatory arbitration clauses in contracts of employment.

This empirical analysis of mandatory employment arbitration, however, demonstrates that, in the post-*Concepcion* period, the situation is more complicated than both proponents and opponents of employment arbitration acknowledge. Specifically, it shows that arbitration in the current period likely suffers from a systemic bias towards businesses who participate in the arbitral system most often. But it also show that this bias likely does not rise beyond the level of

what could be found in similar litigation. Nevertheless, the analysis does, in fact, reveal a concerning rise in summary dismissals during the time period under study. That is, that employee's are much more likely to have their cases disposed of pre-adjudication than they were in prior periods. This is concerning because this rise seems largely to come at the expense of cases making it to final adjudication, and these dismissals appear to be similarly vulnerable to the biases that also impact final rulings. Arbitration may offer a number of benefits in the context of employment dispute resolution, but if large numbers of employee claims are being tossed out before evidence is even presented, the system may be failing to live up to its responsibility of providing a just alternative to litigation.

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