Empirically Investigating the Source of the Repeat Player Effect in Consumer Arbitration

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Abstract: Policymakers, courts, and scholars have long been interested in whether repeat players enjoy an advantage in forced arbitration. Sophisticated empirical studies of consumer and employment awards reveal that there is indeed a repeat player effect: even controlling for other factors, companies that arbitrate more than once boast higher win rates than one-shot firms. However, researchers have not yet tried to determine whether this repeat player effect is a product of experience within the arbitral forum (the "experience" hypothesis) or characteristics of the repeat playing companies themselves (the "defendant-specific" hypothesis). This Article begins to address this issue by analyzing 4,570 consumer arbitration awards from the American Arbitration Association. Using a unique regression specification that includes *both* discrete and continuous random variables (a combination of functional forms not used in previous literature), it finds that the repeat player effect is more consistent with the defendant-specific hypothesis than it is with the experience hypothesis. Indeed, to the extent that repeat-playing businesses enjoy an advantage in arbitration, it likely emanates from company-specific characteristics.

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INTRODUCTION

The role of repeat players in forced arbitration has long been controversial. In the 1980s, the United States Supreme Court began to interpret the Federal Arbitration Act (FAA) broadly, and arbitration clauses became common in consumer and employment contracts. Critics argued that this trend favors repeat-playing companies. Citing Marc Galanter's classic article, *Why the "Haves" Come Out Ahead: Speculations on the Limits of Legal Change*, several influential judicial decisions and law review articles speculated that private dispute resolution was especially hospitable to serially-arbitrating businesses (*Armendariz v. Found. Health Psychcare Servs., Inc.*, 6 P.3d 669, 687 (2000); Schwartz 1997; Sternlight 1997).

However, empirical research into arbitration's repeat player effect is still embryonic. To be sure, several studies have demonstrated that repeat-playing companies outperform their one-shot counterparts (Bingham 1997; Searle 2009; Colvin 2011; Colvin & Gough 2015; Horton & Chandrasekher 2015; Horton & Chandrasekher 2016; Chandrasekher & Horton 2019). Yet, there is no general agreement amongst scholars about the best functional form to use when modelling the repeat player effect and what different functional forms might mean in terms of understanding the repeat player effect. While some scholars have used discrete random variables (such as a single dummy variable or a series of dummies variables) to measure the repeat player effect, others have employed continuous random variables (such as linear counts of the total number of business

appearances). No article has ever stepped back to suggest alternative functional forms nor to identify which of the existing functional forms is most appropriate.

Moreover, the root of the repeat player effect remains unclear. Repeat players might thrive because of their experience within the arbitral forum. For instance, because arbitration is private and non-precedential, frequently-arbitrating defendants might be able to access information that others cannot. We will call this the "experience" hypothesis, since it assumes that repeat-players fare well because of the experience that they gain through arbitrating multiple times. Alternatively, the repeat-player effect could also have nothing to do with experiential learning. Instead, repeat players might win simply because of who they are. Arguably, if the firms that arbitrate often also happen to be powerful, deep-pocketed, and represented by elite counsel, they may win more in *any* forum (arbitration, litigation, small claims court, agency adjudication, etc.) We refer to this as the "defendant-specific" hypothesis. To date, scholars have not attempted to test these rival hypotheses.

This Article takes several steps toward filling these gaps. It uses an originally collected dataset of 14,691 consumer arbitration matters from the American Arbitration Association (AAA) to take a deep dive into arbitration's repeat player effect.[1] Using a simple and straightforward combination of discrete and continuous random variables, we provide evidence that, at least in the consumer arbitration context, the repeat player effect advantage is driven less by a company's arbitration experience and more by a company's identity. We show that once one controls for company size and other time-invariant attributes, defendants gain little from exposure to the arbitral forum. Indeed, the advantage that comes with arbitrating each additional matter is either minimal, or, in some specifications, statistically insignificant.

[1] Our full dataset includes 14,691 matters, of which 4,570 are consumer arbitration awards. In some of the analysis that follows, we rely on the full dataset of 14,691 cases. In our regression analysis, we use the awarded data set.

The article is organized as follows. Section I presents the existing empirical literature on the repeat player effect, with special attention to the particular functional forms that have been used to model the repeat player effect. Section II introduces the dataset that was used to conduct our analysis. Section III presents our main empirical results on the repeat player effect. Section IV contains our discussion. Tables and figures follow the discussion and the conclusion.

I. LITERATURE REVIEW

A. Forced Arbitration and Repeat Players

In 1974, Marc Galanter published *Why the "Haves" Come Out Ahead: Speculations on the Limits of Legal Change*. Galanter's article is justly celebrated for its typology of parties within the legal system. On the one hand, Galanter observed, there are "repeat players," who "have many occasions to utilize the courts." On the other hand, Galanter explained, there are "one shotters," who are only embroiled in litigation a single time.[2] Galanter then argued that repeat players enjoyed systematic advantages over one shotters because they have better access to information, "develop expertise and have ready access to specialists," and "have opportunities to develop . . . relations with institutional incumbents" (Galanter, p. 98).

A decade after Galanter's piece appeared in print, a sea change in the civil justice system sparked interest in his dichotomy between repeat players and one shotters. In the middle of the 1980s, the Supreme Court dramatically expanded the scope of the Federal Arbitration Act (FAA). Companies from all sectors of the economy began to place forced arbitration clauses in their consumer and employment contracts, provoking sustained debate. Several influential critiques of this trend speculated that private dispute resolution exacerbated the asymmetries that Galanter had identified. These commentators claimed that arbitration favored repeat players for three main

[2] As Galanter put it, "the insurance company, the prosecutor, the finance company are [repeat players]," while "[t]he spouse in a divorce case, the autoinjury claimant, [and] the criminal accused are [one shotters]." Page 97.

reasons. First, arbitration is private. Unlike litigation, it does not generate precedent. Thus, repeat players can accumulate information about particular decision-makers and strategies. Second, arbitration is idiosyncratic. Indeed, the normal rules of procedure and evidence do not apply. Arguably, then, it affords repeat players multiple opportunities to hone their skills in this unique universe. Third, arbitrators bill by the hour and are hand-picked by the parties. Accordingly, as David Schwartz observed, "arbitrators have an economic stake in being selected again, and their judgment may well be shaded by a desire to build a 'track record' of decisions that corporate repeat-users will view approvingly" (Schwartz, p. 60-61).

These theories about the repeat player advantage share a common thread. Each one suggests that repeat players flourish within the private forum *because of their experience within it*. Indeed, it is only by arbitrating multiple times that a defendant can stockpile information about case outcomes and individual arbitrators' predilections. Likewise, mastery of arbitration's norms can only be cultivated by arbitrating. Finally, if arbitrators do skew their rulings in the hope of generating future business, they are likely to do so with respect to companies that they know arbitrate frequently. For these reasons, we will call these contentions the "experience" hypothesis of arbitration's repeat player effect.

In the 1990s and 2000s, academics began to use arbitration awards to examine whether arbitration is, in fact, slanted towards repeat players. Lisa Bingham broke ground here with a series of articles that, relying on a sample of AAA employment arbitrations and simple t-tests, provided suggestive evidence that employees won far less often against repeat players than they did against one shotters. Several empirical articles using more sophisticated econometric methods have

followed in Bingham's path, all validating the repeat player effect but with a higher level of persuasiveness.[3]

Yet these studies only go so far. As arbitration's proponents have pointed out for decades, simply showing that repeat players perform well in arbitration does not prove that the process is unfair. Indeed, as Steve Ware argues, it is entirely possible that repeat litigants also excel in the *judicial system*. Thus, the repeat player effect might not be specific to private dispute resolution:

> [L]itigation may have a "repeat-player effect" that equals or even exceeds arbitration's. That is, businesses who litigate often may do better in litigation than businesses who litigate rarely, and the gap between repeat players and "one-shotters" in litigation may be wider than the analogous gap in arbitration. Consequently, evidence of a "repeat-player effect" in arbitration has little relevance without evidence showing whether litigation has a comparable repeat-player effect (Ware, p. 68).

Likewise, no research to date has "control[led] for some rather obvious variables that may explain positive [defendant] outcomes, such as the size of the [company]" (Estreicher, p. 566). Rejoinders like these point towards an alternative explanation for the repeat player effect. Contrary to the experience theory, which assumes that repeat players dominate because of qualities that are inherent in *arbitration*, perhaps repeat players thrive simply because of *who they are*. That is, repeat players may tend to be large companies with deep pockets, sophisticated in-house counsel, and access to elite law firms. We will call this the "defendant-specific" account of the repeat player effect.

B. Econometric Studies of Arbitration Awards

Recently, scholars have brought increasingly sophisticated tools to bear on the repeat player issue. These articles use multiple regression analysis and can be divided into two camps: those that measure the repeat player effect using discrete random variables and those that measure the repeat

[3] See the following section for a review of the empirical arbitration literature.

player effect using continuous random variables. Unfortunately, however, these articles shed no light on the issue of whether the "experience" or "defendant-specific" theory is correct.

i. Modelling Repeat Playing with Discrete Random Variables

In 2011, Alexander J.S. Colvin christened the era of econometric analysis of the repeat player effect by examining 1,213 AAA employment awards. He ran a logit regression of whether or not an employee wins on a simple dichotomous repeat player variable and a series of other control variables. His repeat player variable was equal to 1 if the employee was facing an employer who showed up in the dataset more than one time and equal to 0 if the employee was facing an employer who was only involved in one case.[4] He also included a dummy variable for whether or not the employee was facing a repeat business-arbitrator pair. Colvin found that facing a repeat-playing employer reduced the odds of an employee victory by about 49% ($p < 0.01$). Likewise, the fact that a matter involved a repeat pairing lowered the odds of an employee win by 40% ($p < 0.05$).

Following in Colvin's footsteps, we addressed the repeat player effect in three articles. First, in 2015, we examined 4,839 matters filed by consumers in the AAA. Second, in 2016, we studied 5,883 AAA employment disputes. Third, in 2019, we examined 40,680 cases filed in the AAA, JAMS, ADR Services, Inc., and the Kaiser Office of Independent Administration. Our marquee contribution to the repeat player debate was to abandon the binary distinction between repeat players and one shotters. Instead, we sorted companies into tiers based on the total number of times they appeared in a particular arbitration provider's data, or what we call the company's ***repeat player score***. Specifically, we classified businesses as either one-shotters, low-level repeaters, mid-level repeaters, high-level repeaters, or super repeaters. We then replaced Colvin's single dichotomous variable that indicated repeat player or not, with four dichotomous variables that

[4] Colvin's other control variables were whether or not the employee was unrepresented by counsel and year fixed effects.

indicated whether the plaintiff was facing a low-level repeater (or not), a mid-level repeater (or not), a high-level repeater (or not), or a super repeater (or not). (The omitted category was plaintiffs that were facing one-shotter defendants). We found that in a variety of settings—including AAA consumer and employment cases and JAMS and ADR Services tort disputes—the factor that influenced plaintiff win probability the most was facing a defendant that was on the high-end of the repeat playing distribution (either a high-level or super repeat player defendant). Plaintiffs that faced a low- or mid-level repeating defendant fared no worse in arbitration than plaintiffs that faced one-shot defendants.

ii. Modelling Repeat Playing with Continuous Random Variables

In 2015, Colvin and Mark Gough offered a different way of assessing the repeat player effect using a dataset of 2,802 AAA employment awards. Whereas Horton and Chandrasekher (2015) divided repeat players into different tiers based on their repeat player score and then included dummy variables for the different levels of repeat playingness, Colvin and Gough entered the repeat player score linearly into the regression equation. According to the authors, the repeat player score was meant to "measure employer size, sophistication, and familiarity with arbitration proceedings" (Colvin and Gough, p. 1029). Running a logit regression, Colvin and Gough found that the odds of an employee win fell by 0.3% ($p<0.001$) when the employer repeat player score increased by one unit. The main difference between this approach and the Horton/Chandrasekher approach is that the Colvin/Gough approach restricts the repeat player effect to be linear in the score; that is, each additional level of company sophistication is predicted to yield the *same effect* on the employee win probability—regardless of whether the company is on the very high end or very low end of the repeat playing distribution. On the other hand, the Horton/Chandrasekher dummy variable approach allows for heterogeneity in the treatment effect; that is, the repeat player effect is allowed to vary depending on the level of sophistication of the business.

iii. Limitations of Existing Approaches

Unfortunately, neither our discrete random variable approach nor Colvin and Gough's continuous random variable approach can pinpoint the source of the repeat player effect. For one, our methodology of using discrete random variables only reveals that high-level or super repeat playing firms (like AT&T, Citibank, and Discover Bank) outperform their one shot counterparts. It says nothing about *why* this occurs. It is entirely possible that the experience hypothesis is at work, and these firms reap the benefits of superior information about arbitration and from rulings by self-interested arbitrators. Then again, defendant-specific factors could be driving these results. Indeed, these mammoth companies might be better funded or have top-flight counsel.[5] Finally, both effects might be occurring together.

On the flip side, Colvin and Gough's continuous model merely demonstrates that the odds of a plaintiff win decrease by a small margin when the defendant repeat player score increases by one unit. This does not speak to the dueling explanations of the repeat player effect. Because companies that arbitrate routinely also may be economically powerful and well-represented, it is hard to tell whether the incremental decline in plaintiff win probability is due to changes in a company's total arbitration experience (i.e.—their repeat player score) or instead to some inherent characteristic that makes the company generally successful in all of its arbitrations.

Accordingly, in the next Part, we try to take this analysis to the next level with a regression specification that combines discrete and continuous random variables.

[5] It is possible that the elite law firms themselves are serially arbitrating and that could be a source of the repeat player advantage. Because our data set does not have any measures of business law firm, we cannot test this directly.

II. DATA SET DESCRIPTION

California Code of Civil Procedure section 1281.96 requires institutions that administer "consumer" arbitrations to publish information about their caseload over the past five years. Providers must reveal the type of dispute, the claim amount, the defendant, whether the consumer had a lawyer, the prevailing party, the arbitrator's name, and the amount of the award. To be clear, section 1281.96 does not only apply to California cases; instead, it governs all matters. Using these disclosures, we collected 14,691 consumer disputes that were filed with the American Arbitration Association (AAA) between January 1, 2010 and December 31, 2016.

III. RESULTS

A. General Descriptive Statistics

A quick overview of our data will set the stage for the discussion that follows. As Table 1 shows, in 85% of all cases, the consumer was the initiating party.[6] Information on dispute subtype was missing in about half of the cases (47%). However, for those cases with reported information, the most common issues to arise were new home construction (17% overall), financial services (17%), car sales (6%), telecommunications/wireless communication (4%), and debt collection (4%). Most cases either were settled (6,836 or 47%), awarded (4,570 or 31%) or withdrawn (2,148 or 15%). The remaining 8% of cases were administratively resolved, dismissed, or resulted in an impasse.

Our study investigates the effect of business repeat playing on consumer win rate. As such, all of the regressions in our paper focus exclusively on our 4,570 awarded cases (those that were decided on the merits). Table 2 describes this subsample. Overall, consumers prevailed 1,529 times, which translates into a raw consumer win rate of 33%. As is characteristic of AAA cases, the

[6] For convenience, we will refer to consumers as "plaintiffs" even though in 15% of arbitrations they were the respondent.

dispositions came quickly. On average, cases lasted less than one year (or 255 days).[7] Also, consumers didn't pay a lot for these cases; even though arbitrators charged an average of $3,797 per awarded case, the share that consumers paid was less than that. On average, consumers paid about 38% of the total arbitrator fee.

B. Repeat Player Descriptive Statistics

With that general introduction to the data set, we now turn our focus to describing the main variable of interest: repeat playing. First, we offer some definitions. We define a business as a ***repeat player*** if it arbitrated two or more times across all 14,691 consumer disputes in our data set—regardless of whether the case ended in an award or not. Our reasoning is that we think of every arbitration appearance as an opportunity for a business to learn about how to prevail in arbitration. Relatedly, we refer to the total number of appearances that a business makes in our database as their ***repeat player score***.

Table 3 presents our statistics on repeat player prevalence. Of the 5,034 businesses that arbitrated consumer disputes in the AAA during our sample period, the great majority (83% or 4,168) were one shotters. The remaining 17% of businesses (866 in total) were repeat players. Table 4 gives the frequency table of repeat player scores for all 866 repeat players. Interestingly, *most repeat players were not very sophisticated.* Indeed, the median repeat player score was two—meaning that half of all repeat players entered the arbitral forum exactly two times. Moreover, three-quarters of all repeat players (77%) entered the arbitral forum 5 or fewer times. The range of the repeat player score for the top quartile of repeat players (23%) was wide, spanning from 6 to 1,533. Then, at the very top of the repeat playing distribution were 14 businesses that had repeat player scores greater than 100. These 14 businesses accounted for just 2% of the total repeat player distribution and we refer to

[7] This is short relative to other arbitration providers such as JAMS and Kaiser where the average time to disposition for awarded consumer cases is 380 days and 453 days, respectively. (Chandrasekher and Horton, 2019).

them as *super repeat players*. In Table 4, we list the identity of these super repeat players. Figure 1 presents the histogram of the repeat player score distribution for all repeat players with scores between 2 (inclusive) and 100.[8] As is clear, the distribution of repeat playing appearances is right-skewed with a long right tail.

Counter-intuitively, although repeat players account for only 17% of all businesses, they are involved in the majority of all arbitrations. Indeed, in 72% of all cases (or 10,523 of 14,570), consumers arbitrated against such a company. In fact, facing a relatively sophisticated repeat player was quite common. In 60% of cases (or 8,830), consumers squared off against a firm that was in the top quartile of repeat playing sophistication, and in 37% of cases (or 5,359) consumers confronted a super repeat player.

C. Regression Results

This section is the centerpiece of our paper. After we lay the groundwork by analyzing the data using a model that employs discrete random variables only and a model that employs continuous random variables only, we make our main contribution to the debate over repeat players and consumer arbitration. Specifically, we estimate the repeat player effect using a specification that employs *both* continuous random variables *and* discrete random variables. By doing so, we offer evidence that the defendant-specific hypothesis is potentially a better explanation for the repeat player effect in AAA consumer arbitration than the experience hypothesis.

i. Modelling Repeat Playing with Discrete Random Variables

We begin our analysis by modelling the repeat player effect with a single dummy variable. Our regression model is as follows:

$$Y = \beta_0 + \beta_1 \, Repeat\ Player + \beta_2 Other\ Controls + \varepsilon$$

[8] We omitted the super repeat players from this figure for ease of display.

Because we are interested in investigating the effect on plaintiff win probability, we focus on the 4,570 cases that resulted in an arbitrator award. Our dependent variable *Y* is a dummy variable equal to 1 if a consumer wins the case and 0 otherwise.[9] *Repeat Player* is a dummy variable equal to one if the consumer is facing a defendant that has been in the arbitral forum more than one time and zero if the consumer is facing a one-shot defendant. We also include a series of other variables designed to control for factors that might be correlated with both business repeat player status and consumer win probability. First, we control for repeat pairing by including a dummy variable that indicates whether or not the consumer is facing a business that has appeared before the same arbitrator at least once. Second, because we believe that there could be a repeat player effect on the plaintiff side as well, with consumers who are represented by repeat playing law firms faring better than consumers that are *pro se*, we include a series of dummy variables keyed to the plaintiff's law firm's repeat player and repeat pairing score.[10] Third, we include controls for the plaintiff claim amount, whether or not the consumer initiated the case, whether the case has oral argument or is decided on the papers, whether the case has in-person or telephonic hearings, the length of time that the case takes to resolve, and finally whether the arbitrator is a former judge. Finally, the regression contains fixed effects for the year that the arbitrator decides the case and the specific type of consumer dispute.[11] All regressions in this paper include this same basic set of controls and are estimated using a linear probability model. The standard errors are clustered at the business level.

[9] We define a consumer "win" as an award where either (1) the plaintiff was designated as the "prevailing party" or (2) the plaintiff recovered more than $1 in damages or attorneys' fees, unless (3) the business was listed as the "prevailing party."

[10] Future work will investigate the source of this repeat plaintiff law firm effect in the same way that we are investigating the source of the business repeat player effect in the current piece.

[11] The types of consumer disputes are new home construction, financial services, car sales / lease, telecommunications / wireless / cable, debt collection moratorium and other. There is also a sizeable set of cases in our data set (46%) where the type of consumer dispute is missing. For these disputes, we code their dispute type as "not specified."

We find that facing a business that is a repeat player is associated with a 6.5 percentage point decrease in consumer win probability. (Table 5, column 1). This result is consistent with other papers that also find a negative and statistically significant effect of facing a repeat player on consumer win rate. (Colvin 2011, Colvin and Gough 2015, Horton and Chandrasekher 2015 and 2016, Chandrasekher and Horton 2019).

Other effects are also noteworthy. First, business-arbitrator repeat pairing seems to negatively affect the consumer win rate. Specifically, consumers that face such a pair have a win probability that is 9 percentage points lower than consumers that do not. On the plaintiff side, repeat-playing effects also exist. Plaintiffs that are represented by low level repeat playing law firms tend to do better in arbitration than consumers that are *pro se*. On the other hand, consumers that have a highly sophisticated law firm in terms of total number of appearance in our data set actually *do worse* than consumers that brave arbitration alone. This perverse result is consistent with our findings from our previous papers, and stems from one deviant plaintiff's law firm.[12] There is no detectable plaintiff law firm-arbitrator pairing effect. Our regression results also detected two other statistically significant determinants of plaintiff win rate. First, consumers who initiated the proceedings had a win probability that was 36 percentage points higher than consumers that did not. Second, in-person hearings also advantaged consumers relative to telephonic hearings.

As noted above, our previous articles have built on this baseline by replacing the single repeat player dummy variable in the regression with a series of four dummy variables that indicate the rough quartile of repeat player sophistication in which the business falls. The series of dummy variables indicate whether the consumer is facing a low-level, mid-level, high-level, or super-repeat playing business. The omitted category is businesses that are one-shotters.

[12] A bogus debt collection agency called World Law Group once filed numerous baseless arbitrations on behalf of its clients. It went out of business in 2015.

$$Y = \beta_0 + \beta_1\, LowLevel\ RP + \beta_2 MidLevel\ RP + \beta_3\, HighLevel\ RP + \beta_4 SuperLevel\ RP + \beta_5 Other\ Controls + \varepsilon$$

Using this alternative functional form, we discover that to the extent that a repeat player advantage exists, it only exists for businesses that are at the top of the repeat-playing distribution. That is, consumers facing businesses that are in the low- medium- and high- repeat playing groups fare no worse than consumers facing a one-shot business. However, consumers facing a super repeat playing business (one that is in the top 2% of all repeat playing businesses) fare systematically worse. Specifically, facing a super repeat player is associated with a 21 percentage point reduction in consumer win probability. (Table 5, column 2).

The results from these two discrete models together provide evidence that simply facing a company that is a repeat player negatively impacts a consumer's win probability. This negative repeat player effect operates entirely through the high-end of the distribution—meaning that consumers are only disadvantaged if they face a company that is a super-repeat player.

ii. Modelling Repeat Playing with Continuous Random Variables

In this section, we model business repeat playing with a continuous random variable instead of a discrete random variable. The continuous variable, which we refer to as *appearance counter*, tells us what number appearance the current appearance is for the business.[13] For example, if the consumer

[13] To be clear, our continuous random variable (appearance counter) is slightly different from the continuous random variable used by Colvin and Gough (2015). Their continuous random variable was the repeat player score. The repeat player score is a static variable in the sense that its value stays constant across all observations for a particular defendant. As such, its coefficient tells us how much the plaintiff win probability changes when the repeat player score—i.e. the defendant's overall sophistication as measured by the total number of appearances that the defendant will have accumulated *by the end of the data set*—increases by 1. On the other hand, our appearance counter is dynamic in the sense that its value increases by 1 for every additional appearance that the business makes. As such, its coefficient tells us how much the plaintiff win probability changes when the appearance counter—i.e. the level of sophistication the defendant has accumulated *at the time of the current arbitration case*—increases by 1. While both repeat player variables measure the effect of defendant experience on plaintiff outcome, we prefer the appearance counter variable because its

is facing a defendant repeat player that is on its 5th appearance in the data set, then the value of the appearance counter is 5. If it's the business's 20th appearance, then the value of the appearance counter is 20, and so on. The regression formula is as follows:

$$Y = \gamma_0 + \gamma_1 \, Appearance\ Counter + \gamma_2 \, Appearance\ Counter^2 + \gamma_3 Other\ Controls + \varepsilon$$

γ_1 can be interpreted as the return to arbitration experience. In other words, the coefficient is the average change in consumer win probability that results when a business's arbitral experience increases by one additional appearance. We also include a quadratic function of the appearance counter in the regression to allow the returns to experience to be non-linear. Ours is the first-ever paper to measure the impact of higher-order functions of a repeat player variable on plaintiff win probabilities.

Table 6 contains the regression results. We find that each successive appearance in the arbitral forum by the defendant-business does indeed lower the consumer win probability. Specifically, column 2 shows that for every additional case that the business has under its belt, the consumer win probability falls by one-tenth of a percentage point. The quadratic effect is positive and statistically significant, implying that even though the consumer win probability falls for every additional successive arbitration appearance, the reductions in win rate probability get smaller and smaller as the business's arbitration experience increases. In other words, from the business's perspective, there are diminishing returns to gaining arbitration experience.

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interpretation fits more closely with our idea of testing how each additional appearance of a serially arbitrating defendant contributes to the repeat player advantage, rather than testing how the overall sophistication of a serially arbitrating defendant contributes to the repeat player advantage. Another major benefit of the appearance counter variable is that its value varies *within* defendant, permitting the use of defendant fixed effects, the technique that ultimately allows us to empirically separate the defendant-specific effect from the experience effect.

iii. Specifications with *Both* Discrete and Continuous Measures

In this section, we include both discrete and continuous controls in the same specification in order to separately identify the experience and defendant-specific theories. On the one hand, continuous controls (*appearance counter* and *appearance counter*^{2}) estimate the effect on the consumer win rate of increasing the defendant's arbitration experience by one additional appearance, holding constant all defendant-specific characteristics. On the other hand, discrete controls (either *repeat player dummy* or *repeat playing quartiles*, depending on the specification) estimate the effect on the consumer win probability of facing a repeat player, while holding constant the appearance level. Importantly, these two effects could not be separately estimated without including both sets of variables. The estimating regression is:

$$Y = \gamma_0 + \alpha_1\, Repeat\ Player + \alpha_2 Appearance\ Counter + \alpha_3\, Appearance\ Counter^2 + \alpha_4 Other\ Controls + \varepsilon$$

The results are presented in Table 7. Column 1 of Table 7 reproduces the baseline results from the regression that includes continuous controls alone (from Table 6, column 2). Column 2 of Table 7 then adds in the simple dummy variable. We see that when both the repeat player dummy and the appearance counter variables are estimated together, the effects are both negative and statistically significant. However, the coefficient on the repeat player dummy variable is much larger in absolute value. The relevant coefficients imply that when a consumer faces a repeat player in arbitration, they are disadvantaged in two ways. First, the mere fact of facing a repeat-playing defendant, regardless of that defendant's level of sophistication, leads to a 5 percentage-point average reduction in win probability, right off the bat. Then, amongst consumers that are facing repeat playing defendants, the serial arbitration effect is one-tenth of a percentage point. Alternatively stated, the main disadvantage to a consumer comes from facing a repeat player in the

first place. One unit differences in the defendant's appearance count only minimally impact the plaintiff's win probability.

Column 3 of Table 7 then replaces the repeat player dummy variable with the four dummy variables that indicate the rough quartile that the defendant business is in. Quite surprisingly, when the simple dummy variable is replaced with the more detailed controls for level of repeat playing, the serial arbitration effect (as measured by the appearance counter and its square) *completely disappears*. Specifically, a consumer facing a super repeat player suffers a 21 percentage point reduction in win probability. But the mere fact that a business is appearing for, say, the 101st time rather than the 100th time has no impact at all. This suggests that the repeat player effect stems from *who the defendant is*—more specifically, their presence at the top of the distribution of repeat players—rather than from *arbitration experience*.

A natural extension to the regression with the rough quartile dummies is a specification that uses defendant-specific fixed effects. Rather than sorting companies into tiers, this approach gives each company its own dummy variable.[14] The advantage of this technique is that it allows us to hold constant *any* time-invariant attribute that a particular firm brings to the table (size, assets, etc.) and then focus in on whether they gain from successive entries into the arbitral forum. As Column 4 of Table 7 reveals, once all defendant-specific variation in gone, there is no return to serial arbitration.

[14] The inclusion of defendant fixed effects requires us to drop all one shotter defendants from the regression. Defendant fixed effects cannot be estimated for defendants that do not have at least two appearances in the dataset. Therefore the sample size drops from 4,554 to 2,897.

IV. DISCUSSION

The evidence presented thus far shows that when continuous repeat player random variables and discrete repeat player random variables are included in the same regression model, the coefficients on the discrete random variables are positive and statistically significant whereas the coefficients on the continuous random variables are statistically insignificant. One way to interpret this evidence, indeed the interpretation advanced throughout the paper, is that repeat playing defendants excel because of who they are (i.e.—the defendant-specific hypothesis) and not because of their arbitration experience (i.e.—the serial arbitration hypothesis). Though our research does not specify which one (or ones) of the time-invariant characteristics is at work (ex.—defendant size, assets, industry, or some other factor), it does strongly suggest that some time-invariant characteristic is the driving factor behind the defendant repeat player effect. Nonetheless, there are at least two possible arguments for how the serial arbitration effect could still be at play, and thus partially responsible for, the repeat player effect *within this framework of time-invariant, discrete random variables*. In this section, we lay out these two possible arguments.

First, it is possible that our time-invariant controls are picking up *pre-accumulated serial arbitration experience*. Our data set begins in January 2010, but many defendant companies began arbitrating far before that. Perhaps the prowess of super-repeat players like AT&T and American Express partially emanates from a high-level of previously accumulated serial arbitration experience that, once obtained, continues to pay dividends going forward. If so, our regression specifications would not be able to distinguish that type of time invariant effect from the effect of other defendant-specific characteristics like size, assets, industry and the like. Still, all is not lost. What our data do tell us is that, on average, the serial arbitration experience gained *during our sample period,* from 2010-2016, does not contribute to the defendant repeat player advantage. This is true both for

those defendants that started in 2010 with no prior arbitration experience and those that already had high-levels of prior experience as of 2010.

Second, it is possible that our time-invariant controls are picking up *defendant-attorney serial arbitration experience*. Even though defendants don't gain any advantage to serially arbitrating *within our sample period*, they might have lawyers that either have pre-accumulated serial arbitration experience or are gaining experience through serial arbitration during the sample period. Both of these serial arbitration, lawyer-based advantages would be captured by our time-invariant controls.[15]

CONCLUSION AND FUTURE WORK

What is the root of arbitration's repeat player effect? We have demonstrated that in one important context—AAA consumer cases—repeat players excel because of time invariant characteristics. We call this the defendant-specific hypothesis. What remains to be seen, of course, is which time-invariant characteristics play the largest role in driving the defendant repeat player effect. Future work will focus on investigating the relative contributions of defendant assets, number of employees, and defendant industry.

[15] As previously mentioned, our data set does not include the identity of the defendant law firm, so we cannot directly test for a serial arbitration effect for defendant attorneys.

TABLES AND FIGURES

Table 1. Descriptive Statistics AAA (All Cases)		
Initiating Party	Frequency	Percent
Consumer	12,458	84.8
Business	2,233	15.2
Case Disposition		
Settled	6,836	46.53
Awarded	4,570	31.1
Withdrawn	2,148	14.62
Administrative Resolution	726	4.94
Dismissed	353	2.40
Impasse	58	0.39
Type of Consumer Dispute		
Not-Specified	6,845	46.59
New Home Construction	2,541	17.30
Financial Services	2,497	17.00
Car Sales / Lease	912	6.21
Telecommunications /Wireless / Cable	606	4.12
Debt Collection Moratorium	588	4.00
Other	702	4.78
N	14,691	

Table 2. Descriptive Statistics AAA (Awarded Cases)		
	Mean	Median
Case Length (Days)	255 (157)	216
Total Arbitrator's Fee	$3,797 ($13,694)	$1,059
Plaintiff's Share of Arbitrator's Fee	$1,438 ($6,582)	$0
Claim Amount	$92,505 ($651,512)	$10,000
N	4,570	

Table 3. Repeat Player Prevalence		
	Frequency	Percent
One-Shotter	4,168	82.80
Repeat Player	866	17.20
Total Number of Businesses	5,034	

Table 4. Frequency Table of Repeat Player Appearances			
Repeat Player Score (Defined as Total Number of Appearances)	Frequency (Number of Businesses)	Percent	Cumulative Percent
2	444	51.27	51.27
3	126	14.55	65.82
4	58	6.70	72.52
5	39	4.50	77.02
6	36	4.16	81.18
7	16	1.85	83.03
8	9	1.04	84.06
9	13	1.50	85.57
10	6	0.69	86.26
11	14	1.62	87.88
12	9	1.04	88.91
13	4	0.46	89.38
14	7	0.81	90.18
15	8	0.92	91.11
16	5	0.58	91.69
17	1	0.12	91.80
18	2	0.23	92.03
19	2	0.23	92.26
20	3	0.35	92.61
21	5	0.58	93.19
22	2	0.23	93.42
23	1	0.12	93.53
24	4	0.46	94.00
27	1	0.12	94.11
28	2	0.23	94.34
30	2	0.23	94.57
31	3	0.35	94.92
32	1	0.12	95.03
33	1	0.12	95.15
35	1	0.12	95.27
36	1	0.12	95.38
37	1	0.12	95.50
39	1	0.12	95.61
40	1	0.12	95.73
41	1	0.12	95.84
42	1	0.12	95.96
43	1	0.12	96.07
46	1	0.12	96.19
51	1	0.12	96.30
52	1	0.12	96.42
53	1	0.12	96.54
55	3	0.35	96.88
57	1	0.12	97.00
59	1	0.12	97.11
60	1	0.12	97.23
62	1	0.12	97.34
64	2	0.23	97.58
68	2	0.23	97.81
71	1	0.12	97.92
72	1	0.12	98.04
73	1	0.12	98.15
75	1	0.12	98.27
89	1	0.12	98.38
104 (Hyundai Motor America)	1	0.12	98.50
109 (GE Capital)	1	0.12	98.61
113 (CCA EduCorp, Inc.)	1	0.12	98.73
117 (Credit One Bank, NA)	1	0.12	98.85
132 (Navient Solutions, Inc.)	1	0.12	98.96
159 (Verizon)	1	0.12	99.08
175 (H&R Block)	1	0.12	99.19
212 (Wells Fargo)	1	0.12	99.31
295 (Sallie Mae)	1	0.12	99.42
318 (American Express)	1	0.12	99.54
570 (Santander Bank)	1	0.12	99.65
719 (Discover)	1	0.12	99.77

803 (Citibank)	1	0.12	99.88
1533 (AT&T)	1	0.12	100.00
Number of Repeat Playing Businesses	866	100.00	

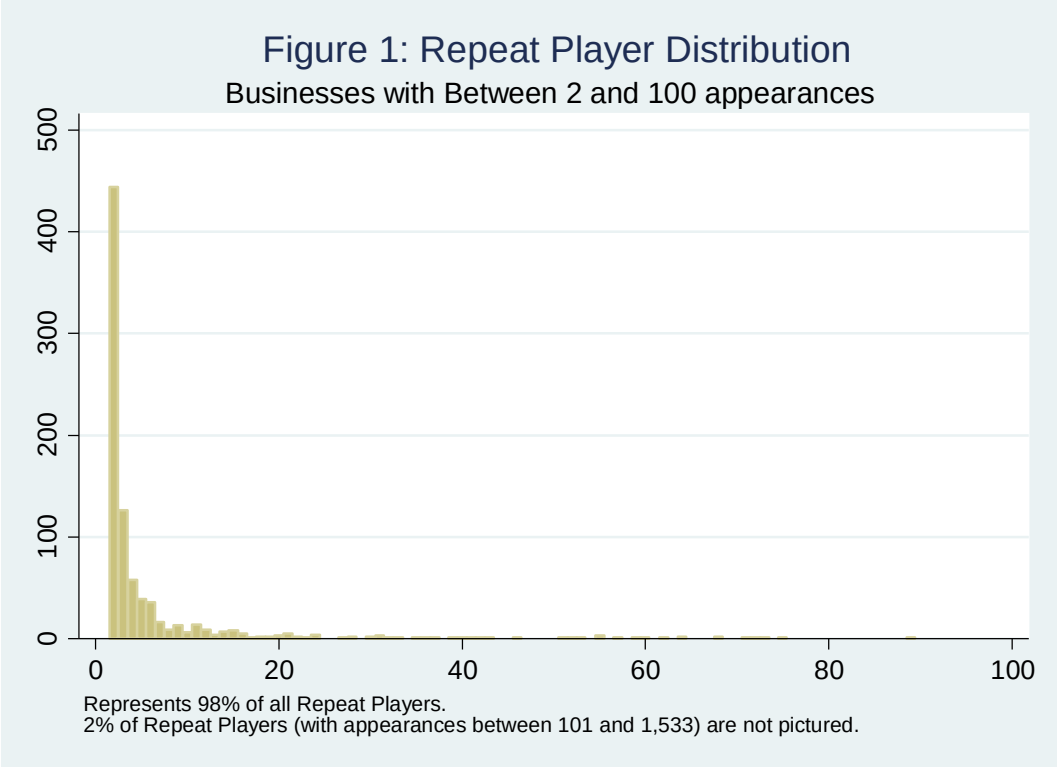

Table 5. AAA Awarded Cases: Linear Probability Model Dependent Variable: Plaintiff Win or Loss (0-1 Variable) **Repeat Player Effect Measured with Discrete Random Variables**		
	(1)	(2)
Business is a Repeat Player	-0.06504** (0.02421)	
Business is a		
Low-Level RP		-0.04437 (0.03068)
Mid-Level RP		0.02477 (0.03512)
High-Level RP		-0.05713 (0.02949)
Super RP *(Reference Group is One-Shot Businesses)*		-0.21318*** (0.05658)
Case involves a Business-Arbitrator Repeat Pair	-0.09241** (0.03057)	-0.03177 (0.02889)
Plaintiff's Law Firm is a . . .		
Low-Level RP	0.08158** (0.02513)	0.07108** (0.02302)
Mid-Level RP	0.04724 (0.03349)	0.03165 (0.03201)
High-Level RP	0.08548 (0.05497)	0.09387 (0.04925)
Super RP *(Reference Group is Pro Se Consumers)*	-0.14942*** (0.03133)	-0.09600* (0.04144)
Case involves a Plaintiff's Law Firm-Arbitrator Repeat Pairing	0.01071 (0.02256)	0.00789 (0.01893)
Plaintiff Claim Amount	0.00000 (0.00000)	0.00000 (0.00000)
Plaintiff Filed the Case	0.36172*** (0.02419)	0.37478*** (0.02316)
In-Person Hearing	0.05342* (0.02150)	0.04006 (0.02138)
Documents-Only Proceeding	0.03881 (0.02175)	0.03935 (0.02043)
Length of Arbitration	0.00423 (0.00684)	0.00323 (0.00627)
Arbitrator is a Former Judge	0.00761 (0.02342)	0.00392 (0.02264)
Constant	-0.50204*** (0.07546)	-0.49462*** (0.07471)
N	4554	4554
adj. R^2	0.217	0.230

Notes:
(1) Standard Errors are clustered at the defendant level.
(2) Fixed effects for year of the arbitrator's award and case sub-type are included in each specification.
(3) $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table 6. AAA Awarded Cases: Linear Probability Model
Dependent Variable: Plaintiff Win or Loss (0-1 Variable)
Repeat Player Effect Measured with Continuous Random Variables

	(1)	(2)
Appearance Counter	-0.00036***	-0.00141***
	(0.00007)	(0.00033)
Appearance Counter^2		0.00000***
		(0.00000)
Case involves a Business-Arbitrator Repeat Pair	-0.07915**	-0.06564*
	(0.03039)	(0.02970)
Repeat Pair Count	-0.01213	-0.01160
	(0.01197)	(0.01170)
Plaintiff's Law Firm is a . . .		
Low-Level RP	0.08570***	0.07938**
	(0.02507)	(0.02424)
Mid-Level RP	0.04480	0.03779
	(0.03371)	(0.03314)
High-Level RP	0.07606	0.06531
	(0.05261)	(0.05308)
Super RP	-0.12886***	-0.10013**
	(0.02941)	(0.03548)
Case involves a Plaintiff's Law Firm-Arbitrator Repeat Pairing	0.00877	0.00313
	(0.02100)	(0.02050)
Plaintiff Claim Amount	0.00000	0.00000
	(0.00000)	(0.00000)
Plaintiff Filed the Case	0.36264***	0.36619***
	(0.02407)	(0.02357)
In-Person Hearing	0.04615*	0.04052
	(0.02184)	(0.02188)
Documents-Only Proceeding	0.03213	0.03235
	(0.02189)	(0.02209)
Length of Arbitration	0.00141	0.00141
	(0.00681)	(0.00676)
Arbitrator is a Former Judge	0.00925	0.00729
	(0.02292)	(0.02296)
Constant	-0.48761***	-0.48268***
	(0.07480)	(0.07477)
N	4554	4554
adj. R^2	0.218	0.221

Notes:
(1) Standard Errors are clustered at the defendant level.
(2) Fixed effects for year of the arbitrator's award and case sub-type are included in each specification.
(3) $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table 7. AAA Awarded Cases: Linear Probability Model
Dependent Variable: Plaintiff Win or Loss (0-1 Variable)
Repeat Player Effect Measured with Discrete AND Continuous Random Variables

	(1)	(2)	(3)	(4)
Appearance Counter	-0.00141***	-0.00127***	-0.00011	0.00049
	(0.00033)	(0.00034)	(0.00051)	(0.00038)
Appearance Counter^2	0.00000***	0.00000**	0.00000	-0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Business is a Repeat Player		-0.04837*		
		(0.02244)		
Business is a . . .				
Low-Level RP			-0.04467	
			(0.03066)	
Mid-Level RP			0.02416	
			(0.03506)	
High-Level RP			-0.05674	
			(0.02917)	
Super RP			-0.20775**	
			(0.07551)	
Business Fixed Effects	No	No	No	Yes
Case involves a Business-Arbitrator	-0.06564*	-0.05514	-0.02342	-0.02057
Repeat Pairing	(0.02970)	(0.02924)	(0.02840)	(0.02410)
Repeat Pairing Count	-0.01160	-0.01244	-0.00830	-0.00227
	(0.01170)	(0.01162)	(0.00869)	(0.00556)
Plaintiff's Law Firm is a . . .				
Low-Level RP	0.07938**	0.07447**	0.07150**	0.04574
	(0.02424)	(0.02402)	(0.02272)	(0.03624)
Mid-Level RP	0.03779	0.03980	0.03222	0.04282
	(0.03314)	(0.03313)	(0.03155)	(0.04222)
High-Level RP	0.06531	0.07243	0.09509	0.11385**
	(0.05308)	(0.05312)	(0.04859)	(0.04063)
Super RP	-0.10013**	-0.09781**	-0.09564*	-0.02443
	(0.03548)	(0.03626)	(0.03881)	(0.02102)
Case involves a Plaintiff's Law Firm-Arbitrator	0.00313	0.00150	0.00875	-0.00517
Repeat Pairing	(0.02050)	(0.02048)	(0.01722)	(0.02097)
Plaintiff Claim Amount	0.00000	0.00000	0.00000	-0.00000*
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Plaintiff Filed the Case	0.36619***	0.36638***	0.37444***	0.05839
	(0.02357)	(0.02346)	(0.02296)	(0.06093)
In-Person Hearing	0.04052	0.03790	0.03888	0.06596*
	(0.02188)	(0.02182)	(0.02186)	(0.02730)
Documents-Only Proceeding	0.03235	0.03352	0.03909	0.01545
	(0.02209)	(0.02208)	(0.02093)	(0.02108)
Length of Arbitration	0.00141	0.00240	0.00354	0.01111
	(0.00676)	(0.00687)	(0.00654)	(0.00883)
Arbitrator is a Former Judge	0.00729	0.00611	0.00305	-0.03779
	(0.02296)	(0.02287)	(0.02255)	(0.02718)
Constant	-0.48268***	-0.44968***	-0.48494***	-0.54097*
	(0.07477)	(0.07649)	(0.07588)	(0.23388)
N	4554	4554	4554	2897

adj. R^2	0.221	0.222	0.230	0.390

Notes:
(1) Standard Errors are clustered at the defendant level.
(2) Fixed effects for year of the arbitrator's award and case sub-type are included in each specification.
(3) $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Appendix Table. AAA Awarded Cases: Linear Probability Model
Dependent Variable: Plaintiff Win or Loss (0-1 Variable)
Repeat Player Effect Measured with Discrete AND Continuous Random Variables
(By Group)

Sample	(1)	(2)	(3)	(4)	(5)	(6)
	One-Shotters and Regular Repeat Players		One-Shotters and Super Repeat Players		Regular and Super Repeat Players	
Business is a Repeat Player	-0.04839*	-0.05040*	-0.19086**	-0.16716*		
	(0.02094)	(0.02334)	(0.07237)	(0.08264)		
Appearance Counter		-0.00016		-0.00044		0.00057
		(0.00396)		(0.00042)		(0.00402)
Appearance Counter^2		0.00002		0.00000		-0.00000
		(0.00007)		(0.00000)		(0.00007)
Super Repeat Players					-0.16414**	-0.14361
					(0.05371)	(0.07446)
Super Repeat Player X Appearance Counter						-0.00084
						(0.00399)
Super Repeat Player X Appearance Counter^2						0.00000
						(0.00007)
Case involves a Business-Arbitrator Repeat Pairing	0.04110	0.02886	-0.07215*	-0.06469*	-0.03276	-0.02606
	(0.05836)	(0.05971)	(0.03032)	(0.02780)	(0.02902)	(0.02787)
Repeat Pairing Count		0.01417		-0.00736		-0.00812
		(0.02976)		(0.00731)		(0.00824)
Plaintiff's Law Firm is a . . .						
Low-Level RP	0.09134***	0.09223***	0.05765*	0.05671*	0.08070*	0.08186*
	(0.02574)	(0.02512)	(0.02499)	(0.02484)	(0.03387)	(0.03270)
Mid-Level RP	0.03785	0.04024	0.04052	0.04111	0.03994	0.04243
	(0.03831)	(0.03743)	(0.03382)	(0.03374)	(0.04275)	(0.04038)
High-Level RP	0.12299*	0.12563*	0.08121	0.07551	0.08999	0.09085
	(0.05048)	(0.04992)	(0.05479)	(0.05583)	(0.06210)	(0.06259)
Super RP	-0.00411	-0.00078	-0.12351*	-0.11207*	-0.08544	-0.08122
	(0.06607)	(0.06775)	(0.05782)	(0.05256)	(0.04513)	(0.04138)
Case involves a Plaintiff's Law Firm-Arbitrator Repeat Pairing	0.02236	0.02289	0.01002	0.00731	0.00888	0.00919
	(0.02623)	(0.02575)	(0.01848)	(0.01682)	(0.02165)	(0.01943)
Plaintiff Claim Amount	0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Plaintiff Filed the Case	0.38031***	0.38003***	0.38551***	0.38539***	0.32316***	0.32216***
	(0.02148)	(0.02133)	(0.02827)	(0.02815)	(0.04206)	(0.04140)
In-Person Hearing	0.04694	0.04665	0.03574	0.03050	0.04039	0.03747
	(0.02418)	(0.02403)	(0.02424)	(0.02573)	(0.02796)	(0.02855)
Documents-Only Proceeding	0.05021	0.05096	0.02220	0.01974	0.04090	0.04070
	(0.02894)	(0.02891)	(0.02037)	(0.02110)	(0.02472)	(0.02543)
Length of Arbitration	0.00446	0.00460	0.00216	0.00201	0.00357	0.00414

	(0.00662)	(0.00656)	(0.00646)	(0.00696)	(0.00917)	(0.00963)
Arbitrator is a Former Judge	-0.00954 (0.03252)	-0.00943 (0.03258)	0.02477 (0.02747)	0.02364 (0.02712)	-0.00707 (0.02483)	-0.00853 (0.02475)
Constant	-0.51652*** (0.09111)	-0.53551*** (0.09900)	-0.53283*** (0.08524)	-0.51117*** (0.08777)	-0.44271*** (0.10491)	-0.44166*** (0.11230)
N	3127	3127	3084	3084	2897	2897
adj. R^2	0.152	0.152	0.284	0.284	0.200	0.199

Notes:

(1) Standard Errors are clustered at the defendant level.
(2) Fixed effects for year of the arbitrator's award and case sub-type are included in each specification.
(3) $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

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