AFFILIATION BIAS IN ARBITRATION: AN EXPERIMENTAL APPROACH

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Abstract

A characteristic feature of arbitration, a growing form of legal adjudication, is that each disputing party appoints an arbitrator. Commentators, however, suggest that party-appointed arbitrators tend to be 'biased' in favor of their appointers. Evaluating this claim from data on historical disputes is problematic due to non-random selection of arbitrators. Here, we use a novel experimental approach to estimate the causal effect of party-appointments. Using survey experiments on arbitration experts around the world we show that professional arbitrators suffer from affiliation effects—a cognitive predisposition to favor the appointing party. At a methodological level, we offer a solution to the problem of measuring affiliation effects when credible observational designs are lacking.

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I. Introduction

Arbitration has crept into nearly every corner of Americans' lives. Scholars refer to this expansion as a "whole-scale privatization of the justice system" (Malin & Ladenson, 1992; Gilles, 2014). This trend goes beyond domestic legal disputes as arbitration has become a central element of economic interdependence. In particular, the growth of arbitration has put arbitration panels in a position to rule on transnational business transactions and key political questions like the ability of governments to tax their citizens or regulate health—long areas of sovereign prerogative (Born, 2011).

What makes this expansion potentially troubling is that systematic features of arbitration make it difficult for arbitrators to be entirely unbiased in their decisions (Garth, 2001). Unfettered by precedent, deprived of strict uniform rules against conflicts of interest, and insulated from any judicial system, some argue that powerful corporations use arbitration to steer cases to friendly arbitrators incentivized by the prospects of sizeable earnings.

This debate is not completely new in the legal academy (Resnik, 2004; Fiss, 1983). What is new, however, is that recent critiques of arbitration come from insiders who otherwise defend this dispute settlement system as an efficient form of legal adjudication (Paulsson, 2010; Van den Berg, 2011). These criticisms have focused on a particularly well-established feature of arbitration—the 'right' of disputants to appoint an arbitrator to a tribunal. While such unilateral *party-appointments* are well established in the field of

arbitration, some now criticize the practice as a source of bias that negatively affects the impartiality and the legitimacy of arbitration proceedings.

Proposals for reform range from eliminating party-appointments altogether to introducing changes in the form of arbitrators' selection, but most seem farfetched or unlikely as a practical matter (Giorgetti, 2014). In short, arbitration is designed to give parties greater control over the dispute resolution process relative to a more formal judicial setting, and parties would be hesitant to give up one of the primary tools for exercising that control—the ability to (partially) choose who hears the dispute. However, a recent proposal that may be more plausible would permit parties to appoint arbitrators, but would prevent nominees from knowing which party appointed them (ABA, 2013).

This "blind appointment" approach attempts to relieve the arbitrator of the possible affiliation or allegiance effects resulting from the nomination process, but maintains unilaterally appointed arbitrators, considered a fundamental 'right' in arbitration (Brower & Rosenberg, 2013).[1] The proposal is in part inspired by ideas ingrained in most legal systems (*e.g.,* "justice is blind"), in some theories of justice (*e.g.,* "veil of ignorance,") and in the highest standards in scientific research (*e.g.,* blind peer-review) (Robertson, 2010), with the general principle being that impartiality can be enhanced by preventing a decision-maker from accessing knowledge that might unduly affect the decision.

In this Article, we use the blind appointments proposal as an entry point into the debate over implicit biases of legal actors, with three objectives in mind. First, we argue that an important issue in the debate over bias is to distinguish between two different mechanisms

[1] Recently, the CPR Institute for Conflict Prevention & Resolution has offered a blinded (screened) option for arbitrator selection under its international rules. See http://www.cpradr.org/About/PressReleasesVideos/tabid/555/ID/934/CPR-CAPTURES-2016-GAR-INNOVATION-AWARD-Press-Release.aspx.

through which the nomination process affects arbitrators' behavior: selection effects and affiliation effects. Prior observational research has shown that arbitrators tend to favor their appointing parties. However, because parties choose arbitrators in part based on how they expect the arbitrator will vote, appointment-driven bias cannot be inferred from these correlations alone. In order to infer evidence of an affiliation effect (the implicit bias of the arbitrator to favor the appointing party) we need to eliminate the selection effect (the strategic decision of the litigant to choose arbitrators likely to be favorable). Therefore, we develop a set of randomized survey experiments designed to isolate affiliation effects in which we directly controlled arbitrators' assigned appointers.

Second, we present the results of these experiments. In short, we presented surveyed arbitrators with a hypothetical choice task related to an investment arbitration case. Participants were randomly told whether they were appointed by one of the parties, by joint agreement of the parties, or simply that they were appointed (without any information about the identity of the appointer). Based on a sample of 257 responses from arbitrators around the world, we found that arbitrators nominated by one of the two parties to the litigation tended to make decisions more favorable to that party compared to arbitrators appointed by the opposite party. We replicated this result in a follow-up experiment. Additionally, we found that arbitrators treated with the 'blind appointment' option exhibited similar responses to joint party appointees and tended to take positions in the middle of the two party-appointees.

Third, based on the results of the experiments, we argue that blinding could be a useful approach to reducing bias. The evidence we find strongly suggests that affiliation effects exist and while there are certainly difficulties in implementing such a proposal, we suggest

that blinding could help ameliorate affiliation biases while still retaining the potential benefits of party-involvement in the appointment process.

The Article proceeds as follows: the next section provides some background to arbitration and the debate over party appointments. Part III describes the design of the initial experiment and its subsequent replication and reports the findings of both. Part IV discusses some of the limitations of our study and the challenges to adopting the blinding proposal. Part V concludes.

II. Background

A. Unilateral Party-Appointments: A Burgeoning Debate.

A debate has emerged among members of the arbitration bar over unilateral party-appointments. Critics of this feature argue that the power and legitimacy of arbitration stems from an appearance of expertise, neutrality, and impartiality. Hence, the predisposition towards one party or the other that results from the system of appointments poses significant challenges to the main legitimating aspects of arbitration (Paulsson, 2010; Van den Berg, 2011). It may also result in unnecessary antagonism, complicated compromises, and inconsistent decisions in the awards of tribunals that all undermine the goal of an independent, rule-based adjudicatory system.

Defenders of the use of party-appointed arbitrators, on the other hand, argue that the traditional party-appointment system is the "keystone" of arbitration because it gives the parties some "ownership" over the process (Perry, 2013). In addition, when tribunals render unanimous decisions, the presence of party-appointees may enhance the credibility of the award in the eyes of both parties since each litigant knows that a trusted appointee was willing to endorse the outcome. Being able to appoint an arbitrator, some argue, is one of

the most attractive aspects of arbitration "as an alternative to domestic litigation" and its elimination would constitute a radical transformation, and potential devaluation, of arbitration (Brower, 2013).

The debate over the convenience of party-appointments has been conducted almost exclusively with anecdotal evidence. However, a recent study using observational data has demonstrated some of the merits of each side of this debate. By comparing cases with and without party-appointments, Puig (2016) shows that tribunals without party-appointed arbitrators tend to handle cases faster and settle more often. Their decisions tend to be unanimous and are less likely to be challenged by a subsequent annulment proceeding. Nonetheless, the rate of arbitrator challenges and resignations in tribunals without party-appointed arbitrators appears to be higher, and the higher rate of resignations in tribunals without party-appointed arbitrators tends to lower the expediency of the process.

What the debate has also shown is a lack of consensus among arbitration professionals for moving away from the system of party-appointments, limiting which reforms would be feasible.[2] Blind appointments, however, may resolve some of the concerns that emanate from the party-appointment system, without eradicating the practice. Under a "blind appointment" system, parties to an arbitration can continue to appoint arbitrators, but a mechanism is introduced to ensure that nominees do not know who appointed them. This proposal maintains the fundamental arbitration feature of party control, but may help to mitigate the implicit bias and adversarial influence of explicitly known party appointments.

[2] A survey of professionals shows "that there is general disapproval of the recent proposals calling for an end to unilateral party appointment." See http://annualreview2012.whitecase.com/International_Arbitration_Survey_2012.pdf.

B. Party Affiliation: Arbitrators and Implicit Biases

Existing empirical evidence points to a number of different ways through which litigants can introduce biases in a legal process (Robertson, 2010). In the arbitration context, the nomination and appointment of arbitrators by the parties is a calculated decision by the litigants. When rules permit litigants to nominate a legal actor, they can ensure that such a person is not too independent minded by selecting someone who has shown reliability and the appropriate decision-making philosophy towards the relevant set of issues.[3] Any influence in the process that result from such practice could be attributed mainly to a selection effect.

Yet, even in the absence of a selection effect, appointees may also find it difficult to maintain impartiality because of implicit preferences for their appointing party. We refer to this as an affiliation effect. Modern psychology research has extensively documented the existence of such "implicit biases" in a variety of settings: from attitudes towards historically disadvantaged groups, to the effect of primes and context cues on expressed beliefs and actions (Greenwald & Banaji, 1995; Nosek & Riskind, 2012). These implicit effects on attitudes often operate outside of conscious cognitive modes—those expressing implicit biases may be entirely unaware of and unable to account for their influence. In this particular instance, an arbitrator may, despite her best intentions to remain unbiased, be nonetheless primed to favor their nominating party simply by knowing that they were selected by that party.

[3] As explained by Hunter (1987, 53) "when I am representing a client in an arbitration, what I am really looking for in a party-nominated arbitrator is someone with the maximum predisposition towards my client, but with the minimum appearance of bias."

Blinding, or removing key information that may affect a legal actor's decision, is commonly suggested as an effective debiasing strategy against affiliation effects in similar settings (Robertson, 2012). In part, blinding is a preferred intervention because the "blind spot" tends to persist even after individuals think through and consider their biases. In fact, experimental research has shown a tendency for people to acknowledge bias more readily in others than in themselves, often increasing polarization in adversarial contexts. Hence, biases typically operate non-consciously, thereby leaving their influence hidden from direct introspection (Wilson and Brekke, 1994 and Wilson et al., 2002).

III. Evidence

A. Context

Before turning to the design and results of our experimental approach, we describe some basics on the appointment of arbitrators, With some exceptions, arbitration tribunals are typically composed of three members—two party appointed arbitrators, and a third arbitrator, usually the chair, appointed by one of the following three methods: a) by agreement of the parties in the proceedings, b) by agreement of the two party-appointed arbitrators; or 3) by an independent designating authority (commonly, the institution administrating the proceedings). All arbitrators are supposed to be independent and impartial, including the two party-appointed arbitrators, and will often sign a declaration affirming their independence and impartiality.

As we discuss above, existing observational research on arbitral proceedings, such as analysis of dissent rates, is insufficient to differentiate between selection and affiliation effects. Moreover, little attention has been given to implicit biases in arbitral decision-

making (Drahozal, 2004; Keer & Naimark, 2001). To our knowledge, no prior experiment of this nature exists using arbitrators as experimental subjects.

B. Design

Our pair of survey experiments were both based around a brief vignette describing a hypothetical investor-state arbitration. Elements of the vignette were randomized for each participant in order to study how arbitrators' responses might vary across different scenarios. Our primary manipulation of interest is the arbitrator's appointing party. Participants could be told that they were appointed by the Respondent, by the Claimant, by the parties (*i.e.,* by agreement of the litigating parties), or simply that they were appointed to the tribunal (followed by a period), with no mention of any appointing method. This last condition is what we refer to as a "blind" appointment case.[4] Figure 1 provides a sample of the vignette that we presented arbitrators in the first experiment, with the key manipulation of interest shown bold.

[4] Note that the survey arbitrator is not explicitly told that their appointing party was hidden from them. The exact text of the treatment simply reads: "You were appointed to the tribunal." While arbitrators who are actually blinded in a dispute would likely know that they were blinded, we chose not explicitly mention blinding in this treatment as we did not want to present arbitrators with a potentially unfamiliar practice that is currently not part of the ICSID proceedings—the applicable rules in our vignette.

Imagine an investor-state dispute being conducted under the 2006 Arbitration Rules of the International Centre for Settlement of Investment Disputes (ICSID). The Claimant is a firm headquartered in a high-income economy. The Respondent is a country classified by the World Bank as a middle-income economy.

The Claimant alleged that the Respondent violated the provisions of a bilateral investment treaty to which the Respondent is a party. Among other arguments, the Claimant argued that the investor and its investments had been treated unfairly and that ultimately the Respondent expropriated the Claimant's investment located within the Respondent's territory. The underlying dispute concerns an infrastructure project undertaken by the Claimant under a concession contract with a governmental agency. The Respondent argued in response that the Claimant had violated provisions of the contract and that the investors received all compensation to which they were entitled.

You were appointed **to the Tribunal [by the Respondent.]/[by the Claimant.]/[by the Parties.]/[.]** After careful consideration of the facts of the case, the tribunal unanimously decided that the Respondent unfairly treated and wrongfully expropriated the Claimant's investment and that the Claimant is entitled to compensation.

In their submissions on costs, both parties have requested that the other party bear the costs of the proceedings in full, including legal fees and expenses. The counsels for both parties behaved professionally and ethically during the proceedings.

Figure 1: Sample experimental vignette showing key manipulation – Experiment 1.

In the first experiment, the vignette included a few additional manipulations in order to examine other potential influences on arbitrator behavior. We randomly varied the implied resource endowments of the Claimant and the Respondent. The Claimant in the arbitration could be a firm headquartered in either a high- or middle-income economy while the Respondent state could be either a middle- or low-income economy. We also varied the type of ruling across four possible conditions reflecting different ways in which a case litigated under the applicable procedural rules in the vignette could be decided: a) the Respondent expropriated the Claimant's property (Claimant wins), b) the Respondent did not expropriate the Claimant's property (Respondent wins), c) the dispute is outside of the tribunal's jurisdiction (Respondent wins on 'technical' grounds), or d) the Claimant's

claims are manifestly without legal merit (case dismissed summarily; *i.e.,* Respondent decisively wins). Each of the conditions had equal probability of being assigned to any individual and the treatments were each randomized independent of one another. Table 1 summarizes the marginal distributions of the number of observations assigned to each treatment condition in the sample.[5]

Table 1:Summary of number observations assigned to each condition – Experiment 1 (N=257)

Variable	Conditions			
	Appointed by the Respondent	Appointed by the Claimant	Appointed by the Parties	Blind Appointment
Appointer	77	50	67	63
	Firm from a high-income economy	Firm from a middle-income economy		
Claimant wealth	135	122		
	Respondent is a middle-income economy	Respondent is a low-income economy		
Respondent wealth	121	136		
	Claimant wins on merits	Respondent wins on merits	Respondent wins on jurisdiction	Claimant is "manifestly without merit"
Dispute outcome	60	66	73	58
Notes: Chi-squared tests for the marginal counts across all four conditions fail to reject the null that the counts are generated by discrete uniform distributions ($p > .10$).				

After being presented with the vignette, survey arbitrators were then asked how they thought the parties' expenses in the dispute, including the cost of legal representation,

[5] We present the marginal distributions of counts (rather than the full factorial joint distribution) as we are primarily interested in the marginal effects of each variable averaged over the distribution of the other treatments. While we do not have enough power to credibly estimate the effect of a particular unique combination of all four treatments, this is not the quantity we are interested in. We do have sufficient observations to estimate marginal effects for a single treatment. In this sense, our experiment is very similar to the "conjoint" multi-attribute choice experiment that is increasingly common in social science surveys (Hainmueller et. al., 2014).

should be apportioned in such a case. Arbitrators could choose to have one party reimburse the other for either *all* or *some* of their expenses or have each party bear their own expenses. Participants were then asked to briefly discuss the reasoning behind their decision in an open-ended question.

We chose in the first vignette to ask the surveyed arbitrators to rule on the allocation of costs rather than on the actual merits of a case for two important reasons. First, summarizing the arguments of the parties such that participants would have enough information to render an educated decision on the merits of a full case would require an impossibly long vignette. We were conscious of the fact that participants (busy lawyers and arbitration professionals) would likely not have the time or interest to spend hours on our survey. Prompting arbitrators to render a clear decision on costs given the result is known allowed us to use a vignette that participants could easily read and analyze in a practical amount of time.

Second, we needed an outcome that would generate variation in responses. Had we chosen an outcome on which there is obvious and clear legal guidance, we would expect arbitrators to all reach more or less the same outcome. In the context of the procedural rules applicable in our vignette (ICSID 2006 Arbitration Rules), costs are an ideal outcome as there is little precedential guidance for how they should be allocated. The arbitration rules grant significant discretion to tribunals in how they decide the parties should pay the costs. Some tribunals follow the principle that "costs follow the event," and the losing party should compensate the winner's legal fees. Others choose to have each party bear their own costs. While the former appears to be the most common approach,[6] it is rare that costs are

[6] See "Decisions on Costs in International Arbitration" 2015. ICC Commission Report.

fully borne by one party or another. There is a meaningful amount of open-endedness in the decision such that it could plausibly be swayed by extra-legal considerations.[7]

We conducted the survey in the fall of 2015 and recruited participants by collecting publicly available e-mail addresses of arbitrators and lawyers specializing in arbitration (throughout the paper we refer to these participants as arbitrators). These addresses were largely obtained from lists published on the websites of arbitration institutions and the directories of other professional organizations. We chose to recruit from this specific set of potential arbitrators, rather than from a typical sample of individuals from the general population, in order to make our results as generalizable as possible to the population of potential arbitrators around the world. Moreover, competently completing the vignette itself requires some familiarity with international arbitration to understand in the first place, making typical convenience samples useless. While not all participants have served as arbitrators, based on survey responses given after we administered the vignette all respondents indicated some expertise in arbitration, either in investor-state or in commercial arbitration. To increase our response rate, we sent several reminders to participants, but promised only an advanced circulation of any articles summarizing the research as the reward. We obtained approval from both, the University of Arizona and Harvard's institutional review boards.

[7] We are aware, however, that costs may appear to be an ancillary element of an arbitral award compared to the actual merits of a dispute. Therefore, in our replication experiment, we created a vignette to test for effects on how arbitrators would reason through how much should be awarded in damages. Please see below for further discussion on the topic.

C. Analysis and Results

A total of 538 individuals responded to the survey e-mail, but not all arbitrators completed the survey or answered all of the questions.[8] We therefore only received 257 complete responses to the investor-state vignette. While this may appear to be a very low response rate, this does not invalidate our experimental design. Random assignment of treatment allows us to obtain an unbiased estimate of the treatment effect for arbitrators in the sample regardless of how individuals were selected into the sample.[9] Moreover, if the factors driving non-response are not associated with individuals' effect sizes we can treat our effect estimates in sample as representative of the average effect in the population. Nevertheless, we thoroughly discuss the generalizability of the results in the next section discussing our sample composition.

In allocating costs, arbitrators are first and foremost driven by the outcome of the case. In practice, winning parties do not bear costs of losing parties and the vast majority of arbitrators followed this convention. In effect, there are only three reasonable outcome choices of interest that were available to arbitrators: losing party pays all, losing party pays some, and each party pays their own costs. However, perhaps due to an accidental mis-click on the part of the arbitrator, thirteen of our arbitrators indicated that they would have the winner reimburse the loser. While one could drop these individuals, doing so would risk inducing bias as we would be controlling for a post-treatment variable—the survey response. To avoid post-treatment bias, we leave these arbitrators in the analysis (and treat

[8] In this experiment, we sent out 28,832 recruitment e-mails. However, some of these e-mails returned with a delivery failure message as the addresses were no longer active. Because arbitrators could choose to stop the survey at any time, we could not force responses for all questions in the survey.

[9] Moreover, as we discuss in Table 1, the distributions of treatment assignment are not statistically distinguishable from uniform, suggesting that randomization held for the observed sample.

them as choosing neither of the three outcome options), preferring to err on the side of slightly more measurement error in the outcome (and therefore variance) than systematic bias.

In our analysis, we collapse the outcome variable into these three relevant response categories. Folding over our outcome this way has the important benefit of improving estimation efficiency by reducing the total number of outcome categories considered and allowing us to pool results from vignettes where the Claimant won and ones where the Respondent won. We therefore focus on estimating the average effect of being appointed by the winning versus the losing party, averaging over the separate Claimant and Respondent effects. [10]

The affiliation bias hypothesis suggests that arbitrators who were told that they were appointed by the winning party will be more punitive towards the loser in terms of cost allocation than those who were told they were appointed by the losing party. To analyze the effect of being appointed by the dispute winner versus the loser, we simply take the difference in means for each of the three response indicators of interest between arbitrators assigned to each treatment condition.[11] Because treatment is randomized, these differences-

[10] While it would be possible to estimate separate effects for Claimant compared to Respondent appointees, we have insufficient power to meaningfully detect much of a difference between the two. To obtain greater precision, we find it more appropriate to estimate a single "pooled" winner vs. loser effect given the constraints of the sample size.

[11] With a trichotomous outcome variable, another approach commonly used by researchers is to assume a model for an underlying latent variable that is then observed in a coarsened form. These ordered dependent variable models (such as ordinal logit or probit), however, would require us to make additional functional form assumptions relating the covariates to the outcome. Moreover, the proportional odds assumption for ordered logit may not be satisfied in this case as the effect of treatment is not constant across units. Our results suggest that arbitrators that would choose to split costs are unaffected by treatment, but those who would choose reimbursement are moved to select a more punitive option. Because we have an experiment, any outcome modeling approaches (such as a logit model) are unnecessary at best and misleading at worst. See Volfovsky, Alexander, Edoardo M. Airoldi, and Donald B. Rubin. "Causal inference for ordinal outcomes." arXiv preprint arXiv:1501.01234 (2015) for more on the challenges of estimating causal effects with multichotomous outcome variables.

in-means are unbiased estimates for the causal effect of the appointer treatment on the probability that an arbitrator would select a particular response category. Figure 2 plots the estimated treatment effect of winner versus loser appointment on the three outcome categories. On average, arbitrators were about eighteen percentage points more likely to award all costs to the winning party when they were appointed by the winner rather than the loser. However, assignment to the winning party did not appear to change arbitrators' propensity to cost-shift in the first place. When an arbitrator chooses to follow the unwritten rule of "costs follow the event," the winner's appointee is more likely to punish the losing party by having them reimburse *all* of the winner's costs while the loser's appointee is more likely to protect their appointing side by having them pay only *some* of the winner's costs. This is consistent with the affiliation bias story. While arbitrators do not completely advance their appointing party's interests, when room for discretion arises, they appear to be more likely to choose outcomes that are more favorable to the side that appointed them.

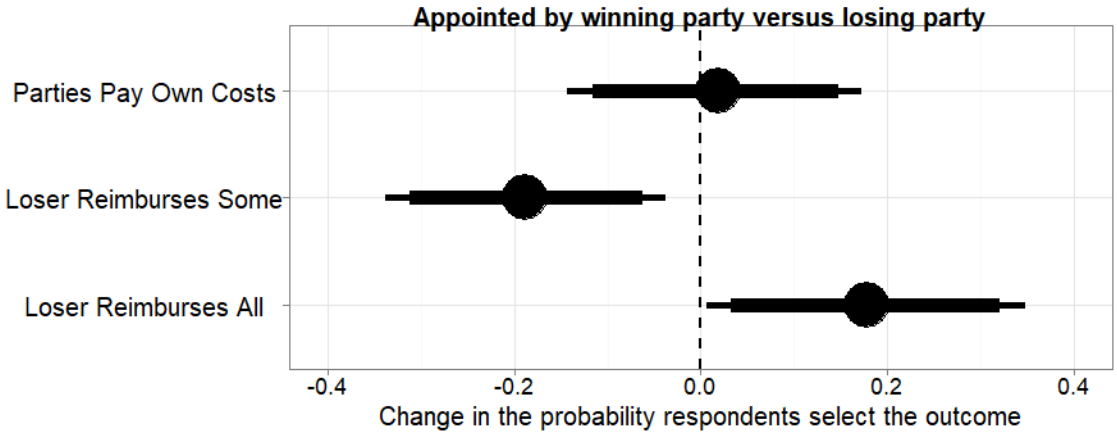


Figure 2: Estimated average effects of winning party appointment – Experiment 1[12]

[12] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 72 appointed by winning party, 55 appointed by losing party.

It is worth noting that these results are likely a very conservative test of affiliation effects. Arbitrators taking the survey are not actually participating in a months-long proceeding that may reinforce party allegiances, nor do they face any potential costs or benefits to how they rule in the vignette. The bias we are able to detect is purely implicit and inherent in the 'role' assignment. Moreover, if arbitrators are sensitive to the stigma of being perceived as not impartial, then they would likely try to attenuate any overt expressions of bias in their responses. If there is any form of social desirability bias influencing survey arbitrators, then it would likely bias the experiment against finding a difference between different treatment conditions. Nevertheless, we find strong evidence for a party affiliation effect.

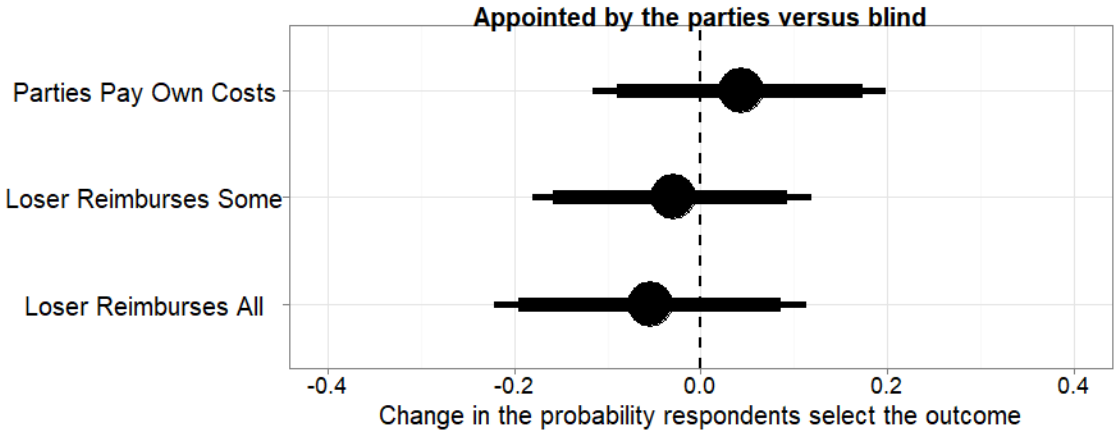


Figure 3: Estimated average effects of party appointment versus blind appointment - Experiment 1[13]

Additionally, we wanted to consider whether blind appointees behave differently from those appointed jointly by the parties. Figure 3 plots the estimated differences-in-means for each cost allocation option—between the arbitrators who were told they were appointed by the parties and those who were not given any appointer information: the blinded

[13] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 67 appointed jointly by the parties, 63 appointed blind.

condition. While in our sample, party appointees were slightly more likely to not cost shift, this difference is not statistically significant at any conventional rejection level. Overall the magnitude of any difference between these two groups appears to be quite small, suggesting that blinded arbitrators are likely to behave much like the joint party appointees when given discretion over some allocation between the parties. This suggests that blinding could have the benefits of having the parties agreeing to an arbitrator without facing the costs associated with such agreement.

One concern with the original experiment that was raised by a reviewer is that it may be difficult to generalize the observed behavior in response to a vignette on cost to real world arbitral decisions on actual questions that implicate the substance of the case. Because fees are a comparatively less significant component of the decision relative to the merits and damages and arbitrators are much less constrained in how they choose to award costs, it may be that observed biases dissipate when arbitrators consider questions of greater importance to the case. While our argument for choosing to look at costs is precisely that the absence of constraints (*i.e.,* discretion) is most likely to generate affiliation bias and it is important to note that legal costs are not an insignificant component of the parties' overall expenses in a dispute, we did consider it valuable to determine whether our result is robust to different choices of legal questions.

Therefore, we conducted a replication study in March of 2017 designed to evaluate how arbitrators would reason through a decision on damages, that is, how much should the Claimant be awarded given that the tribunal has found the Respondent at fault. As in the first experiment, we chose to ask arbitrators to decide on a discrete question on damages to keep the vignette within reasonable length and the task as clearly defined as possible.

Asking arbitrators to evaluate the entirety of an actual case would still be unreasonable in the time and space allotted for the experiment. However, a decision on damages provides a tougher test of the affiliation bias hypothesis. Arbitrators certainly face greater legal constraints and actual precedential guidance when evaluating how much a successful Claimant should be awarded in compensation from the Respondent. Moreover, the amount of damages received by a Claimant is certainly a central component of any arbitral dispute and highly salient to the litigating parties. Therefore, this vignette serves as an initial test of how well our affiliation bias finding carries over more substantive decisions that comprise an arbitral award.

Figure 4 outlines the vignette we used in this second experiment. In this vignette, a similar number of participants were presented with a similar set-up as in the first experiment. Arbitrators were told that the tribunal to which they were appointed had decided unanimously that the Claimant investor was entitled to damages due to the Respondent state's violation of the 'fair-and-equitable-treatment' standard of a hypothetical treaty. They were then tasked with deciding how much the Claimant should be owed in damages in terms of two possibilities: one proposed by the Claimant and another proposed by the Respondent in the case. The key legal question arbitrators had to evaluate was whether the Claimant should be awarded compensation for lost future profits (the Claimant's argument) or whether the award should be based exclusively on the liquidation value of the firm (the Respondent's argument). According to the vignette, both parties cited relevant ICSID precedents for their positions. For a frame of reference, we included a link to the precedents and an article describing the typical award amounts in ICSID disputes. As with the first vignette, our goal was to provide enough information that

arbitrators would feel comfortable providing some answer but not so much information that there would be zero variation in arbitrators' responses. We also re-tested our original question on costs.

Because our interest was in replication, in this vignette we only manipulated two elements: the appointing party and the amount of the submission on damages. As before, all treatments were randomly assigned independently of survey arbitrators' characteristics. However, to increase statistical power for detecting the affiliation effect, we chose to assign two-thirds of arbitrators to a party-appointed condition (rather than one-half as in the first experiment). We included the manipulation on damages to evaluate whether differences in the magnitude separating each of the parties' proposals affected the choice of damages or moderated the affiliation bias effect. We wanted to ensure that our findings would not be driven exclusively by the particular values we chose for the Claimant's and Respondent's submissions. However, we found no evidence for effect modification or a statistically significant effect of the size of the proposed damages on the probability the arbitrator would choose either the Claimant's or Respondent's position. Table 2 summarizes the number of observations assigned to each treatment condition.

Table 2: Summary of number observations assigned to each condition – Replication experiment (N=248)

Variable	Conditions			
	Appointed by the Respondent	Appointed by the Claimant	Appointed by the Parties	Blind Appointment
Appointer	88 (90)	79 (79)	45 (45)	36 (38)
	US$25,001,050.00	US$12,500,525.00	US$6,250,262.50	
Respondent's proposed damages	85 (86)	70 (73)	93 (93)	

Notes: Counts denote the number of observations among respondents who answered the question on damages. Counts in parentheses denote the number of observations among respondents who answered the question on costs. Chi-squared tests for the marginal counts across all four conditions fail to reject the null that the counts are generated by the distributions we specified for randomization ($p > .10$).

Imagine an investor-state dispute being conducted under the 2006 Arbitration Rules of the International Centre for Settlement of Investment Disputes (ICSID). The Claimant investor alleged that the Respondent state violated the provisions of a bilateral investment treaty to which the Respondent is a party. Among other arguments, the Claimant argued that the Respondent violated the treaty's fair and equitable treatment provisions and mistreated the Claimant's investment. You were appointed **to the Tribunal [by the Respondent.]/[by the Claimant.]/[by the Parties.]/[.]** After careful consideration of the facts of the case, the tribunal (you and your fellow arbitrators) unanimously decided that the Respondent unfairly treated the Claimant's investment in violation of the treaty and that the Claimant is entitled to compensation.

You are now asked to decide on the amount of damages owed to the Claimant by the Respondent. The parties have agreed that the tribunal's task is simply to pick one of the two positions of the parties' experts and decide how the expenses should be apportioned in this dispute. In its relevant part, the bilateral investment treaty provides as follows:

1. A Tribunal may award monetary damages and any applicable interest, only.
2. A Tribunal may also award costs in accordance with the applicable arbitration rules.
3. A Tribunal may not order a Party to pay punitive damages.

The Claimant has argued that they should be compensated for lost future profits that would have been realized had the measure not taken place plus interest. The Claimant justifies this claim on the grounds that the enterprise operated profitably for a period of almost three years prior to the violation. The Claimant cites Metalclad v. Mexico ICSID Case No. ARB(AF)/97/1 which considered a minimum presence of at least two or three years necessary for an award of future profits. The Claimant's expert has calculated damages for US$50,002,100.00 based on the discounted cash flow value of the expected returns from the Claimant firm's ten-year investment plan. The Respondent has argued that the enterprise had not operated for a sufficient period of time to establish itself as a "going concern" and that the ability of the enterprise to generate future earnings was uncertain and compromised. The Respondent cites Tecmed v. Mexico ICSID Case No. ARB(AF)/00/2, arguing that the tribunal in that dispute ruled that the Claimant's operating history of two and a half years was insufficient to establish enough objective data on profitability to apply a discounted cash flow analysis. Therefore, any estimate of future profits would be highly speculative. The Respondent instead proposes that damages should be based on the liquidation value of the firm and their expert has calculated damages for **[US$25,001,050.00, US$12,500,525.00, US$6,250,262.50].**

In ICSID disputes, the average award for Claimants who are awarded damages is about US$45.6 million. The median award is US$10.9 million (See: Franck, S. D., & Wylie, L. E. (2015). Predicting outcomes in investment treaty arbitration. Duke Law Journal, 65(3), 494-527.). Throughout the proceedings, both disputing parties were cooperative and the counsels for both parties behaved efficiently, professionally and ethically. The parties have not agreed on how and by whom the expenses shall be paid.

Figure 4: Sample vignette in the Experiment 2.

After reading the vignette, arbitrators were asked to choose the amount that the Respondent should pay to the Claimant in damages because of the treaty violation. We constrained our participants to choosing exclusively between the Claimant's proposal and the Respondent's proposal. In practice, investment arbitrators have freedom to choose the actual quantum awarded and rarely give the exact value proposed by either party. However, to increase statistical power in our experiment, reduce variance in the outcome, and increase the response rate by simplifying the task, we decided to limit arbitrators to two choices. This also forced arbitrators to consider the arguments of the parties instead of simply taking the shortcut of splitting the difference between the two proposals. Since we were interested in seeing whether our original results on costs replicated, we also asked our arbitrators to decide on how the parties' costs should be apportioned using the same outcome choices as in the first vignette.

We recruited our arbitrators from the same pool of e-mail addresses as in the first, removing any arbitrators who explicitly unsubscribed from the list or notified us that they were not interested in participating in any surveys.[14] A total of 644 arbitrators consented to participate in the survey. However, as before not all arbitrators finished the survey. Only 248 of the arbitrators who began completed the survey and responded to the question on damages.[15]

[14] A total of 25,965 recruitment e-mails were sent out in the second wave. As in the first experiment, many addresses were inactive or unavailable. Because we did not store identifying information from the first wave, we could not exclude arbitrators from the first experiment. However, we do not think that there are likely any interference effects between experiments given the year and a half-long gap between them.

[15] 252 arbitrators finished the survey and answered the question on costs.

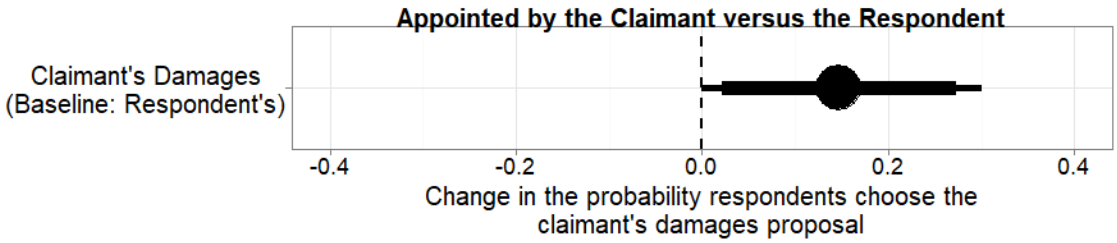


Figure 5: Estimated average effects of Claimant versus Respondent appointment on damages award – Experiment 2[16]

Figure 5 plots the estimated effect of being appointed by the Claimant (the dispute winner) versus the Respondent (the dispute loser) on the probability of choosing the Claimant's (high) proposed damages. On average, arbitrators appointed by the Claimant were about 15 percentage points more likely to choose the Claimant's damages proposal compared to arbitrators appointed by the Respondent (p = .055). While the result is just barely insignificant at the .05 level, we can reject the null at just about any slightly higher rejection threshold. The evidence from the follow-up is strongly suggestive that affiliation bias remains even for more substantive questions and beyond simply decisions on costs. However, it may be the case that the effect is attenuated somewhat by reduced discretion and greater precedential constraints associated with decisions on damages versus costs. A study with greater sample size and power would be needed to assess the magnitude of any such difference. As before, there does not appear to be a sizeable difference between blind and joint-party appointees in their choice of damage awards (Figure 6).

[16] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 88 appointed by the Respondent, 79 appointed by the Claimant.

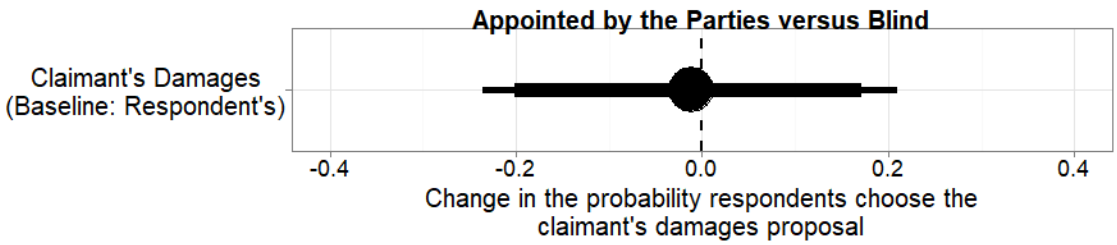


Figure 6: Estimated average effects of joint party v. blind appointment on damages award –Experiment 2[17]

A promising result from our replication is that surveyed arbitrators' answers to the costs question are nearly identical to those from our original study. In the replication, we only exposed arbitrators to a "Claimant wins" condition (while in the original study most outcomes were conditions where the Claimant failed to obtain an award). Nevertheless, as shown in Figure 7, arbitrators appointed by the winner (Claimant) were about fifteen percentage points more likely to have the Respondent reimburse all of the Claimant's costs. Again, the affiliation bias drives the magnitude of the cost award, but does not affect arbitrators' initial decision on whether costs should follow the event.

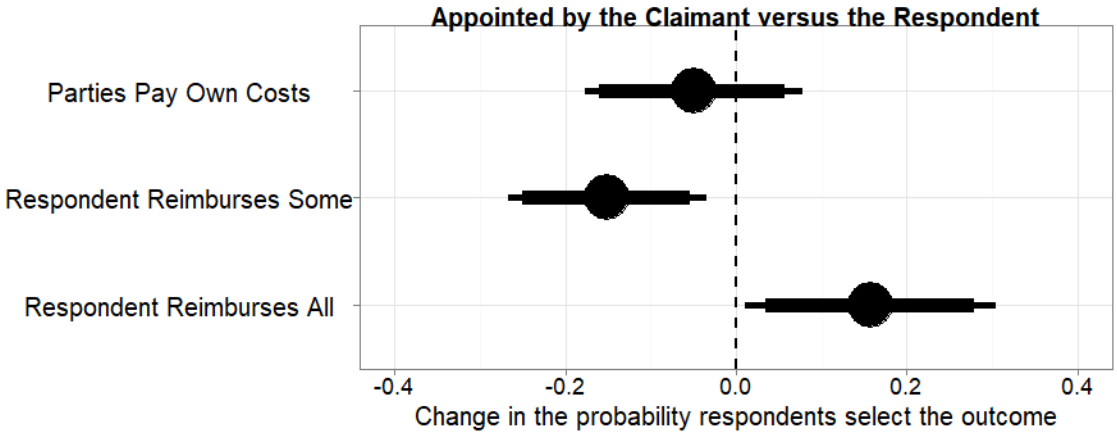


Figure 7: Estimated average effects of winning party appointment – Investor-state experiment 2[18]

[17] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 45 appointed by the Parties, 36 blind appointments.

[18] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 90 appointed by Respondent, 79 appointed by Claimant.

Finally, we found in the replication experiment that blind appointees differed somewhat from joint appointees in how they apportioned costs (Figure 8). Joint appointees were significantly less likely to choose to have the Respondent reimburse all the Claimant's costs relative to blind appointees. While we saw weak evidence of this in the first experiment, the difference between the two groups is much larger in the replication. However, because the sample size allocated to the blind and joint appointee conditions is smaller (since we wanted more power to detect the affiliation bias effect), it is important to not read too much into this finding. In general, with smaller samples, we can expect more variable and potentially more extreme estimates. Nevertheless, this surprising result does suggest one possible caveat to the observed similarity between blind and joint appointees that we observed in the first experiment – when one party wins outright, joint party appointees may have an aversion to extreme cost awards and favor a slightly more middle-of-the road compromise relative to blind appointees.

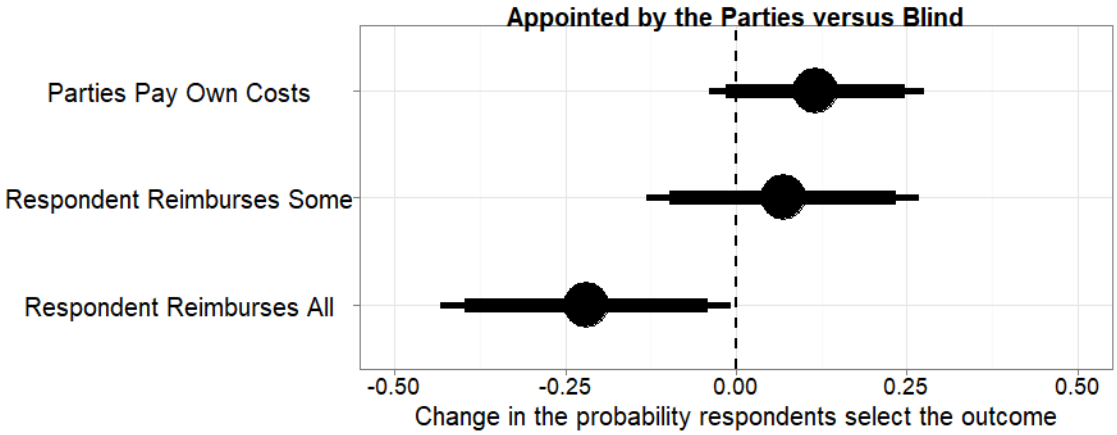


Figure 8: Estimated average effects of party appointment versus blind appointment Experiment 2[19]

[19] Thin lines denote 95% bootstrapped confidence intervals (5000 iterations). Thick lines denote 90% bootstrapped confidence intervals. Number of observations: 45 appointed jointly by the parties, 38 appointed blind.

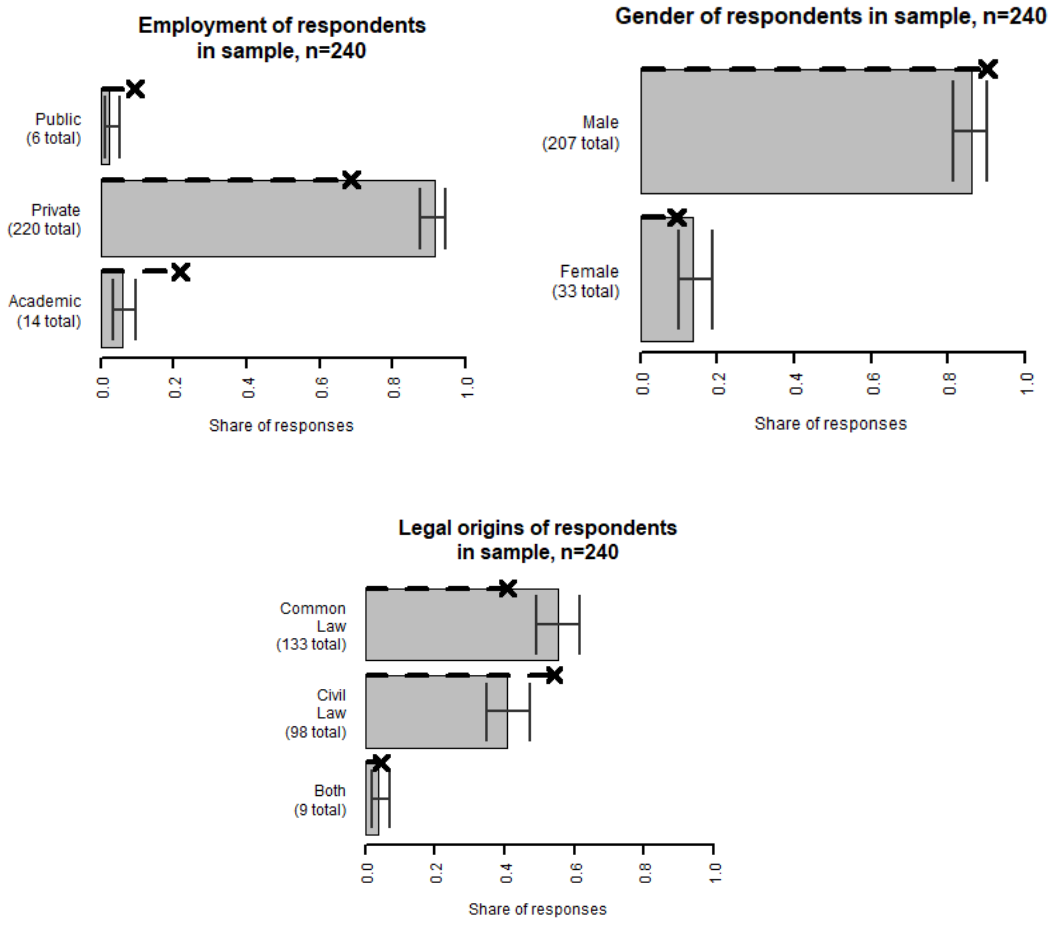
We summarize the results of the experiments in Table 1. Overall, the evidence across both experiments strongly suggest a meaningful affiliation effect when arbitrators are tasked with allocating some amount between the parties—a very fundamental aspect of arbitration. When given heavy discretion, as is the case for cost awards, party appointees tend to give the party that appointed them a more favorable outcome. Winning party appointees demand more from the loser while losing party appointees try to mitigate their appointer's losses.

Table 3: Summary of treatment effects of party appointments.

Treatment Conditions	Experiment 1: Award on Costs		
	Parties pay their own costs	Losing party pays some of the winner's costs	Losing party pays all of the winner's costs
Appointed by winning party v. losing party	0.019 (0.080)	-0.188* (0.076)	0.178* (0.088)
Appointed jointly by the parties v. blind	0.043 (0.081)	-0.030 (0.076)	-0.054 (0.086)
Observations: Appointed by Winner: 72, Appointed by Loser: 55, Appointed by the Parties: 67, Blind Appointment: 63			
	Experiment 2: Award on Damages		
	Claimant's damages proposal		
Appointed by winning party (Claimant) v. losing party	0.147+ (0.076)		
Appointed jointly by the parties v. blind	-0.011 (0.112)		
Number of observations: Appointed by Winner (Claimant): 79, Appointed by Loser (Respondent): 88, Appointed by the Parties: 45, Blind Appointment: 36			
	Experiment 2: Award on Costs		
	Parties pay their own costs	Losing party (Respondent) pays some of the winner's costs	Losing party (Respondent) pays all of the winner's costs
Appointed by winning party (Claimant) v. losing party	-0.050 (0.066)	-0.151* (0.060)	0.157* (0.075)
Appointed jointly by the parties v. blind	0.117 (0.080)	0.070 (0.101)	-0.219* (0.109)
Observations: Appointed by Winner (Claimant): 79, Appointed by Loser (Respondent): 90, Appointed Parties: 45, Blind: 38			
Point estimates denote differences in the share of arbitrators choosing each outcome. Bootstrapped standard errors in parentheses (5000 iterations). * = p < .05, + = p < .10			

D. External Validity and Sample Composition.

The experimental design allows us to isolate the treatment effect from selection biases common to observational analyses. Our estimates have high "internal validity". We are confident, by design, that the observed association is not due to unobserved factors. Moreover, we obtained a large sample of a very selective group of legal actors—to our knowledge the largest set of experiments conducted in this population. However, it is also important to assess the "external validity" of our estimates—the extent to which our findings can be generalized to the population of interest.



Crosses denote proportion among ICSID arbitrators 2010-2015. Lines denote 95% confidence intervals.

Figure 9: Characteristics of arbitrators in experimental sample.

In the case of our experiment, we want to generalize how arbitrators behaved in response to our vignette to the population of arbitrators serving on investment arbitration tribunals— the context of current debate and the experiment. If non-response to our survey were random, then we could treat the observations as a random sample from the true population and our sample average treatment effect is an unbiased estimate of the population average treatment effect. Even in the case that non-response is correlated with arbitrators' characteristics, our experiment still yields an unbiased estimate of the sample average treatment effect. This still captures the average effect of treatment within a theoretically interesting sub-population within the total set of arbitrators. While testing for non-random non-response is impossible, we assess how representative our sample is by comparing the background characteristics provided to us by survey arbitrators to those in the population of interest.

We consider three sets of covariates that we were able to measure for a large subset of our arbitrators: gender, country's legal tradition, and employment background. Of our 257 arbitrators from the first vignette, 240 also provided information on these covariates. We then obtained a list of all 188 arbitrators that served on an ICSID tribunal constituted between 2010 and 2015 to act as our target population. For each of these variables, we calculated the proportion of arbitrators in each category and compared these target proportions to the proportions within our sample. Figure 9 compares the two covariate distributions.

Overall, the distribution of ICSID arbitrators in recent cases is predominantly male, consistent with prior accounts of the lack of women in arbitration appointments. Only 9.5 percent of arbitrators who served on at least one ICSID tribunal in the 2010-2016 period

were women. This skew is also evident in our sample, which is very close to the population distribution. However, our sample does contain a slightly larger share of women arbitration experts—about 14 percent. Nevertheless, it is unlikely that this small discrepancy is sufficient to give misleading estimates, particularly as we find no statistically significant difference ($p > .1$) in treatment effect magnitude between men and women.

Arbitrator nationality is the second variable we considered. Consistent with empirical accounts of the distribution of arbitrator nationalities (*e.g.* Puig, 2014), the vast majority of arbitrators were nationals of European or North American countries. However, a fair number of arbitrators were also from of South American, Asian or African countries. Our sample is not exclusively comprised of nationals from a single state, which bodes well for the generalizability of our findings. One concern for our sample is that because the survey was administered in English, we may be more likely to get arbitrators from English-speaking countries and, in particular, countries with English common law traditions. This could be an issue if arbitrators trained in different legal cultures approach the question of cost allocation differently. Indeed, the principle that "costs follow the event" or "loser pays" is extremely well-entrenched in some countries of the common-law tradition (Woodroffe, 1997).

Determining each arbitrator's legal training is challenging given that nationality is not a perfect proxy—many arbitrators attend foreign law schools to obtain a masters or doctorate abroad after an original law degree. However, for the data that we were able to collect, it does appear to be a reasonable initial proxy. For each arbitrator who reported their nationality or nationalities we coded whether that country's legal system has a

common or civil law tradition using the dataset of La Porta et. al. (2008).[20] We did the same for the 188 arbitrators in our actual target population. Among ICSID arbitrators, slightly more than half come from exclusively civil law backgrounds while about 41 percent had common law backgrounds.

Within our sample, these proportions are essentially reversed, suggesting that arbitrators with English common law backgrounds were a bit more likely to respond to our survey. However, the magnitude of this difference is not particularly large. We also do not find evidence of effect heterogeneity for arbitrators with common versus civil law backgrounds. Testing for an interaction between our treatment does not yield a difference in effect that is statistically distinguishable from zero at commonly used thresholds ($p > .1$). While this does not disprove the possibility of effect heterogeneity, it does suggest that our observed evidence for bias is unlikely to hold for only one major legal tradition. Because any effect heterogeneity is likely small or non-existent, the difference between arbitrators' legal origins in the sample and in the population does not meaningfully impact external validity.

Finally, we consider our arbitrators' career backgrounds and expertise. Respondents to our survey had four options to indicate their current area of employment: private law, academia, government, or other. These categories were chosen to reflect the most common career backgrounds of international arbitrators as found by Costa (2011). Arbitrators who answered "other" were re-coded into one of the three remaining categories based on their open-ended response. Most arbitrators answering 'other' described themselves as independent arbitrators and were recoded as being in the private sector. A few also noted

[20] We treat La Porta et. al. (2008)'s coding of "socialist" legal systems as another form of a civil law system.

employment in an international organization and were recoded as public-sector employees (along with those selecting "government"). We also coded all 188 ICSID arbitrators in our target population into one of the three career categories based on their most recent area of employment using publicly available information (e.g. websites, CVs).

As shown in Figure 9, the majority of arbitrators work in the private sector. While this is also the case among ICSID arbitrators, a larger share of the target population originates in academia or the public sector. About 92 percent of our arbitrators indicated private sector employment compared to 69 percent in the ICSID group. While this does suggest a potential limitation of our sample, it is the case that arbitrators with backgrounds in private sector comprise the largest sector of the overall international arbitration pool. Even if there is a difference in the response of private sector arbitrators relative to those employed in other areas, our experiment credibly identifies the treatment effect for a highly meaningful sub-group of arbitrators—those employed in the private sector. Moreover, as with the other two factors considered, we do not find strong evidence that treatment effects differ between private sector and non-private arbitrators. Therefore, despite the over-representation of private sector arbitrators, our sample does not appear to be particularly idiosyncratic in a way that could give misleading results compared to the international arbitration population at large.

IV. Blind Appointments in Arbitration: Possibilities and Challenges

Despite the current skepticism by commentators (Duarte, 2012; Giorgetti, 2014), our evidence suggests that blinding could be an effective debiasing alternative to correct the observed affiliation effects that result from the current practice of unilateral party appointments. Our methodological contribution shows that the pro-appointer attitudes

among arbitrators cannot be explained solely by selection effects. Therefore, blind appointments would very likely help to mitigate the overall level of party bias in arbitration.

There are other benefits with blind appointments. Chiefly, parties maintain the ability to make unilateral party appointments, one of the oft-cited advantages of arbitration compared to more conventional forms of litigation. Moreover, the practice of blinding need not be implemented solely for unilaterally party-appointed arbitrators but can be easily extended to all members of an arbitration tribunal. For the litigating parties, it may be possible to infer which side made each appointment (as one is known to each), but individual arbitrators will have significant uncertainty over the source of their appointment, having to consider the possibility not only of being a Claimant or a Respondent appointee, but also the potential of being a joint appointee or an appointee by the neutral arbitral institution.

To be sure, some limitations of our experimental approach are clear as there are significant implementation challenges to blinding in practice. Specifically, our experiment is unable to assess how exactly implicit biases of individual arbitrators affect outcomes of the collective body. Prior research on this question suggests that in deliberations, biases tend to be reinforced—an effect known as the "bias accentuation effect" (Fiedler & Kutzner, 2016). Hence, the implicit biases of the two party-appointed members may not simply cancel one another out as often assumed, but rather become further entrenched throughout the arbitration process. As to the implementations challenges and limitations of blinding, in this final section we provide some final remarks.

A. Implementation Challenges.

Best practices and professional norms can be developed to encourage blinding without introducing radical changes to the current practice of arbitration. However, among the most important limitations is the administration and enforcement of such a practice. Duarte (2012) notes that the "main problem with [blind appointments] is that it is not so reliable in practice, where it would be extremely easy to find out which party appointed who." The most obvious issues stem from the fact that professional norms still consider it proper to interview the arbitrators in advance of the nomination (although some arbitrators will refuse a pre-appointment interview) (Bishop & Reed, 1998; Carter, 2000). This is understandable given that substantial sums of money might rest in the hands of arbitrators. In fact, with limited tools to assess potential decision-makers (CVs, website information, or word-of-mouth), and without access to substantial decisions to assess judicial philosophy (especially in fields such as commercial arbitration) or, many times, guiding precedent, it may be advisable to allow some form of limited exchange between parties and potential arbitrators.

This particular challenge, however, should not be insuperable. For one, potential parties to contractual disputes can include specific guidelines in arbitration clauses on how to conduct such interviews without frustrating blind appointments. Parties could agree not to interview arbitrators at all or to agree to an interview phase that allows both parties to conduct preliminary interviews on all potential, previously identified candidates. Once each party has made a decision on the appointee, arbitrators can be notified with a joint communication from the parties to the litigation or from the arbitral institution, seeking at all times to ensure that nominees do not know which party nominated them.

Arbitral institutions can also issue appointment policies that address 'blind' interviewing. In fact, professional best practices on interviewing arbitrators are just emerging and could start including direction on how arbitral institutions can act as intermediates between the litigating parties and the potential nominee to prevent any indiscretion (CIArb, 2011). Arbitral institutions can even facilitate the infrastructure for enabling the interviews that prevent the nominee from recognizing the interviewing party and counsel (*e.g.,* use of an intermediary or written interrogatories).

A second obstacle is that some arbitration rules provide that the institution disclose the particular method of constitution of the tribunal (ICSID 2006, Reg. 23). What exactly should be disclosed can be subject to interpretation. While there are reasons to opt for a broad interpretation (*e.g.,* transparency of the process) we see very little value in arbitrators knowing the specific party making the appointment, because the arbitrator's ethical and professional obligations are vis-à-vis both parties—not only the nominating party (*e.g.,* International Bar Association (2014), General Standard 1). Moreover, such information could be concealed until the final decision is issued if transparency is a concern.

Finally, a more mechanical matter: arbitral institutions must transmit to each member of the tribunal any communication received from either party. In doing so, institutions could ensure that in transmitting information to the members of the tribunals the information on the source of appointment is redacted. Operational policies could even be established across arbitral institutions for requiring appointments made in a separate confidential document that is not shared with the nominees.

B. Limitations of Blinding in Arbitration

If implemented successfully, blinding could ameliorate affiliation effects. However, it remains to be seen whether that reform will substantially reduce the partisanship observable in many settings of arbitration like investor-state arbitration. There are many reasons for that, but the main one is that selection effects would still persist under blinding. Litigants are aware of the preferences of specific arbitrators, especially of repeatedly appointed arbitrators, and appoint this core group precisely because they are more predictable and effective in signaling a particular position, by, for example, voting or dissenting (Puig, 2016). In fact, some arbitrators might prefer to signal their political preference clearly via their decisions—hence less willing to compromise and less reluctant to dissent when in disagreement with the majority.

As a more general point, blinding as a solution to bias should always take context into account. Some arbitration proceedings like investment arbitrations take place in a close-knit community of legal actors and repeat-players who interact routinely. In such contexts, arbitrators rely heavily on social capital and individual reputations to remain a part of the community. Once members of the profession develop a reputation, this information is passed on and translates into other nominations precisely because of such leanings. Hence, while the thick social structure of the arbitral community may be valuable for the implementation of a blind appointment proposal by facilitating the dissemination and development of new professional norms, it may also create additional hurdles by accentuating the selection bias and confidentiality challenges. Nevertheless, in the long run, blinding might help move arbitration toward a more rule-based system.

V. Conclusion

Within the field of arbitration, the frequent use of party-appointed arbitrators is likely to result in litigant-induced biases. Disentangling selection effects (parties appointing friendly arbitrators) from affiliation effects (arbitrators changing behavior in response to their appointment) is particularly challenging using observational data. While the former is inherent in the system of party appointment, the latter could be ameliorated by "blinding" arbitrators—restricting party appointees from knowing the source of their appointment throughout the arbitration proceedings.

Through novel survey experiments, we provide a methodological solution to the problem of measuring affiliation effects in a world confounded by selection effects. As our survey results show, assignment to being appointed by one of the parties in a dispute directly changes the behavior of arbitrators. Hence, the appointment itself is the cause of some of the bias towards one's appointing party. Apparent patterns of bias in real decisions are unlikely to be simply an effect of parties filtering their appointees on the basis of known, prior attitudes. Normatively, these results provide support for implementing "blinding" within arbitration proceedings. While the implementation challenges do not seem insuperable, the effectiveness of blinding may also be heavily dependent on the context of arbitration. Hence, our article should be a precursor for assessing affiliation bias and the impact of blind appointments in more realistic settings. The proposed methodology—we believe—is a significant contribution to exploring questions on judicial politics and the psychology of decision-maker biases.

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