Winning or losing in investor-to-state dispute resolution: the role of arbitrator bias and experience

Winning or Losing in Investor-to-State Dispute Resolution:

The Role of Arbitrator Bias and Experience

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Abstract: When an investor sues a state for alleged breaches of its obligations under an investment treaty or a trade agreement with investment provisions, all that should matter for who wins the case are the merits of the claim itself. Alas, investor-to-state dispute settlement (ISDS) does not take place in a vacuum. Such cases are decided by a tribunal typically consisting of three arbitrators, one each nominated by the two parties while the president is mutually agreed upon. We demonstrate that the kind of involvement of these arbitrators in previous ISDS cases matters for the case under dispute. Specifically, we show that what we label the president's pro-investor appointment bias – the number of times they have previously been nominated by an investor minus the number of times they have represented respondent states – raises the likelihood that an investor wins an ISDS case. The same holds for the pro-investor appointment bias of state-appointed arbitrators. Given the president's crucial role, the main implication of our findings is that presidents should be drawn from among those who have not systematically represented more one side than the other in previous cases.

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1. INTRODUCTION

The widely perceived legitimacy crisis of investor-state dispute settlement (ISDS) is often attributed to ad-hoc arbitration tribunals established under the auspices of institutions such as the International Centre for Settlement of Investment Disputes (ICSID), which is part of the World Bank in Washington, DC. Public debate, notably in Europe, suggests that these tribunals are dominated by self-interested arbitrators operating under opaque circumstances. As noted by Rogers (2014: 226), "critics hypothesize that investment arbitrators favor their appointing party in a self-interested effort to increase the likelihood of future appointments." More problematically still, the suspicion is that private arbitrators with considerable interest in serving corporate clients favor the claimants and take position against respondent states (Pauwelyn, 2015). In other words, ISDS is suspected to systematically favor investors over respondent states. Respondent states with relatively low per-capita income and poor governance are expected to be in a particularly weak position when multinational corporations bring alleged breaches of commitments made in international investment treaties to 'private' arbitration (see Behn et al., 2017 for a discussion of the economic development bias hypothesis).[1] Behn et al. (2017) themselves find a pro-developed state rather than anti-developing state bias in their analysis and also find that if poorer states on average lose cases this is because such states often have poor governance which is the more important predictor of losing the case than their development status.

By contrast, little to no attention has been paid to the prior experience and what we later on define as the appointment bias of arbitrators in ISDS cases. What analysis there is has instead mostly focused on their gender (the vast majority of them are men) or whether they come from developed countries (the vast majority of them do), none of which we find persuasive as explanatory factors. In order to fill this gap, we make use of rich ISDS-related information collected

by UNCTAD for 739 investment disputes (as of end-2016). The database informs not only about tribunal decisions in favor of private investors or respondent states; it also names the arbitrators handling the specific case. We use the case-specific information on the composition of tribunals to test the hypothesis that what we label 'biased' arbitrators – those who systematically served the interests of one particular side in past cases – shape the outcome of ISDS. In particular, we hypothesize that 'biased' presidents of arbitration tribunals increase the probability of investor wins in ISDS. We also hypothesize that the two parties can improve their chances of winning by appointing arbitrators who are more biased toward, respectively, respondent states or investors (have served more in previous cases on the same side as the one who is appointing them now) and by appointing arbitrators who are more experienced (have served on more previous cases, independently on which side).

After reviewing the related literature in Section 2, we derive our hypotheses in Section 3. Section 4 introduces our empirical model to test these hypotheses. Our estimation results are presented in Section 5. We find that the presidents of arbitration tribunals play an important role for ISDS outcomes. The probability of investor wins increases when presidents are 'biased' in the sense of having predominantly served as claimant-appointed arbitrators in previous cases. However, this effect becomes weaker, and in fact disappears, if state-appointed arbitrators are extremely experienced, i.e. have been involved in ISDS very frequently before, or if they have an appointment history that extremely favors respondent states, i.e., if they have previously represented respondent states many times more than investors. The probability of investor wins also increases if respondent states appoint arbitrators that have served more claimants than states in the past. Finally, there is some evidence that investor wins become more likely if arbitrators appointed by claimants are more experienced but there is too much noise in the data for us to be

able to reject, on statistical grounds, the null hypothesis of no effect across all robustness test models. Section 6 concludes with discussing policy implications of our analysis.

2. RELATED LITERATURE

It has become common that international investment agreements – bilateral investment treaties (BITs) as well as plurilateral trade and cooperation agreements containing investment chapters – include binding provisions on investor-state dispute settlement (ISDS). These provisions allow private foreign investors to evade national courts in the host countries and instead revert directly to international arbitration by ad-hoc tribunals, e.g., established under the auspices of the ICSID, in order to raise claims for financial compensation against host-country governments that have allegedly broken treaty obligations.

ISDS provisions were widely regarded as a 'technical' issue until they first received public attention in the wake of controversial ISDS decisions under the umbrella of the North Atlantic Free Trade Agreement (NAFTA) (Neumayer, 2001). A massive surge in public attention followed the heated debate on broad-based cooperation agreements such as the Transatlantic Trade and Investment Partnership (TTIP). In academic research, the major question was whether ISDS provisions could help attract foreign direct investment (FDI) to host countries in which deficient national institutions appeared to be unable or unwilling to ensure the rule of law.[2] The empirical evidence on whether 'legal delegation' (Allee and Peinhardt, 2010) to international arbitration tribunals induces higher FDI inflows has remained ambiguous.[3] At the same time, some recent studies indicate that FDI inflows are negatively affected once the host country faces compensation claims before arbitration tribunals. Allee and Peinhardt (2011: 401) find that "governments suffer

notable losses of FDI when they are taken before ICSID." Focusing on differences in the FDI response from BIT-partner and non-partner countries, Aisbett et al. (2017) find that BITs stimulate bilateral FDI flows from partner countries only as long as the host country has not previously had a claim brought against it to arbitration.[4]

According to Allee and Peinhardt (2011), host countries suffer particularly large losses of FDI when international arbitration tribunals consider the compensation claims of private claimants to be justified. Consequently, it is of considerable relevance for respondent states to know what determines the decisions of tribunals in favor of either of the two parties involved in ISDS. However, the empirical literature on the determinants of ISDS outcomes is still in its infancy, predominantly because the number of disputes with sufficient documentation was fairly small until the recent boom of ISDS cases.

Some authors argue that ISDS generally favors private claimants over respondent states. For instance, the analysis of trends in legal interpretation by Van Harten (2012: 214) suggests that arbitrators tend to adopt expansive interpretations of contentious issues of jurisdiction, thereby leaning to the position of private claimants rather than respondent states. However, Van Harten (2012) does not consider actual ISDS outcomes, i.e., tribunal decisions in favor of either of the two parties involved.

As concerns ISDS outcomes, the academic debate has mainly focused on whether respondent states at lower levels of economic development and/or with poor governance face systematically higher risks of investor wins in ISDS proceedings. According to Franck (2009: 435), the development status of respondent states "does not have a statistically significant relationship with outcome." She concludes that ISDS does not discriminate against lower-income countries.[5] Franck (2014) comes to the same conclusion when controlling for the level of democracy, as an

indicator of the quality of governance, in the respondent state. In contrast, Pelc (2017) finds that richer respondent states fare better in ISDS proceedings and argues that the striking difference to Franck (2014) may be due to the larger sample of ISDS cases.[6]

Previous empirical research has paid limited attention to the role of arbitrators in shaping ISDS outcomes. This represents an important gap since it has been suspected that the defining characteristics of ISDS proceedings, notably the "asymmetrical claims structure and absence of institutional markers of judicial independence create apparent incentives for arbitrators to favour the class of parties (here, investors) that is able to invoke the use of the system" (Van Harten, 2012: 219).

Pauwelyn (2015) and Costa (2011) focus on some personal characteristics of ICSID arbitrators, comparing them with WTO panelists. Inter alia, they find that relatively few ICSID arbitrators are from developing countries (see also Waibel and Wu, 2011). The private sector or academia represent the professional background of most ICSID arbitrators, while most WTO panelists have a governmental background. Moreover, the share of ICSID arbitrators with legal expertise (and a degree in law) is higher than the corresponding share of WTO panelists. It is also shown that "the pool or network of ICSID arbitrators is clearly more closed and dense, with a much higher repetition rate (…) than that of WTO panelists" (Pauwelyn, 2015: 774f.; see also Costa 2011).

Franck (2009) accounts for the 'development status' of (presiding) arbitrators in order to address concerns that the disproportionate representation of arbitrators from rich Western countries biases ISDS outcomes in favor of claimants who are typically based in similarly rich home states, sharing Western legal concepts and norms.[7] She concludes from her analysis of just about 50 concluded cases of ISDS that these concerns tend to be unfounded, considering the statistically

insignificant link between the development status of presiding arbitrators and ISDS outcomes.[8] Using information on 131 concluded cases, Kapeliuk (2010) provides evidence on ISDS outcomes by focusing on the decision patterns of so-called elite arbitrators, somewhat arbitrarily defined as having served on at least four ICSID tribunals. Inter alia, the descriptive statistics speak against the hypothesis that arbitrators who have been reappointed repeatedly are biased in favor of private investors.[9]

Langford et al. (2017) provide an interesting social network analysis on the 3,910 known individuals involved as arbitrators, counsel, expert witnesses or tribunal secretaries in the 1,039 investment arbitration cases they analyze. Their particular interest lies in the analysis of so-called 'double hatting', where the same people serve in the same or different roles in multiple sequential or even simultaneous cases, which raises potential conflict of interest issues. They show that double hatting "is not a common or widespread practice" but instead is "practiced so consistently by a highly visible and powerful core of some of the most influential actors in the system" (Langford et al., 2017: 328). The authors do not estimate the impact of the individuals' characteristics on arbitration outcomes, hence their study is not directly relevant to ours. However, one inference we draw for our own study is that in a robustness test we exclude cases on which arbitrators with the highest consolidated experience or appointment bias serve. This allows us to check whether our results are driven by a similar core set of a few individuals.

By accounting for arbitrators and the legal counsel of investors and states in a multiple regression analysis of the determinants of ISDS outcomes, Franck and Wylie (2015) is more closely related to our empirical analysis below.[10] Their analysis provides only weak evidence that arbitrators matter for ISDS outcomes.[11] This may be partly due to the still relatively few observations as Franck and Wylie miss the particularly large number of newly initiated ISDS cases

since 2012.[12] More importantly perhaps, the coverage of personal traits of arbitrators focuses on the tribunal's gender composition and 'development status' (as defined above), rather than the experience of arbitrators and whether they have systematically represented more one side than the other in the past. As we explain in more detail in Section 3, we address this limitation by constructing case-specific measures of the experience and what we call 'appointment bias' of all three arbitrators. In assessing the importance of these measures for ISDS outcomes, we also account for conditional effects, e.g., by interacting the personal traits of different arbitrators involved in a specific case.

3. THE IMPACT OF THE APPOINTMENT BIAS AND EXPERIENCE OF ARBITRATORS ON ISDS OUTCOMES

In this section, we develop hypotheses regarding the effect that the involvement of arbitrators in previous cases has on ISDS outcomes. To explain our reasoning, it is important to introduce readers to the database we draw on, namely UNCTAD's database on ISDS which included 739 cases by the end of 2016 (http://investmentpolicyhub.unctad.org/ISDS). The database provides case-specific information on the private claimant and the respondent state, the economic sector of the dispute, the year when the case was filed for international arbitration, the current status, and the outcome for concluded cases. While some cases of ISDS date back to the early 1990s, it was only in 2003 that the number of new disputes exceeded 30 for the first time. Throughout the period of observation, 471 cases have been concluded; 257 were still pending by end-2016.[13]

Crucially, the database provides information on the arbitrators constituting the ad-hoc tribunals. Typically, three arbitrators are involved in each case: one arbitrator is appointed by the

private claimant, another one is appointed by the respondent state, and the third serves as the president. Only if a party fails to appoint their arbitrator will the arbitrator be designated to them by the arbitrating institution (Puig, 2014).[14] The president is normally appointed by mutual agreement between the two parties though they can also be appointed by the arbitrating institution, particularly so if the two parties cannot agree on a president. Ideally, we would thus have 2,217 observations on arbitrators (739 cases x 3 arbitrators). However, 388 observations (17.5%) are missing. A large share of the missing observations (34%) concerns cases that were still pending, most probably because tribunals had not yet been established; 41% of missing observations relate to cases that were settled or discontinued, most probably before arbitrators were appointed. Importantly, missing observations are relatively few for cases decided in favor of either the investor or the respondent state.

Some of the 426 arbitrators named in the database performed all three functions – i.e., as president, as a representative of the claimant, and as a representative of the respondent state – during the period of observation. 11% of all arbitrators fall into this group.[15] However, most arbitrators specialized and performed just one function. About half of all arbitrators were active exclusively as representatives of either claimants or respondent states.

We presume that the two party-appointed arbitrators tend to serve the interests of the party they are representing. To be clear: all arbitrators, including those appointed by the parties, are required to be impartial and independent and they cannot be biased in a legal sense as otherwise they are by law required to withdraw from the case (Puig, 2014; Frank and Wylie, 2015). However, that does not mean that they are neutral or disinterested. Clearly, the parties appoint certain individuals and not others with certain expectations regarding them. Individuals can be known or suspected to tend toward investor interests or toward respondent state interests in their view of the

world and their interpretation of contested issues without them being biased in a legal sense, just like candidates for the Supreme Court in the United States and elsewhere are known to tend toward conservative or liberal viewpoints. As Kapeliuk (2010: 67) argues, an arbitrator is "often selected due to his or her perceived predisposition to a party and its legal position". More cynically, party-appointed arbitrators may also tend to serve their party's interests in order to maximize the chances that they will be appointed again by, respectively, the same or other investors or the same or other respondent states (Rogers, 2014).

Based on our presumption that the other two arbitrators tend to serve the interests of the party they are representing, we hypothesize that the presidents of tribunals play a critically important role in deciding on investor-state disputes. As Langford et al. (2017: 304) have put it: "(…) the president of an arbitration represents the most prestigious role in arbitration, possesses the most responsibility in case management, and exercises the most influence in the final decision as they are usually not appointed solely by one party". Presidents set the agenda, impact upon the style of arbitration, decide on procedural issues and mediate between the two party-appointed arbitrators (Franck, 2009: 443f.).

Arguably, presidents are most likely to be neutral toward either party when they have not served as the representative of either claimants or respondent states in previous cases. A relatively large number of presidents belong to this group (34% of all presidents; see Figure 1). However, the average number of 1.7 cases over which this type of president presided is considerably smaller than for all other types. The second largest group of 47 presidents has previously represented both claimants and respondent states in other cases. This group presided over 54% of all cases, i.e., each president in this group handled 7.1 cases on average. The remainder consists of those who have

previously represented only claimants (27) or only respondent states (34). This group handled 32% of all cases, thus presiding on average over 3.2 cases.

Based on the case-specific composition of arbitration tribunals and the history of previous cases, we define two characteristics of each arbitrator in a tribunal, namely what we call (pro-investor) appointment bias and experience. We define appointment bias as the number of previous cases an arbitrator has served as an investor's appointee minus the number of previous cases the same arbitrator has served as a respondent state's appointee, whereas experience is defined as the accumulated number of cases the arbitrator has been involved in, no matter on what side or as president.

Our particular focus is on the characteristics of arbitrators that serve as presidents in a particular case. We hypothesize that presidents who represented more often investors than states in the past (i.e., who are biased toward investors in our definition) are more likely to find the case in dispute in favor of the investor. Similarly, party-appointed arbitrators with stronger bias in favor of claimants should, all other things equal, increase the chances of the claimant winning the dispute. Exactly because arbitrators appointed by parties are chosen for a reason, we agree with Waibel and Wu's (2011) and Puig's (2014) argument that repeated appointment by one of the parties can serve as a proxy variable for their otherwise unobserved tendency to prefer one party over the other in the face of legal disputes.

As for arbitrators' experience, as Ashenfelter (1987: 342) notes, "a key determinant of the parties' preferences for an arbitrator is usually the extent of the arbitrator's 'experience' in deciding related arbitration cases."[16] More specifically, the comparison of ICSID arbitrators and WTO panelists by Pauwelyn (2015) suggests that "experience and track record" are relatively important selection criteria in ISDS, explaining the higher repetition rates of party-appointed ICSID

arbitrators. We therefore hypothesize that the more experienced the investor-appointed arbitrator is the more likely is an outcome in favor of the investor in the disputed case. The opposite holds for the experience of the state-appointed arbitrator. Moreover, we additionally hypothesize that well experienced state- or investor-appointed arbitrators can mitigate or strengthen the effect that the appointment bias of presidents has on arbitration outcomes. Their accumulated expertise should help them increase the probability that the outcome is in favor of their client despite the president's appointment bias in the direction of the investor or the state. The same goes for greater (pro-investor) bias in party-appointed arbitrators, which should reinforce any effect that president bias has. By contrast, the president's experience (as opposed to bias) is not expected to have an effect since experience itself does not suggest that a president is inherently more inclined to decide in either party's favor.

In sum, we test the following hypotheses on the effects of the arbitrators' experience and appointment bias on the outcomes of ISDS:

H1 (main hypothesis): The president's appointment bias is hypothesized to be critically important for ISDS outcomes. The decisions of arbitration tribunals are more likely to be in favor of private investors when the president has been appointed relatively more by claimants than by respondent states in the past, and vice versa.

H2: Similarly, if the party-appointed arbitrators are more biased toward the claimant this also increases the chances of the claimant winning the dispute, and vice versa.

H3: Tribunal decisions are more likely to be in favor of private investors if claimant-appointed arbitrators are more experienced and less likely in favor of private investors if state-appointed arbitrators are more experienced.

H4: The experience and bias of party-appointed arbitrators condition the effect of the president's appointment bias on ISDS outcomes. Specifically, state-appointed arbitrators with more experience and negative pro-investor bias (i.e., stronger bias toward respondent states) are expected to mitigate the impact of the president's bias in favor of private investors. Conversely, claimant-appointed arbitrators with more experience and stronger bias toward investors are expected to exacerbate the impact of the president's bias in favor of private investors.

4. DATA AND ESTIMATION MODEL

As mentioned in the previous section, we have coded our data from UNCTAD's database on ISDS. In some contrast to the impression given in public debate, of the 471 concluded cases, tribunal decisions were more often in favor of respondent states (173 cases) than in favor of private investors (125). Arguably, it was also in the interest of respondent states that 48 cases were discontinued, particularly when tribunals dismissed the case for lack of jurisdiction.[17] The remaining 114 cases have been settled among the parties of the dispute.

To test our hypotheses we code a dependent variable that is set to one for ISDS cases decided in favor of the claimant, i.e., the private investor, and zero for cases decided in favor of the respondent state as well as for discontinued cases. It is typically in the interest of respondent states when arbitration tribunals dismiss the case for lack of jurisdiction or cases are discontinued for other reasons. This coding is also consistent with UNCTAD's own classification. However, we perform a robustness test in Section 5 by excluding discontinued cases from state wins. Furthermore, we also estimate ordered logit models which consider settled cases as a third and discontinued cases as a fourth outcome category.

The appointment bias and experience of the three arbitrators handling a specific case represent our explanatory variables of principal interest. Appointment bias and experience are not systematically correlated with each other so we include both characteristics simultaneously in all estimations. To control for the generally rising trend of arbitrators' experience built into our measure and any temporal trend in appointment biases, we include period-specific fixed effects of typically three years into all estimations.[18]

In addition, we include a number of control variables capturing potentially important characteristics of respondent states as well as the home countries of the claimants. On the respondent state side, we account for the country's GDP and GDP per capita. Economically large and rich respondent states can afford to invest more in their legal defense which may lower the chances of investor wins. Since arbitration tribunals may be less inclined to decide against respondent states with high quality national institutions, we include a measure of the respondent state's rule of law. As argued by Schultz and Dupont (2015: 1160), one of the "functional effects of investment arbitration is that it serves to make up for deficient rule of law in the host state." The panel analysis of Freeman (2013) suggests that a larger number of ISDS cases are brought against countries with relatively weak domestic institutions that could have ensured property rights and the rule of law.[19]

On the part of claimants, private investors may have better chances to win when they are based in rich and large home states because such states are suspected to push for treaties with stronger investor protection (Allee and Peinhardt, 2014; Simmons, 2014). We therefore control for the GDP and GDP per capita of the country in which claimants are located. We also include two dummy variables to take into account that investor wins may be more likely when the claimant is based in the European Union or, alternatively, in one of the NAFTA member countries. Investors

based in these two country groups account for most outward FDI stocks and also for the largest shares of all ISDS cases throughout the period of observation. The governments of advanced FDI source countries in Europe and America had a particularly strong interest plus the necessary bargaining power to conclude IIAs offering strong protection of investors which could have improved their chances to win disputes. The appendix lists summary variable statistics (Table A1). Finally, to account for unobserved heterogeneity across the economic sectors in which claimants operate, we include sector fixed effects in all estimations. For similar reasons, we include dummy variables for the type of treaty that gives rise to ISDS (bilateral investment treaty, NAFTA, Energy Charter treaty, other) and for the arbitration institution that handles the ISDS case (UNCITRAL, ICSID, ICSID AF[20], other).

Given the binary nature of our dependent variable (investor wins or not), we employ logit estimation though we show in the robustness test section that our results are robust toward using probit or a linear probability model instead. Subsequently, we augment the estimation model by interaction terms in order to account for the conditional effects predicted by our fourth hypothesis. Since with non-linear estimators like the logit the existence of conditional effects cannot be reliably inferred by assessing the statistical significance of the interaction term coefficient (Ai and Norton, 2003), we evaluate these models by plotting predicted marginal effects.

As a caveat, we acknowledge that we cannot identify causality in our study based on observational data. It is possible that some factor omitted from our estimation model is correlated with our experience variables of interest and has an independent effect on our outcome variables. For example, it could be that well organized and well financially equipped investors are inherently more likely to win an ISDS case and have recourse to more experienced arbitrators.[21] We see less

reason for similar concern regarding our appointment bias variables but equally cannot exclude the possibility of spurious correlation in this regard either.

5. DESCRIPTIVE STATISTICS AND ESTIMATION RESULTS

Before we present the results from our logit estimations, we discuss some stylized facts that provide a first descriptive overview of arbitrator appointment bias and experience. Table 1 provides period averages for our case-specific measures of arbitrators' experience and appointment bias. The evidence for all cases of ISDS in column (1) indicates that respondent state-appointed arbitrators are slightly more experienced, on average, than claimant-appointed arbitrators and presidents. What is more, state-appointed arbitrators are more strongly biased toward respondent states in their prior appointment history than claimant-appointed arbitrators are biased toward private investors. This is striking insofar as public debate on ISDS focuses almost exclusively on the partisanship and self-interest of claimant-appointed arbitrators. Compared to the appointment bias of party-appointed arbitrators, the appointment bias of presidents is much weaker on average. This was to be expected, recalling that both parties have to agree on the president. All the same, on average presidents were biased somewhat in favor of claimants meaning that they have served more often as claimant-appointed arbitrators than state-appointed arbitrators in previous cases.

Figure 2 reveals that strong appointment bias of arbitrators is not a common phenomenon in ISDS proceedings (see also Nunnenkamp, 2017). Taken together, the appointments of all three types of arbitrators are unbiased or just slightly biased in 54% of all cases, defined as the difference between the number of previous appointments by claimants and the number of previous appointments by respondent states to be just one or minus one. This share is particularly high for

presidents (62%). Nevertheless, it may be problematic for respondent states that the appointment bias of presidents of arbitration tribunals is more often in favor of claimants than in favor of states (143 versus 84 cases). Claimants and respondent states were represented by more strongly biased arbitrators in a similarly large number of disputes and, not surprisingly, the bias is in their direction: respondent states tend to appoint arbitrators that more often represented states than claimants in past cases and vice versa for arbitrators appointed by claimants.

Columns (2) and (3) of Table 1 point to some striking differences in terms of arbitrators' experience and appointment bias between cases decided in favor of the claimant and cases decided in favor of the respondent state. First of all, the president's appointment bias toward the claimant appears to be relatively strong for cases decided in favor of the claimant. Second, the experience and appointment bias of arbitrators representing the state are relatively weak in such cases. Third, and most surprisingly perhaps, the experience and appointment bias of arbitrators representing the claimant are slightly weaker, rather than stronger, in cases decided in favor of the claimant.

Baseline estimation results

Descriptive statistics provide first insights but only a multivariate estimation model can test our hypotheses. Table 2 presents results from our baseline model. Model 1 includes the experience and bias measures for all three sets of arbitrators, model 2 additionally includes the respondent state and claimant's home state control variables. In model 1, we find that only the appointment bias of the tribunal's president exerts a statistically significant effect. It is in the expected direction: the more often presidents had been appointed by claimants in previous cases relative to having been appointed by states, the more likely it is that claimants win the case under observation (and vice

versa for greater appointment bias toward respondent states). This provides evidence in favor of our first hypothesis.

Including control variables in model 2 confirms the statistically significant effect of the president's appointment bias from model 1. In addition, we now find statistically significant effects in the expected direction for the appointment bias of state-appointed arbitrators and the experience of claimant-appointed arbitrators, while neither the appointment bias of claimant-appointed arbitrators nor the experience of the state-appointed arbitrator have a statistically significant effect. This therefore only partially supports our third hypothesis.

According to the average marginal effect based on model 2, an increase in the appointment bias of the tribunal's president by one standard deviation is associated with an increase in the likelihood of an investor win, which is on average 38 per cent, by 8.2 percentage points.[22] An increase in the experience of the claimant-appointed arbitrator by one standard deviation is associated with an increase in the likelihood of an investor win by 6.5 percentage points. An increase of state appointed arbitrators' bias towards investors by one standard deviation is associated with increasing the chance of an investor win by about 13.5 percentage points.

As concerns the control variables, with one exception we find no statistically significant effects. Most notably, the risk of investor wins in international arbitration of investment disputes does not appear to be higher for respondent states with relatively low GDP per capita. Broadly in line with Franck (2014) and Behn et al. (2017) we find that the risk of investor wins in international arbitration is lower for respondent states with better national institutions to enforce the rule of law. Most likely, this is because international arbitration tribunals are more inclined to deny jurisdiction and suspect 'frivolous' litigation by private investors for respondent states that adhere to the rule of law domestically.[23]

Conditional effects

To test our fourth hypothesis, we now include interaction terms to account for possible conditional effects. In models 3 and 4, results for which are reported in Table 3, we interact the president's appointment bias with, respectively, the experience and appointment bias of respondent state-appointed arbitrators. As we mentioned before, in non-linear models conditional effects cannot be simply inferred by assessing the sign and statistical significance of interaction term coefficients. We therefore plot the predicted average marginal effects of the president's appointment bias across the range of the conditioning variables based on models 3 and 4 in Figure 3. The top left graph in Figure 3 reveals that the marginal effect of the president's appointment bias on the likelihood of an investor win increases with increasing appointment bias of the state-appointed arbitrator. However, it is clear from this graph that only appointing an arbitrator with an extremely low value on (pro-investor) appointment bias makes a difference, that is, appointing an arbitrator who has many more times represented a respondent state than an investor in the past. In fact, at the 5th percentile of appointment bias of the state-appointed arbitrator the positive effect of the president's appointment bias disappears. By contrast, for the most part of the relevant range of the conditioning variable there is no change to the marginal effect of the president's appointment bias. A similar picture emerges for the conditioning effect stemming from the experience of state-appointed arbitrators, as the top right graph in figure 3 reveals. Only appointing the most experienced of arbitrators can make the positive effect of the president's appointment bias disappear.

In models 5 and 6, also reported in Table 3, we interact the president's appointment bias with, respectively, the experience and appointment bias of claimant-appointed arbitrators. The bottom left graph in Figure 3 shows that, contrary to expectation, the marginal effect of president

bias decreases rather than increases with increasing bias of the claimant-appointed arbitrator. However, the marginal effects of the president's appointment bias are never statistically significantly different across the relevant range of claimant-appointed arbitrator bias. Finally, the bottom right graph in Figure 3 reveals that the experience of the claimant-appointed arbitrator also exerts no conditioning effect on the president's effect.

In sum, we only find very limited support for our fourth hypothesis: respondent states can mitigate the detrimental effect of president bias on the likelihood that investors win an arbitration case by appointing extremely experienced arbitrators or those with extreme negative appointment bias, that is, a previous appointment history in favor of respondent states. We find no statistically significant evidence for other conditioning effects.

Robustness tests

In this sub-section, we subject our estimation results to a number of robustness tests, results for which are reported in Table 4. Given the limited evidence for conditional effects, we focus on the robustness of our baseline model instead. For ease of comparison, the baseline model 2 (with other control variables included) from Table 2 is shown again in column (1) of Table 4.

In models r1 and r2, we follow Behn et al. (2017) and employ alternative governance measures. Specifically, in model r1 we use the International Country Risk Guide's (ICRG's) investment profile index to assess a respondent state's ability to provide adequate protection to a foreign investor's property rights. In model r2 we employ ICRG's index on bureaucratic quality as bureaucracies that are impartial and more efficient should be better able to oversee laws and regulations than bureaucracies where drastic changes in policy or interruptions in government

services are more frequent. Interestingly, only the indicator for property rights protection has a negative and highly significant association with the probability of investor wins in international arbitration whereas the estimated coefficient of a respondent state's bureaucratic quality is not statistically significant.

In model r3, we use an alternative definition to our preferred definition of appointment bias of arbitrators. Our preferred definition has pro-investor appointment bias increasing in the difference in the actual number of times an arbitrator has previously been appointed by an investor minus the actual number of times he has previously been appointed by a respondent state. One can alternatively define pro-investor appointment bias as increasing in the relative share of times an arbitrator has previously been appointed by an investor minus the number of times he has previously been appointed by a respondent state.[24] In this robustness test, we therefore divide our appointment bias variable by an arbitrator's experience score, thus creating an alternative relative appointment bias variable that runs from -1 (always having served respondent states in past appointments) to 1 (always having served investors). Even with these alternative definitions, we find that the relative appointment biases of the state appointed arbitrator and of the president have the expected positive and statistically significant effect, which is entirely consistent with our baseline model results though the coefficients are not directly comparable across these two estimation models of course.

Returning to our preferred definition of appointment bias, in model r4 we no longer consider the appointment bias and experience separately for each of the three arbitrators. Instead, we use a 'consolidated' measure of appointment bias for the arbitration tribunal as a whole, by summing up the biases of the three individual arbitrators. The modified measure of experience is given by the difference between the claimant-appointed arbitrator's experience and the state-

appointed arbitrator's experience.[25] As can be seen, the coefficient on the consolidated appointment bias is statistically significantly positive, if only at the 10% level, consistent with the baseline model in which the biases of the president and the respondent state-appointed arbitrator had positive effects. The modified measure of experience does not reach statistical significance at conventional levels.

In models r5 and r6, we return to the standard measurement of the appointment bias and experience for individual arbitrators. However, we exclude cases of ISDS with extraordinarily high and low values of the modified measure of experience (model r5) or the consolidated measure of appointment bias (model r6). Specifically, we exclude cases with the highest and lowest five percent of modified experience or consolidated appointment bias in order to test whether our baseline results were driven by outliers. Next, we exclude cases brought against the richest respondent states. In model r7, all countries classified as high-income by the World Bank for the majority of years in our sample period are dropped. ISDS cases initiated prior to 2002 are excluded from model r8.[26] In model r9 we modify the definition of state wins. We exclude cases that were discontinued and consider only those cases as state wins that were explicitly decided in favor of respondent states by the arbitration tribunals. In model r10 we employ multiple imputation to impute missing values of arbitrator's experience and bias, which results in an increase in observations from 289 to 341.

Strikingly, we find that the effects of the appointment bias of the president and of the state-appointed arbitrator are robust across all these variations to our baseline model specification.[27] The consolidated appointment bias of model r4 is of course not directly comparable to the baseline model but is also statistically significant with the expected positive sign. The effect of the experience of the claimant appointed arbitrator does not vary much across model specifications

and would thus be robust in the definition introduced by Neumayer and Plümper (2017) but its statistical significance is sensitive to model specification. We infer from this that we cannot, on statistical grounds, reject the null hypothesis of no effect with sufficient confidence across model specifications.

We report further robustness tests in Table 5. In models r11 and r12 we estimate, respectively, a linear probability and a probit model. Results are practically identical in terms of sign and statistical significance of coefficients. The coefficients are not comparable across the models in substantive terms. If we calculate marginal substantive effects for one standard deviation increases in explanatory variables we find that the results are very similar. We therefore conclude that our findings do not depend on the logit estimator.

Lastly, in the remaining robustness test models reported in Table 5 we redefine our dependent variable. So far we have used a binary dependent variable of investor wins versus state wins. We now include ISDS cases that were concluded by settlements among the parties as an intermediate outcome category. In an additional estimation, we also distinguish between major wins and minor wins to account for the fact that even if an investor wins a case, it may not get close to what it had originally claimed for in terms of financial compensation. We create three ordered dependent variables. The first one consists of three categories and is ordered along the combined state win plus discontinued cases; settlement; and, finally, investor win dimension (model r13). The second one consists of four categories and is ordered along the state win; discontinued case; settlement; and, finally, investor win dimension (model r14). The third one consists again of four categories but this time ordered along the combined state win plus discontinued cases; settlement; minor investor win (defined as investor receives less than the median of the amount awarded as share of the amount claimed); and, finally, major investor win (defined as investor receives an

award that is equal to or greater than the median of the amount awarded as share of the amount claimed) dimension (model r15).[28]

Consequently, we estimate three variants of an ordered logit model suitable for ordered outcome dependent variables. Such models depend on the validity of what is known as the proportional odds or parallel regression assumption. A Wald test developed by Brant (1990) fails to reject the proportional odds or parallel regression assumption for the model as a whole as well as for each variable separately with few exceptions.[29] As in the baseline estimation model, we find that the outcomes of international arbitration are more likely to be in favor of private investors and less likely to be in favor of respondent states if the president of the tribunal is biased toward the claimant and if the claimant-appointed arbitrator is more experienced. The effect of the appointment bias of the state-appointed arbitrator becomes marginally statistically insignificant in model r13 but on the whole our baseline model results are again robust across these additional permutations to the baseline model specification.

6. CONCLUSION

If whether investors win or lose in investor-state dispute settlement were dependent merely on the merit of the investor's claim against the respondent state, the composition of the arbitration tribunal would not matter. The prior experience of arbitrators as well as whether they have represented in previous cases relatively more the side of the respondent state or the side of the investor would be of no significance. Alas, our analysis demonstrates that this is not the case. Given the president's crucial veto power, we have argued that what we call their 'appointment bias', defined as the number of times they have previously represented an investor in ISDS cases minus the number of

times they have represented a respondent state in such cases, should increase the odds that the investor wins its case. Our empirical analysis of all concluded ISDS cases in UNCTAD's database has corroborated this hypothesis across and therefore independent of multiple ways of specifying our estimation model. This suggests that presidents are not as neutral as they should be. Having served more the interests of one party over the other in the past suggests they prefer an investor's over a respondent state's standpoint in relevant matters of legal dispute since we see no other reason why this particular prior experience should otherwise impact the outcome of the case in which they now serve as president. We find the same effect for the appointment bias of state-appointed arbitrators, but interestingly not for the appointment bias of claimant-appointed arbitrators.

By contrast, on the whole we find no statistically significant evidence that experience matters independently of model specification though in some estimations we have found that more experienced claimant-appointed arbitrators – those who have served on more prior ISDS cases, independently on which side – help investors win their case. The experience of the respondent state-appointed arbitrator, by contrast, was found not to have any independent effect. However, appointing arbitrators with extreme experience helps respondent states to mitigate the detrimental impact that a biased president has on their own chances to win the case. The same goes for appointing arbitrators with extreme negative appointment bias, that is, with an appointment history tilted toward having represented respondent states more than investors.

The immediate implications for the two parties of ad-hoc investment arbitration are clear. From the investor's perspective, seek to agree with the respondent state on a president who has previously represented many more times an investor than a respondent state. From the respondent state's perspective, the opposite holds for the appointment of presidents. Whilst appointing an extremely experienced arbitrator on their side or an arbitrator with an appointment history very much tilted

toward respondent states helps mitigate the impact of a president with an appointment bias in favor of investors, clearly it is better to avoid a biased president in the first place.

From an outsider's viewpoint, our results would call for avoiding appointment bias, particularly in the crucial position of president, and mitigating its effects on ISDS decisions. This policy implication might be addressed in several ways. Greater transparency, e.g., by following the Rules on Transparency in Treaty-Based Investor-State Arbitration adopted by the United Nations Commission on International Trade Law (UNCITRAL) in July 2013, may provide a first step to alert the parties involved (and also the general public) about potential conflicts of interest of arbitrators.[30] Consequently, the selection of arbitrators may receive more attention in the future, notably by respondent states that appear to have neglected selection issues in the past. Rogers (2014) observed that many parties of the ICSID convention, especially relatively poor respondent states, waived their right to make nominations to the list of arbitrators and, thereby, strengthen their influence on the composition of arbitration tribunals.

Stricter codes of conduct for arbitrators could provide another step in the direction of avoiding biased ISDS decisions. While all arbitrators are already required to be independent and impartial, "with the large number of new cases, the disclosure requirements for ICSID arbitrators might usefully be expanded" (ICSID Secretariat, 2004: 12). In order to mitigate bias, it appears to be particularly important to require arbitrators "to disclose, not only any past or present relationships with the parties, but more generally any circumstances likely to give rise to justifiable doubts as to the arbitrator's reliability for independent judgment" (ibid: 13). While data constraints prevented us from addressing so-called double hatting of arbitrators, where the same people serve in the same or different roles in multiple sequential or even simultaneous cases, in conjunction with

appointment bias, this issue clearly deserves greater attention in codes of conduct addressing potential conflicts of interest.[31]

More controversially, it has been argued that biased ISDS decisions would be easier to avoid if ad-hoc investment tribunals were replaced by a permanent investment arbitration court. For instance, van Harten (2012: 218) favors a permanent court over ad-hoc arbitration because of "certain institutional safeguards of judicial independence such as secure judicial tenure, objective methods of appointment of judges to specific cases, and restrictions on outside remuneration by the judge." However, as discussed in more detail by Rogers (2014), a court system would not necessarily resolve biased ISDS decisions. As mentioned above, candidates for any court, including the Supreme Court in the United States, tend to have different viewpoints, including on so-called frivolous litigation by private investors or excessive market interventions by the state. Hence, just like ad-hoc tribunals, permanent investment courts may tend toward investor interests or toward respondent state interests – especially if it is realistically assumed that the judges sitting in the permanent court were drawn from the "highly visible and powerful core of some of the most influential actors in the [current] system" (Langford et al., 2017: 328). Therefore, our empirical findings may suggest a more practical solution: the creation of a pool of potential candidates who can function as presidents drawn from those and only those who have not systematically over-represented investors or respondent states in previous cases.

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NOTES

[1] See also the discussion in Gallagher and Shrestha (2011) and Schultz and Dupont (2015). Conflicts of interest, favoring investors, and a lack of transparency are supposed to encourage 'strategic litigation' (Pelc, 2017), which aims not only at obtaining financial compensation for alleged breaches of treaty obligations by the respondent state but also at deterring the regulation of business activities by host-country governments. The mere fear of being sued and ending up on the losing side may result in 'regulatory chill', that is, states shying away from regulatory measures and policies for fear of being dragged by foreign investors before a private arbitration tribunal (Neumayer, 2001).

[2] A related literature discusses investment treaties as a possible solution to investor-state hold-up problems. See Markusen (2001) or Turrini and Urban (2008).

[3] See, for instance, Berger et al. (2011, 2013) and the literature given there.

[4] According to Wellhausen (2015), the negative effects of disputes are limited to FDI flows from the particular source country where the foreign investor raising the claim is based.

[5] See Gallagher and Shrestha (2011) for a critical assessment of Franck's (2009) analysis and conclusions.

[6] However, the focus of Pelc (2017) is on whether the recent trend toward strategic litigations and so-called indirect expropriations, rather than direct takings, can explain why the win rates of private claimants declined over time. See also Schultz and Dupont (2015) for descriptive statistics suggesting that higher-income countries have better chances to fend off compensation claims in ISDS proceedings.

[7] As discussed in more detail in Behn et al. (2017), this reasoning is based on Posner and De Figueredo (2005) who report in the context of the International Court of Justice that judges are more likely to vote for a disputing state that shares a similar level of economic development with the judge's home state. Judges are supposed to be 'sympathetic' with comparable states and/or to consider shared interests of home states with similarly advanced states.

[8] In contrast, Behn et al. (2017) report a significantly positive effect of the GDP per capita of the presiding arbitrator's home state on the probability of investor wins. The sample underlying the ordered logit model of Behn et al. is much larger than that of Franck (2009).

[9] Kapeliuk (2010) also finds no evidence supporting the view that arbitrators render compromise awards, by 'splitting the difference' with regard to claims for financial compensation, in order to maximize their chances of reappointment in future cases of ISDS. It should be noted, however, that the evidence is largely based on just 43 of the 131 concluded cases – namely those with involvement of elite arbitrators (105) having been finally resolved by a publicly known award on the merits.

[10] In addition, Waibel and Wu (2011) perform multiple regressions on the determinants of ISDS outcomes in an unpublished working paper. They account for the personal background of arbitrators as well as repeated appointments by claimants or respondent states. Inter alia, Waibel and Wu (2011) find that arbitrators (notably, the presidents of tribunals) with a career in the private sector and with repeated appointments by claimants are more likely to affirm jurisdiction, i.e. accepting the case for the tribunal to decide on its merits. In contrast to our analysis below, Waibel and Wu do not distinguish between the arbitrators' experience and bias on a case-by-case basis.

[11] In contrast, the investors' identity and the expertise of the parties' lawyers appear to be more important.

[12] Franck and Wylie (2015) include awards that were publicly available by the end of 2011. As a result, the number of observations underlying the reported regressions is about 50-100.

[13] The current status was unknown for 11 cases; another 11 cases were concluded but the arbitration tribunal's decision was "in neither party's favor", i.e., the tribunal found a liability but awarded no damages.

[14] Our database does not allow us to distinguish between those appointed by parties and those dedicated to them.

[15] In a few cases, arbitrators are listed as the "sole arbitrator" in the database. In some other cases, the function of arbitrators is "unknown." Note that these listings count for an arbitrator's experience, i.e., the number of cases being involved in any function (see below).

[16] See also Bloom and Cavanagh (1986) for an analysis of arbitrator selection.

[17] Jurisdiction may be denied, for instance, when the tribunal finds that the investor's asset does not constitute a 'covered investment', that the claimant is not a 'covered investor', or that the dispute arose before the relevant investment treaty entered into force or falls outside the relevant ISDS provisions (UNCTAD, 2016). See also Schultz and Dupont (2015) who define state wins as arbitral decisions that either decline jurisdiction or deny the investor any compensation.

[18] The first period consists of two years only. We do not include year-specific fixed effects since some years see only investor wins or only state wins. These cases would be dropped due to multicollinearity between the year-specific fixed effects and the dependent variable in these years.

[19] In contrast, Pelc (2017) argues that most disputes no longer result from direct takings by host countries with weak rule of law but from policy regulations (so-called indirect expropriation) in democratic states with relatively strong institutions.

[20] "ICSID AF" refers to the ICSID Additional Facility Rules, applying to disputes that fall outside the scope of the ICSID Convention (e.g. if one of the involved parties is not an ICSID member state or a national of an ICSID member state). For details see: https://icsid.worldbank.org/en/Pages/icsiddocs/ICSID-Additional-Facility-Rules.aspx

[21] We are grateful for an anonymous referee for pointing this out.

[22] Average marginal effects are marginal effects estimated for each observation at its observed value and then averaged.

[23] Interestingly, if we exclude the respondent state's rule of law as an explanatory variable, its per capita income becomes statistically significant. Plausibly, therefore, a finding that poorer states are more likely to lose ISDS cases spuriously picks up the effect of weak institutions on ISDS case winning.

[24] This relative appointment bias measure is not our preferred measure because it will give the same appointment bias score to an arbitrator who has previously been appointed only once, namely by one party, as to an arbitrator who has previously been appointed many times by this party (and never by the other). In our view, relative appointment bias has low construct validity since it does not capture the theoretical rationale behind measuring appointment bias, namely that repeated appointment by one of the parties can serve as a proxy variable for their otherwise unobserved tendency to prefer one party over the other in the face of legal disputes.

[25] As argued in Section 3, the president's experience should not play an unconditional role for tribunal decisions in favor of either party. The baseline results are in line with this reasoning.

[26] Note that our measures of experience and bias take relatively low values by construction in the early periods of our analysis when the number of ISDS was still relatively small.

[27] Adopting the formal definition and measure of effect robustness of Neumayer and Plümper (2017), we find that the estimated degree of robustness for the average marginal effect of the appointment bias of the respondent state is always at least 0.8 (except in model r6 where it is 0.45) and for the average marginal effect of the president's appointment bias it is always at least 0.8.

[28] We lose a few observations in this model as the share awarded relative to the original claim could not be established in all investor win cases.

[29] In models r13 and r15, the appointment bias of the state appointee fails to meet the parallel regression assumption test. If we re-run these models with multinomial logit, which does not depend on the parallel regression assumption, we find that this variable has a positive and statistically significant effect on investor wins (model r13) and both major and minor investor wins (r15) but a negative (and statistically insignificant) effect on the probability of a settlement in both models.

[30] UNCITRAL rules require the disclosure of a wide range of information submitted to and issued by investment tribunals. Furthermore, UNCITRAL finalized and adopted the Mauritius Convention on Transparency in July 2014. This convention supplements existing treaties with respect to transparency-related obligations; it "establishes a mechanism through which all parties to existing investment treaties can efficiently and effectively update the procedural rules governing investor-State arbitrations under those treaties so as to effectively implement the Transparency Rules and better take into account the public interest nature of these disputes" (Johnson, 2014: 2).

[31] As noted by García-Bolívar (2010: 5), some arbitrators have been challenged recently because there were doubts about their independent judgment: "In most cases where those challenges were made, the arbitrators served as legal counsel in other investment treaty cases. Most of these challenges have not been successful."

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TABLE 1 'Experience' and 'Bias' of presidents and party-appointed arbitrators

	(1)	(2)	(3)	(4)
	All cases	Decided in favor of:		Discontinued
		Claimant	Respondent state	
Presidents:				
Bias	0.63	1.15	0.44	-0.38
Experience	7.61	5.37	6.12	5.56
Observations	622	121	160	32
Arbitrators representing claimants:				
Bias	3.15	1.57	2.03	2.53
Experience	7.74	4.86	5.38	6.81
Observations	608	113	147	32
Arbitrators representing states:				
Bias	-5.84	-1.42	-3.82	-4.22
Experience	8.72	4.00	6.09	7.41
Observations	599	113	147	32

Note. Experience = accumulated number of cases involved in any function up to year t-1; Bias = accumulated number of cases involved by representing claimants minus accumulated number of cases involved by representing respondent states, up to year t-1. Settled and pending cases as well as cases without information on the current status and cases decided in favor of neither party are included in 'All cases.'

Source: UNCTAD, ISDS database.

TABLE 2 Baseline results

	Model 1	Model 2
Experience state appointee	0.0277	0.0507
	(0.0345)	(0.0363)
Experience claimant appointee	0.0418	0.0452*
	(0.0271)	(0.0267)
Experience president	-0.0144	-0.0253
	(0.0284)	(0.0304)
Appointment bias state appointee	0.0594	0.0883**
	(0.0426)	(0.0427)
Appointment bias claimant appointee	-0.0780	-0.0707
	(0.0502)	(0.0516)
Appointment bias president	0.149**	0.133**
	(0.0620)	(0.0670)
ln GDPpc respondent state		0.0300
		(0.234)
ln GDP respondent state		-0.0718
		(0.121)
Rule of law respondent state		-0.903***
		(0.253)
ln GDPpc investor home		-0.0878
		(0.322)
ln GDP investor home		0.0768
		(0.119)
EU investor dummy		-0.164
		(0.509)
NAFTA investor dummy		-0.743
		(0.683)
Observations	290	289

Note. Period, sector and treaty and institution dummies included in all estimation models. Constant included but not reported. Statistical significance at the one, five, and ten percent level is indicated by ***, ** and *, respectively.

TABLE 3 Conditioning effects

	Model 3	Model 4	Model 5	Model 6
Experience state appointee	0.0481	0.0820**	0.0485	0.0506
	(0.0398)	(0.0336)	(0.0374)	(0.0364)
Experience claimant appointee	0.0410	0.0436	0.0449*	0.0456*
	(0.0270)	(0.0268)	(0.0272)	(0.0272)
Experience president	-0.0312	-0.0299	-0.0319	-0.0257
	(0.0280)	(0.0311)	(0.0316)	(0.0310)
Appointment bias state appointee	0.0795*	0.113***	0.0864*	0.0884**
	(0.0455)	(0.0393)	(0.0445)	(0.0428)
Appointment bias claimant appointee	-0.0587	-0.0696	-0.0452	-0.0703
	(0.0523)	(0.0543)	(0.0550)	(0.0520)
Appointment bias president	0.202***	0.254***	0.188**	0.137
	(0.0771)	(0.0882)	(0.0766)	(0.0885)
ln GDPpc respondent state	0.0619	0.0207	0.0360	0.0301
	(0.237)	(0.233)	(0.235)	(0.234)
ln GDP respondent state	-0.0852	-0.0610	-0.0766	-0.0725
	(0.123)	(0.119)	(0.124)	(0.122)
Rule of law respondent state	-0.924***	-0.944***	-0.910***	-0.902***
	(0.258)	(0.263)	(0.250)	(0.254)
ln GDPpc investor home	-0.0861	-0.0457	-0.0693	-0.0876
	(0.322)	(0.333)	(0.318)	(0.322)
ln GDP investor home	0.0703	0.0634	0.0561	0.0756
	(0.116)	(0.117)	(0.114)	(0.117)
EU investor dummy	-0.175	-0.147	-0.149	-0.166
	(0.509)	(0.520)	(0.502)	(0.510)
NAFTA investor dummy	-0.681	-0.642	-0.678	-0.741
	(0.677)	(0.694)	(0.665)	(0.681)
Bias president * Bias state appointee	0.0170**			
	(0.00747)			
Bias president * Experience state appointee		-0.0158***		
		(0.00605)		
Bias president * Bias claimant appointee			-0.0207	
			(0.0129)	
Bias president * Experience claimant appointee				-0.000496
				(0.00642)
Observations	289	289	289	289

Note. Period, sector and treaty and institution dummies included in all estimation models. Constant included but not reported. Statistical significance at the one, five, and ten percent level is indicated by ***, ** and *, respectively.

TABLE 4 Robustness tests 1

	baseline	r1	r2	r3	r4	r5	r6	r7	r8	r9	r10
Experience state appointee	0.0507	0.0289	0.0286	0.00239		0.0548	0.0227	0.0474	0.0611	0.0482	0.0394
	(0.0363)	(0.0396)	(0.0394)	(0.0214)		(0.0390)	(0.0443)	(0.0399)	(0.0385)	(0.0365)	(0.0343)
Experience claimant appointee	0.0452*	0.0456	0.0536	0.0257		0.0985**	0.0578*	0.0493*	0.0434	0.0466	0.0377
	(0.0267)	(0.0336)	(0.0338)	(0.0198)		(0.0443)	(0.0342)	(0.0281)	(0.0280)	(0.0285)	(0.0265)
Experience president	-0.0253	-0.0541	-0.0565	0.00535		-0.0155	-0.0156	-0.0237	-0.0196	-0.0309	-0.0227
	(0.0304)	(0.0358)	(0.0350)	(0.0239)		(0.0329)	(0.0346)	(0.0324)	(0.0319)	(0.0308)	(0.0277)
Bias state appointee	0.0883**	0.126**	0.110**	0.633**		0.133***	0.179***	0.0821*	0.0996**	0.0787*	0.0632*
	(0.0427)	(0.0498)	(0.0471)	(0.308)		(0.0452)	(0.0555)	(0.0463)	(0.0452)	(0.0424)	(0.0376)
Bias claimant appointee	-0.0707	-0.0789	-0.0835	-0.261		-0.119*	-0.118	-0.0439	-0.0532	-0.0696	-0.0558
	(0.0516)	(0.0569)	(0.0542)	(0.373)		(0.0676)	(0.103)	(0.0597)	(0.0533)	(0.0519)	(0.0489)
Bias president	0.133**	0.197**	0.183**	1.222***		0.160**	0.196**	0.144*	0.124*	0.107*	0.113*
	(0.0670)	(0.0771)	(0.0745)	(0.419)		(0.0817)	(0.0790)	(0.0749)	(0.0701)	(0.0641)	(0.0679)
Consolidated experience					-0.0134						
					(0.0194)						
Consolidated bias					0.0445**						
					(0.0226)						
ln GDPpc respondent state	0.0300	0.00678	-0.0891	0.103	0.0693	0.279	0.360	0.155	-0.0196	0.0376	-0.0290
	(0.234)	(0.215)	(0.230)	(0.212)	(0.204)	(0.215)	(0.236)	(0.252)	(0.260)	(0.240)	(0.207)
ln GDP respondent state	-0.0718	0.0259	0.112	-0.0834	-0.123	-0.183	-0.166	-0.145	-0.122	-0.0566	-0.102
	(0.121)	(0.141)	(0.132)	(0.111)	(0.108)	(0.120)	(0.132)	(0.134)	(0.139)	(0.121)	(0.108)
Rule of law respondent state	-0.903***			-0.907***	-0.900***	-1.049***	-1.071***	-1.046***	-1.118***	-0.861***	-0.898***
	(0.253)			(0.255)	(0.236)	(0.286)	(0.277)	(0.292)	(0.300)	(0.260)	(0.242)
Property rights respondent state		-0.252***									
		(0.0760)									
Impartial bureaucracies respondent state			-0.332								
			(0.262)								
ln GDPpc investor home	-0.0878	-0.180	-0.208	-0.110	-0.107	-0.201	-0.271	0.106	-0.0416	-0.0595	0.0256
	(0.322)	(0.306)	(0.308)	(0.305)	(0.297)	(0.348)	(0.344)	(0.353)	(0.339)	(0.349)	(0.308)
ln GDP investor home	0.0768	0.127	0.118	0.0527	0.0640	0.178	0.0744	0.109	0.0850	0.0541	0.0601
	(0.119)	(0.140)	(0.136)	(0.116)	(0.117)	(0.138)	(0.126)	(0.135)	(0.132)	(0.118)	(0.111)
EU investor dummy	-0.164	-0.444	-0.344	-0.0578	-0.0775	0.182	0.135	-0.273	-0.203	-0.226	-0.224
	(0.509)	(0.534)	(0.519)	(0.480)	(0.460)	(0.538)	(0.524)	(0.561)	(0.551)	(0.535)	(0.485)
NAFTA investor dummy	-0.743	-1.030	-0.652	-0.468	-0.579	-0.576	-0.321	-0.946	-0.613	-0.676	-0.885
	(0.683)	(0.715)	(0.710)	(0.653)	(0.642)	(0.712)	(0.685)	(0.747)	(0.745)	(0.707)	(0.666)
Observations	289	245	245	289	289	259	259	234	251	254	341

Note. Period, sector and treaty and institution dummies included in all estimation models. Baseline is model 2, Table 2. In models r1 and r2, alternative governance measures are used. In model r3, a relative measure of appointment bias is used. In model r4, consolidated measures for bias and experience are used. In models r5 and r6, cases with the highest and lowest five percent of consolidated experience or bias are excluded. In model r7, high-income respondent states are dropped. Model r8 excludes ISDS cases initiated prior to 2002. In model r9 discontinued cases are no longer considered as state wins. In model r10, we impute missing values of arbitrator's experience and bias. Constant included but not reported. Statistical significance at the one, five, and ten percent level is indicated by ***, ** and *, respectively.

TABLE 5 Robustness tests 2

	baseline	r11	r12	r13	r14	r15
Experience state appointee	0.0507	0.0101	0.0304	0.0392	0.0459	0.0437
	(0.0363)	(0.00700)	(0.0205)	(0.0295)	(0.0292)	(0.0278)
Experience claimant appointee	0.0452*	0.00715	0.0251	0.0261	0.0261*	0.0218
	(0.0267)	(0.00496)	(0.0159)	(0.0162)	(0.0151)	(0.0165)
Experience president	-0.0253	-0.000963	-0.0166	-0.00578	-0.00848	-0.00935
	(0.0304)	(0.00437)	(0.0178)	(0.0210)	(0.0180)	(0.0215)
Appointment bias state appointee	0.0883**	0.0151**	0.0490**	0.0495	0.0611*	0.0544*
	(0.0427)	(0.00749)	(0.0236)	(0.0311)	(0.0313)	(0.0289)
Appointment bias claimant appointee	-0.0707	-0.0122	-0.0403	-0.0347	-0.0312	-0.0342
	(0.0516)	(0.00937)	(0.0292)	(0.0253)	(0.0237)	(0.0277)
Appointment bias president	0.133**	0.0164**	0.0827**	0.0982**	0.0730*	0.0781*
	(0.0670)	(0.00644)	(0.0390)	(0.0449)	(0.0384)	(0.0435)
ln GDPpc respondent state	0.0300	0.00951	0.00558	0.0559	0.0652	0.108
	(0.234)	(0.0460)	(0.130)	(0.178)	(0.173)	(0.192)
ln GDP respondent state	-0.0718	-0.0118	-0.0442	-0.0413	-0.0479	-0.0505
	(0.121)	(0.0230)	(0.0688)	(0.0916)	(0.0904)	(0.102)
Rule of law respondent state	-0.903***	-0.170***	-0.528***	-0.813***	-0.763***	-0.891***
	(0.253)	(0.0477)	(0.144)	(0.198)	(0.194)	(0.211)
ln GDPpc investor home	-0.0878	-0.0205	-0.0542	-0.117	-0.0517	-0.0195
	(0.322)	(0.0622)	(0.181)	(0.214)	(0.189)	(0.216)
ln GDP investor home	0.0768	0.0116	0.0468	0.0799	0.0879	0.0334
	(0.119)	(0.0218)	(0.0672)	(0.101)	(0.0896)	(0.0931)
EU investor dummy	-0.164	-0.0216	-0.0962	-0.210	-0.383	-0.0150
	(0.509)	(0.0966)	(0.286)	(0.364)	(0.329)	(0.365)
NAFTA investor dummy	-0.743	-0.124	-0.457	-0.843	-0.990**	-0.813
	(0.683)	(0.130)	(0.389)	(0.547)	(0.495)	(0.531)
Brand global chi^2 test (p-value)				0.268	0.501	0.127
Observations	289	289	289	361	361	353

Note. Period, sector and treaty and institution dummies included in all estimation models. Model r11 is estimated with a linear probability model. In model r12 probit is used. In model r13, the dependent variable consists of three categories and is ordered along the combined state win plus discontinued case; settlement; and, finally, investor win dimension. In model r14, the dependent variable consists of four categories and is ordered along the state win; discontinued case; settlement; and, finally, investor win dimension. In model r15, the dependent variable consists of four categories and is ordered along the combined state win plus discontinued cases; settlement; minor investor win; and, finally, major investor win dimension. An investor win is coded as major (minor) if an investor gets >=p(50) (<p(50)) of the amount awarded as share of the amount claimed. Models r11 and r12 include constant but not reported. Statistical significance at the one, five, and ten percent level is indicated by ***, ** and *, respectively.

APPENDIX

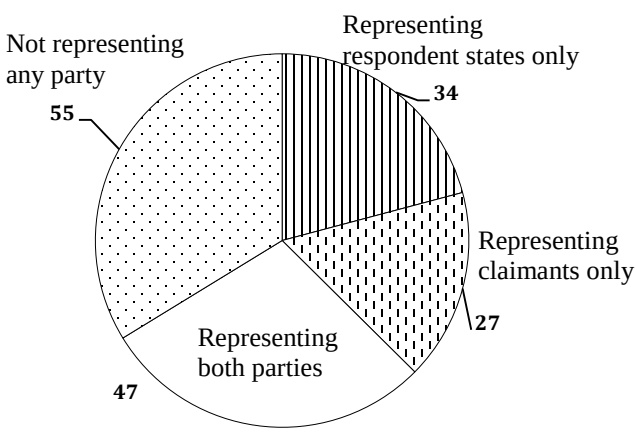
TABLE A1 Summary variable statistics

Variable	Mean	Std. Dev.	Min	Max
Investor win (baseline dependent variable)	0.38	0.49	0	1
Experience state appointee	5.51	9.13	0	62
Experience claimant appointee	5.37	7.62	0	35
Experience president	5.86	6.87	0	31
Bias state appointee	-2.96	8.01	-57	10
Bias claimant appointee	1.92	4.20	-6	27
Bias president	0.61	3.25	-27	10
ln GDPpc respondent state	8.74	1.06	5.37	11.00
ln GDP respondent state	25.61	1.74	20.46	30.37
Rule of law respondent state	-0.25	0.85	-2.08	1.81
ln GDPpc investor home	10.46	0.60	7.99	11.58
ln GDP investor home	28.02	1.83	22.22	30.41
EU investor dummy	0.49	0.50	0	1
NAFTA investor dummy	0.33	0.47	0	1

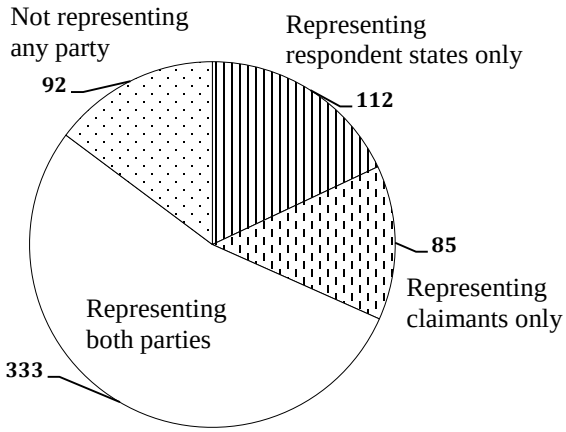
Note. N=289.

FIGURE 1 Number of presidents performing other functions and number of cases they have been involved in as president (based on 162 presidents and 622 cases with information on president)

a) Number of presidents

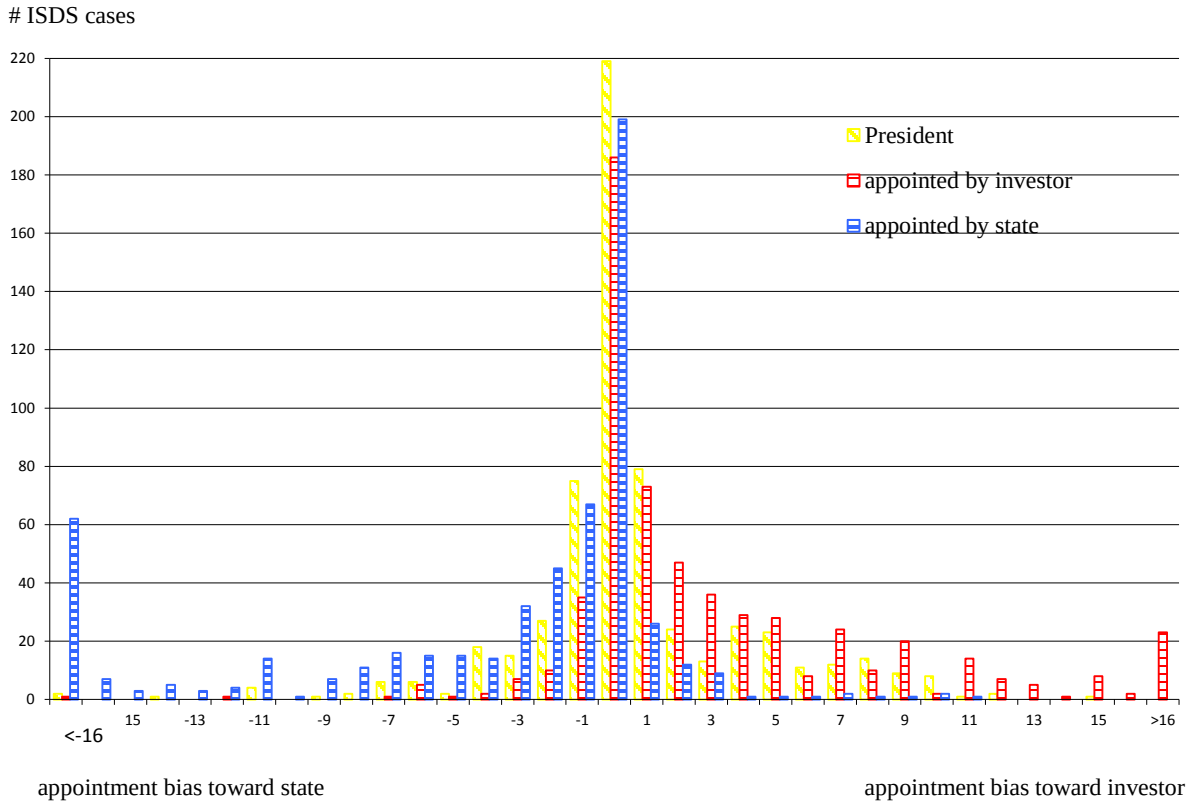


b) Number of cases



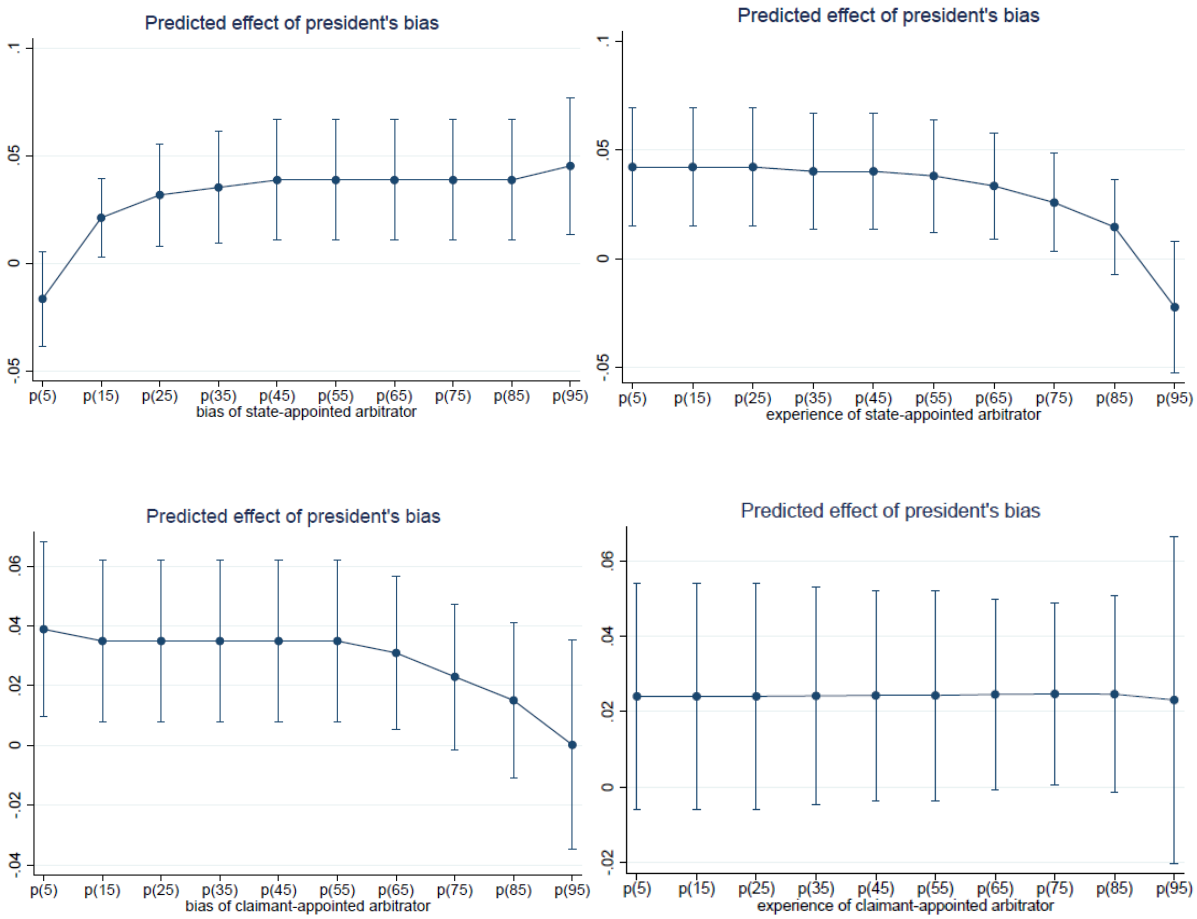
Source: UNCTAD, ISDS database.

FIGURE 2 Distribution of ISDS cases according to bias of arbitrators



Source: UNCTAD, ISDS database.

FIGURE 3 Predicted effect of president's bias conditioned by bias and experience of party-appointed arbitrators



Note. On the x-axis, percentiles of the conditioning variable are shown, respectively.