

# Does participation in international organizations increase cooperation?

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**Abstract** Recent research asserts that public commitments to international institutions promote behavior that is consistent with institutional purposes. Evidence for this proposition is based almost entirely on studies that compare the behavior of states that have and have not ratified treaties. This paper evaluates instances in which some member states temporarily experience increased entanglement with an IO because they or their nationals serve in a position of authority. Unlike selection into IOs, selection into positions of authority is often governed by a common, observable, and partially exogenous process. I exploit exogenous exit, random assignment to different term lengths, and competitive elections in three contexts: the International Criminal Court (ICC), the UN Human Rights Commission (UNHRC), and the UN Security Council (UNSC). The evidence implicates that acquiring a position of authority can make states more willing to reject U.S. advances to sign non-surrender agreements, adopt domestic legislation that changes the penal code (ICC case), ratify legally binding treaties (UNHRC case), and contribute to peacekeeping missions (UNSC case). On the other hand, there is no evidence that UN institutions successfully select more cooperative states for positions of authority. Similar research designs can gainfully be employed to identify the causal effects of other forms of institutional participation.

**Keywords** International organizations · ICC · United Nations · UN Human Rights Council · UN Security Council

**JEL Codes** F53 · F55 · F60 · K33 · N40

## 1 Does participation in international organizations increase cooperation?

A rapidly growing literature asserts that even international institutions that lack international enforcement mechanisms can affect state behavior. By ratifying treaties, states make public commitments that allow domestic and international actors to point

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out inconsistencies between past promises and current behavior (e.g., Dai 2005; Kelley 2007; Simmons 2009; Tomz 2007). If these promises are more than just cheap talk, then states that are members of international institutions should behave differently from those that are not.

Empirical tests of this proposition are plagued by a problem that is well-understood but difficult to remedy: congruence between institutional purposes and state behavior may occur simply because those states already inclined to cooperate are also most likely to select into institutions. The canonical empirical strategy is to regress an indicator of IO/treaty membership on some measure of the behavior that the IO/treaty seeks to promote.

Instead, I evaluate instances in which some governments temporarily experience increased entanglement with an IO because they or their nationals serve in a position of authority. The conventional wisdom is that governments pursue such positions primarily as a means for policy influence, exchange, prestige, or patronage. Yet, in the process governments frequently align themselves publicly with the IO's purposes. Moreover, high profile positions raise the salience of an institution, thus increasing the opportunity for domestic and international actors to hold governments accountable for their promises. If public commitments to the IO's purposes are more than just cheap talk, then governments that serve in temporary positions of authority should cooperate more than similar governments that do not have such positions.

This research design also faces a selection problem: the same reasons that lead a government to seek and to be successful in finding a position of prominence may influence cooperation. Yet, unlike selection into IOs, selection into positions of authority is often governed by a common, observable, and (sometimes) partially exogenous process, thus offering opportunities to identify causal effects. Sometimes these processes are fully exogenous, such as draws by random lot or exit after a two-year term (as in the UN Security Council). Other settings provide information about the selection process. For example, if positions of authority are allocated via competitive elections, then we observe not just who gets the position but also how many votes they and the losers received. We may exploit this in a regression discontinuity (RD) design, which identifies a causal effect by comparing states that just won or lost. In many settings, a full-fledged RD design may be impossible as there are only a few dozen competitive candidates for positions of authority. Yet, even in that circumstance actually observing the common selection process improves causal inference vis-à-vis dozens of distinct unobservable selection processes; as in treaty ratifications.

I apply such research designs in three settings in order to examine the impact of IO participation in ways other than comparing the behavior of members and non-members. First, I take advantage of exogenous exit to examine whether non-permanent UN Security Council (UNSC) members temporarily increase their contributions to UN peacekeeping operations. There is an extensive literature that links nonpermanent membership with gains in bilateral or multilateral aid (e.g., Kuziemko and Werker 2006; Dreher et al. 2009a) but none that examines whether it may also encourage greater public goods contributions. I find that non-permanent memberships lead to significant and substantively important yet temporary increases in peacekeeping contributions.

Second, I examine whether countries that have a national elected as a judge to the International Criminal Court (ICC) are more likely than countries whose candidates were narrowly defeated to cooperate with the ICC by defying U.S. efforts to ratify

nonsurrender agreements and adopting implementation legislation. Consistent with the findings from Kelley's (2007) seminal analysis, the results show that commitments to the ICC do not just screen states but also constrain. Yet, this study identifies this effect not based on a comparison between ratifiers and nonratifiers but on variation in entanglement among the Rome ratifiers.

Third, I exploit competitive elections and random assignment to different term lengths to examine whether participation in the UN Human Rights Council (UNHRC) makes countries more likely to ratify human rights treaties. Aspiring UNHRC member states make explicit and verifiable commitments to ratify treaties. I find that elected states kept significantly more of their pledges than those states that were narrowly defeated in their quest for membership. Moreover, states that were randomly assigned to two- and three-year terms kept more pledges than states who were assigned to a one-year term.

In each case, I find significant and substantively important effects of holding temporary positions of authority. The Security Council evidence is the most convincing, as it is based on a large amount of data over many years. Together, I take this as evidence for the theoretical argument that even weak increases in the levels of entanglement with an IO can yield tangible short-term behavioral changes. By contrast, there is no evidence that competition for positions of authority affects cooperation. Indeed, UN member states appear not to select candidates based on past or present cooperative behavior. Moreover, I found no evidence that positions of authority yield lasting effects. Peacekeeping contributions were back to their pre-membership levels two years after the UNSC term ended. Thus, it is unlikely that temporary positions of authority socialize states into more cooperative behavior.

This article goes beyond the existing literature in showing that commitment effects occur not just as a consequence of ratification but also in the course of interactions with IOs. This matters as it suggests potential avenues for how relatively inexpensive manipulations of institutional design could affect state cooperation. By contrast, the existing literature has largely treated institutional membership, such as non-permanent positions in the UN Security Council, as mechanisms of exchange. Methodologically, the note introduces research designs to identify causal effects that have not yet been applied to the study of IOs and international relations. There are opportunities for applying these types of designs to other cases. Yet, there are also challenges, such as small sample sizes, which make the inferences more model dependent than one would like in an ideal regression discontinuity design. The conclusion offers thoughts about the possibilities and limitations of pursuing research designs other than cross-sectional time series analysis for identifying institutional effects on cooperation.

## 2 Positions of authority and cooperation with IOs

Why would governments that acquire a position of authority for themselves or a national increase their cooperation with an IO? There are many objectives that governments have in pursuing positions of authority, not all of which imply increased cooperation. First, governments may seek positions of authority for policy influence. Nonpermanent members are unlikely to be pivotal in UNSC votes (O'Neill 1996; Voeten 2001). Yet, they may be able to affect the agenda, exert influence on the wording of resolutions, promote pet principles, or acquire policy relevant information

(Malone 2000). Similarly, winning a UNHRC seat does not affect the probability of being publicly shamed by the institution (Lebovic and Voeten 2006). Nevertheless, governments might use membership to affect the text of resolutions or the agenda. By contrast, having a national as an ICC judge serves no obvious policy purposes.

Second, governments sometimes use positions of authority as a means of exchange. Non-permanent UNSC members obtain more U.S. aid (Kuziemko and Werker 2006), more World Bank projects (Dreher et al. 2009a), and more IMF programs with relatively fewer conditions (Dreher et al. 2009b). This presumes that votes in an institution are valuable to others with access to resources and that the vote is directly controlled by the governments (as opposed to an independent national, such as an ICC judge).

Third, positions of authority carry private benefits to elites. UNSC and UNHRC memberships increase the size and relevance of diplomatic missions. Individual postings at IOs are attractive retirement positions for elites who have outlived their domestic usefulness. The ability to “promote” domestic elites to international organizations can be useful to governments and comforting to current elites, given that these positions are generally well compensated and in attractive locales.

None of these rationales imply that governments should cooperate more with an IO. After election, governments can achieve their goals by pursuing their own policy agendas or that of their benefactors. This is, however, different if governments are at least partially motivated by a desire to enhance their standing, reputation, or prestige. Such motives are frequently part of public justifications for seeking positions of authority. For example, David Malone finds that “The dominant view at the UN is that countries aim for membership in the council to underscore their international prestige” (Malone 2000). The behavioral literature on UN office seeking has long viewed these primarily as “badges of prestige” (Weigert and Riggs 1969).

It is not clear how one gains prestige or standing from attaining high office or what such prestige is good for. For the present purpose, the first issue is more important than the latter. Standing has two facets: credibility and esteem (American Political Science Association 2009). Esteem refers to perceptions that a government stands for something deemed desirable by relevant domestic or international audiences. Credibility refers to the perception that a government follows through on what it proclaims to stand for. IOs generally embody a set of goals deemed laudable by their membership. Increasing ones entanglement with an IO may help associate oneself more strongly with those goals. Governments may believe that a national as an ICC judge increases the belief that it is a strong advocate for international criminal justice. Yet, this also offers domestic and international actors additional opportunities for pointing out inconsistencies between the associated goal and behavior. In order to protect credibility, governments may align their behavior more closely with the IOs’ goals when they or their nationals serve in a position of authority.

International and domestic “naming and shaming” mechanisms are central in the theoretical literature on the effects of weak human rights institutions on state behavior (e.g., Hafner-Burton 2008) and in international political economy (e.g., Tomz 2007). Simmons (2009) distinguishes three mechanisms through which ratification of human rights treaties that lack strong enforcement provisions may alter behavior. The first, litigation, does not apply here. The other two do theoretically operate.

First, there may be an agenda-setting effect due to increased salience. Simmons argues:

“It is one thing not to initiate policy change on the national level and quite another not to respond once a particular right is made salient through international negotiations. Silence is ambiguous in the absence of a particular proposal, but it can easily be interpreted as opposition in the presence of a specific accord.” (Simmons 2009, 128)

A government’s decision not to participate in a peacekeeping mission may raise few eyebrows in ordinary times. Yet, if domestic media and opposition forces are paying increased attention to UNSC decisions due to non-permanent membership, then this decision will at least demand an explanation. Moreover, it offers the UN Secretary-General an opportunity to highlight the responsibilities that come with non-permanent UNSC membership. Similarly, the pledges that are part of the process of acquiring UNHRC membership create a moment in which governments have to consider whether they want to promise that certain treaties should be ratified, regardless of whether ratification previously appeared on the agenda.

Second, public promises of cooperative behavior combined with the salience brought by high profile authority positions can trigger mobilization. Simmons (2009) documents that human rights NGOs like Amnesty International mobilize around treaties. Human rights NGOs use UNHRC membership in a similar way. For example, Amnesty International publishes and (where necessary) criticizes the pledges UNHRC candidates make, organizes letter campaigns calling on countries to fulfill their election pledges, is actively involved in the universal periodic review process that comes with UNHRC membership, and regularly highlights the pledges in the annual reports for UNHRC members.<sup>1</sup>

The claim here is that the accountability mechanisms that allow international and domestic actors to hold governments accountable for their commitments to institutional purposes should be temporarily intensified when governments or their nationals hold a position of authority. If these commitments matter, then states should become more cooperative with IO purposes during their tenure. Such accountability politics could be triggered even if the acquisition of positions of authority were motivated by purely material purposes as long as there is some mechanism that forces states to make promises and that holds them accountable for such promises.

This mechanism is not the only way in which participation in IOs could affect state behavior. Constructivists argue that continued interaction in IOs socializes governments to start identifying with the IO’s goals (Johnston 2001). The process of socialization aims to induct actors into the norms and rules of a given community, leading to sustained compliance based on the internalization of new norms. There is some evidence that individuals that are delegated to IOs are socialized into the goals of their organizations. Alger (1963) showed with a pre-test and a post-test that diplomats who interacted with each other in the UNGA changed notions about how the institution works, although they did not necessarily internalize the institution’s norms. Yet other studies reveal that UN delegates became less internationalist in their orientations and more attuned to the national interests they were representing (Ernst 1978; Peck 1979). Similarly, extensive exposure to the European level does not seem to increase supranationalism among EU officials (Beyers 2005; Hooghe 2005).

<sup>1</sup> <http://www.amnesty.org/en/united-nations/human-rights-council> (accessed June 11, 2011).

Moreover, the aggregation of individual socialization processes may not penetrate foreign policy. This is especially improbable for the ICC, given that judges are not representatives of their governments or supported by government bureaucracies.

An important difference between the observable implications of the commitment and the socialization mechanisms is that socialization is a gradual process that is likely to emerge slowly but also have more lasting impacts. The commitment mechanism provides little ground for expecting that states continue to increase peacekeeping contributions after they have exited the UN Security Council. By contrast, socialization should be enduring. On the other hand, short stints in positions of authority may be insufficient to trigger socialization. Where possible, I examine whether the effects of institutional membership are enduring or temporary.

### 3 UNSC non-permanent membership and peacekeeping contributions

Previous research has shown that non-permanent UNSC members obtain more U.S. aid (Kuziemko and Werker 2006), more World Bank projects (Dreher et al. 2009a), and more IMF programs with relatively fewer conditions (Dreher et al. 2009b). We thus know that states use their temporary positions of authority to extract favors. This suggests that UNSC decisions matter to the powerful: Why else would they pay a premium to non-permanent members? It also suggests that non-permanent membership may be little more than a prize that carries no further commitments. Yet, most observers presume that UNSC membership is also paired with a desire to enhance standing or prestige (e.g., Malone 2000). Thus, it is at least plausible that UNSC non-permanent membership may also encourage more cooperative behavior.

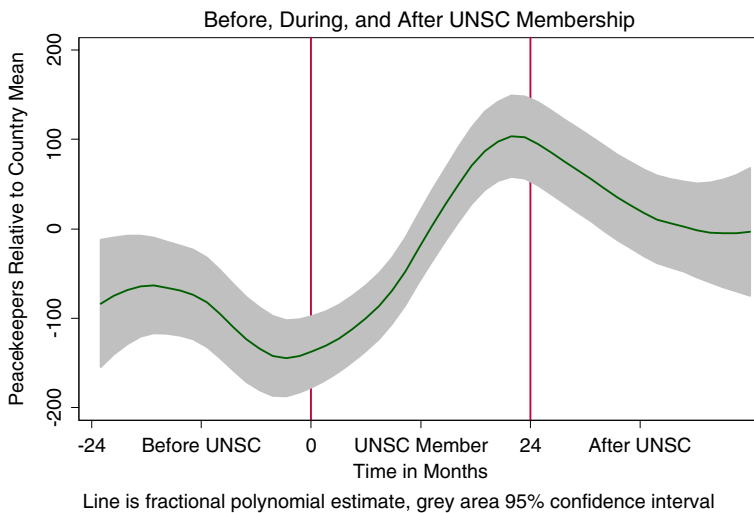
A natural way to evaluate this hypothesis is to ask whether non-permanent members increase their peacekeeping contributions. Volunteering peacekeepers is costly, as peacekeeping is a risky endeavor and domestic publics are not always tolerant of casualties endured in foreign adventures under UN auspices. Yet, contributing peacekeepers is also a strong show of support for the most important decisions taken by the UNSC. It is thus a good test of whether temporary increases in entanglement lead to more costly cooperative behavior.

Figure 1 provides a simple but powerful test of this proposition. The figure examines whether countries employ more peacekeepers during their non-permanent UNSC membership compared to the two years preceding and after their membership. The dependent variable is taken relative to the mean number of peacekeepers for each country during the 1991–2009 period.<sup>2</sup>

The number of peacekeepers a country employs increases sharply and continuously during countries' tenure to about 100 more than the average at the end. The membership effects are quite large. The mean number of peacekeepers for this sample is 514 so an additional 100 peacekeepers is a substantial relative increase. The effect is not caused by a few outliers<sup>3</sup> and applies to most countries. 64 % of countries had more peacekeepers

<sup>2</sup> I thank James Lebovic for sharing this data.

<sup>3</sup> I excluded outlier cases (countries that had increased peacekeepers by 1,000 or more in any period) and found the same pattern.



**Fig. 1** Average levels of peacekeepers

during their non-permanent membership than in the two preceding years and 67 % had more during their tenure than in the two years immediately afterward.

Dreher et al. (2013) find that within Africa and Asia states that contribute more troops are more likely to be elected to the UNSC. This may raise concerns about selection bias. However, non-permanent membership is non-renewable and has a fixed two-year term. This means that member states have no control over exit. Thus, the difference between membership levels and post-membership levels can be interpreted as a causal effect. The decline in peacekeepers after exit from the Council is not immediate, perhaps because it takes some time to withdraw peacekeepers. Nevertheless, the evidence suggests that the effect is not permanent.

Table 1 analyzes the hypothesis using a multiple regression specification commonly used in the literature that examines the effects of UNSC membership on aid (Kuziemko and Werker 2006; Dreher et al. 2009a, b). The dependent variable is the natural log (+1) of the number of troops a country contributes to UN peacekeeping missions in a given year.<sup>4</sup> All analyses include a lagged dependent variable and fixed country effects. This is appropriate in this context because the main exogeneity is that non-permanent membership has a fixed duration (2 years) and is non-renewable: that is it should lead to changing incentives over time within countries.<sup>5</sup> The model includes dummies for the 2 years before and after the UNSC election. Moreover, there are dummy variables for whether a country had lost a UNSC election and the

<sup>4</sup> The natural log is taken as the distribution of troop contributions is highly skewed. Nevertheless, the results are similar without transformation.

<sup>5</sup> Analysts are sometimes skeptical about fixed effects with lagged dependent variables due to the potential for Hurwicz or Nickell bias: that the fixed effects bias the estimate of the lagged dependent variable downwards. The main results also hold in a fixed effect specification without lagged dependent variable and in a random effects specification with a lagged dependent variable. They also hold in an error-correction specification where the dependent variable is differenced. Moreover, they hold when using monthly data where the number of time periods is so large that any Nickell bias should be very small. Finally, concerns about Nickell bias should be larger when estimating various time trends on each other as opposed to the effect of an indicator variable.



**Table 1** Country fixed effects regressions on natural log of yearly peacekeeping contributions by UNSC membership status

Variables	(1)	(2)	(3)	(4) Only UNSC members
Lagged DV	0.694*** (0.0263)	0.610*** (0.0263)	0.559*** (0.0261)	0.341*** (0.0521)
Two years before UNSC	0.0465 (0.130)	0.127 (0.130)	0.136 (0.138)	
UNSC	0.224** (0.118)	0.262** (0.111)	0.255** (0.117)	0.353** (0.175)
Two years after UNSC	-0.0663 (0.119)	-0.0265 (0.117)	-0.0356 (0.131)	
Two years before lost	-0.0774 (0.238)	-0.211 (0.232)	-0.221 (0.237)	
Term after lost election	-0.365 (0.279)	-0.441* (0.247)	-0.496* (0.278)	
Two years after term following lost election	0.0571 (0.166)	0.0323 (0.158)	0.0745 (0.160)	
First year after UNSC				0.349* (0.187)
Second year after UNSC				0.160 (0.196)
Peacekeepers region (natural log)		0.451*** (0.0433)	0.406*** (0.0571)	
Polity			0.0157 (0.0139)	
GDP per Capita (natural log)			-0.0448 (0.0336)	
Population (natural log)			1.053* (0.545)	
Agreement with the US in UN			-0.887* (0.475)	
Observations	2,288	2,288	1,861	385
R-squared	0.498	0.536	0.519	0.153
Number of countries	130	130	116	70

Dependent variable is the natural log of the average number of peacekeepers a country employed during a year. The average is calculated from monthly totals. The model is estimated using STATA's xtreg procedure with the robust option for standard errors. Model (4) only includes states in the two years before, during, and after UNSC membership.

Robust standard errors in parentheses (one-tailed for directional hypotheses) \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

years before and after this election.<sup>6</sup> While the results presented in the table use annual peacekeeping data, similar results hold with monthly data.<sup>7</sup>

Since exit is exogenous, control variables are not necessary for identification. Nevertheless, control variables can enhance the efficiency of estimates. I therefore

<sup>6</sup> A lost election is defined by having acquired at least 5 votes in an election. This is to rule out cases where countries get one or two "protest votes" even though they had not campaigned. This does not include cases where countries campaigned but did not participate in the election because they realized they would not get sufficient votes.

<sup>7</sup> The monthly data may be plagued by problems with unit roots. Although the main independent variable is not a trend variable but an indicator variable, there are other trend variables in the model whose coefficients become unreliable. For the annual data, we can reject the null-hypothesis from the augmented Dickey-Fuller test that all panels contain unit roots ( $p = .000$ , Inverse chi-squared test based on STATA command xtunitroot).



present results both with and without controls. Given the strong regional dimension to sending peacekeepers, I include the number of peacekeepers that are currently employed from a country's region as a control (Lebovic 2010). Since compensation for peacekeeping is fixed but the costs are not, I include GDP per capita, with the expectation that wealthier countries (whose soldiers are more expensive) are less likely to contribute. Democracies are more likely to participate in peacekeeping operations (Lebovic 2004). More populous nations are expected to contribute more as are countries that more frequently agree with the U.S. in UN General Assembly votes. The rationale behind the latter expectation is that the U.S. has a heavy say in what peacekeeping missions are approved. Countries whose foreign policy objectives are more closely aligned with those of the U.S. may also believe more frequently that a peacekeeping mission serves their interests.<sup>8</sup>

Table 1 presents strong support for the hypothesis that states contribute more peacekeepers when they are non-permanent members. A state that is a non-permanent member increases its peacekeeping commitment on average by between 22 and 26 % (depending on the specification). Estimates on the annual data were 2.2 % a month.<sup>9</sup> This is a substantial effect. Peacekeeping contributions during UNSC membership are significantly higher (at the 5 % level) than they were the 2 years before UNSC membership and the 2 years after membership.

There is no evidence of increases by candidates just before the election, regardless of whether these candidates eventually won or lost. This also holds if I limit the analysis to only those elections that included multiple candidates who acquired more than 10 votes.<sup>10</sup> States that lost an election on average decreased their peacekeeping commitments after the result although this effect is not significantly different from zero in all specifications. This could be part of a "middle-finger effect" in which domestic support for participation in peacekeeping operations declines after being snubbed.

The fourth model looks only at countries in the 2 years before, during, and after their UNSC memberships. The estimate of the effect of serving on the Council is now slightly higher: on average a UNSC member increases its peacekeeping contributions by 35 % on an annual basis during its membership. In the first year after UNSC membership, the average contributions are still up, presumably because longer lasting troop commitments were made during a state's tenure. In the second year after membership, contributions are back to pre-UNSC levels. The difference between the second year after membership and the membership period is significant at conventional levels. This confirms the insights from Fig. 1: UNSC non-permanent membership increases peacekeeping contributions only temporarily. There is no evidence of a socialization effect.

<sup>8</sup> I calculate voting coincidence with the U.S. by computing an index of agreement where identical votes are counted as full agreement whereas an abstention paired with a no vote count as .5 agreement. This is similar to "S-scores" or other Affinity indicators that are generally used in the literature.

<sup>9</sup> Results available from the author.

<sup>10</sup> This finding does not necessarily contradict the finding by Dreher et al. (2013) that states from Africa and Asia are more likely to get elected if they contribute more troops. Aside from the fact that finding is based on a different period and is restricted to two continents, the finding here says that states that get elected or compete for election do not increase their contributions just before the election. It could be that states are rewarded for long-term cooperative behavior.

#### 4 Regression discontinuity

The UNSC's prohibition on immediate re-election has been exploited by numerous political scientists and economists as a source of exogeneity (e.g., Kuziemko and Werker 2006; Dreher et al. 2009a, b). Most international organizations do not, however, prohibit re-election. There is another institutional feature that may help resolve selection issues in estimating effects of institutional entanglements: many institutions elect their members via competitive elections. This means that the selection process is observable: we know which states wanted positions of authority and how much support these states received from others. This information may prove useful to alleviate concerns that states self-select into these positions and that the membership tends to reward states that are more cooperative.

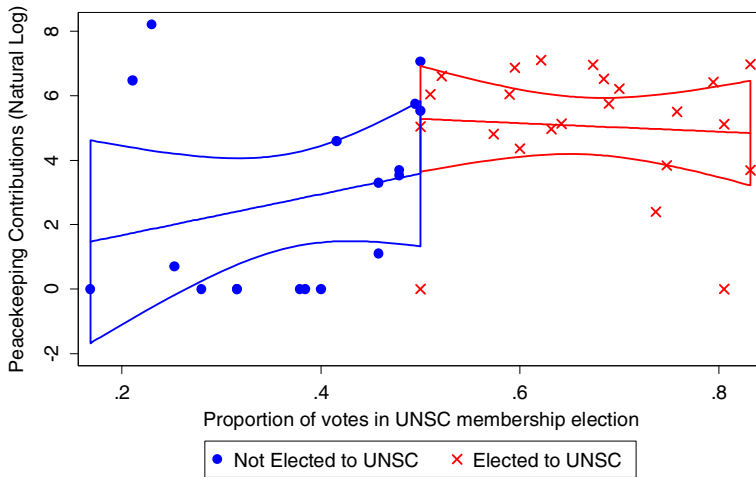
Political scientists have used competitive elections as a way to identify causal effects using a regression discontinuity (RD) design (e.g., Lee 2008; Eggers and Hainmueller 2009; Gerber and Hopkins 2011) although not in the context of international organizations. The RD design is a special case of matching where the selection variable is observed and thus does not need to be estimated based on covariates (Heckman et al. 1999). This gives RD an internal validity advantage over other quasi-experimental approaches (Imbens and Lemieux 2008; Lee 2008). The canonical application is in educational research, where students sometimes enter a treatment only if they exceed a target test score. The effect of the program can then be evaluated by examining if a discontinuity in student performance occurs at the threshold level for admittance. Yet, vote shares can also be used for that purpose. States that were just elected to the UNSC and states that by a narrow margin failed to acquire enough votes are similar in their desire to acquire a position of authority and they are nearly similar in their ability to attract support for this. We may be able to identify a causal effect of institutional membership on subsequent behavior by comparing these states.

A major impediment to employing this model in the context of international institutions is that there are usually only a limited number of states that participate in elections. During the entire period of analysis, there were only 41 states that partook in competitive elections for UNSC membership. Figure 2 compares the natural log of peacekeeping contributions of the 23 states that won these elections to the 18 states that lost.<sup>11</sup> The x-axis displays the proportion of the votes each state received. If states selected states based on their likely cooperative behavior, the slope should be positive and steep. If getting elected matters, then we would expect a discontinuity right around the .5 mark. Such a discontinuity would indicate that it is not simply the case that the UN membership selects more cooperative states but that getting elected makes a difference too.

There is some graphical evidence for that proposition in Fig. 2. In addition, I ran a regression analysis including lagged peacekeeping contributions and the vote proportions (both in linear and quadratic). Getting elected boosts peacekeeping contributions by about 95 % over the two year period, which is an estimate that is similar in size to that from model 4 in Table 1.<sup>12</sup> Yet the 95 % confidence interval is large (10–200 %) and Fig. 2 also displays the potential weaknesses of the RD approach. Ideally we would like

<sup>11</sup> UNSC elections are run within regions, which means that sometimes there may be three states vying for two positions, thus explaining the higher number of winners.

<sup>12</sup> The estimate is significant at the 5 % level (one-tailed). Results available from author.



**Fig. 2** Peacekeeping contributions and UNSC membership

to only compare states that narrowly lost and won. States that handily lost elections are likely quite different than states that narrowly lost. However, there are too few data points. This makes the analysis “model-dependent,” meaning that it hinges on the correct specification of the relationship between how many votes a state gets and how cooperative a state is.

Recognizing this weakness is important but it does not fully negate the advantages of observing what states wish to get selected and how much support they get. This is useful information for causal inference even if it rarely comes in a form that allows for an ideal-typical RD design. Given that the UNSC case is better identified using exogenous exit, I will illustrate the potential and limitations of an RD-like design using a different example: the election of ICC judges.

## 5 National judges and cooperation with the ICC

In a prominent article in the *American Political Science Review*, Judith Kelley (2007) argued that commitments to the ICC may make states more willing to undertake costly actions in support of that institution. She found that states that ratified the Rome Treaty and have a strong domestic rule of law are much less likely to sign a Bilateral Immunity Agreement (BIA) with the United States. A BIA undermined the ICC as it promised not to surrender Americans to the newly created ICC. This request was backed up by the American Service-Members’ Protection Act, sometimes labeled “The Hague Invasion Act,” which authorizes the President to use “all means necessary and appropriate to bring about the release of any US or allied personnel being detained or imprisoned by, on behalf of, or at the request of the International Criminal Court” and obliges the US to withdraw military aid from ICC state parties that did not sign BIAs.<sup>13</sup>

<sup>13</sup> Exceptions were made for NATO states and some other allies (see: <http://www.state.gov/t/pm/rls/othr/misc/23425.htm>).

Kelley acknowledges that her research design suffers from a selection problem that cannot be easily resolved: how do we know that there isn't some unobserved factor that makes some states both more likely to ratify the Rome Treaty and refuse to sign BIAs.<sup>14</sup> I propose a different design: rather than comparing ratifiers and non-ratifiers we may compare states whose nationals served as judges on the first court with those who had no judges on the court. A few examples illustrate that having a national as a permanent judge plausibly works as a treatment that intensified the commitments of some states but not others.

The first case is raised by Kelley (2007). Costa Rica's president blocked the nomination of Elizabeth Odio-Benito for political reasons (she had been a vice-president for the opposition party). Consequently, Odio-Benito was nominated by Panama and elected. The blockage of Odio-Benito's nomination spurred domestic mobilization and increased public support for the ICC, thus tying the government's hands when faced with U.S. demands to undermine the court (Kelley 2007, p583–4).

Another example is Bolivia, which signed a BIA in July 2003, thereby receiving a six-month waiver from U.S. sanctions, but did not ratify the BIA. In response, the U.S. cut military assistance in 2004.<sup>15</sup> The fact that Bolivia had a national on the court (former minister of Justice René Blattmann) featured prominently in the ratification debates and in appeals to the Bolivian Chamber of Deputies by NGOs, such as Amnesty International<sup>16</sup> and the Coalition for the International Criminal Court.<sup>17</sup> Sacha Llorenti, a parliamentarian and president of Bolivia's National Human Rights Assembly argued that: "Bolivia would be the only country in the world to agree to such a pact that also has a judge on the court."<sup>18</sup>

Even in the only country with a judge on the court that did ratify a non-surrender agreement,<sup>19</sup> Ghana, this fact was emphasized in parliamentary debates: "It will be the hallmark of double standards for Ghana to ratify the Rome Statutes that established the International Criminal Court, nominate its Vice-President and turn around to ratify an agreement that obviously undermines the integrity of the Court."<sup>20</sup>

By contrast, ten of the twenty-five states whose candidates failed in their bids to acquire judgeships signed non-surrender agreements with the United States. This difference in

<sup>14</sup> Kelley discusses alternative approaches to estimation, such as Heckman selection models, but argues that there are no obvious variables that can be excluded from the ratification equation and thus that the effect of ratification cannot be separately identified, although she highlights that domestic rule of law does not significantly predict ratification.

<sup>15</sup> Ribando (2006). This happened before Evo Morales was elected in December 2005 on an anti-American platform.

<sup>16</sup> See: <http://www.amnesty.org/en/library/asset/AMR18/002/2004/en/45f6cfe7-d630-11dd-ab95a13b602c0642/amr180022004en.html> (accessed June 19, 2009).

<sup>17</sup> <http://www.iccnw.org/documents/Boliviare sistBIA01Jun04.pdf>

<sup>18</sup> Quoted in: "U.S. Threatens Bolivia in Effort to Secure Criminal Court Immunity." Pacific News Service, March 3 2005. [http://news.pacificnews.org/news/view\\_article.html?article\\_id=40d8f93957008266edbc544c21df75be](http://news.pacificnews.org/news/view_article.html?article_id=40d8f93957008266edbc544c21df75be) (accessed June 19, 2009).

<sup>19</sup> Ghana did not sign a BIA treaty but concluded an executive agreement stating that it would not extradite U.S. nationals to the jurisdiction of the ICC. Since these are treated as equivalent by the US State Department and NGOs such as ICCnow, I also do not highlight this distinction.

<sup>20</sup> Parliamentary debate, October 29, 2003. Quoted in: [http://www.iccnw.org/documents/HighOfficialQuotes\\_Current.pdf](http://www.iccnw.org/documents/HighOfficialQuotes_Current.pdf). Ghanaian judge Kuenyahia is Vice-President of the ICC.

proportions is significant (at the 5 % level) but not conclusive about causality. It is possible that the Assembly of State Parties was simply successful in electing judges from countries whose governments were already so committed to the Court that they would not undermine it by signing a BIA. The RD design investigates this.

### 5.1 The ICC elections

The first ICC elections were held on February 7, 2003. On that day, a few non-surrender agreements were already in force but none that involved the 43 states that had put forth candidates for the 18 judgeships.<sup>21</sup> To be elected, a judge had to obtain a two-thirds majority of the votes (56) of the Assembly of States Parties. Each state issued as many votes as there were judgeships to be awarded in a given round of voting. Votes were cast through secret ballots. Moreover, states were required to elect a minimum number of candidates from each regional group, gender, and legal expertise (criminal law and international law).<sup>22</sup> It took 33 rounds of voting to elect all 18 judges.<sup>23</sup> The group of states whose candidates narrowly lost includes countries that have been among the most active supporters of the ICC such as Argentina, Belgium, Spain, Switzerland, and Portugal. No judge received more than 78 % of the vote. Elected judges were assigned by lot to terms of 3, 6, and 9 years. All analyses focus on state behavior that occurred between the first and second elections (January 26 2006).<sup>24</sup>

A critical assumption in any RD analysis is that states cannot precisely control the number of votes they get around the threshold (Lee 2008). Caughey and Sekhon (2011) show that this assumption was not satisfied in the context of U.S. House elections: bare winners were significantly different from bare losers on important covariates. They showed this by plotting p-values from a comparison of covariate means in a sample of elections that came within a 0.5 % margin. In the absence of sufficient data, we perform a parametrized version of this test. If elected judges are from states that are observably different in a way that is not captured by vote margins, then the Elected variable should significantly correlate with observables even after controlling for vote margins.

Figure 3 produces the p-values for regressions of the treatment variable on all covariates from Kelley (2007) controlling for the number of votes each states' candidates received (as a second order polynomial). These include general variables such as GDP and Polity scores, and specific ones: such as whether countries could expect a cut in military aid if they refused to sign a BIA. I also include two variables that were significant in Simmons and Danner's (2010) analysis of Rome Treaty ratification: internationalized armed conflict on its territory between 1988 and 2003 and whether a country has a common law regime.

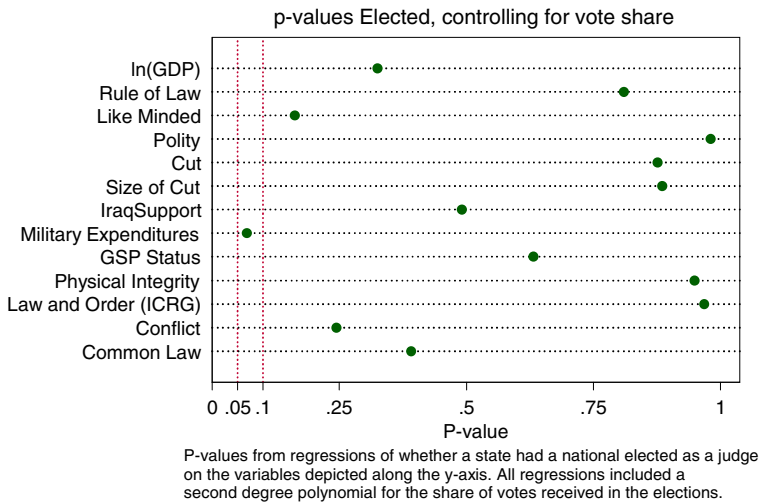
None of the regressions on these 13 covariates yields a p-value smaller than .2. Thus, there is no evidence that having a judge elected is correlated with the

<sup>21</sup> Results are available here: [http://untreaty.un.org/cod/icc/elections/results/1st\\_round.htm](http://untreaty.un.org/cod/icc/elections/results/1st_round.htm) (accessed July 2, 2009).

<sup>22</sup> For more detail see: ICC/ASP/1/4 "Elections of the Judges of the International Criminal Court": <http://www.iccnw.org/documents/NoteElectionJudges200212Eng.pdf> (accessed July 2, 2009).

<sup>23</sup> The last elected judge was Claude Jorda (France) who defeated Nigerian candidate Adolphus Karibi-White (the other candidates had been withdrawn).

<sup>24</sup> Five of the six judges that sat for reelection were reelected in 2006, the sixth lost. There were only 10 candidates at the second election.



**Fig. 3** Covariate balance

covariates that the literature identifies as important for cooperation with the ICC after controlling for the proportion of votes a state's candidate received. This obviously only alleviates concerns that there are observable differences between members and non-members.

As pointed out in the discussion of the UNSC, the assumptions needed for causal inference grow stronger the further subjects are from the threshold value. Some analysts prefer a non-parametric analysis of RD designs that are limited only to states that are right around the threshold, although this is not necessarily preferable to a parametric approach (Lee and Lemieux 2009). If the relationship between the forcing and the outcome variables is truly linear, then a simple OLS regression that includes the forcing variable and an indicator for whether a state had a judge elected is an unbiased and the most efficient estimator.

Given the small number of observations, it is not feasible to limit the analysis to those observations to within 5 or 10 % of the threshold. Obviously, the assumption of local linearity (for observations close to the threshold) is less strong than assuming that the relationship is linear across a wider range of observations. Thus, assessing the robustness of the findings to higher-order polynomials becomes more important. I present results with quadratic relationships. In addition, including covariates can help to eliminate some bias that results from including a wider range of observations (Imbens and Lemieux 2008). This makes the inferences here more model dependent than they would be in an ideal RD design. Nevertheless, observing the variable that selects states into a treatment condition is still an extremely valuable asset.

With a small sample, base-line covariates can help improve the efficiency of estimates and may help eliminate some bias when including observations that are relatively far removed from the threshold (Imbens and Lemieux 2008). Again, this makes the inferences more model dependent than one would like in an ideal RD setting. I include covariates that were significant in Kelley's (2007) analysis: the natural log of GDP per capita, whether a state would be subject to military sanctions, the rule of law (from the World Bank), whether a state belonged to the "like-minded" group during the negotiations

for the Rome Treaty, and levels of democracy (Polity scores).<sup>25</sup> I also include military expenditures as this variable was the only one with a p-value below .25 in Fig. 3.

Table 2 reports the results from a logit regression.<sup>26</sup> I include both the proportion of votes states received on the first ballot and the average they got on the first and the highest vote total as indications of support from the membership. In all models states whose candidates were elected were indeed significantly less likely to sign bilateral agreements than states whose candidates were not elected. The estimated effect of having a judge is very large although imprecisely estimated. Holding the other variables at their means, a state whose judge was elected is on average 45 percentage points less likely to sign a BIA (based on model 2). Another useful comparison may be to look at states whose candidates received large proportions of the vote but were not elected. Among the top half (12) vote getters whose candidates failed to be elected five ratified a non-surrender treaty, in contrast with one out of the 18 states whose judges won with less than twenty percentage points.

If we were to take the absence of a relationship between vote shares and BIA signing as evidence that elections do not select nationals from more cooperative states, it is plausible to generalize to the full ICC membership. This relies on the tenuous assumption that the states that put forth candidates for judgeships are similar to those that do not. Model (5) replicates model 5 from Kelley's article with dummies for whether a judge was elected and whether a state's candidate lost. There is a statistically significant negative effect of having a judge and no significant effect of having a candidate but losing. This suggests that any ICC member states who would be assigned a judge would have been less likely to sign a BIA, although the validity of this inference does depend on stronger assumptions. One cannot extrapolate further. It would be silly to suggest that offering Sudan an ICC judge would alter its behavior.

## 5.2 Cooperation with the court

To further investigate this issue, I developed a scale that captures broader cooperation with the ICC based on three pieces of domestic legislation and two treaties.

First, the ICC is based on the complementarity principle, which leaves first responsibility for war crimes trials to states. For this to work properly, states must adopt the crimes articulated in the Rome Statute into their domestic penal codes and must identify criminal responsibility and fair trial guarantees in accordance with the Statute. Second, states must enact cooperation legislation, which guarantees cooperation in investigations and access for the ICC. Amnesty International has coded whether each state drafted or adopted each type of legislation in January 2006, just

<sup>25</sup> Below are the descriptive statistics on these variables

Variable	N	Mean	Std. Dev.	Min	Max
Ln (GDP)	41	25.15414	1.867464	21.50534	28.1402
Military Aid cut	41	.3902439	.4938648	0	1
Rule of Law	41	.2514634	.9500436	-1.4	1.98
LikeMinded	41	.6829268	.471117	0	1
Polity	41	7.512195	3.099371	0	10

<sup>26</sup> For the application of logit models to regression discontinuity models, see Berk and De Leeuw (1999). A linear probability model returns nearly identical results.



**Table 2** Logistic regression on whether state signed BIA

Variables	(1)	(2)	(3)	(4)	(5)
	Votes on 1st ballot		Average 1st and Highest ballot		
Elected	-6.411** (3.441)	-5.740** (3.118)	-7.152** (4.101)	-6.402** (3.814)	-2.235* (1.205)
Vote proportion	0.150* (0.0833)	0.0790* (0.0425)	0.143 (0.0939)	0.277 (0.244)	-
Vote proportion <sup>2</sup>	-	-0.000700 (0.000863)	-	-0.00178 (0.00324)	-
Ln (GDP)	-0.603 (0.610)	-0.800 (0.696)	-0.683 (0.607)	-0.778 (0.681)	-0.397 (0.251)
Military aid sanctions	-2.096 (1.557)	-2.075 (1.637)	-1.719 (1.430)	-1.940 (1.558)	-2.365** (1.015)
Rule of law (World Bank)	-3.243** (1.600)	-3.502** (1.757)	-2.858** (1.444)	-3.215** (1.623)	
LikeMinded	-1.075 (1.690)	-1.066 (1.759)	-0.598 (1.560)	-0.847 (1.689)	
Polity	0.182 (0.325)	0.0638 (0.360)	0.0679 (0.292)	0.0484 (0.353)	
Military expenditures	-0.348 (0.734)	-0.198 (0.779)	-0.182 (0.671)	-0.162 (0.752)	-2.540 (1.738)
GSP status					3.818*** (1.096)
Size of cut					0.133 (0.0903)
IraqSupport					0.808 (0.797)
Lost election					-0.361 (0.852)
Constant	11.33 (14.06)	22.66 (17.26)	13.62 (13.88)	14.36 (14.99)	8.280 (5.451)
Observations	41	41	41	41	99

The dependent variable is whether a state signed a Bilateral Immunity Agreement with the United States. The key independent variable Elected equals 1 when a state's national served as an ICC judge. The models are estimated using the logit command in STATA 12. Models 1 and 2 use the number of votes a candidate received on the first ballot in ICC elections, models 3 and 4 are based on the average between the highest and the first total. Model 5 uses the full ICC membership and thus excludes the election variables given that not all ICC members had nationals run for judgeships.

Standard errors in parentheses \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (one-tailed for directional hypotheses)

before the second ICC election (Van der Pas 2006). Third, Simmons and Danner (2010) coded whether states had adopted crimes against humanity in domestic penal codes by June 2005. This indicator is similar but not identical to the one coded by Amnesty International.

Fourth, states may ratify the Agreement on the Privileges and Immunities of the Court (APIC), which ensures access and immunity for ICC employees. Finally, as discussed above, states may ratify a BIA. In this instance, not ratifying is the cooperative outcome.

I combined the five indicators in a summary scale. For each indicator, a draft/signature confers one point and an enactment/ratification two points. The resulting 10-point scale is roughly normally distributed (mean 5.8, standard deviation 2.8) with high reliability (Cronbach's alpha is .76).

Figure 4 graphically illustrates the relationship between vote proportions, electoral status and the cooperation scale among states whose candidates received at least 10 % of the vote. There is no obvious linear or non-linear relationship between vote shares and cooperation. The data are noisy but states with judges have higher average cooperation scores. Table 3 reports the linear regression results on the composite cooperation scale. Aside from the covariates introduced before, I follow Simmons and Danner (2010) in including an indicator for whether a country experienced an international or an internationalized armed conflict on its territory between 1988 and 2003, acknowledging that among ICC state parties the experience of war is a powerful incentive to enact legislation to prevent recurrence of atrocities.<sup>27</sup> Moreover, countries with common law systems are generally much less reluctant (or slower) in signing human rights treaties and adopting legislation, presumably because once these are signed they have greater consequences as judges are more powerful, independent, and have a broader interpretive role (Simmons 2009).

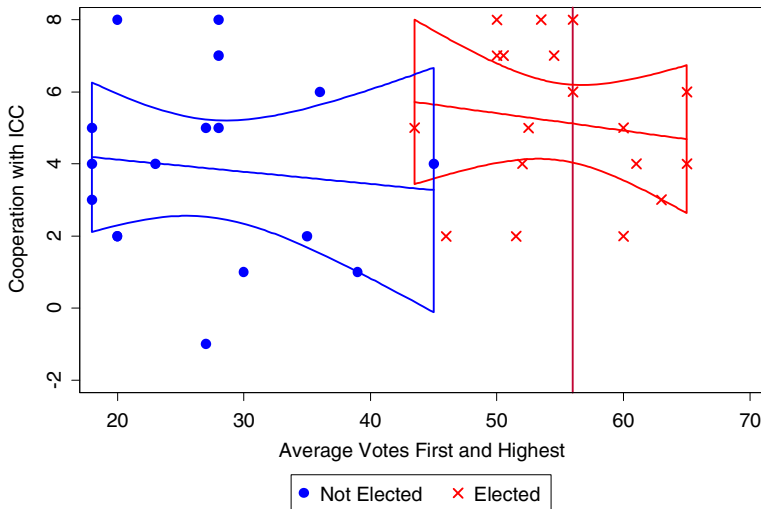
The results reveal a significant and sizeable effect of having a national elected as judge. Countries with nationals as judges had about three points extra on the ten-point scale, amounting to an additional piece of legislation enacted in support of the ICC. The results are robust to controlling for the highest number of votes received. Model (5) generalizes to the full ICC membership using Kelley's model and finds a similar effect for that population. Again, the inference is more model dependent than in an ideal RD situation.

For illustrative purposes, it is useful to compare similar countries for whom election outcomes differed. For example, the Swiss candidate received 45 votes on the first ballot and the German candidate only 43. Neither was sufficient to get elected in the first round. Nevertheless, the German candidate (Hans-Peter Kaul) was elected in a later round. Germany scores four points higher on the cooperation scale than Switzerland. Kaul was a life-long diplomat who was involved in the creation of the ICC and actively participated in debates on how Germany should implement the Rome statute (e.g., Kaul 2005).

## 6 Pledges and UN human rights council membership

The widely criticized UN Human Rights Commission was replaced in 2006 by a new UN Human Rights Council (HRC). The main impetus for reform was a desire to

<sup>27</sup> PRIO/Uppsala Armed Conflict Dataset. Danner and Simmons interact this variable with democracy. Doing so does not generate different results here.



**Fig. 4** Cooperation with ICC and judgeships

create higher standards of accountability. While the reforms were not as far-reaching as many desired, they contained some interesting features. First, members are elected directly and individually by secret ballot in the UN General Assembly. Second, candidate countries make pledges that detail the human rights initiatives they would undertake if elected. Third, member countries' human rights records are reviewed under the universal periodic review mechanism.

To the extent that member state pledges contain verifiable promises, such as ratifying binding international human rights treaties, these reforms introduce an opportunity for evaluating whether IO participation increases the extent to which countries uphold their commitments to cooperate. Earlier studies indicate that states that have ratified a human rights treaty are more likely to be held accountable (Lebovic and Voeten 2006) and thus that there are costs to ratification, although voting in the Council remains highly political (Hug and Lukács 2013). All states that are successfully elected are reviewed and are thus held accountable internationally for their promises.<sup>28</sup> If states alter their behavior in response to institutional membership, then we would expect the states that narrowly won elections to keep more of these promises than the states that narrowly lost.

There is some anecdotal evidence for this. For example, the Philippines pledged to "strengthen domestic support for the ratification of the Optional Protocol to the Convention against Torture" (CAT). The optional protocol provides for "a system of regular visits undertaken by independent international and national bodies to places where people are deprived of their liberty, in order to prevent torture and other cruel, inhuman or degrading treatment or punishment." Philippine President Arroyo highlighted in the ratification document that: "[...] The Philippine government is morally obliged to strengthen the country's compliance with international human rights instruments since the Philippines is a current member of the U.N. Human Rights Council."<sup>29</sup> The chairman

<sup>28</sup> Every UN member state will be reviewed every 4 years but all states undergo a review during their HRC membership.

<sup>29</sup> <http://www.apt.ch/npm/asiapacific/Philippines1.pdf>, April 22, 2008 (accessed June 8, 2011).

**Table 3** Linear regression on cooperation index with ICC

Variables	(1)	(2)	(3)	(4)	(5)
	Votes on 1st ballot		Avg 1st and Highest Vote Totals		
Elected	3.024** (1.346)	3.042** (1.370)	3.609** (1.634)	4.024** (1.739)	2.041*** (0.647)
Vote proportion	-0.0599 (0.0391)	-0.0378 (0.113)	-0.0729 (0.0464)	0.00120 (0.110)	
Vote proportion <sup>2</sup>		-0.000288 (0.00137)		-0.00109 (0.00147)	
Ln (GDP)	0.574** (0.250)	0.577** (0.255)	0.540** (0.242)	0.560** (0.245)	0.632*** (0.124)
Democracy	0.311 (0.259)	0.297 (0.272)	0.342 (0.261)	0.301 (0.268)	
Conflict	1.361* (0.747)	1.323 (0.780)	1.399* (0.751)	1.308* (0.766)	
Common law	-1.184 (0.874)	-1.189 (0.888)	-1.209 (0.871)	-1.210 (0.877)	
Military aid sanctions	1.150 (0.797)	1.134 (0.813)	1.177 (0.798)	1.118 (0.808)	1.592*** (0.568)
Rule of law	0.989 (0.693)	0.977 (0.706)	1.014 (0.693)	0.986 (0.699)	
Like minded	0.682 (0.830)	0.669 (0.845)	0.652 (0.825)	0.617 (0.832)	
Military expenditures	-0.0482 (0.0523)	-0.0501 (0.0539)	-0.0427 (0.0511)	-0.0500 (0.0524)	
Lost					-0.0486 (0.0420)
GSP					0.779 (0.530)
Size of cut					-2.740*** (0.582)
IraqSupport					-0.0172 (0.0122)
Constant	-9.673 (6.370)	-10.12 (6.813)	-8.397 (6.207)	-10.02 (6.626)	0.0126 (0.489)
Observations	42	42	42	42	-12.13*** (2.922)
R-squared	0.619	0.619	0.620	0.627	98
					0.560

The dependent variable is a ten-point cooperation scale with the ICC. The models are estimated using the regress command in STATA.

Standard errors in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (one-tailed for directional hypotheses)

of the Presidential Human Rights Committee, Eduardo R. Ermita, noted that this ratification was “part of the momentum on human rights matters the Arroyo government promised to continue after undergoing the Universal Periodic Review at the United Nations Human Rights Council.”<sup>30</sup> Similarly, the Philippines promised to ratify the Second Optional Protocol to the International Covenant on Civil and Political Rights, which abolishes the death penalty. Soon after election to the Council, the legislature adopted domestic legislation that abolished the death penalty and ratified the protocol.<sup>31</sup>

During the first elections for the newly created Human Rights Council, there were 68 candidates for 47 seats.<sup>32</sup> By lot, states were assigned to 1, 2, or 3 year terms. This allows us to evaluate two questions. First, did states that got elected keep more commitments than states that were narrowly defeated? Second, did elected member states that were randomly assigned to a shorter term keep fewer of their pledges (in the same three-year period) than states that were assigned to a longer term?

Unfortunately only 28 states submitted pledges that contained verifiable commitments to ratify one or more international human rights treaties. These states made an average of 2.5 pledges of which 56 % were kept (meaning that ratification of the treaty followed within three years of making the pledge). Figure 5 plots the commitments kept as a function of the number of votes each state received on the initial ballot. Again, there is no relationship between the number of votes received and the proportion of commitments a state keeps. Nevertheless, there are significant differences based on HRC member state status. The 21 states that were elected upheld 61 % of their commitments. The 7 states that failed to win elections upheld 42 % of their commitments. States that were elected to only a one-year term upheld 36 % of their commitments against 64 % for states to whom the lot assigned two- or three-year terms. These differences are statistically significant at the 5 % level.<sup>33</sup>

Table 4 uses regression analysis to estimate the effects of HRC election on the proportion of commitments made. The regression controls for the number of votes the country acquired and the square root of the vote total to check whether the observed differences occur because states likely to uphold more commitments acquire more votes. Models 3 and 4 also control for the number of commitments made (given that it is harder for states to uphold a larger proportion if they make more commitments), and for differences in democracy. The democracy (Polity) variable is included because the literature suggests that democracies have greater audience cost and should thus be more likely to keep their commitments regardless of whether they are elected.

As before, vote shares are insignificant. Moreover, states that made verifiable commitments were no more likely to get elected and received no more votes than states that made no such commitments, despite the efforts by Amnesty International and others to distribute lists of countries that had and had not made verifiable commitments.<sup>34</sup>

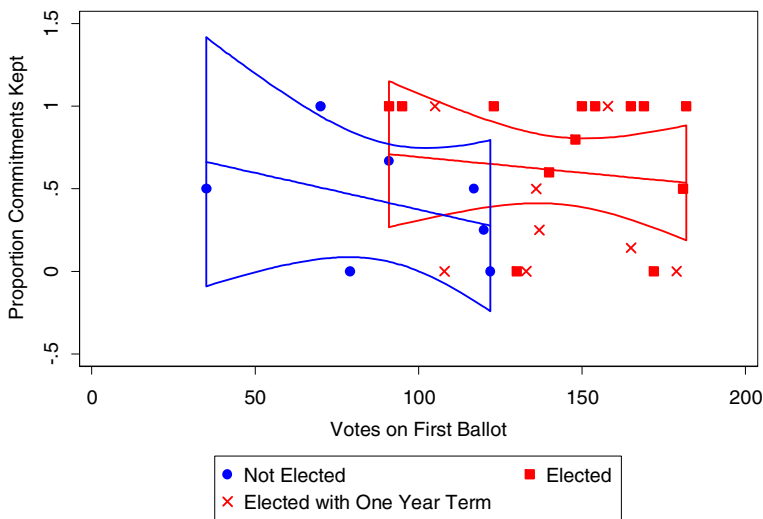
<sup>30</sup> Ibid.

<sup>31</sup> 20 November 2007.

<sup>32</sup> Later elections have been much less competitive.

<sup>33</sup> I examined covariate imbalance as in Fig. 3 for GDP per capita, democracy (Polity), CIRI empowerment and physical integrity rights indicators, and common law. These are all important determinants of treaty status in the literature. Elected status was not significant in any regression.

<sup>34</sup> Amnesty International, *Amnesty International's Guide to UN Human Rights Council Candidates* (2006).



**Fig. 5** Human rights commitments and elections

By contrast, getting elected does exert an influence on behavior as does the assignment by lot to a short term. Countries that were elected and randomly assigned to a two- or three-year term upheld about 50 percentage points more of their commitments than countries that were not elected. This amounts on average to one additional human rights treaty that is ratified, plausibly resulting in meaningful long run effects on rights (Simmons 2009). Countries that were assigned to a one-year term upheld 37 percentage points fewer commitments than their counterparts that

**Table 4** Linear regression on proportion of commitments held

Variables	(1) First round	(2) Maximum votes	(3) First round	(4) Maximum votes
Elected	0.528** (0.238)	0.530** (0.259)	0.478** (0.256)	0.505** (0.266)
One year term	−0.414** (0.179)	−0.420** (0.184)	−0.369* (0.195)	−0.366* (0.196)
Votes	−0.00583 (0.0117)	−0.00771 (0.0123)	−0.00507 (0.0121)	−0.00728 (0.0126)
Votes squared	1.08e-05 (4.66e-05)	2.02e-05 (4.80e-05)	1.06e-05 (4.82e-05)	1.92e-05 (4.91e-05)
Number of commitments made			−0.0300 (0.0556)	−0.0415 (0.0518)
Polity			−0.0101 (0.0193)	−0.0125 (0.0191)
Constant	0.847 (0.670)	0.912 (0.696)	0.920 (0.700)	1.068 (0.732)
Observations	28	28	28	28
R-squared	0.255	0.241	0.273	0.278

Linear regressions on the proportion of treaties that a state actually ratified after promising ratification while standing for HRC election. Estimated using regress in STATA 12.

Standard errors in parentheses \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (\*\*\*)  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$  (one-tailed for directional hypotheses)

were elected to longer terms, thus making them barely distinguishable from the group of countries that failed to get elected. The sample is small but this does indicate that HRC membership increased treaty commitments.

## 7 Conclusions

Seemingly innocuous increases in the extent to which states are entangled with the purposes of an IO can have meaningful behavioral consequences. Simply acquiring a position of authority in an IO can make states more willing to incur the wrath of the U.S. (BI Agreements), adopt domestic legislation that changes the penal code (ICC case), ratify legally binding treaties (UNHRC case), and even send its own soldiers into dangerous peacekeeping missions (UNSC case). The evidence from this latter case is the most credible given that it is based on a large number of cases with data over many years and clearly concerns a costly act of cooperation.

The analyses suggest that holding a position of authority as opposed to competing for such a position exerts influence on behavior. There is little point in becoming more cooperative out of a desire to obtain high office given that there is no descriptive evidence that UN elections reward such behavior. Yet, once states are in, the increased entanglement with the institution can be used by domestic and international actors to hold them accountable. The absence of incentives to cooperate in order to win high office puts a limit to the degree to which positions of authority can be used to increase overall cooperation. Peacekeeping contributions could benefit greatly if states thought their chances of UNSC non-permanent membership would improve markedly with increased cooperation. Overall peacekeeping would benefit even more if there were evidence consistent with the socialization story that participation leads to enduring increased cooperation. Instead, I only find evidence for a temporary boost in cooperation. Thus, the news contained in this note is not all good for IOs.

The utilization of the regression discontinuity design, random assignment to different term lengths, and exogenous exit all ensure high internal validity of the case studies. The small sample size makes the RD design heavily parametrized. Similar issues are likely to occur in future applications of RD designs to international affairs. On the other hand, it is a major advantage to observe the selection process, which is almost always hidden in IR applications. Even if we have to make assumptions about the functional relationship between the selection variable and outcomes over a wider range of cases than we would like, these assumptions are arguably less strong than assuming away the role of unobservables in selection.

External validity is another concern. Most notably, the findings only apply to the group of states that have a desire to obtain positions of authority and receive at least a modicum of support for this. This is a reasonably sized group of states but it is not the universe of states. As noted earlier, we could not extrapolate that Sudan would behave differently if only it had a judge on the ICC. Given that elections appear not to select more cooperative states, the desire to stand for election is a more serious limiting factor for the scope of the findings. It could be that some states avoid member status because they do not want to be held accountable for their promises. Indeed, this is what may have kept the U.S. from participating as a candidate in the 2006 UNHRC elections. Yet, among those that stand for election, those that win behave differently than those that just lost.



The general message this article seeks to convey is that scholars should look beyond the effects of IO/treaty membership towards the behavioral effects of participation in IOs. First, unlike ratification, selection into participation is often (but not always) governed by common, observable, and sometimes partially exogenous processes. This allows for more plausible identification strategies of causal effects. For example, the universal periodic review is conducted at set intervals, thus making their timing exogenous. Some commissions or courts use majority vote, allowing for regression discontinuity designs to evaluate the impact of decisions. Some regions use rotation as a basis for UNHRC membership. If membership encourages treaty ratification then this yields a plausible instrument.

Second, studying participation may yield more precise insights into what features of institutions are responsible for behavioral changes. For example, this study revealed that it is not the competitiveness of elections but the actual effects of membership that led to changes. Similarly, it would be useful to evaluate review systems or other monitoring effects within regimes in order to evaluate what works and what can be improved.

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