

Adaptation to International Organizations*

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Abstract

This article examines how international regime complexity affects diplomacy. We argue that diplomats modify their behavior in anticipation of changes to their social environment, driven by psychological pressures to conform rather than learning or socialization. We test this theory of diplomatic adaptation with a natural experiment. Once every four years, states undergo Universal Periodic Review, an assessment of their human rights records at the UN Human Rights Council. Delegates also convene annually for the International Labour Conference to debate the International Labour Organization's policies. Both events are held at the Palais des Nations in Geneva, many delegates represent their states at both forums, and the timing of UPR was set by lottery. We leverage this lottery and the names of over 37 thousand delegates to assess how exposure to UPR – at both the state and individual levels – impacts debate at the ILC. We show that government delegates use human rights language more in ILC debates before UPR and, contra socialization theory, emphasize human rights less often after UPR. These effects are driven by the exposure of individual diplomats to UPR.

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1 Introduction

The past few decades has witnessed a rapid proliferation in the number of international institutions. There are now hundreds of international organizations and thousands of multilateral treaties governing nearly every conceivable policy issue in all regions of the world (Pevehouse et al., 2020; Schmidt, 2024). This growth has generated a dense network of overlapping global governance institutions, often characterized by competing mandates, contradictory rules, and ambiguous obligations.

The emergence of this “international regime complexity” has sparked debate over its merits and pitfalls (Alter and Meunier, 2009). On one hand, overlapping institutions may counteract one another. Concerns abound over institutional competition (Qian, Vreeland and Zhao, 2023), forum shopping (Busch, 2007), the prospect of outside options limiting institutional leverage over states (Lipsy, 2017), and additional opportunities for powerful states to abuse their institutional privileges (Drezner, 2009). However, overlapping institutions can also generate positive complementarities. Multiple institutions governing the same issue area present states with more opportunities for socialization and the diffusion of policy idea (Johnston, 2008; Greenhill, 2015). Overlapping institutions can also collaborate with one another and fill governance gaps unaddressed by prior agreements, deepening international cooperation (Clark, 2021; Keohane and Victor, 2011).

In this article, we examine how states and their diplomats navigate regime complexity in a case where positive complementarity is most likely to exist: the overlap of the UN Human Rights Council (HRC) and the International Labour Organization (ILO). Both institutions’ seek to promote human rights through public, deliberative policy making aimed at fostering the development, adoption, internalization, and enforcement of international laws and norms (Weisband, 2002; Carraro, 2019). These overlapping institutions should consequentially reinforce each other through processes of policy learning and socialization. The policies, practices, and discourses states are exposed to at the HRC should affect, and be reflected in, their subsequent words and deeds at the ILO.

We develop an alternative theory of diplomatic adaptation. We argue that when diplomats are required to represent their states at international forums, they adopt social roles and discourse consistent with the expectations of individuals operating in those institutions. When diplomats no longer

represent their states in these forums, their social roles change and they adapt their language and behavior to correspond to their new environment and responsibilities. As a result, behavior and discursive change during periods of institutional exposure that appear consistent with social learning and socialization is, in fact, tactical short-term adaptation reflecting an effort to behave diplomatically.

We test our argument using a natural experiment occurring in Geneva, Switzerland. Since 2008, the UN Human Rights Council has implemented the mechanism of Universal Periodic Review (UPR), a program in which each state undergoes a review of its human rights practices roughly once every four years. Reviews entails a 3.5 hour “interactive dialogue” in which delegates first present their government’s human rights record and then field comments, questions, and critiques from other state delegates. Each summer, the ILO hosts the International Labour Conference (ILC), an annual summit where thousands of delegates convene to debate labor rights issues and revise international labor standards. UPR and the ILC are both hosted at the Palais des Nations in Geneva, many delegates represent their governments at both forums, and the schedule of UPR was set by lottery in 2007. Whether delegates represent their states at UPR before or after the ILC is thus determined by chance.

We leverage this natural experiment to assess how exposure to UPR affects diplomats’ discourse in a corpus of 2,653 speeches made by government officials and representatives of workers and employers during ILC debates from 2008 through 2022. Theories of socialization and policy learning indicate that government diplomats should emphasize human rights more during debates at the ILC after they have represented their countries at UPR. We find, however, that the opposite is true. Diplomats emphasize human rights more in ILC debates before their country undergoes review at UPR, and mention human rights less often once review has passed. UPR does not affect the behavior of delegates representing workers and employers. We then show that this effect is driven by individual-level exposure to UPR. We use the names of over 37 thousand delegates that have represented their states at UPR or the ILC to compare cases in which the same delegates represent their states in both forums to those where states are represented to different delegates.

Diplomats modifying their language in one forum *in anticipation of* exposure to another is not consistent theories of socialization and learning that expect behavioral change to follow social in-

teraction. Furthermore, debate at the ILC has no bearing on the content or outcome of UPR; consequently, there is no strategic advantage to changing discourse at the ILC in advance of UPR.¹ We contend that this evidence is most consistent with our theory of diplomatic adaptation. When diplomats' social roles change, they adapt their language to suit their new environment.

This article advances research on international organizations and diplomacy in two respects. First, we provide a direct test of how variation in institutional overlap affects diplomatic practices, helping to advance our understanding of the consequences of regime complexity (Alter and Rautiala, 2018). Such evidence is difficult to obtain, because states typically enter institutions at their own discretion, confounding the relationship between institutional exposure and behavioral outcomes (Chaudoin, Hays and Hicks, 2018). Our research design addresses these concerns and provides strong evidence of positive complementarity when diplomats themselves anticipate periods of overlap. Second, by bringing the regime complexity and social psychology literatures into conversation with one another, we provide novel theory and evidence of how institutional exposure impact patterns of discursive change and norm diffusion. Contrary to research showing that tactically adopted language can set the stage for subsequent norm internalization (Schimmelfennig, 2001; Risse, Ropp and Sikink, 2013), we find no evidence of persistent change. Our finding thus speak to both the prospective power of overlap as well as to its potentially ephemeral nature.

2 The Promise and Peril of Institutional Overlap

How does the overlap of international organizations affect state behavior? Existing scholarship provides three alternative perspectives. One account suggests that institutional overlap should not impact state behavior. By this account, international organizations are not independent variables, capable of shaping government action; instead, they simply reflect governments' preexisting preferences and policies (Downs, Rocke and Barsoom, 1996). International institutions are epiphenomenal, the byproduct of the distribution of power, domestic politics, or other factors that determine foreign policy (Krasner, 1982). Government delegates being exposed to one institution should consequently

¹While labor practices and the ratification of ILO conventions are often raised during UPR, we have yet to find a single case of statements being made during ILC debates being referenced at UPR.

have no impact on their behavior elsewhere.

A second account predicts overlap to undercut institutional impact. There are several mechanisms by which overlap could be counterproductive. Early work on the politics of regime complexity emphasized how jurisdictional overlap generates legal ambiguity and incoherent authority (Raustiala and Victor, 2004). Overlapping institutions could consequentially present outside options or facilitate forum shopping, offering states an escape from the constraints of existing institutions (Alter and Meunier, 2009; Busch, 2007; Morse and Keohane, 2014). Institutional overlap could thus weaken the leverage institutions have vis-a-vis member states (Lipsy, 2017; Clark, 2022). The creation of overlapping institutions could generate inter-institutional competition, diverting financial and political support from donor states and limiting institutional power and autonomy (Qian, Vreeland and Zhao, 2023). These considerations suggest that overlap could make institutions less effective.

A final possibility is that overlapping institutions reinforce each other. When institutions' are complementary – that is, when their mandates and rules require similar policy change from member states – they can address each other's shortcomings and be more adaptable to changing policy circumstances than any politically feasible comprehensive regime (Keohane and Victor, 2011). More institutions also provide more opportunities for the enforcement of standards and for policymakers to learn best practices and be socialized to international ideas and norms (Dobbin, Simmons and Garrett, 2007). Socialization is a process through which states learn how to behave appropriately in international institutions through their interaction with peer states and, by doing so, internalize the norms and principles represented by such behaviors (Johnston, 1998). The central tenet of socialization theory is that engagement within IOs should precede changes in states' behaviors (Finnemore, 1993; Greenhill, 2015). Thus, socialization theory predicts that the effect of overlap should occur *after* institutional exposure.

3 A Theory of Diplomatic Adaptation

We develop an alternative to these accounts. We bring theories of regime complexity into conversation with insights of social psychology to argue that diplomats align their behavior and discourse

in anticipation of future social interactions. Once these interactions pass, so too does the social incentive to conform, and diplomats revert their behavior back to the status quo ante. We develop this argument in the context of the international human rights regime. We first explain why this context constitutes one of complementary institutional overlap. We then explain the psychological mechanisms underlying our argument.

3.1 Complementary Overlap

The international human rights regime consists of a multitude of international laws and institutions developed over the past century. Its membership is universal and its institutional architecture is expansive and complex. The principals expressed in the Universal Declaration of Human Rights, for example, are core tenets of customary international law and integrated into the mandates of international institutions as diverse as the African Union, the Council of Europe, and the UN Security Council. The regime includes 18 major multilateral human rights treaties, including near-universal agreements like the International Covenant on Civil and Political Rights, the International Covenant on Economic, Social and Cultural Rights, or the Convention Against Torture, all of which have more than 170 state parties, as well as smaller agreements that have attracted fewer ratifications.² A panoply of actors support these norms and laws, including the so-called Treaty Bodies (e.g., the Human Rights Committee of the ICCPR), panels of experts who receive complaints, investigate abuse, and issue rulings that shape subsequent interpretation of international law, and UN Special Procedures, independent experts tasked by the UN High Commissioner of Human Rights to report on specific substantive issues. The International Labour Organization, founded in 1919 as part of the Treaty of Versailles and later incorporated into the UN as one of its specialized agencies, is the oldest intergovernmental organization with a rights mandate. While the focus of these institutions are diverse, they are united by a common focus in promoting justice and respect for individual or collective rights.

Recognizing this complexity, in 2006 the UN General Assembly created the UN Human Rights

²For instance, the International Convention on the Protection of the Rights of All Migrant Workers and Members of their Families concluded in 1990 currently has 57 state parties; and the Optional Protocol to the Convention on the Rights of Persons with Disabilities concluded in 2006 has 100 state parties. For data on all 18 treaties, see, <https://indicators.ohchr.org>.

Council to “promote the effective coordination and the mainstreaming of human rights within the United Nations system.”³ As part of the HRC, the UNGA established the mechanisms of Universal Periodic Review, a process in which each UNGA member state undergoes a public review of its compliance with international human rights laws and norms roughly once every four years. Like the HRC, UPR was designed to “[c]omplement and not duplicate other human rights mechanisms.”⁴

Several UPR design features facilitate its reinforcement of existing human rights institutions, including the ILO. Deference, a key mechanism by which IOs coordinate their policies with one another, is built into the design of UPR (Pratt, 2018). UPR does not promulgate new regulations, mitigating concerns of rule inconsistency; instead, it encourages states to comply with or deepen their cooperation with *other* institutions to which they *already* belong. UPR is thus designed to be incentive compatible with states’ pre-existing policy preferences. The ILO has been among the greatest beneficiaries of this deference. Shah and Sivakumaran show that more UPR recommendations encourage states to ratify ILO treaties (659 recommendations) and implement ILO outputs (147 recommendations) than any other UN specialized agency, fund, or programme (2024, 808). They likewise identify 18 recommendations encouraging states to seek assistance, including grants, from the ILO (Ibid.). UPR, in short, was designed to reinforce the work of existing human rights institutions.

We argue that UPR increases the salience of human rights norms in other human rights institutions.⁵ Prior works has identified similar complementarities across international institutions. Kelley argues that when election monitoring organizations reach similar conclusions, “their consensus can bolster their individual legitimacy as well as the legitimacy of the international norms they stress” (Kelley, 2009). In an examination of UPR’s impact on the efficacy of the UN Treaty Bodies, Carraro argues that “repetitions are likely to enhance these bodies’ credibility by showing that they reach similar conclusions” (Carraro, 2025, 203). Greenhill likewise shows that overlapping institutions facilitate the spread of human rights norms (Greenhill, 2015). We claim that UPR should have a corresponding

³UNGA Res 60/251 “Human Rights Council” (15 March 2006) A/RES/60/251.

⁴HRC 5/1 “Institution-building of the United Nations Human Rights Council” (18 June 2007) A/HRC/RES/5/1.

⁵Carraro notes that “no empirical research has been conducted thus far to assess whether the UPR encroaches on the work of the treaty bodies, nor on the consequences of possible overlaps for the effectiveness of the UN human rights machinery” (Carraro, 2022); Carraro (2025) provides evidence that UPR strengthens the work of the UN Treaty Bodies. We provide the first research on how UPR affects the ILO.

strengthening impact on human rights in the ILO.

3.2 Diplomatic Representation, Social Roles, and Conformity

While prior research and the design of UPR indicates that it should reinforce the importance of human rights in other international institutions, we refine these claims by specifying how this operates in practice. Diplomats, as human beings and not abstract states, must navigate these overlapping institutions and represent their government across different settings and forums. We argue that the design of UPR incentivizes diplomats to adopt human rights language in their work during the period before review occurs. Once review passes, this incentive ceases.

UPR is designed to exact social pressure on diplomats themselves. UPR is a *peer review* mechanism. Each review consist of a 3.5-hour “interactive dialogue” where delegates representing the state under review first present their government’s human rights record and then respond to questions, comments, and criticisms, and other recommendations from representatives of other UN members states. These recommendations – and whether the state under review accepts them – are recorded in an outcome report, which forms the basis of the state’s next review.

States take interactive dialogue seriously. Governments are required to submit formal reports documenting their human rights records at least six months before their review data. Preparations for review begin far before this deadline, putting human rights on diplomats minds. When review occurs, state send large diplomatic delegations with cabinet- or ministerial-level representation. Other states participate actively in interactive dialogue. According to data from UPR Info, states issued a total of 57,966 recommendations over the course of 387 reviews during UPR’s first two cycles from April 2008 through November 2016, an average of 150 recommendations per review.

Existing research on UPR conceptualizes human rights recommendations as instances of “naming and shaming” in which diplomats identify and publicize their peers’ transgression of human rights norms, seeking to increase the social costs (or shame) of noncompliance (Carraro, 2017; Terman and Voeten, 2018). This research emphasizes how the effects of shaming at UPR operate through diplomatic relations. For instance, Terman (2023, 27-94) develops a “relational theory of international

shaming” and tests this argument, in part, by demonstrating that states’ decisions to issue recommendations at UPR are shaped by their geopolitical relations with the state under review. UPR thus leverages social and diplomatic relations to pressure state to comply with human rights laws and norms.

Research in social psychology has shown that exposure to these types of social environments generates strong incentive for individuals to conform with group norms. When individuals find themselves exposed to social pressures like those provided at UPR, their behavior can begin to conform with group expectations both due to strategic efforts to gain social approval and the dynamics of behavioral mimicry that follows from subconscious tendencies to replicate social expectations (Cialdini and Goldstein, 2004, 609-611). When diplomats find themselves exposed to the expectations of new institutions, it presents challenges to maintaining consistency across their social roles, which is necessary to maintain a positive self-conception.⁶ A psychological need to maintain normative consistency indicates that when diplomats are required to represent their government positively in a human rights review, these same representations would be more likely to carry over into their presentations in other forums. Once UPR passed, the social incentive for conformity and consistency would pass, reducing the need for diplomats to align their behavior at UPR with their behavior in other forums.⁷ Barring learning or socialization, the pressure for diplomats to conform with UPR should pass with review.

4 Observable Implications

Several observable implications follow from the different theoretical perspectives outlined in the preceding section. First, research that views institutions as epiphenomenal to state power and interest predict UPR to have no impact discourse at the ILC. This is our null hypothesis. Second, if institutional overlap produces institutional competition, UPR may offset or substitute the functional purpose of the ILO in the area of human rights. If this is the case, then UPR could lead to a decrease in

⁶On the relationship between roles and personalities in foreign policy decision-making, see, Cuhadar et al. (2017).

⁷These implications follow from self-esteem protection and self-categorization theory (Turner, 1987). When diplomats associate with a certain in-group (here, UPR) they are more likely to comply with group-based expectations; when the association ceases that incentive no longer holds.

human rights language used at UPR.⁸

The prevailing theory is that of socialization, which anticipates states adopting more human rights language after exposure to UPR. Given the prominence of this theory, we illustrate its empirical expectations Figure 1 alongside the implications of our theory of diplomatic adaptation. The top pane of Figure 1 illustrates how delegates to ILC adopting more human rights languages after exposure to UPR. The bottom pane of 1 illustrates how diplomats increasing human rights language prior to UPR, and decreasing it thereafter. The empirical analyses that follow are aimed at testing these two competing hypotheses.

5 Outcome Data and Measurements

We test these hypotheses in the context of the International Labor Conference (ILC). The ILC is the ILO's annual tripartite "parliament of labor," where each member state's government, worker, and employer delegations gather to debate priorities, vote for new conventions and resolutions, and deliver statements based on their national and organizational positions. The conference typically lasts for two weeks, consisting of multiple plenary and breakout sessions. We focus on members' participation in the plenary sessions, which include agenda items such as the Director-General's annual report. At each ILC, the ILO Director-General tables a formal "Report of the Director-General," an agenda-setting document that takes stock of world-of-work trends, outlines the Organization's priorities, and advances a theme for debate between members.⁹ These reports are published on the ILO website and distributed to conference members in advance so that delegations can prepare their comments.

Discussion of the Director-General's report is taken up in plenary meetings, which constitutes

⁸We welcome comments on how to adjudicate between this hypothesis and the expectations of our theory of diplomatic adaptation. At first glance, the expectations of the two are similar. However, our theory indicates an increase in human rights discourse before UPR, while a substitution effect does not expect this empirical pattern. We are interested in other differences that readers might expect. We are still working through how to disentangle the implications of these theories, but are inclined toward our own because it seems like there is little theoretical reason so expect UPR to conflict with and substitute for the ILO. Nevertheless, the possibility exists and we would like to test for it explicitly.

⁹For example, titles of these reports indicated the agenda-setting purpose they serve: "Tackling the global jobs crisis" (2009), "Work in a changing climate" (2017), and "Jobs, rights and growth" (2025).

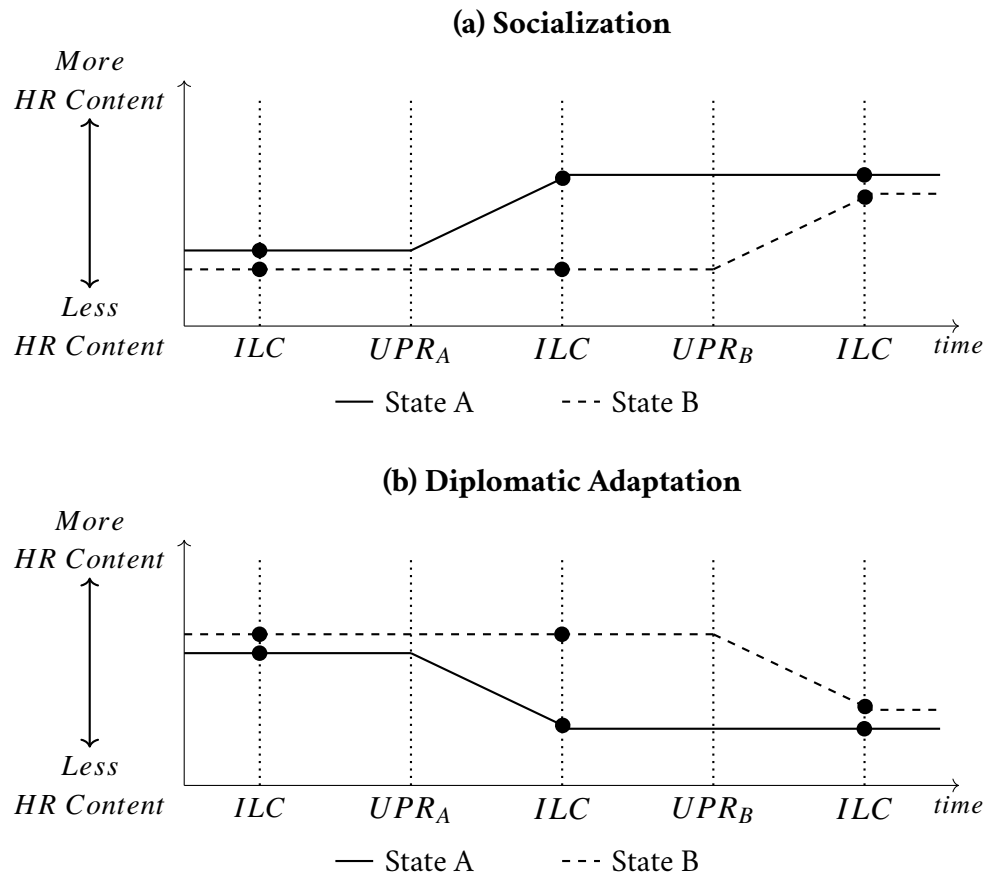


Figure 1: *Observable Implications*. Top plot illustrates expectations of socialization theory in which delegates use more human rights language during ILC after exposure to UPR. Bottom plot illustrates expectations of diplomatic adaptation theory in which delegates increase human rights discourse before UPR and reduce usage of this language once UPR has passed.

a major component of the conference. Typically, more than 70% of the non-logistical plenaries are dedicated to these discussions each year. Governments, employers, and workers intervene from the floor after registering in advance for the speaker list, and make their comments on the major issues facing the global governance of labor rights raised by the ILO Director-General. The ILO publishes the verbal transcript (translated into English) of the speeches made by all members after the annual conference. We emphasize that these discussions draw a universal audience, without selection or barrier to attendance; therefore, they function as a common forum where governments speak alongside employers and workers, making the ILC an ideal forum to observe whether and how delegates publicly link labor questions to the adjacent human rights norms.

To measure variation in human rights language employed in these speeches, we first collect data on speeches made by government, worker, and employer delegates in the ILC between 2008 and 2021.¹⁰ In order to ensure that the topic and content of the speeches are comparable, we focus on speeches made in plenary meetings. Theoretically, plenary meetings are ideal forums to distinguish observable implications between the adaptation and socialization hypothesis, as the audience of delegates' speeches is not self-selected or pre-screened due to any political considerations. In total, we identified 2,653 plenary speeches, which resulted in a corpus of 1.82 million words. Using information from ILC delegation lists, we also measure individual-level covariates associated with each speaker, including their nationality, affiliation (government, worker, or employer), and job title.

We are interested in measuring the degree to which delegates adopt human rights languages when speaking in the ILC. We construct outcome measures in two ways. First, we create a binary measure, that takes 1 if a speaker uses the term "human rights" in their speech to the ILC and is otherwise 0. In many cases where speakers refer to human rights, they seek to make a direct connection between human rights and labor rights. For example, during the 2008 ILC, the Finnish Minister of Labor underscored the ILO's "valuable work in promoting social justice and human rights amongst various

¹⁰We note that transcripts of conference proceedings are not available for 2015 to 2017 due to clerical reasons in the ILO. The 2020 conference was postponed by a year due to the COVID pandemic. Moreover, we remove speeches made by INGO and IO representatives from our data, focusing on the *within* country comparisons between government delegates, who are exposed to the institutional overlap between the ILC and UPR, and worker/employer delegates, who are not exposed to such overlap.

UN organizations”; similarly, in 2010, the Argentinean of Minister of Labor, Employment and Social Security stated that “without work, the future [of human rights] will be bereft of social inclusiveness.”

Second, because a narrow focus on the term “human rights” excludes other terms that reflect an attention to human rights norms, we use GloVe word embeddings to create a human rights lexicon. Following similar procedures by [Thrall \(2025\)](#), we first obtain vector representations of “human” and “rights.” We then take the average of these two embeddings and identify the 100 terms in our corpus with the cosine similarity most similar to this averaged embedding. Finally, we inspect these 100 words, removing words that are too vague, common, introduce other ideas. This leaves us with a list of 47 keywords.¹¹ Using this lexicon, we construct (1) a count measure for the frequency of “human rights words” used by speakers and (2) a log-transformed frequency measurement to capture how much content in a delegate’s speech relates to human rights.

Our unit of analysis is the government-worker-employer triad year. For each ILO member state (187 out of the 193 members in the UN system) in a given year, we document three possible entries: one for the speech made by the government representative, one for the worker representative, and one for the employer representative.

6 Research Design: The UPR Lottery

Research on the effects of international laws and institutions on state behavior is often confounded by the problem of selection on unobservables ([Lupu, 2013](#); [Chaudoin, Hays and Hicks, 2018](#)). States choose when to ratify treaties and enter international organizations, making it difficult to discern whether associations between institutional membership and policy outcomes are due to institutions themselves or states’ pre-existing preferences and characteristic ([von Stein, 2005](#); [Simmons and Hopkins, 2005](#)). Two unique features of the design and implementation of UPR help address these problems: its universality and a lottery used to assign states’ review dates. Here, we explain these features

¹¹These keywords are “rights”, “human”, “fundamental”, “respect”, “dignity”, “freedom”, “right”, “principles”, “protection”, “basic”, “guarantee”, “justice”, “ensure”, “protect”, “life”, “freedoms”, “essential”, “decent”, “ensuring”, “democracy”, “standards”, “guarantees”, “equality”, “exercise”, “violation”, “defend”, “principle”, “protecting”, “conditions”, “promoting”, “guaranteeing”, “enjoy”, “violations”, “guaranteed”, “values”, “core”, “respecting”, “migrant”, “law”, “bargaining”, “migrants”, “violate”, “exploitation”, “universal”, “discrimination”, “trafficking”, and “respected”.

and how we leverage them to estimate the effect of institutional overlap on discourse at the ILC.

On March 15, 2006, the United Nations General Assembly adopted resolution 60/251, establishing the Human Rights Council and the mechanism of Universal Periodic Review. The resolution charged the HRC to undertake UPR in a manner that “ensures universality of coverage and equal treatment with respect to all States.” In pursuit of these twin goals, the HRC held a lottery on September 21, 2007, to determine the order states would undergo review.¹² This lottery differed from a simple random ordering in three respects.

First, the HRC block randomized by region. The first cycle of UPR occurred over 12 sessions, beginning in April 2008 and ending October 2011; 16 UN member states underwent review during each session.¹³ To ensure geographic representation within each session, the HRC set a fixed number of states to be reviewed from each UN regional grouping during each session.¹⁴ For example, during UPR’s first session, four states were reviewed from Africa, two from Asia, three from Latin America and the Caribbean, two from Eastern Europe, and three from Western Europe and Other States. This approach to the lottery not only ensures equal geographic treatment, it also improves the precision of statistical estimates, because region is associated with many factors – socio-economic development, regime type, conflict legacy, and more – that affect government human rights policies and preferences.

Second, following HRC resolution 5/1 of June 2007, states that were HRC members in 2007 or elected to join the HRC in 2008 were required to undergo review during the first session of the last year of their term on the HRC. For example, states whose HRC terms ended in 2007 or 2008 were assigned the earliest sessions in 2008, and states whose term ended in 2010 were assigned to the earliest sessions in 2010.¹⁵ This rule determined the year that 56 states underwent UPR. Critically, this rule was set *after* these states joined the HRC and, we argue, is exogenous to their human rights

¹²The modalities of the lottery are detailed in the HRC’s report on its sixth session; UN Human Rights Council, “Report of the Human Rights Council on its 6th session,” 2007, Geneva, paragraphs 291-294 (page 132) and Annex V (pages 205-209), available at, <https://digitallibrary.un.org/record/626770?ln=en>. On June 17, 2011, the HRC adopted decision 17/119 fixing this order for all subsequent UPR cycles.

¹³The number of states reviewed each session was reduced to 14 after the first cycle; however, the order was not changed.

¹⁴See Appendix A for block randomization schedule and states’ regional groupings.

¹⁵States that were HRC members in 2007 are bolded in the list of regional groupings provided in Appendix A, and the year their term expired is provided in parentheses.

preferences.

Third, the order of some states was not determined by lottery. The HRC invited states to volunteer to undergo UPR first. Only two states accepted this invitation: Switzerland and Colombia. These states were reviewed in the third UPR session. Haiti was moved to the end of the first cycle following the 2010 earthquake. South Sudan was then placed in the order after Haiti at the end of the second cycle once it gained independence in 2011.

With one exception, all states complied with their assigned review dates during the study period.¹⁶ Only once – Israel during the second cycle – did a state refused to show up on its assigned review date. Even in this case, however, Israel’s noncompliance was short lived: it skipped it’s initial assigned review during the 15th UPR session on January 29, 2013, but then agreed to participate in a make-up review during the 17th session on October 29, 2013.

We argue this lottery renders whether states undergo UPR before or after the ILC independent of other determinants of their discourse at the ILC. Consequentially, differences between speeches by government delegates before and after UPR can be interpreted as causal effects of UPR. We use a two-year window to define whether a delegate participates in the ILC before or after UPR. We select two years as the temporal window for our main analyses, given that each round of UPR lasts four and a half years, so two years indicate the midpoint between a country’s last and next round review. However, our analyses are robust to alternative selections of the window period.

In addition to the UPR lottery, we leverage two additional components of the ILC and UPR in our research design. First, we use speeches at the ILC by a country’s employee and workers delegates as a pure control group. We categorize these speeches as before or after UPR using the same procedure used for government delegates. Our comparison between government and nongovernment delegates’ speeches operates under the assumption that non-government delegates do not participate in or receive monitoring pressures from the UPR process. To validate this assumption, we further collect the names of all unions and employer associations that have ever participated in the ILO monitoring process, and compare them with the list of NGO participants in the UPR. While we find no

¹⁶The United States skipped its fourth review on November 7, 2025. This case occurred after the study period.

crossover between nonstate participants in the ILO with UPR,¹⁷ it may be possible that worker and employer delegates strategically integrate more discussions of human rights when their government is facing such pressure in order to attract more attentions. While such a scenario suggests possible bias in our estimation of the adaptation effect, it also implies that our measurements capture a lower bound for the relative shift of government delegates' attention to human rights issues, thus leading to underestimates.

Second, we use the lists of government delegations to UPR and ILC to identify cases in which ILC delegations include individual diplomats who did (or will) represent their government at UPR. Altogether, 10,338 diplomats represented their governments at UPR during the study period. Meanwhile, there are 66,439 total delegates listed as participating in the ILC; 26,763 of these are government delegates, and the remaining 39,676 are representatives of worker and employer organizations. We detail the technical aspects of this analysis below; here, we note that comparing the relationship between the content of ILC speeches and UPR in cases with delegate overlap to those without it provides insight into the individual-level mechanism that our theory expects.

The credibility of this research design depends on the comparability of units assigned to undergo UPR before and after the ILC. Appendix Figures [SM.9](#) and [SM.10](#) provide consistent evidence of covariate balance across all different samples: the full sample, only those within the to year window, and those with and without delegate overlap. This evidence provides some assurance that the natural experiment produced by the UPR lottery is valid.

7 Diplomats Mention Human Rights More at ILC Before UPR

We motivate the choice of our research design by first showing the descriptive trends of human rights references in the ILC. As shown in [Figure 2](#), government representatives mention human rights more often before their states undergo UPR. The black lines represent the LOESS curve for the average proportion of speeches by government delegates that mention human rights. Gray lines represent this same curve for nongovernment delegates (e.g., delegates representing workers or employees)

¹⁷The claim is currently based on an analysis of the 2008 delegate lists and requires further validation.

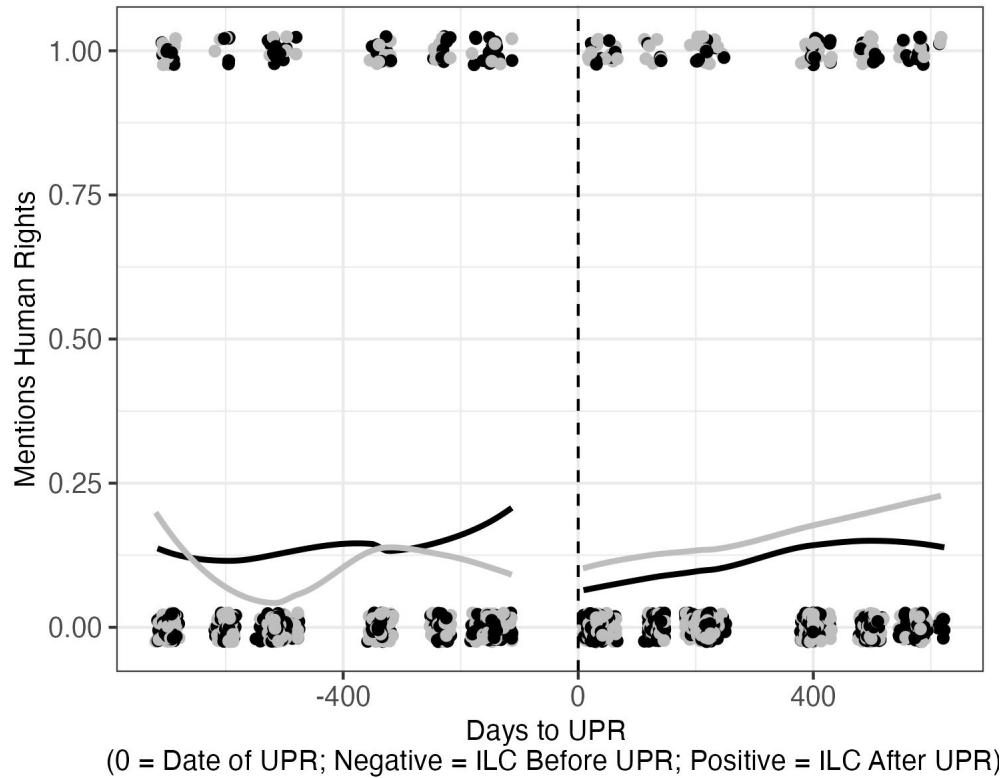


Figure 2: *Mentioning of Human Rights at ILC, Before and After UPR.* Figure plots smoothed (LOESS) curve of whether delegates mention the term “human rights” during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country’s employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

from specific countries. Scatter plots indicate the distribution of the raw data. Note that the six clusters along the top and bottom of the graph reflect the fact that UPR occurs in three two-week sessions per year. The x-axis represents the number of days from a state’s closest UPR date to which the delegate made these comments at the ILC. For example, a value of -100 on the x-axis means that the delegate gave their speech at the ILC 100 days before their state had UPR. Likewise, a value of 100 means that the delegate’s speech at the ILC occurred 100 days after their state underwent the UPR. The graph includes all speeches falling within a four-year window, from two years before UPR to two years after UPR.

To test these patterns statistically, we employ a linear regression model specified in equation 1. Crucially, we interact an indicator for whether a delegate is a government representative (Gov. Rep.

in Table 1) with an indicator of whether the closest UPR review for the country of that delegate was in the last two years (POST UPR). The coefficient on the interaction (GOV. REP. \times POST UPR) is the average treatment effect on government delegates (i.e., the ATT). Under our assumption, nongovernment delegates serve as a pure control, which allows us to be on firmer ground when we interpret the differences during the pre-review period and the lack of difference between government and nongovernment mentions of human rights in the post-review period.¹⁸ Our theoretical expectation is that the interactive effect between POST UPR and GOV. REP. will be negative and statistically significant.

$$\mathbf{1}(\text{Mention Human Rights}) = \tau(\text{Gov. Rep}_{it} \times \text{Post UPR}_{it}) + \mathbf{X}^T \boldsymbol{\beta} + \alpha_i + \gamma_t \quad (1)$$

Model (1) in Table 1 is a simple OLS regression with both the base variables and the interaction term. Model (2) adds a series of covariates.¹⁹ Model (3) adds country and year fixed effects and drops covariates. Model (4) is the full model, which includes both fixed effects and covariates. Models (5) through (8) assess the sensitivity to the results in Model (4) to the bandwidth. In Models (5) and (6), we increase the bandwidth to three years and then include all observations. In Models (7) and (8), we reduce the bandwidth to one year and then six months.

We make three observations based on the results in Table 1. First, estimated coefficients are highly stable across models (1) through (6) – where the bulk of observations (about 2,000 of them) remain the same – for both the interaction term and the “Post-UPR” indicator variable. Second, the coefficient on the interaction term increases in magnitude as the window becomes narrower (e.g. compare models (5) to (4) to (7) to (8)). Third, in terms of substantive significance, the estimates on the interaction term are sizable relative to the base rate of the outcome in the sample. For example, human rights are mentioned in 14.5% of speeches across all observations and in 16.2% of speeches by government delegates before UPR. In comparison, the range of estimates indicates that government delegates mention human rights 6 percentage points to 9.9 percentage points less often after review, a reduction

¹⁸The set up is similar to a two-period difference-in-differences design, though we note that this is not a DiD design because we do not always observe the same units (delegates) in both pre and post periods.

¹⁹These are V-Dem’s liberal democracy, freedom from torture, freedom from forced labor, and civil society participation indices (Coppedge et al., 2024); log GDP per capita and log population are estimated by Fariss et al. (2022).

Table 1: UPR affects whether delegates say “human rights” at ILC

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|-----------------------------------------|---------------------|----------------------|----------------------|----------------------|----------------------|---------------------|---------------------|
| | Delegate Mentions “Human Rights” at ILC | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 0.052** (0.026) | 0.056** (0.026) | 0.055*** (0.021) | 0.055*** (0.020) | 0.046*** (0.018) | 0.050*** (0.018) | 0.016 (0.031) | -0.044 (0.086) |
| Gov. Rep. | 0.037 (0.025) | 0.034 (0.024) | 0.025 (0.022) | 0.016 (0.021) | 0.013 (0.020) | 0.019 (0.020) | 0.043 (0.030) | 0.097 (0.059) |
| Liberal Democracy | | -0.032 (0.069) | | -0.345 (0.242) | -0.172 (0.202) | -0.162 (0.193) | -0.114 (0.410) | -1.056 (0.759) |
| Freedom from Torture | | -0.025* (0.013) | | -0.041 (0.040) | -0.019 (0.033) | -0.004 (0.030) | -0.028 (0.059) | -0.027 (0.098) |
| Civil Society Part. | | 0.086 (0.063) | | 0.380* (0.197) | 0.187 (0.150) | 0.167 (0.144) | 0.303 (0.365) | 1.136* (0.628) |
| Freedom from Forced Labor | | 0.078 (0.059) | | 0.388 (0.301) | 0.373 (0.256) | 0.301 (0.243) | 0.332 (0.414) | 0.532 (0.711) |
| Log GDP PC | | 0.022** (0.009) | | 0.046 (0.094) | 0.047 (0.079) | 0.067 (0.079) | 0.078 (0.158) | 0.193 (0.254) |
| Log Population | | -0.011** (0.006) | | -0.218 (0.200) | -0.139 (0.174) | -0.189 (0.165) | -0.228 (0.326) | -0.043 (0.610) |
| Post UPR × Gov. Rep. | -0.079** (0.035) | -0.086** (0.034) | -0.075*** (0.028) | -0.077*** (0.027) | -0.062*** (0.023) | -0.073*** (0.024) | -0.086** (0.039) | -0.143** (0.069) |
| Country FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 3 Years | Full Sample | 1 Year | 6 Months |
| Observations | 1,818 | 1,791 | 1,818 | 1,791 | 2,177 | 2,266 | 916 | 473 |
| Adjusted R ² | 0.002 | 0.007 | 0.057 | 0.062 | 0.059 | 0.053 | 0.051 | 0.066 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

of roughly 50%.

Figure 2 and Table 1 jointly reflect a consistent empirical pattern: when government delegations anticipate the UPR, they use more HR language in the ILC to signal commitment to human rights. This could both be because states are adapting to the incentives produced by UPR; or because the reporting process in UPR makes human rights more salient for their country in the lead up to review. This is strong evidence against a learning or socialization hypothesis, which would predict states (or their representatives) internalizing human rights discourse *after* representing their states at UPR and

then echoing this language at other forums. Once the UPR is in the rearview mirror, state delegates mention human rights less often.

8 Diplomats Increase Human Rights Discourse Before UPR

We next examine whether UPR pressure shapes not only whether government delegates invoke human rights, but also how much of their ILC remarks are couched in human-rights language. We consider two outcomes: (a) the raw frequency of words in our human-rights lexicon and (b) the percentage change of human-rights lexicon (log-transformed). Figures SM.4 and SM.5 plot LOESS curves for these outcomes by government vs. non-government delegates, centering the x-axis on the closest UPR date. The same pattern from Figure 2 reappears: government delegates ramp up human-rights language as their state approaches review and use relatively less after review, while worker/employer trends remain comparatively flat.

To formalize these patterns, we re-estimate equation 1 with each lexical outcome. Tables 2 and 3 report results across the same sequencing of models and pre/post bandwidths used in the previous section. As before, the key parameter is the interaction term, which captures the differential post-review shift among government delegates relative to non-government delegates from the same country-year. Across the two outcomes, the interaction is consistently negative and statistically significant in the main specifications. For the count outcome in table 2, the post-review differential ranges from roughly -2.7 to -2.9 words (e.g., in Models 3 and 4), with similar or larger magnitudes in the tighter windows (-3.8 to -4.1 in Models 7 and 8). On the logged outcome in table 3, the interaction is negative and significant in Models 2–4 and remains directionally stable elsewhere. Substantively, these results indicate that after UPR has passed, government delegates devote less of their ILC floor time to human rights language than they did beforehand, net of contemporaneous changes among their domestic social partners.

In addition, similar to our first finding, the anticipatory effect is more pronounced closer to the UPR event. In Table 2, tightening the window to one year (Model 7) strengthens precision and size; the 6-month window (Model 8) yields similar direction and a sizable coefficient, albeit with wider

intervals due to fewer observations. For the logged specification (Table 3), effects are strongest and most precise in the two-year window with fixed effects (Models 3–4) and attenuate when we pool far-from-review years (Models 5–6). These patterns aligns with an anticipation mechanism: language intensifies before review and recedes after, with effects concentrating in the immediate review horizon.

Is it possible that the increase in the use of human rights language stems not from adaptation, but from changes in the length of speech? States devote diplomatic efforts in preparing for their UPR reviews, from which they can repurpose the scripts prepared for the UPR and append them to the ILC speech. We argue that this is unlikely to happen given the time limit imposed on delegate speech in the ILC’s plenary meetings. Moreover, we conduct a placebo test in Appendix C.2 which shows that UPR pressure does not impact the length of speeches at the ILC across various specification. Taken together, these results support our theoretical expectation that states adapt their public rhetoric in anticipation of UPR scrutiny rather than being durably socialized by it. Governments increase both the incidence and intensity of human rights framing at the ILC before review, and then dial it back after review, relative to co-national worker/employer delegates.

9 Delegate Overlap Increases Impact of UPR

We now assess whether the effect depends on the direct exposure of individual diplomats to UPR. Our proposed mechanism suggests that the effect of adaptation is moderated by whether there is delegate overlap between the ILC and UPR, that is, whether any government official attending the ILC also attends the closet UPR when their state is up for review. For each delegate listed in the ILC roster, we code a dummy variable of `MATCHED` that equals one when both the first and last name of a delegate simultaneously match a delegate entry in the UPR *within* each word-boundary.

A non-trivial challenge in the matching process is to balance false negatives against false positives.²⁰ To catch legitimate variants, we incorporated in our name-matching algorithm a compact

²⁰E.g., due to nicknames (e.g., “Daniel” vs. “Dan”), initialization (e.g., “James Smith” vs. “J. Smith”), transliteration (e.g., “Mohammed” vs. “Muhammed”), and OCR errors, the same delegate could be listed under different names in the ILC and UPR attendance records. At the same time, using overly fuzzy matching criteria also lead to different but similar names

Table 2: UPR affects number of human rights words at ILC

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|----------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|--------------------|
| | Human Rights Lexicon Count | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 1.664* (0.899) | 1.781** (0.872) | 1.675** (0.659) | 1.601** (0.662) | 0.951 (0.613) | 0.870 (0.598) | 2.122** (0.971) | 2.083 (3.182) |
| Gov. Rep. | 1.645* (0.858) | 1.902** (0.844) | 1.958*** (0.631) | 1.871*** (0.645) | 1.589** (0.627) | 1.616*** (0.611) | 2.065* (1.072) | 2.138 (1.976) |
| Liberal Democracy | | -4.416 (3.049) | | 5.182 (7.413) | 3.324 (6.034) | 3.678 (5.805) | 6.544 (12.678) | 18.090 (24.928) |
| Freedom from Torture | | -0.998** (0.449) | | -1.453 (1.159) | -0.996 (0.952) | -0.446 (0.880) | -0.413 (1.719) | -1.405 (3.442) |
| Civil Society Part. | | 8.035*** (2.314) | | 6.991 (7.216) | 7.495 (6.000) | 5.292 (5.755) | 6.293 (12.973) | 17.474 (19.800) |
| Freedom from Forced Labor | | 1.132 (2.144) | | 20.888*** (8.009) | 17.685*** (6.544) | 17.631*** (6.380) | 20.306 (15.953) | 38.830 (34.147) |
| Log GDP PC | | 1.547*** (0.317) | | 5.712* (3.074) | 5.695** (2.891) | 6.727** (2.749) | 7.271 (5.079) | 9.699 (10.008) |
| Log Population | | -0.244 (0.213) | | -8.296 (5.856) | -6.443 (5.410) | -8.285 (5.114) | -1.880 (9.734) | -5.469 (17.645) |
| Post UPR × Gov. Rep. | -3.137** (1.243) | -3.506*** (1.213) | -2.728*** (0.871) | -2.872*** (0.872) | -2.069** (0.810) | -2.005** (0.794) | -3.757*** (1.340) | -4.119* (2.398) |
| Country FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 3 Years | Full Sample | 1 Year | 6 Months |
| Observations | 1,818 | 1,791 | 1,818 | 1,791 | 2,177 | 2,266 | 916 | 473 |
| Adjusted R ² | 0.003 | 0.019 | 0.158 | 0.161 | 0.156 | 0.162 | 0.158 | 0.139 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

fallback by removing surname particles, hyphens, and spaces. We also combine a fuzzy Jaro-Winkler threshold (0.85) with a hand-coded list of transliteration variants. As a result, word boundaries and particle filtering jointly cut false positives, while the compact and fuzzy passes reduce false negatives from hyphenation, spacing, or transliteration differences. Finally, we manually exclude the remaining false positives in the matching output by visual comparison of both the matched names and other identifying covariates (e.g., job titles).²¹

to be paired together (e.g., “Pham Quoc Tru” vs, “Pham Quoc Tran”).

²¹Our data cleaning process for delegate matching between the ILC and UPR may still leave out false negative cases.

Table 3: UPR affects percent of human rights words in ILC speeches

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|-------------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|--------------------|-------------------|
| | Log(Human Rights Lexicon Count + 1) | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 0.066 (0.048) | 0.064 (0.047) | 0.064* (0.034) | 0.053 (0.034) | 0.013 (0.032) | 0.014 (0.032) | 0.086* (0.048) | 0.036 (0.163) |
| Gov. Rep. | 0.078* (0.045) | 0.097** (0.044) | 0.109*** (0.033) | 0.103*** (0.033) | 0.092*** (0.033) | 0.105*** (0.033) | 0.118** (0.048) | 0.121 (0.090) |
| Liberal Democracy | | -0.234 (0.152) | | 0.314 (0.379) | 0.157 (0.321) | 0.139 (0.316) | 0.163 (0.564) | 0.670 (1.281) |
| Freedom from Torture | | -0.047* (0.025) | | -0.051 (0.059) | -0.036 (0.048) | -0.009 (0.045) | -0.025 (0.089) | -0.050 (0.180) |
| Civil Society Part. | | 0.452*** (0.126) | | 0.314 (0.331) | 0.396 (0.293) | 0.319 (0.283) | 0.622 (0.514) | 0.823 (0.867) |
| Freedom from Forced Labor | | 0.059 (0.114) | | 0.921** (0.459) | 0.910** (0.390) | 0.835** (0.385) | 0.921 (0.875) | 2.008 (2.048) |
| Log GDP PC | | 0.076*** (0.017) | | 0.280* (0.160) | 0.322** (0.151) | 0.410*** (0.152) | 0.349 (0.247) | 0.414 (0.497) |
| Log Population | | 0.003 (0.011) | | -0.572* (0.324) | -0.332 (0.292) | -0.393 (0.282) | -0.133 (0.506) | 0.449 (1.010) |
| Post UPR × Gov. Rep. | -0.116* (0.065) | -0.129** (0.063) | -0.096** (0.045) | -0.097** (0.045) | -0.052 (0.043) | -0.058 (0.043) | -0.123* (0.063) | -0.117 (0.112) |
| Country FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 3 Years | Full Sample | 1 Year | 6 Months |
| Observations | 1,818 | 1,791 | 1,818 | 1,791 | 2,177 | 2,266 | 916 | 473 |
| Adjusted R ² | 0.001 | 0.015 | 0.170 | 0.171 | 0.170 | 0.178 | 0.165 | 0.025 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

For each government entry, we take the `MATCHED` measure from the ILC roster and sum by country and year to create a `OVERLAP` variable. In other words, `OVERLAP` is a count measure for the number of people in a state's delegation to the ILC who were also in the state's delegation to its nearest UPR. That is, we use the list of UPR delegates that has the closest date to a given years ILC. For example,

These cases may arise when delegates' first names recorded in the ILC and UPR rosters differ significantly due to particular nicknames (e.g., "Robert" vs. "Bob") or when their last names change significantly due to, for example, changes in marital status. Thus, our measure is a lower bound for the delegate overlap across the ILC and UPR. Undetected delegate overlaps, therefore, would only create attenuation bias that lead to the effect of our proposed mechanism to be understated.

if a state's nearest UPR is 400 days before ILC, we match the names of the delegation at the current ILC and the names of the delegates it sent to UPR 400 days before.

We begin, as before, with descriptive analyses. Figures [SM.6](#), [SM.7](#), and [SM.8](#) replicate the LOESS curves and scatterplots above, but plot observations with and without overlap separately. Across mention incidence, raw counts, and logged counts, the pre/post-UPR patterns mirror the main figures: government speech becomes more human rights forward as review approaches and eases after, while co-national worker/employer trajectories remain comparatively flat.

In [Table 4](#), we re-estimate the model specification using full set of covariates and a two-year window (i.e, model (4)) from [Table 1](#) to [3](#) for samples of delegates in which there are either no overlapping delegates and those in which there is at least one overlapping delegate. Our expectation is that the interactive effect between `GOV. REP.` and `POST UPR` should be insignificant when there are no delegate overlaps between ILC and UPR; and negative and statistically significant when there is. We show it does.

The interaction term is statistically indistinguishable from zero when there are no overlapping delegates across models (1), (3), and (5) in [table 4](#). In comparison, the interaction term increases in magnitude and becomes statistically significant in model (2), (4), and (6). This pattern is consistent with an anticipatory ramp-up in human rights framing that is specific to individual government delegates and specific to the period before their state's review. In short, delegate overlap amplifies the pre-review government differential in human rights language at the ILC, aligning with our mechanism that individual-level exposure to UPR transmits incentives into adjacent international forums.

10 Conclusion

This article has proposed that institutional overlap can shape state behavior through diplomatic adaptation: when government delegates anticipate their exposures across adjacent international forums, they adjust their rhetoric accordingly. In the overlap between the UPR and the ILC, we find that government delegates become more likely to invoke human rights, and to do so more extensively, before their states undergo review at the UPR. This emphasis, however, sharply declines after review has

Table 4: Delegate Overlap Amplifies Effects of UPR Pressure Pre-Review

| | <i>Dependent variable:</i> | | | | | |
|---------------------------|----------------------------|---------------------|------------------------|----------------------|----------------------|---------------------|
| | Mention | | Lexicon Count | | log(Count + 1) | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post UPR | 0.055 (0.046) | 0.053* (0.027) | 2.085 (1.326) | 1.710** (0.846) | 0.088 (0.072) | 0.058 (0.045) |
| Gov. Rep. | -0.053 (0.039) | 0.042 (0.026) | -0.628 (1.092) | 2.906*** (0.806) | -0.007 (0.061) | 0.159*** (0.039) |
| Liberal Democracy | 0.113 (0.415) | -0.498 (0.318) | 8.623 (15.956) | -3.225 (8.910) | 0.762 (0.794) | -0.151 (0.452) |
| Freedom from Torture | -0.146* (0.086) | -0.034 (0.047) | 2.194 (3.022) | -2.709** (1.353) | 0.196 (0.161) | -0.116* (0.066) |
| Civil Society Part. | 0.442 (0.488) | 0.469* (0.239) | -25.133 (16.049) | 13.987* (8.286) | -2.073** (0.825) | 0.861** (0.341) |
| Freedom from Forced Labor | -0.317 (0.615) | 0.503 (0.328) | 33.476 (25.638) | 25.033** (9.935) | 1.203 (1.285) | 1.144** (0.536) |
| Log GDP PC | 0.226 (0.163) | 0.025 (0.138) | -2.946 (5.533) | 10.272** (4.385) | 0.004 (0.317) | 0.461** (0.202) |
| Log Population | -0.520 (0.410) | -0.028 (0.282) | -30.766*** (11.141) | 0.914 (9.517) | -2.219*** (0.655) | 0.087 (0.476) |
| Post UPR × Gov. Rep. | -0.064 (0.051) | -0.069** (0.034) | -1.932 (1.489) | -2.953*** (1.102) | -0.064 (0.081) | -0.097* (0.056) |
| No. Overlapping Delegates | =0 | >0 | =0 | >0 | =0 | >0 |
| Country FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years |
| Observations | 598 | 1,193 | 598 | 1,193 | 598 | 1,193 |
| Adjusted R ² | 0.053 | 0.057 | 0.102 | 0.185 | 0.154 | 0.186 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

passed. Furthermore, this pattern does not appear among worker and employer representatives, and appears stronger where the same officials participate across both institutional settings. These findings distinguish our argument from both socialization and rational choice accounts: while institutional overlaps can generate pressures for diplomatic behaviors, they also do not necessarily produce the durable normative change after socialization. Instead, the evidence is more consistent with a process

in which diplomats adapt to the shifting normative and professional demands of partially connected institutional environments, calibrating their rhetoric in anticipation of scrutiny and scaling it back once that moment of exposure has passed.

These findings hold important implications for research on regime complexity and norm diffusion, as they suggest that behavioral convergence across institutions may rest on a different foundation than is often assumed. Scholarship on overlapping institutions frequently emphasizes reinforcement through learning, persuasion, and socialization (Elkins and Simmons, 2005). Our evidence instead points to a more contingent mechanism in which diplomats alter their discourse strategy because they move across partially connected institutional environments with different audiences and expectations. This discursive shift is substantively important in its own right: Public rhetoric is one of the principal ways states signal priorities, define compliance, and possibly constrain actions taken in the future (Schimmelfennig, 2001). Changes in discourse can therefore shape what issues receive attention, what kinds of demands appear appropriate, and how state behavior is interpreted by other actors, even when those changes do not immediately translate into policy reform. Overlap, in this view, matters not because it necessarily deepens commitments, but because it changes the incentives and role demands facing representatives at particular moments in time. If norm-consistent rhetoric can be generated through adaptation rather than internalized preference change, then scholars should be cautious about treating discursive convergence as evidence of durable socialization. Institutional complementarity may exist, but it may be episodic, actor-specific, and reversible rather than cumulative and self-reinforcing.

We examine a likely case of institutional overlap, in which elite discourse is situated in a normatively adjacent and visible multilateral setting. Future work should investigate whether similar patterns of anticipatory adaptation emerge in other regime complexes, whether they extend from rhetoric to implementation, and if there exist certain conditions under which temporary adaptations can calcify into more durable change. More broadly, the findings invite a rethinking of how scholars interpret behavioral shifts in international organizations. Not all norm-consistent conduct is evidence of socialization. Some of it may instead reflect diplomats adapting strategically to shifting

institutional roles. Recognizing that distinction is essential for understanding both the possibilities and the limits of contemporary global governance.

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Supplemental Materials (to appear online)

A UPR Lottery Details

Figure SM.3 provides the distribution of the number of states from each regional group assigned to each session during the first cycle. This is the schedule used for block randomization by region during the UPR lottery.

| Session/ year | African Group | Asian Group | GRULAC | WEOG | EEG | Total |
|------------------|------------------|----------------|--------|------|-----|-------|
| 1-2008 | 4 | 4 | 3 | 3 | 2 | 16 |
| 2-2008 | 5 | 5 | 2 | 2 | 2 | 16 |
| 3-2008 | 4 | 4 | 3 | 3 | 2 | 16 |
| 4-2009 | 5 | 5 | 2 | 2 | 2 | 16 |
| 5-2009 | 4 | 4 | 3 | 3 | 2 | 16 |
| 6-2009 | 5 | 5 | 3 | 2 | 1 | 16 |
| 7-2010 | 4 | 5 | 3 | 2 | 2 | 16 |
| 8-2010 | 4 | 4 | 3 | 3 | 2 | 16 |
| 9-2010 | 4 | 5 | 3 | 2 | 2 | 16 |
| 10-2011 | 5 | 4 | 3 | 2 | 2 | 16 |
| 11-2011 | 4 | 5 | 2 | 3 | 2 | 16 |
| 12-2011 | 5 | 4 | 3 | 2 | 2 | 16 |
| Total | 53 | 54 | 33 | 29 | 23 | |

Figure SM.3: *Regional Distribution of States per Session*. Table provides the number of states that were required to be from each region when drawing lots (A/HRC/6/22, page 209).

Regional Groupings

The states categorized in each regional grouping are listed below. Bolded states are those that were serving or had been elected to serve on the HRC in 2007 when the lottery was conducted. The year these states were required to undergo their first review is noted in parentheses.

African Group (53 States)

- **Algeria (2007)**; **Angola (2010)**; Benin; Botswana; Burkina Faso; Burundi; **Cameroon (2009)**; Cape Verde; Central African Republic; Chad; Comoros; Congo; Côte d'Ivoire; Democratic Republic of the Congo; **Djibouti (2008)**; **Egypt (2010)**; Equatorial Guinea; Eritrea; Ethiopia; **Gabon**

(2008); Gambia; **Ghana (2008)**; Guinea; Guinea-Bissau; Kenya; Lesotho; Liberia; Libyan Arab Jamahiriya; **Madagascar (2010)**; Malawi; **Mali (2008)**; Mauritania; **Mauritius (2009)**; **Morocco (2007)**; Mozambique; Namibia; Niger; **Nigeria (2009)**; Rwanda; Sao Tome and Principe; **Senegal (2009)**; Seychelles; Sierra Leone; Somalia; **South Africa (2007)**; Sudan; Swaziland; Togo; **Tunisia (2007)**; Uganda; United Republic of Tanzania; **Zambia (2008)**; Zimbabwe.

Asian Group (54 States)

- Afghanistan; **Bahrain (2007)**; **Bangladesh (2009)**; Bhutan; Brunei Darussalam; Cambodia; **China (2009)**; Cyprus; Democratic People's Republic of Korea; Fiji; **India (2007)**; **Indonesia (2007)**; Iran (Islamic Republic of); Iraq; **Japan (2008)**; **Jordan (2009)**; Kazakhstan; Kiribati; Kuwait; Kyrgyzstan; Lao People's Democratic Republic; Lebanon; **Malaysia (2009)**; Maldives; Marshall Islands; Micronesia (Federated States of); Mongolia; Myanmar; Nauru; Nepal; Oman; **Pakistan (2008)**; Palau; Papua New Guinea; **Philippines (2007)**; **Qatar (2010)**; **Republic of Korea (2008)**; Samoa; **Saudi Arabia (2009)**; Singapore; Solomon Islands; **Sri Lanka (2008)**; Syrian Arab Republic; Tajikistan; Thailand; Timor Leste; Tonga; Turkmenistan; Tuvalu; United Arab Emirates; Uzbekistan; Vanuatu; Viet Nam; Yemen.

Latin American and Caribbean States (GRULAC) (33 States)

- Antigua and Barbuda; **Argentina (2007)**; Bahamas; Barbados; Belize; **Bolivia (2010)**; **Brazil (2008)**; Chile; Colombia; Costa Rica; **Cuba (2009)**; Dominica; Dominican Republic; **Ecuador (2007)**; El Salvador; Grenada; **Guatemala (2008)**; Guyana; Haiti; Honduras; Jamaica; **Mexico (2009)**; **Nicaragua (2010)**; Panama; Paraguay; **Peru (2008)**; Saint Kitts and Nevis; Saint Lucia; Saint Vincent and the Grenadines; Suriname; Trinidad and Tobago; **Uruguay (2009)**; Venezuela (Bolivarian Republic of).

Western Europe and Other States (WEOG) (29 States)

- Andorra; Australia; Austria; Belgium; **Canada (2009)**; Denmark; **Finland (2007)**; **France (2008)**; **Germany (2009)**; Greece; Iceland; Ireland; Israel; **Italy (2010)**; Liechtenstein; Luxembourg; Malta; Monaco; **Netherlands (2007)**; New Zealand; Norway; Portugal; San Marino; Spain; Sweden; **Switzerland (2009)**; Turkey; **United Kingdom of Great Britain and Northern Ireland (2008)**; United States of America.

Eastern European States (EEG) (23 States)

- Albania; Armenia; **Azerbaijan (2009)**; Belarus; **Bosnia and Herzegovina (2010)**; Bulgaria; Croatia; **Czech Republic (2007)**; Estonia; Georgia; Hungary; Latvia; Lithuania; Moldova; Montenegro; **Poland (2007)**; **Romania (2008)**; **Russian Federation (2009)**; Serbia; Slovakia; **Slovenia (2010)**; The former Yugoslav Republic of Macedonia; **Ukraine (2008)**.

B Descriptive Trends of Human Rights Usage in the ILC

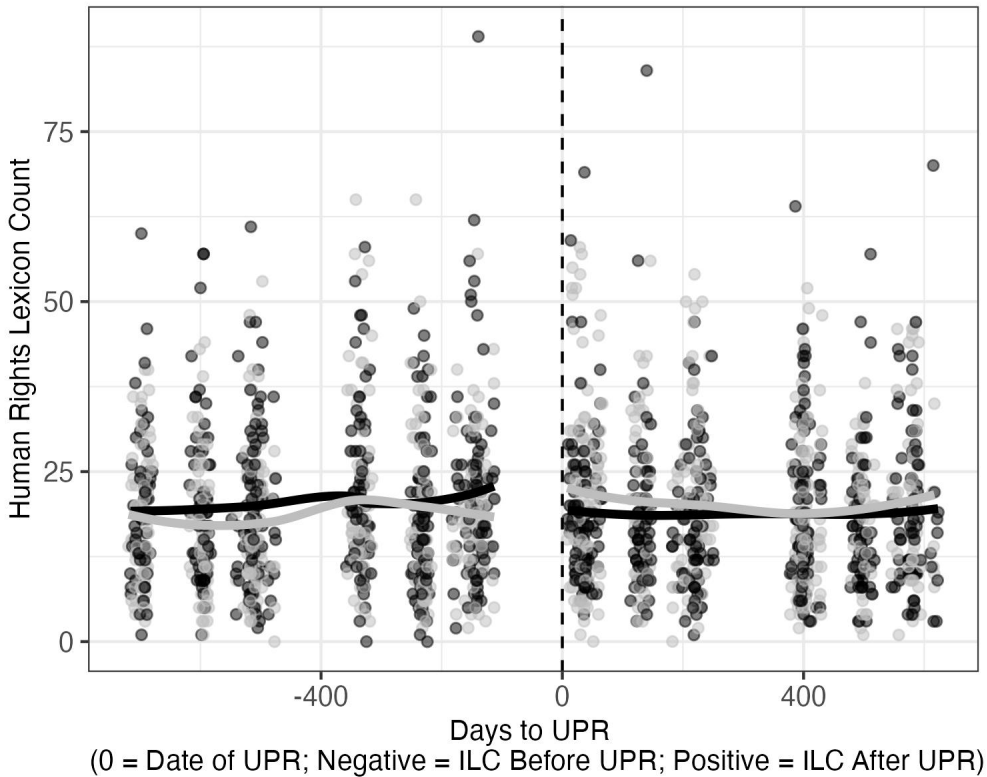


Figure SM.4: *Count of Human Rights Words at ILC, Before and After UPR.* Figure plots smoothed (LOESS) curve of the count of human rights terms delegates use during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country’s employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

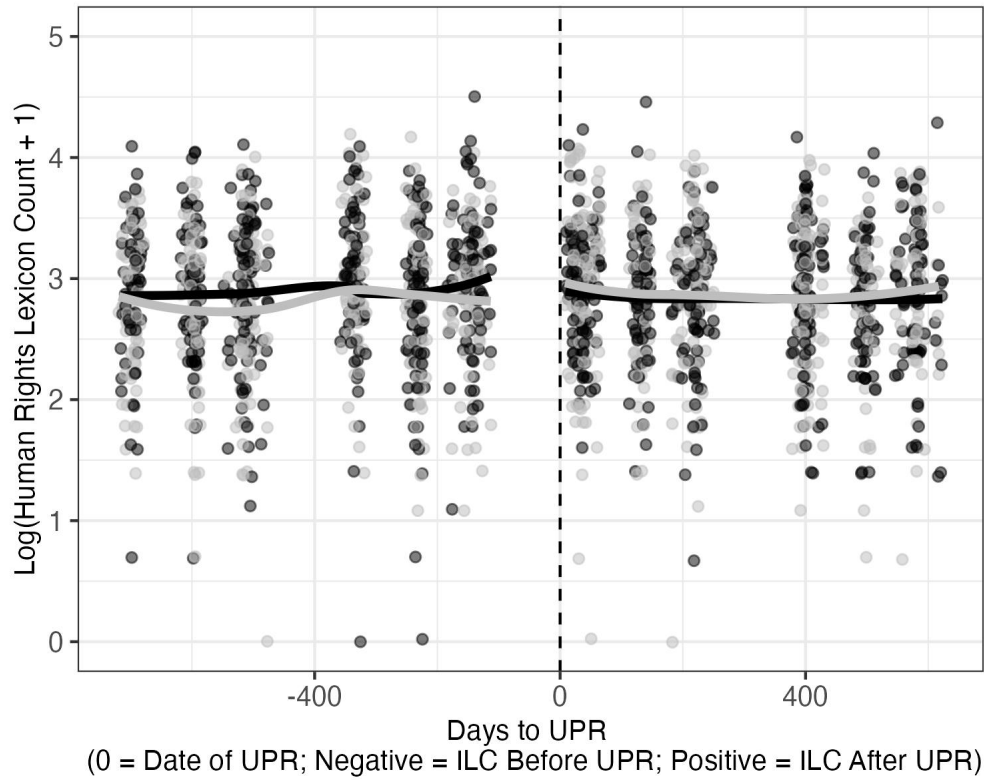


Figure SM.5: *Logged Count of Human Rights Words at ILC, Before and After UPR*. Figure plots smoothed (LOESS) curve of the logged count of human rights terms delegates use during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country's employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

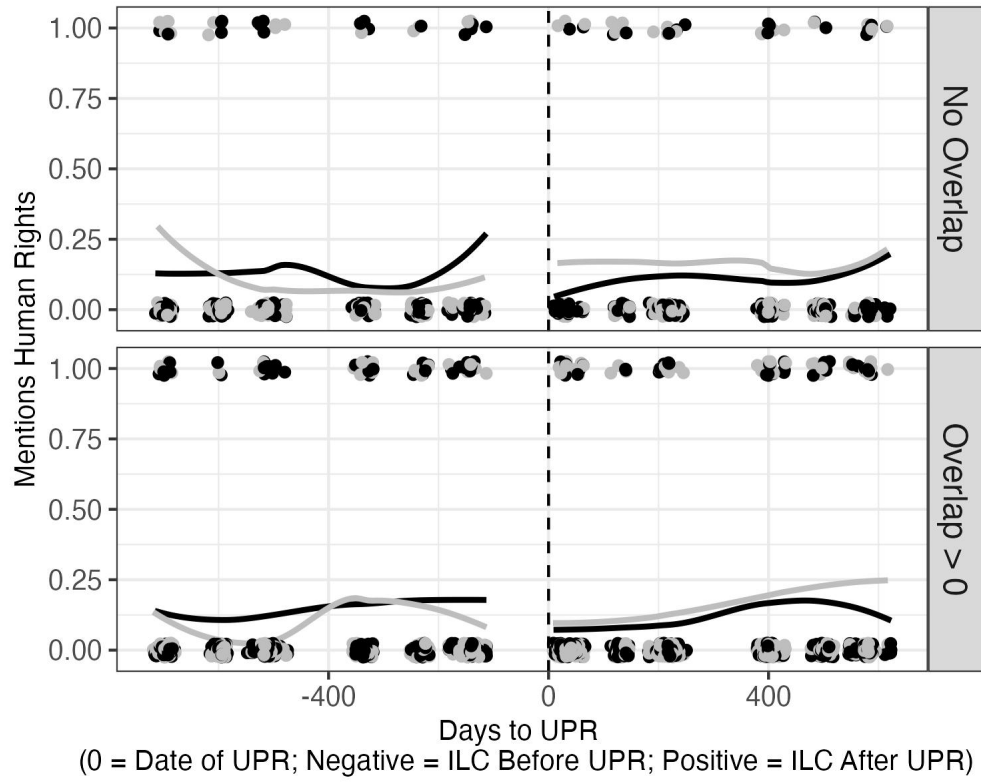


Figure SM.6: *Mentioning of Human Rights at ILC, Before and After UPR, among Delegations with at Least One Member at both ILC and UPR.* Figure plots smoothed (LOESS) curve of whether delegates mention the term “human rights” during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country’s employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

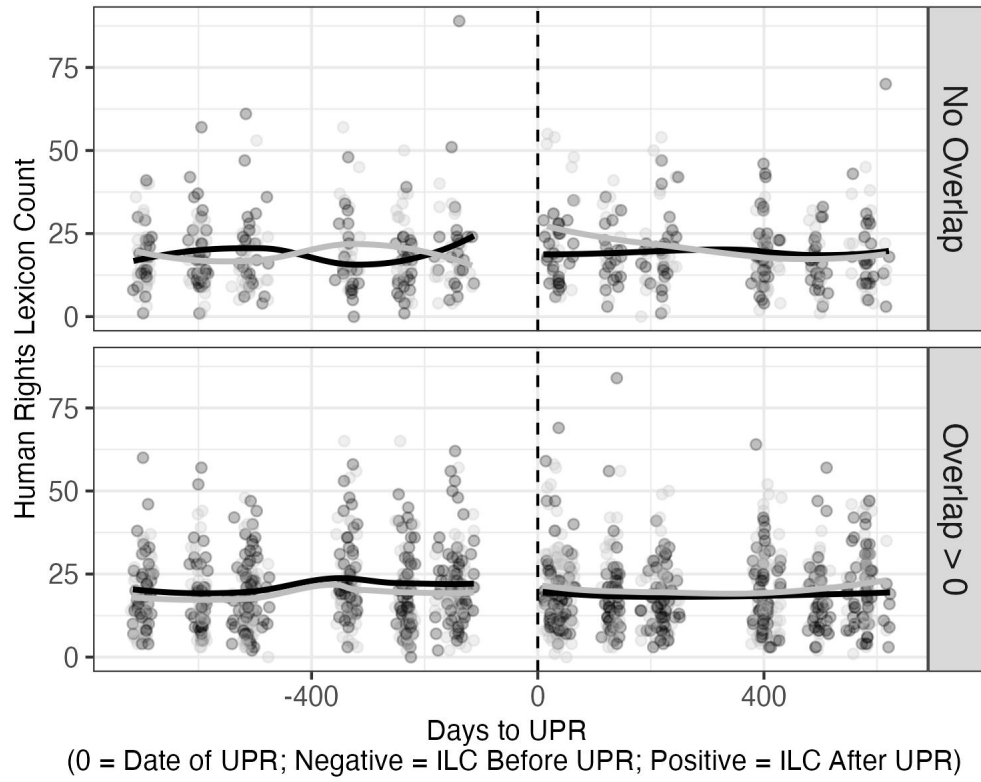


Figure SM.7: *Count of Human Rights Words at ILC, Before and After UPR, among Delegations with at Least One Member at both ILC and UPR.* Figure plots smoothed (LOESS) curve of the count of human rights terms delegates use during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country's employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

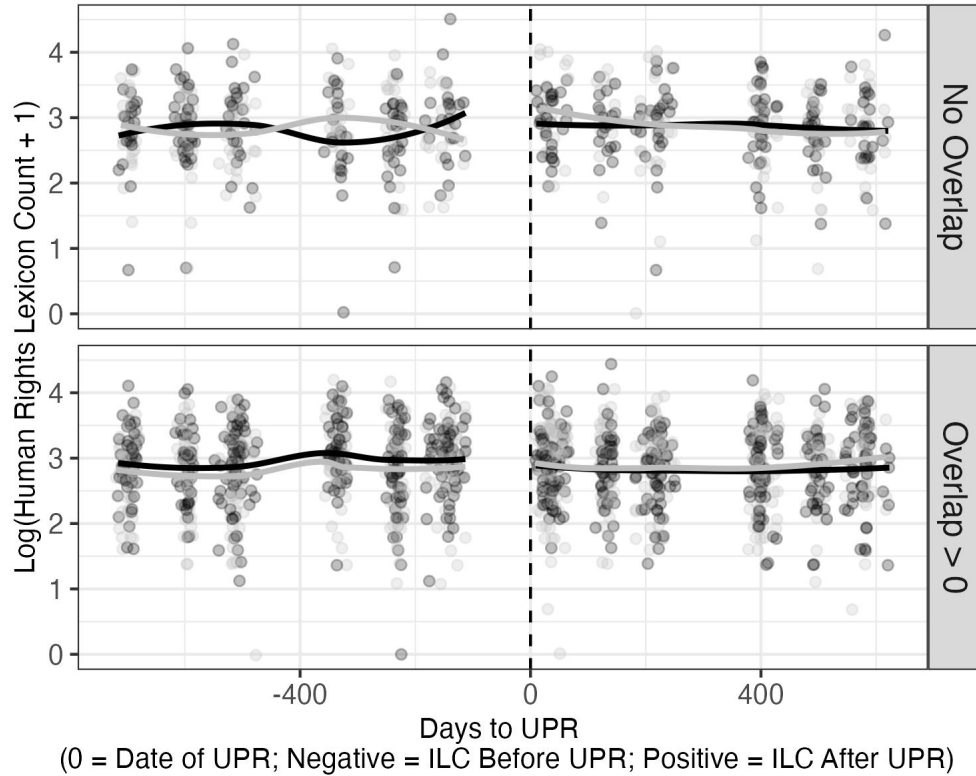


Figure SM.8: *Logged Count of Human Rights Words at ILC, Before and After UPR, among Delegations with at Least One Member at both ILC and UPR.* Figure plots smoothed (LOESS) curve of the logged count of human rights terms delegates use during their speeches at the ILC. Black lines represent this average for government delegates. Grey lines represent this average for a country’s employer and worker delegates. Observations to the left of the vertical dashed line are of speeches at the ILC held before a government underwent UPR; values to the right of this line are from speeches held after a government underwent UPR.

C Research Design Validation

C.1 Balance Plots

In this section, we present equivalence tests to assess covariate balance in pre/post-UPR groups. Figure SM.9 presents this for the full sample (left) and all observations that undergo UPR within two years of giving speeches at ILC (right). Figure SM.10 presents these same tests for delegates without any overlap in their country’s delegations to the ILC and UPR (left) and delegates with at least one common member at both the ILC and UPR (right). In each plot, vertical dashed lines mark the convention 0.36σ threshold proposed by Hartman and Hidalgo to assess imbalance. Across both plots,

there is no evidence of imbalance.

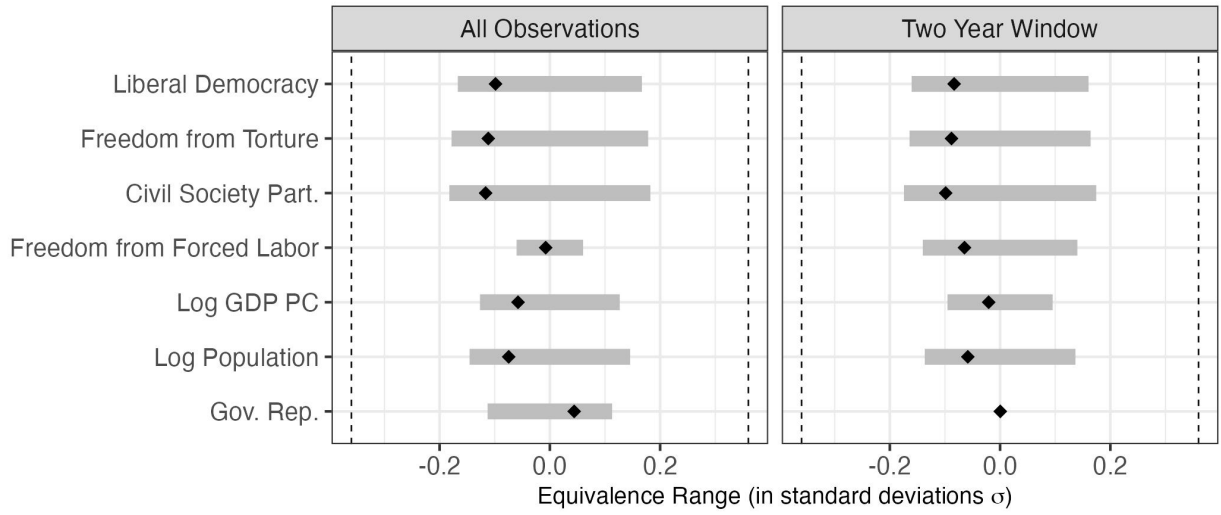


Figure SM.9: *Equivalence Tests, Main Analyses*. Balance plots for observations being exposed to UPR before vs. after ILC.

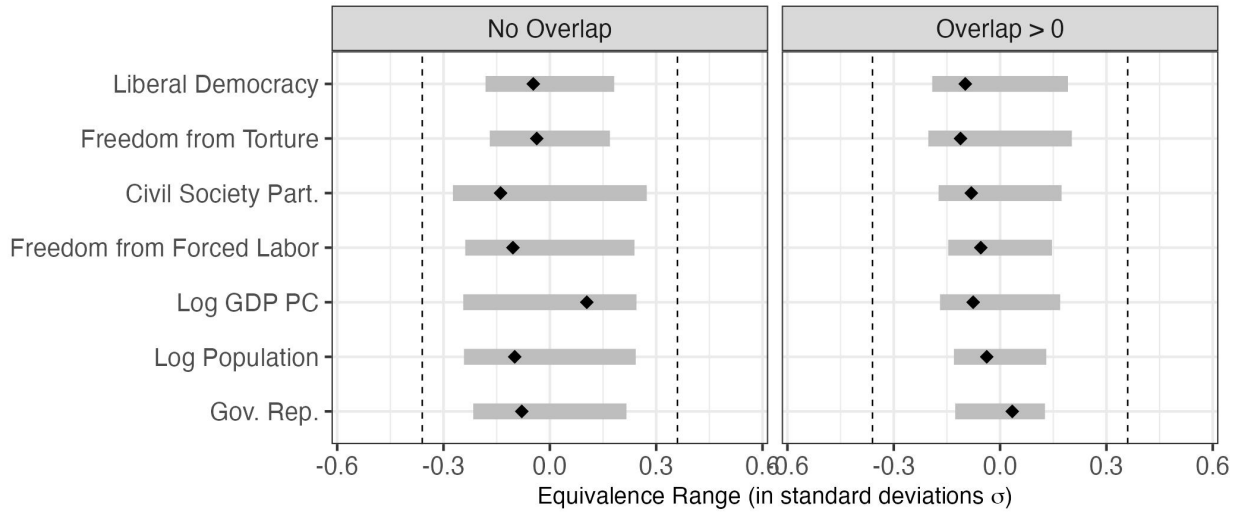


Figure SM.10: *Equivalence Tests, Overlap Analyses*. Balance plots for observations being exposed to UPR before vs. after ILC with and without overlapping delegations at UPR and ILC.

C.2 Placebo Analysis 1: Impact on ILC Word Count

We present the first placebo test of our main analysis in this section. Here we use the word count of delegates' speeches at the ILC as a placebo outcome. We argue that the content of their speeches should be affected by UPR, not how much they say. These analyses mostly validate our findings, though there is one area of exception.

Tables [SM.5](#) and [SM.7](#) provide consistent evidence that although human rights language is impacted by UPR, overall word count is not. In both tables the only interaction term that is statistically significant is the one in which the set of observations included in the analysis all have UPR within 6 months of ILC. This is the same analysis where we have the weakest results in [Table 1](#).

In [Tables SM.6](#) and [SM.8](#), which replicate the delegate overlap analysis using word count as an outcome, there are some signs that length of speech is affected by UPR as the number of overlapping delegates increases. In [Table SM.6](#), we find a statistically significant effect once there is more than one delegate overlapping between a states UPR and ILC delegations; in [Table SM.8](#), we find a statistically significant effect once there is any delegate overlap. Comparing the coefficients reported in [Table SM.8](#) with those in [Table 4](#), we can see that the estimates in the placebo analysis at most half as large in terms of magnitude.²² This suggests that these placebo analysis still validate our findings, though the interpretation of the overlap analysis is not as simple as I would like. Ideally, all interaction terms reported in [Tables SM.6](#) and [SM.8](#) would be null.

²²Comparing the coefficients in analyses with (non-logged) count variables as an outcome is not straightforward, because the base rates of total word count and total human rights word count are different.

Table SM.5: UPR does not impact length of speeches at ILC

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|-----------------------------|------------------------|-----------------------|-----------------------|------------------------|-------------------------|----------------------|-----------------------|
| | Word Count of Speech at ILC | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 6.467 (14.325) | 4.493 (13.445) | -5.880 (8.929) | -7.877 (9.096) | -4.571 (8.542) | -0.954 (8.615) | 2.577 (13.583) | -25.478 (43.659) |
| Gov. Rep. | 35.222** (14.360) | 35.851*** (13.737) | 29.017*** (10.556) | 28.968*** (10.671) | 28.903*** (9.949) | 32.189*** (9.899) | 24.365 (15.014) | 42.260* (24.273) |
| Liberal Democracy | | -14.864 (42.215) | | 95.918 (120.953) | 75.627 (100.739) | 61.613 (99.552) | 178.084 (197.164) | 288.423 (494.609) |
| Freedom from Torture | | -6.052 (6.664) | | -7.132 (17.839) | -16.938 (14.337) | -0.712 (15.887) | 1.627 (26.343) | 8.394 (62.670) |
| Civil Society Part. | | 110.884*** (35.608) | | 124.537 (107.736) | 151.378* (90.961) | 112.823 (90.637) | 68.320 (192.522) | 166.402 (414.972) |
| Freedom from Forced Labor | | 58.360* (34.161) | | 191.410 (136.952) | 171.209* (103.015) | 186.337* (102.567) | 41.905 (215.589) | -98.193 (555.120) |
| Log GDP PC | | 2.891 (5.307) | | 102.551* (55.069) | 63.215 (52.958) | 81.773* (49.191) | 89.334 (71.638) | 308.503* (172.193) |
| Log Population | | -4.695 (3.161) | | -129.118 (113.301) | -173.033* (104.130) | -207.550** (102.333) | -98.376 (155.457) | 69.026 (304.227) |
| Post UPR × Gov. Rep. | -15.623 (20.418) | -19.398 (19.682) | -5.517 (14.722) | -8.397 (14.817) | -8.743 (12.915) | -13.469 (12.976) | -10.502 (21.375) | -28.907 (31.758) |
| Country FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 3 Years | Full Sample | 1 Year | 6 Months |
| Observations | 1,818 | 1,791 | 1,818 | 1,791 | 2,177 | 2,266 | 916 | 473 |
| Adjusted R ² | 0.005 | 0.025 | 0.191 | 0.195 | 0.199 | 0.207 | 0.163 | 0.321 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table SM.6: Length of speeches at ILC is unaffected by delegates direct exposure to UPR

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|-----------------------------|-----------------------|------------------------|-----------------------|-----------------------|----------------------|-------------------------|---------------------------|
| | Word Count of Speech at ILC | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | -2.349 (20.040) | 15.809 (18.928) | 6.443 (15.829) | -17.283 (12.552) | -20.650 (18.022) | -10.917 (31.171) | 35.867 (33.548) | 211.671** (99.060) |
| Gov. Rep. | 16.829 (20.577) | 10.804 (19.208) | 45.573*** (16.527) | 39.526*** (13.414) | 37.558** (17.232) | 57.227** (22.867) | 67.991** (30.257) | 50.321 (52.255) |
| Liberal Democracy | 36.406 (74.468) | 174.516 (170.570) | -29.632 (51.106) | 79.635 (173.510) | 200.408 (383.258) | 556.806 (603.986) | 1,485.922* (773.903) | 3,443.010* (2,051.715) |
| Freedom from Torture | -18.439 (11.701) | -15.395 (33.902) | -1.869 (8.082) | -7.694 (21.884) | -22.017 (33.015) | -62.555 (49.929) | -28.707 (83.983) | -460.378 (321.058) |
| Civil Society Part. | 164.064*** (51.927) | -22.127 (152.608) | 92.232** (43.432) | 99.530 (142.082) | 132.195 (245.458) | 312.744 (277.440) | 373.032 (370.022) | 64.889 (577.957) |
| Freedom from Forced Labor | -36.596 (60.127) | 524.027 (324.343) | 102.350*** (38.696) | 152.292 (170.866) | 534.502* (307.596) | 237.463 (472.525) | -821.254 (586.699) | 1,194.093 (872.504) |
| Log GDP PC | 3.711 (8.003) | -68.465 (72.956) | 2.355 (6.289) | 182.290** (90.581) | 223.161 (148.941) | 209.636 (175.315) | 103.221 (159.229) | 1,155.471 (901.230) |
| Log Population | -13.194*** (4.953) | -234.527 (161.346) | -1.740 (3.591) | 28.189 (169.027) | 128.751 (284.834) | 313.090 (623.533) | 781.277 (1,075.263) | 1,952.157 (1,830.282) |
| Post UPR × Gov. Rep. | -7.413 (28.317) | -4.539 (23.867) | -24.513 (23.910) | -13.108 (19.753) | -12.312 (28.783) | -41.037 (37.198) | -120.328*** (41.937) | -102.074 (66.353) |
| No. Overlapping Delegates | 0 | 0 | >0 | >0 | >1 | >2 | >3 | >4 |
| Country FE | - | ✓ | - | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | ✓ | - | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years |
| Observations | 598 | 598 | 1,193 | 1,193 | 701 | 376 | 200 | 98 |
| Adjusted R ² | 0.023 | 0.238 | 0.028 | 0.187 | 0.208 | 0.478 | 0.615 | 0.106 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table SM.7: **UPR does not impact logged length of speeches at ILC**

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|---------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|--------------------|--------------------|
| | Log Word Count of Speech at ILC | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 0.016 (0.022) | 0.013 (0.021) | -0.002 (0.013) | -0.005 (0.013) | -0.0004 (0.013) | 0.006 (0.013) | 0.005 (0.020) | -0.026 (0.063) |
| Gov. Rep. | 0.049** (0.021) | 0.051** (0.020) | 0.044*** (0.015) | 0.044*** (0.015) | 0.047*** (0.015) | 0.052*** (0.015) | 0.042** (0.021) | 0.072** (0.035) |
| Liberal Democracy | | -0.025 (0.062) | | 0.216 (0.171) | 0.142 (0.138) | 0.105 (0.136) | 0.392 (0.261) | 0.644 (0.682) |
| Freedom from Torture | | -0.012 (0.010) | | -0.025 (0.026) | -0.028 (0.021) | -0.012 (0.021) | -0.018 (0.037) | -0.028 (0.095) |
| Civil Society Part. | | 0.169*** (0.053) | | 0.176 (0.145) | 0.228* (0.128) | 0.202 (0.125) | -0.001 (0.239) | 0.056 (0.516) |
| Freedom from Forced Labor | | 0.102* (0.053) | | 0.306 (0.197) | 0.213 (0.158) | 0.221 (0.161) | 0.173 (0.326) | 0.186 (0.890) |
| Log GDP PC | | 0.007 (0.008) | | 0.157** (0.078) | 0.120* (0.069) | 0.155** (0.066) | 0.114 (0.099) | 0.359 (0.223) |
| Log Population | | -0.008 (0.005) | | -0.173 (0.151) | -0.181 (0.146) | -0.195 (0.146) | -0.117 (0.215) | 0.164 (0.425) |
| Post UPR × Gov. Rep. | -0.030 (0.030) | -0.036 (0.028) | -0.017 (0.020) | -0.020 (0.020) | -0.020 (0.019) | -0.028 (0.019) | -0.024 (0.028) | -0.054 (0.045) |
| Country FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | - | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 3 Years | Full Sample | 1 Year | 6 Months |
| Observations | 1,818 | 1,791 | 1,818 | 1,791 | 2,177 | 2,266 | 916 | 473 |
| Adjusted R ² | 0.004 | 0.030 | 0.210 | 0.215 | 0.215 | 0.229 | 0.172 | 0.181 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table SM.8: Log length of speeches at ILC is unaffected by delegates direct exposure to UPR

| | <i>Dependent variable:</i> | | | | | | | |
|---------------------------|---------------------------------|--------------------|---------------------|---------------------|---------------------|---------------------|----------------------|--------------------|
| | Log Word Count of Speech at ILC | | | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post UPR | 0.004 (0.030) | 0.024 (0.027) | 0.016 (0.025) | -0.018 (0.019) | -0.022 (0.029) | 0.001 (0.053) | 0.038 (0.055) | 0.314** (0.153) |
| Gov. Rep. | 0.030 (0.030) | 0.021 (0.028) | 0.062*** (0.024) | 0.058*** (0.018) | 0.066*** (0.024) | 0.093*** (0.035) | 0.084** (0.041) | 0.061 (0.077) |
| Liberal Democracy | 0.046 (0.108) | 0.325 (0.268) | -0.045 (0.076) | 0.167 (0.240) | 0.387 (0.492) | 0.599 (0.701) | 2.091 (1.268) | 5.223* (3.109) |
| Freedom from Torture | -0.031* (0.018) | -0.017 (0.053) | -0.005 (0.011) | -0.024 (0.033) | -0.061 (0.050) | -0.113 (0.076) | -0.056 (0.120) | -0.706 (0.515) |
| Civil Society Part. | 0.252*** (0.076) | -0.131 (0.240) | 0.136** (0.064) | 0.177 (0.187) | 0.259 (0.312) | 0.516 (0.392) | 0.443 (0.558) | 0.090 (0.903) |
| Freedom from Forced Labor | -0.012 (0.085) | 0.904** (0.445) | 0.152** (0.061) | 0.217 (0.244) | 0.816* (0.435) | 0.660 (0.671) | -0.745 (0.835) | 1.631 (1.341) |
| Log GDP PC | 0.008 (0.012) | -0.102 (0.107) | 0.006 (0.009) | 0.269** (0.124) | 0.391* (0.207) | 0.454 (0.299) | 0.314 (0.232) | 1.633 (1.313) |
| Log Population | -0.019*** (0.007) | -0.319 (0.223) | -0.004 (0.005) | 0.027 (0.219) | 0.173 (0.358) | 0.813 (0.938) | 1.283 (1.573) | 2.688 (2.818) |
| Post UPR × Gov. Rep. | -0.018 (0.041) | -0.011 (0.034) | -0.044 (0.034) | -0.030 (0.026) | -0.045 (0.037) | -0.080 (0.055) | -0.176*** (0.064) | -0.147 (0.101) |
| No. Overlapping Delegates | 0 | 0 | >0 | >0 | >1 | >2 | >3 | >4 |
| Country FE | - | ✓ | - | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | - | ✓ | - | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-Post Window | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years | 2 Years |
| Observations | 598 | 598 | 1,193 | 1,193 | 701 | 376 | 200 | 98 |
| Adjusted R ² | 0.027 | 0.265 | 0.031 | 0.207 | 0.223 | 0.297 | 0.426 | 0.110 |

Notes: Country-delegate-period cluster robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.