**The Impact of Preferential Trade Agreements on Governmental Repression Revisited**

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**Abstract**

Previous research suggests that most treaties are ineffective in ensuring countries' compliance with human rights standards. In contrast, preferential trade agreements (PTAs) comprised of 'hard law' can withhold economic benefits and, thus, can have a real potential to substantially reduce human rights violations. The following article questions this presumption as existent work on the effects of PTAs on human rights standards neglects a selection process underlying the implementation of these treaties. That is, countries being aware of the 'shadow of the future' already take into account what may happen at the succeeding enforcement stage when establishing a particular PTA. This implies that states agree on 'hard' human rights standards in PTAs only if they have a general propensity to abide by human rights in the first place. For testing the empirical implications of their argument, the authors collected new data on human rights compliance rates and PTAs in 1976/77-2009, and employ genetic matching techniques. The results show that PTAs are unlikely to affect human rights compliance when controlling for the selection problem. Furthermore, we show that countries' ratification of human rights treaties seems to be a simple attempt to achieve a good reputation, while post-ratification compliance is hardly given afterwards.

**Keywords:** international institutions; preferential trade agreements; human rights; compliance; selection

**Introduction**

According to the latest Amnesty International (2011) report, people suffered from abuse, were tortured, or were constrained in their freedom of expression in almost 100 countries worldwide during 2010. The question that may immediately emerges against this background asks, 'why does this occur, i.e. why do some countries torture their people while other countries comply with human rights standards and, consequently, refrain from abusing, torturing, or repressing their people?' A thorough and systematic answer to this question is certainly of interest to policy makers and non-governmental organizations alike, and, in fact, scholars sought of addressing this before (e.g., Cottier 2002; Goodman and Jinks 2003; 2004; Gould 2004; Hafner-Burton and Tsutsui 2005; Hathaway 2002; Hill 2010; Keith 1999; Mitchell and McCormick 1988; Moravcsik 1995; Neumayer 2005; Risse, Ropp, and Sikkink 1999; Schwarz 2004; Sen 2004).

The majority of this literature, however, claims that most human rights treaties are ineffective in ensuring countries' compliance with human rights standards. Primarily, this stems from the lack of enforcement mechanisms that characterizes most, if not all, international human rights agreements. In contrast, preferential trade agreements (PTAs) if they comprise 'hard human rights standards', i.e. if trade benefits can be made conditional on member states' compliance with international human rights, might be able to induce domestic policy changes and substantially reduce human rights violations.[1] The logic here is simple: PTAs pertain to the highly attractive gains from trade and demanding tough standards in these kinds of treaties can withhold economic benefits or impose economic sanctions in the case of non-compliance (Hafner-Burton 2005).[2]

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[1] The term hard human rights standard in contrast to soft human rights standard implies that non-compliance with the standard can be sanctioned by withholding trade benefits. Soft standards, in contrast, refer to the simple mentioning of human rights standards in a treaty without an enforcement mechanism being attached to the standard.

[2] Although our study empirically focuses on the compliance with human rights standards, the theoretical argument could easily be applied to other policy issue areas such as environmental degradation, labor standards, or the impact of any international institution in general. For example, see Bechtel and Tosun (2009) who

While this idea seems to constitute good news for the impact of PTAs in particular, from a more general perspective it also supports the assertion that international institutions or regimes do (at least) have the potential to influence adverse governments via coercion (and maybe also via other mechanisms such as issue linkage, or reputational effects) toward showing 'good behavior.' Arguably, the pre-requisite is that this logic holds and is empirically valid to the extent that international institutions, regimes, and PTAs as the focus of this research, are indeed effective instruments for enforcing states' compliance with human rights standards.

The following article questions this presumption as existent work on PTAs neglects a selection process underlying the implementation of these treaties. Since countries should be aware of the 'shadow of the future', they should already take into account what may happen at the succeeding enforcement stage when establishing a particular PTA. This implies that states agree on hard human rights standards in PTAs only if they have a general propensity to abide by human rights in the first place anyway.

If this selection process indeed holds it is crucial for examining the effectiveness of PTAs (or other international institutions and regimes) in promoting human rights (or other standards such as labor or environmental standards), that both theory and empirics acknowledge first the factors that motivate countries to include (hard) human rights standards and those that do not require the inclusion of any human rights clauses. Otherwise, it may well be that the findings we obtain are spurious or biased. Due to this rationale, the literature on the rational design of international institutions (e.g., Koremenos, Lipson, and Snidal 2001; Koremenos 2005) serves as the foundation of our research as it emphasizes that states take into account what may happen at the succeeding enforcement stage already when they establish a particular regime (see also von Stein 2005; Simmons and Hopkins 2005; Bernhagen 2008; Hill 2010). Consequently, scholars must consider countries' preferences for the establishment of

examine the impact of PTAs in the field of environmental protection. Due to the availability of data, however, we exclusively focus on PTAs and human rights compliance in this work.

international institutions, regimes, and PTAs when studying their effects in order to avoid biased inferences, since these are just 'two sides of the same coin' (e.g., Fearon 1998; Koremenos et al. 2001; Koremenos 2005).

For empirically testing our argument, we collected new data on human rights compliance rates and PTAs in 1976/77-2009, and employ genetic matching techniques for addressing selection. The results confirm that PTAs are unlikely to affect human rights compliance when controlling for the selection problem. Further, countries' ratification of human rights treaties seems to be a simple attempt for achieving a favorable reputation, while post-ratification compliance is hardly given afterwards (see also Hollyer and Rosendorff 2011). Our research provides important insights into the effectiveness of PTAs as enforcement mechanisms of human rights standards, and potentially into treaties dealing with other issue areas or international institutions in general. In fact, our results suggest that international agreements do not have much coercing power *per se*, but are only means to enforce law – such as human rights standards as in our case – if governments are willing to do so in the first place.

The article proceeds as follows. The next section summarizes the existent literature on the broader framework to which our research seeks to contribute to: do international institutions *affect* world politics or rather *reflect* them? We then discuss our theoretical argument, highlighting that the previous research on PTAs and human rights standards has not yet accounted for persistent selection effects, although we believe that there is a genuine need for addressing this shortcoming. Here, we also outline what kinds of implications this has for the impact of hard (or any) human rights commitments in PTAs on the compliance with human rights standards in turn. Afterwards, we describe the data used for our empirical analysis. The succeeding sections present our research design, the estimation strategy as well as our results and robustness checks. We finish the article with a comprehensive discussion of our findings and implications for future studies and policy makers.

**Do International Institutions Have the Potential to Affect World Politics?**

The broader and more fundamental research question underlying our paper is whether international institutions and agreements *affect* world politics or rather only *reflect* them (see Boehmer, Gartzke, and Nordstrom 2004). On the one hand, if we find evidence that countries join international institutions and agreements such as PTAs after taking into account their expectations on compliance already at the formation stage, this would lend support to a realist point of view claiming that these institutions do not have either an independent or a substantial effect on countries' courses of action. Put differently, these institutions then merely reflect existing interests (Downs, Rocke and Barsoom 1996; see also Hollyer and Rosendorff 2011). On the other hand, if international institutions and agreements were to influence countries' policy decisions, perhaps independent of their design or underlying country-specific preferences, we could subscribe to an institutionalist perspective, which posits that international institutions are in fact more than 'signaling devices' (Boehmer, Gartzke, and Nordstrom 2004).

Having said that, a great amount of literature would confirm that international institutions do indeed affect countries' domestic policy decisions, and that they have a real and independent impact. For example, several studies show that international organizations exert influence by significantly reducing the risk of military conflict between countries if they share a common membership in these institutions (Boehmer et al. 2004; Dorussen and Ward 2008; Oneal and Russett 1999). Membership in international institutions also positively influences international trade (Ingram, Robinson, and Busch 2005), environmental quality (Ward 2006; Spilker 2012), or democratization processes (Pevehouse 2002).

Furthermore and directly pertaining to our focus on PTAs and human rights compliance, Hafner-Burton (2005) examines the conditions under which PTAs may be effective in preventing domestic human rights abuses. She argues that PTAs with 'hard' human rights standards have the potential to rely on coercion (instead of reputational effects) to influence

their member countries towards establishing and respecting human rights standards. This stands in contrast to both PTAs with 'soft' standards and regular human rights treaties, which generally lack enforcement mechanisms. This distinction seems important, since it shows that only those international agreements, which link material benefits to the compliance with human rights standards, can induce their member countries to demonstrate a more substantial respect toward human rights. By analyzing data on PTAs and the level of compliance with human rights for 1976-2002, Hafner-Burton (2005) finds empirical support for her theoretical arguments.

Contrary to this more optimistic view on the effectiveness of international institutions, von Stein (2005) actually obtains evidence that international treaties have little constraining power. In her analysis of states' commitment to Article VIII of the International Monetary Fund (IMF) treaty, von Stein (2005) demonstrates that the factors, which make it more likely that states enter into internationally binding agreements in the first place, affect countries' propensity for compliance afterwards.[3] While this suggests that treaties merely have screening purposes than actual constraining power, it is in line with Downs, Rocke and Barsoom (1996) who contend that the high levels of compliance we usually observe in international governance do not necessarily mean that a deep level of cooperation has been reached.[4] Instead, it is more likely that countries demonstrate compliance with institutional laws and regulations by making agreements rather shallow to ensure encompassing membership. Under these circumstances, the policy changes demanded by international institutions simply denote (denote/indicate) the 'lowest common denominator' and, thus, rather small hurdles to clear.

The 'lowest common denominator' argument essentially points to the idea that countries

[3] However, Simmons and Hopkins (2005) criticize von Stein's methodological approach. They show that even if one accounts for the screening effects of Article VIII of the IMF treaty, there is still significant constraining power to the treaty. We will come back to this methodological disagreement in our research design section below.

[4] This is somewhat in contrast to Chayes and Chayes (1993) who argue that non-compliance is the exception because countries have an inherent interest not to violate agreements to which they have committed themselves. If non-compliance occurs, however, it happens unintentionally due to rule ambiguity or capacity limitations. Compliance can therefore be increased through transparency, clear rule interpretation, or capacity building.

build in their expectations about compliance when negotiating international agreements. Hence, it becomes difficult to determine what the effect of a specific treaty may be or how it may look like when the typical country that ratifies the treaty 'possesses more of the state-level characteristics known to be associated with good human rights practices than the typical nonratifier' (Hill 2010: 1161).

The idea of incorporating expectations about compliance is at the heart of the rational design literature (e.g., Koremenos, Lipson, and Snidal 2001; Koremenos 2005). By mainly focusing on uncertainty and flexibility, this literature argues that states will opt for international forms of cooperation that can be described as 'soft law' when facing conditions of uncertainty. In turn, this occurs at the expense of agreements with clear enforcement mechanisms that pertain to the 'hard law' category (Abbott and Snidal 2000; Koremenos 2005). Consequently, these arguments posit that countries are aware of the 'shadow of the future' when designing international agreements. While these agreements may not necessarily address the problem situation at hand effectively, they actually fit countries' pragmatic anticipation on the prospective policy change that will be possible given the (potential) member countries' current policy level. We will rely on this assertion in the next section when elaborating why PTAs are unlikely to be effective in causing policy changes in issue areas such as human rights, labor standards, or environmental protection.[5]

**The Impact of Preferential Trade Agreements on Government Repression – A Selection Perspective**

Previous literature suggests that PTAs, in contrast to standard labor rights treaties or human rights agreements in general, have a built-in enforcement mechanism if they are comprised of hard law standards i.e., if economic benefits from these PTAs are in some way conditional

[5] As emphasized, our argument is more general, since it not only refers to human rights issues, but essentially to any policy area international institutions deal with. The succeeding section exclusively focuses on human rights, though, since our empirical work is restricted to the compliance with these.

upon member states' actions toward human rights. More specifically, the possibility to rely on trade sanctions in case of non-compliance allows 'hard law' PTAs to coerce member countries to change their domestic policies as specified in the respective agreement (Hafner-Burton 2005). Although this argumentation sounds plausible, we claim that it ignores a crucial step: it does not account for the formation stage of these treaties/agreements, i.e., the stage in which countries negotiate and decide the terms to be included and the level of commitment to be reached in any institutional agreement or PTA. Moreover, this line of reasoning does not take into account countries' willingness to comply in the first place with treaty regulations afterwards. If states are selective when deciding whether to include (hard) human rights standards in a PTA or not, any analysis on the effectiveness of PTAs in promoting human rights is likely to be biased unless we control for this selection effect. In fact, following the rational design literature (e.g., Koremenos, Lipson, and Snidal 2001; Koremenos 2005), we would expect countries to be 'forward looking' as they should take into account what may happen at the succeeding enforcement stage already when they establish a particular institution, regime, treaty, and so forth.

Correspondingly, we expect states to include hard human rights standards in PTAs only if they themselves expect to subsequently comply with the human rights standards postulated in the agreement. This should hold because including tough human rights standards while knowing that one is unlikely to abide by these standards brings about two risks: First, violating these standards might simply involve reputational costs for the country since it thereby signals to the present as well as to other (potential) treaty partners that it does not necessarily adhere to its international obligations. Second, the country might risk that the partner country in the PTA does opt for enforcement. If the counterpart of a violating country in a PTA observes the violation of the human rights standards and is willing to use trade sanctions to enforce them, the violating country will not only suffer from reputational costs and damages but it will also lose out from the gains of trade. Being aware of this enforcement

risk, a country that does not have a good human rights record should simply not include any clause that may lead to the enforcement of human rights standards. Since a PTA without human rights standards ensures the same amount of trade liberalization and, thus, the same potential gains from trade but does not involve any reputational costs or the risk of enforcement in the event of non-compliance with a human rights clause, there is no a-priori reason to actually add such human rights standards.

Following this logic, we expect that only countries with a good human rights record should be willing to add a hard human rights clause to a PTA they are entering. Similarly, this also implies that we should not observe any impact of hard human rights standards in PTAs on states' degree of human rights compliance once we control for the selection effect of including (hard) human rights clauses in a PTA.

We illustrate our rationale with two cases. In 2003, the European Union (EU) established a PTA with Chile and Egypt, respectively. Whereas the treaty with Chile included hard human rights standards, the one with Egypt did not contain any reference to human rights at all. Following our argument, the selection process can explain these outcomes during PTA formation. In the case of the EU-Chile agreement, it was (and still is) unlikely that any (major) human rights violations would emerge since both countries were and still are highly committed to democratic values. Hence, both the EU and Chile should face little difficulties when signed the PTA containing hard human rights standards, since they knew in advance that an enforcement situation was/is unlikely to ever emerge. In contrast, when the EU and Egypt entered into their PTA, the calculations for the two actors may have been somewhat different: as Egypt would have been unlikely to change its human rights practices even if it had agreed on a PTA with a (hard) human rights clause, the EU would have been faced with the option to enforce the human rights standards. Enforcement, however, would have been associated with vast reputational costs for Egypt and would have led to potential losses in the gains from trade for both actors. Having said that, Egypt (and the EU as well) could simply

circumvent this dilemma by establishing a PTA containing no human rights commitments, which, in turn, does neither risk losing any gains from trade nor begets any reputational costs.

Arguably, this means that countries take into account the probability of enforcement when establishing PTAs. We therefore claim that countries are aware of the 'shadow of the future' and that they should conclude PTAs with (hard) human rights standards only in those cases, in which they are likely to subsequently comply with the PTA's human rights regulations. Hence, and perhaps paradoxically, the inclusion of (hard) human rights standards primarily occurs in those cases in which they are hardly necessary. In contrast, countries should rely on soft or no human rights standards when they expect that they (and their treaty partner) will violate human rights.

To recap, our argument implies more generally that analyzing the effectiveness of PTAs in enforcing compliance with human rights standards is equivalent to a selection bias problem if we do not consider the determinants that compel countries to include a (hard) human rights standard or not in the PTAs in the first place. Since states would only conclude hard commitment PTAs when they expect that they will be able to comply with these standards anyway, the effect of PTAs with hard human right standards might have been overestimated by previous studies. Overall, this implies that PTAs *per se* might not be effective in promoting human rights standards once we control for the selection process underlying the conclusion of PTAs.

## Research Design

### *Data*

To examine the effect of (hard) human rights standards in PTAs on countries' compliance with human rights, we analyze partly new data. More specifically, we used the country-year as the unit of analysis and merged existing data on human rights compliance with newly compiled information on PTAs as well as their inclusion of human rights standards between

1976 and 2009 (1977 constitutes the 'effective' starting year for the time period under study, since most of our variables are lagged by one year). We also added information from existing datasets on various covariates described in detail below. Due to the methodological approach that we describe below, observations with missing values had to be deleted case-wise. Ultimately, our time-series cross-sectional data are comprised of 4,117 country-years for 174 countries with 249 PTAs in total.

*Dependent Variable*

We measure the extent to which a country complies with human rights standards via its level of political repression. Following Hafner-Burton (2005: 615), political repression is operationalized by the level of political terror, i.e., data on 'murder, torture, or other cruel, inhuman, or degrading treatment or punishment; prolonged detention without charges; disappearance or clandestine detention; and other flagrant violations of the right to life, liberty, and the security of the person'. This variable draws on two data sources. The first one is Poe and Tate (1994) who offer data on 153 governments' reported levels of political terror from 1976 to 1993; the second one pertains to Gibney et al. (2005) and Gibney et al. (2011) who collected repression data from 1980 to 2009 across a somewhat different sample of 141 states and territories. The information in both sources was compiled via content analysis of annual human rights reports issued by Amnesty International and the U.S. State Department. Based upon information that Gibney shared with us in personal email exchanges, we constructed a 5-point ordinal scale measure that combines the information of both Gibney et al. (2011) and Poe and Tate (1994), and ultimately follows Gibney et al.'s (2011) operationalization of the Political Terror Scale. Table 1 gives an overview about this item.

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*Explanatory Variables*

We consider states' decisions to include human rights standards in PTAs as our core factor of interest and we operationalize this via two different dichotomous variables. More specifically, we coded 249 different PTAs using content analysis of all formal PTA contracts,[6] where the 'explicit adoption of human rights language and principles, and whether the benefits accorded by the contract formally depend on those principles' (Hafner-Burton 2005: 615) signaled if a PTA in question is comprised of a) hard human rights law or b) any human rights law. Our first variable, *PTA Law*, thus receives a value of 1 in a specific country-year if state membership in a PTA also requires compliance with either hard or soft law human rights practices (0 otherwise). Our second variable, *PTA Hard Law,* measures state membership with PTAs supplying hard standards: an observation takes on a value of 1 in a specific year if a state belongs to a PTA with hard law human rights standards.[7] Using these two treatment variables allows us to test whether PTAs that incorporate human rights standards with an enforcement mechanism (i.e. hard standards) differ from those PTAs that incorporate human rights standards in general and from those PTAs that do not incorporate any human rights standards at all.

Next to the core variables, we also control for other influences on the level of political repression. We follow Hafner-Burton's (2005) approach here. First, in order to differentiate the impact of human rights standards in PTAs from a countries' general commitment to international human rights agreements, we include information on ratification, succession, and accession to the *International Covenant on Civil and Political Rights* and the *Convention against Torture*, respectively. The final variable, the *Human Rights Ratification* variable, is an

[6] This includes treaties, protocols, and other forms of amendments.
[7] Space limitations prevent us from explaining in-depth our coding procedures, but these are listed in the online codebook. Having said that, the descriptive statistics in Table 2 below actually demonstrate that our codings are very close to Hafner-Burton's operationalizations – although our variables do not fully match her items.

ordinal variable ranging from 0 to 2. Those values are derived from the total number of the two treaties that a state has ratified into national law by any given year.

Second, scholars are often concerned that population pressure can exacerbate resource scarcity and, thus, increase the likelihood that a 'government will use repression to control civil violence' (Henderson 1993; Poe and Tate 1994). We therefore consider a second control variable that measures a state's population density per square kilometer. We retrieved the data from the World Bank Development Indicators and lag it for our model estimations.

A combination of democratic values, democratic institutions, transparency, and the promotion of civil liberties are crucial determinants of countries' tendency to comply with human rights accords (Henderson 1991; Poe et al. 1999; Cingranelli and Richards 1999; see also Mitchell and McCormick 1988; Keith 1999; Gould 2004; Hathaway 2002; Schwarz 2004; Neumayer 2005). Related to, albeit differently, regime transitions and political instability influence the regime type of a country and, in turn, could affect the propensity toward repression by the government (Henderson 1991; Poe et al. 1999; Cingranelli and Richards 1999; see also Mitchell and McCormick 1988; Keith 1999; Gould 2004; Hathaway 2002; Schwarz 2004; Neumayer 2005). For capturing these two rationales, we incorporate the following variables: *Political Stability* counts the number of years since a state has undergone a structural regime transition, defined as a movement on *polity2* of three points or more (Marshall and Jaggers 2002). Note that this item also corrects for temporal dependencies as it essentially measures the movement of a country's *polity2* score over time (see Beck et al. 1998).We operationalize a country's *level of democracy* via the *Polity2* item of the *Polity IV* data (Marshall and Jaggers 2002).[8] Both variables are lagged by one year.

Forth, *GDP* measures a country's gross domestic product per capita in constant U.S. dollars. Mitchell and McCormick (1988) argue for the 'simple poverty thesis,' i.e., a lack of economic resources creates a fertile ground for political conflict and governmental political

[8] Missing values were imputed using data from Gleditsch (2008), who does not omit values of microstates.

repression. Furthermore, 'in an advanced economy where people are likely to have fewer grievances, political stability is often achieved more easily, reducing the likelihood of human rights violations' (Pritchard 1989; Henderson 1991). The data for this variable stem from the World Bank Development Indicators, We log it due to its skewed distribution and lag it by one year.

Finally, we draw on the sum of a state's total exports and imports of goods and services measured as a share of GDP in order to operationalize *Trade*. This variable controls for the impact of financial and market transactions on countries' tendency to comply with human rights (Cingranelli and Richards 1999; Richards et al. 2001).[9] Similar to *GDP* above, the World Bank Development Indicators provides us with the necessary information, while the final variable is logged and lagged by one year. Table 2 summarizes the descriptive statistics.

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*Methodological Approach: Genetic 1:1 Matching with Replacement*

As argued above, PTAs that actually comprise (hard) human rights standards – and, consequently, countries that agree to include these terms in their PTAs – are unlikely to be a random sample. In fact, and according to our theory, tough or any kind of human rights standards are systematically included in PTAs due to certain kinds of country interests or domestic characteristics (see Hill 2010). Previous research dealt with this problem either through an instrumental variable approach or the use of selection estimators. However, Gilligan and Sergenti (2007) demonstrate that these purely parametric strategies are

[9] In order to address this mechanism, Hafner-Burton (2005: 617) additionally considers a variable on countries' inflows and outflows of foreign direct investment. We decided to drop this 'investment' variable due to three reasons. First, it theoretically addresses the same concerns as *Trade*, which we do include. Second, Hafner-Burton's variable is statistically insignificant throughout any model estimation, rendering it unlikely that this item will crucially affect our results. Finally, the World Bank Development Indicators as the source for the investment variable suffer a lot from missing values. In turn, this makes it difficult to consider this item for our methodological approach that requires *ex-ante* that missing values do not exist and we lack convincing grounds for imputing the missing information.

inaccurate in addressing non-random assignments, since they rely on unverifiable modeling assumptions and are generally not able to deal with the influence of other existent covariates. In turn, this may lead to the underestimation of the actual effect of human rights laws in PTAs and thus, the results are potentially biased.

Matching is a more effective solution to these problems as it corrects for the non-random assignment while controlling for the existence of confounding factors. More specifically, matching pre-processes the data to form quasi-experimental contrasts by sampling a subset of comparable cases from the overall pool of observations. The observations contained in this subset resemble each other as closely as possible, i.e., the differences due to confounding factors are reduced to a minimum. The only – and actually crucial – exception is that these 'most-similar' cases differ in whether they received the treatment or not. Given our empirical set-up, please note that we rely on two different treatments: *PTA Law* and *PTA Hard Law*. After the matching, we can estimate the effect of the treatment by analyzing the matched sample using parametric methods in order to control for any remaining imbalances (see Ho et al. 2007; Morgan and Winship 2007). Here, we use orderd logit models, and also cluster the standard errors by country to correct for the bias due to non-constant variances and for taking into account intra-group correlations.

**Empirical Findings**

In a first step, we employ genetic one-to-one matching with replacement (Diamond and Sekhon 2008; Sekhon 2007). Thus, we obtain a matched sample of 3,172 observations for any reference to human rights standards in PTAs and of 2,754 observations for a reference to hard human rights standards in PTAs due to the fact that our original data identified 1,586 (1,377) PTA country-years that did include any human rights standards (hard law human rights standards). Depending on the treatment, we used the following variables to match observations from the treatment group with those from the control group: *Trade* and *Polity2*

were used for that sample using *PTA Law* as the treatment; *Trade*, *Polity2*, and *Human Rights Ratification* were used for that sample using PTA Hard Law as the treatment. This set of variables proved to be optimal with regard to the overall achieved balance.

We refrained from matching on all explanatory variables due to two reasons. First, this would not avoid matched datasets with still significant imbalances. Second and in the words of Ho et al (2007: 216f), 'the theoretical literature emphasizes that including variables only weakly related to treatment assignments usually reduces bias more than it will increase variance, and so most believe that all available control variables should always be included. However, the theoretical literature has focused primarily on the case where the pool of potential control units is considerably larger than the set of treated units. Some researchers seem to have incorrectly generalized this advice to all datasets. If, as is often the case, the pool of potential control units is not much larger than the pool of treated units, then always including all available control variables is bad advice. Instead, the familiar econometric rules apply about the trade-off between the bias of excluding relevant variables and the inefficiency of including irrelevant ones: researchers should not include every pre-treatment covariate available.' Our approach, thus, corresponds to the general genetic algorithm used by Sekhon (2007: 12ff), which maximizes the smallest *p-value* for *T-Tests* in each iteration of the matching procedure.

Before and after we conducted the matching, we assessed the degree of distributional balance of our explanatory items between the treatment and the control group. Figures 1 and 2 depict our findings via two common balance statistics. With regard to the left panel in either figure, a standardized bias within [-0.25; 0.25] indicates that a variable is well balanced (Ho et al. 2007: 220). In terms of the second panel in either figure, we report the *p-values* of simple *T-Tests* (0.10 as threshold level) for identifying if real differences between the treatment and the control group do persist. Arguably, the distributions of most explanatory variables significantly differ between the treated and the control groups before we matched

observations. After the matching, however, our sample displays a substantially improved balance to the extent that we can hardly distinguish between observations in either group and the only real difference between observations actually is the treatment. More specifically, most standardized biases range within [0.25; -0.25] and the *p-values* are mostly well above the value of 0.1. Note, however, that these two balance statistics also show that differences between the treatment and the control group seem to carry on for *Human Rights Ratification* in our matched samples. We will address this point in the robustness section below.

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As indicated above, Ho et al. (2007: 211f) suggest using the same parametric estimator for the matched data one would have employed in the first place, i.e., before the matching. Due to the use of the matched sample, however, the importance of the functional form that is characteristic for any parametric estimator is significantly lowered, specification assumptions matter less, and the reliability of the results is more strongly given. Tables 3 and 4 summarize our results from the final models.

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Tables 3 and 4 in here

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In total, we estimate eight models to demonstrate that our findings are robust and take the censoring of the impact of PTAs' reference to (hard) human rights standards on political repression into account. We postulated that the impact of hard law references to human rights compliance in PTAs is likely to disappear as soon as we account for selection. Both *PTA Law* and *PTA Hard Law* are highly insignificant throughout Models 1-8. As demonstrated in

Tables 3 and 4, adding or excluding controls from the models does not alter this result. The confidence in the empirical support for our theory is further increased by Figure 3, where we calculated first differences for scoring any of the five values of *Political Repression* while increasing *PTA (Hard) Law* from 0 to 1 and holding all other variables at their respective means.

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Figure 3 essentially mirrors our findings from Tables 3 and 4, since neither *PTA Law* or *PTA Hard Law* exert any significant or substantial impact on either value of *Political Repression*. In fact, we observe increases or decreases in the predicted probability that are close to 0 and the 90% confidence intervals permanently cross that threshold as well. Hence, the impact of *PTA Law* or *PTA Hard Law* on states' levels of human rights compliance cannot be distinguished from 0 as soon as we take into account the underlying selection process. This contradicts Hafner-Burton (2005), who finds that hard law human rights standards improve a country's compliance, but is strongly in line with our theoretical rationale that controls for selection. Countries are aware of the 'shadow of the future' and, hence, already take into account what may happen at the succeeding enforcement stage when establishing a particular PTA. This implies that states agree on hard human rights standards in PTAs only if they have a general propensity to abide by human rights in the first place. If this general propensity does not exist, hard or any law references to human rights compliance are not included in PTAs, which in turn leads to the observed insignificance of either *PTA Law* or *PTA Hard Law*.

We also obtain interesting findings with regard to the control variables. As expected by the existent literature and found in various earlier studies, *Trade*, *GDP*, and the democracy measure all exert a substantial and significantly negative impact on a country's level of political repression. More specifically, when moving from the minimum toward the maximum

of *Trade*, the predicted probability of reaching the most repressive level of political terror decreases by about 26% on average across each category of *Political Repression* (Table 3; 18% for Table 4). When moving from the minimum toward the maximum of *GDP*, the predicted probability of reaching the most repressive level of political terror decreases by about 24% on average across each category of *Political Repression* (Table 3; 25% for Table 4). Finally, when moving from the minimum toward the maximum of *Polity2*, the predicted probability of reaching the most repressive level of political terror decreases by about 18.5% on average across each category of *Political Repression* (Table 3; 19% for Table 4). The findings obtained for *Population Density* and *Political Stability*, respectively, should be treated with caution, however, as they seem to depend on model specifications.

Most surprisingly, *Human Rights Ratification* is constantly positive throughout our models and highly significant. When moving from the minimum toward the maximum of this variable, the predicted probability of reaching the most repressive level of political terror increases by about 12% on average across each category of *Political Repression* (Table 3; 13% for Table 4). In other words, if a country ratified either the *International Covenant on Civil and Political Rights* or the *Convention against Torture* (or both), we observe a substantial increase in the abuse, repression, or torture in that country. Although this is contrary to many expectations, it does support other studies suggesting that international treaties that lack enforcement mechanisms do not notably influence the course of states' action significantly (e.g., Drezner 2003; Goodman and Jinks 2004; Downs et al. 1996; Eaton and Engers 1999). Furthermore, the fact that *Human Rights Ratification* is positively signed points to a free-riding problematic: Both the *International Covenant on Civil and Political Rights* and the *Convention against Torture* are global treaties, open to ratification to any state. Thus, we face a relatively large group of countries. Within this context, enforcement, i.e., instruments of monitoring and sanctioning states' behavior, is relatively hard to organize and also very costly. Hence states can participate in either treaty without actually doing much

against human rights abuses at home. Furthermore, since joining any of the two agreements might yield a reputation as a 'good state', monitoring and sanctioning would be essential in order to prevent states from free-riding (Fearon 1998: 270). Therefore, countries' ratification of human rights treaties merely seems to be a simple attempt to achieve a favorable reputation, while post-ratification compliance is hardly given afterwards (see also Hollyer and Rosendorff 2011).

**Robustness**

To test the robustness of our findings, we also employed other econometric instruments that are not reported here.[10] First, Clarke (2005) shows that the inclusion of control variables may actually increase the bias instead of decreasing it. Similarly, the balance statistics from above indicate that we were unable to match the samples perfectly for *Human Rights Ratification*. However, Tables 3 and 4 demonstrate that making amendments in this regard, i.e., dropping all controls or only *Human Rights Ratification,* does not affect the substance of our findings.

Second, we replaced the dependent variable with a) Hafner-Burton's (2005) civil liberty item and b) a variable that exclusively rests on information from Amnesty International, i.e., the values here have not been combined with data from the U.S. State Department. These robustness tests, however, did not differ significantly from our main results reported here.

Third, the dependent variable's fifth category, which refers to a level of terror that has expanded to the whole population, has only 259 observations in the original sample, meaning that this value could be an outlier category that biases our results. In order to address this, we recoded *Political Repression* by grouping the fourth and fifth category together. Similarly, we also considered all models using a less stratified dependent variable by employing a dichotomous item for *Political Repression* with the value of 0 combining categories 1–2 and

[10] Due to space limitations, most of the robustness checks are not listed in the paper, but are available from the authors upon request.

the value of 1 combining categories 3-5 of the original variable. Both changes did not alter our core results.

Finally, although *Political Stability* should address temporal dependencies due to its operationalization as a yearly count item, some of these dependencies might persist. We therefore estimated all models with a torture-years variable (i.e., time in years elapsed since a country scored the value 3 or higher on *Political Repression*) and different sets of cubic splines (Beck, Katz, and Tucker 1998). However, and as demonstrated in Models 4 and 8, our core finding essentially stays the same.

**Conclusion**

This article has sought to expand our understanding of the impact of PTAs on countries' level of political repression and compliance with human rights standards. While the recent literature suggests that PTAs can substantial decrease human rights abuses when they include hard human rights standards, it has tended to treat PTAs, their impact, and their surrounding influences in an undifferentiated or truncated manner. We therefore developed a theoretical model that considers PTAs in a selection process: countries agree on including hard law human rights standards in PTAs only if they intend to comply with these afterwards anyway, i.e., if there is a general tendency to abide by human rights in the first place.

Our findings suggest that PTAs – regardless of what kind of legalization level they employ for demanding human rights compliance – do not have any impact on countries' level of political repression. In addition, countries seem to systematically abuse other forms of human rights law such as the *International Covenant on Civil and Political Rights* and the *Convention against Torture* for free-riding. Therefore, our research implies that the policy community has to think carefully about what kind of treaties and institutions it establishes, and with whom, in order to reach most effective outcomes as PTAs do not seem to be helpful in this context.

Against this background, although this analysis here demonstrates a great deal of empirical support for our theoretical model developed in the paper, other important questions remain. For example, although our results show that PTAs and other human rights treaties do not significantly lower countries' human rights abuses, we did not explicitly examine the treaty characteristics of the latter. Future research has to unveil their effects. We should also refrain from concluding that the treaties in place today are more or less futile. As the work of Simmons (2009) shows, it may not be the treaty itself that enforces standards but rather that domestic groups use the leverage provided by a treaty to pressure their governments for enforcement. In this view, 'stakeholders and their allies use treaties to strengthen their rights claims [… as] treaties assist in the process of political mobilization of groups who stand to gain from their provisions' (Simmons 2009: 357).

**Table 1**. Political Repression Worldwide, 1977-2009 (Original Sample before Matching)

| *Value* | *Description* | *Frequency* | *Percent* |
|---|---|---|---|
| *1* | Countries under a secure rule of law, people are not imprisoned for their view, and torture is rare or exceptional. Political murders are extremely rare. | 721 | 17.51 |
| *2* | There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beatings are exceptional. Political murder is rare. | 1,211 | 29.41 |
| *3* | There is extensive political imprisonment, or a recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without a trial, for political views is accepted. | 1,294 | 31.43 |
| *4* | Civil and political rights violations have expanded to large numbers of the population. Murders, disappearances, and torture are a common part of life. In spite of its generality, on this level terror affects those who interest themselves in politics or ideas. | 632 | 15.35 |
| *5* | Terror has expanded to the whole population. The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. | 259 | 6.29 |
| *Total* | | 4,117 | 100.00 |

**Table 2**. Descriptive Statistics, 1977-2009 (Original Sample before Matching)

| | *N* | *Min* | *Std.Dev.* | *Min* | *Max* |
|---|---|---|---|---|---|
| *Repression* | 4,117 | 2.635 | 1.126 | 1 | 5 |
| *PTA Law* | 4,117 | 0.381 | 0.486 | 0 | 1 |
| *PTA Hard Law* | 4,117 | 0.334 | 0.472 | 0 | 1 |
| *Human Rights Ratification* | 4,117 | 1.136 | 0.826 | 0 | 2 |
| *Population Density* | 4,117 | 109.088 | 309.067 | 1.322 | 6,913.429 |
| *Political Stability* | 4,117 | 23.986 | 30.348 | 0 | 199 |
| *Polity2* | 4,117 | 1.897 | 7.306 | -10 | 10 |
| *GDP* | 4,117 | 7.465 | 1.583 | 4.390 | 10.749 |
| *Trade* | 4,117 | 4.157 | 0.552 | 1.844 | 6.082 |

**Table 3**. The Impact of Human Rights in PTAs on Human Rights Compliance, 1977-2009

| | *Model 1* | *Model 2* | *Model 3* | *Model 4* |
|---|---|---|---|---|
| *PTA Law* | 0.119<br>(0.235) | -0.069<br>(0.208) | 0.147<br>(0.209) | 0.057<br>(0.190) |
| *Human Rights Ratification* | | 0.663<br>(0.148)*** | | 0.586<br>(0.137)*** |
| *Population Density* | | 0.001<br>(0.000)*** | 0.001<br>(0.000)** | 0.001<br>(0.000)** |
| *Political Stability* | | -0.010<br>(0.007) | -0.010<br>(0.007) | -0.008<br>(0.008) |
| *Polity2* | | -0.115<br>(0.017)*** | -0.087<br>(0.016)*** | -0.079<br>(0.016)*** |
| *GDP* | | -0.444<br>(0.107)*** | -0.428<br>(0.111)*** | -0.277<br>(0.097)*** |
| *Trade* | | -1.009<br>(0.215)*** | -0.900<br>(0.216)*** | -0.826<br>(0.199)*** |
| *Years since Torture* | | | | -0.448<br>(0.041)*** |
| *Spline 1* | | | | 0.001<br>(0.000)*** |
| *Spline 2* | | | | (omitted) |
| *Spline 3* | | | | -0.002<br>(0.000)*** |
| *N* | 3,172 | 3,172 | 3,172 | 3,172 |
| *Log Pseudolikelihood* | -4,694.442 | -3,869.324 | -3,953.933 | -3,504.877 |
| *Wald* $\chi^2$ | 0.26 | 113.59*** | 110.37*** | 452.07*** |

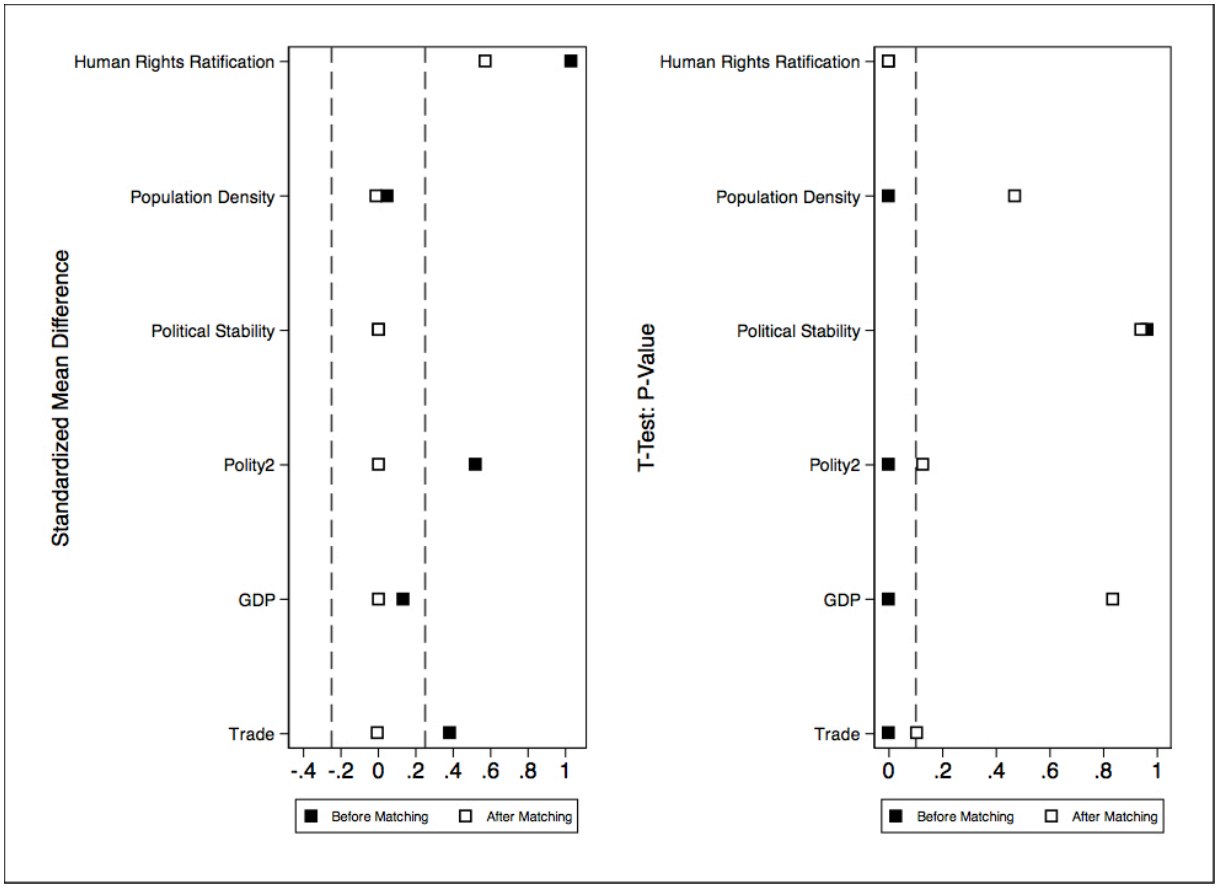
**Note**: table entries are coefficients; robust standard errors clustered on country in parentheses; * significant at 10%; ** significant at 5%; significant at 1% (two-tailed).

**Table 4**. The Impact of Hard Human Rights in PTAs on Human Rights Compliance, 1977-2009

| | *Model 5* | *Model 6* | *Model 7* | *Model 8* |
|---|---|---|---|---|
| *PTA Hard Law* | -0.058<br>(0.250) | -0.123<br>(0.214) | -0.041<br>(0.212) | 0.191<br>(0.195) |
| *Human Rights Ratification* | | 0.734<br>(0.156)*** | | 0.672<br>(0.160)*** |
| *Population Density* | | 0.001<br>(0.001) | 0.001<br>(0.001) | 0.001<br>(0.001) |
| *Political Stability* | | -0.013<br>(0.006)** | -0.013<br>(0.006)** | -0.010<br>(0.006)* |
| *Polity2* | | -0.124<br>(0.016)*** | -0.088<br>(0.016)*** | -0.086<br>(0.016)*** |
| *GDP* | | -0.412<br>(0.111)*** | -0.461<br>(0.115)*** | -0.289<br>(0.101)*** |
| *Trade* | | -0.761<br>(0.236)*** | -0.661<br>(0.238)*** | -0.675<br>(0.220)*** |
| *Years since Torture* | | | | -0.425<br>(0.040)*** |
| *Spline 1* | | | | 0.001<br>(0.000)*** |
| *Spline 2* | | | | (omitted) |
| *Spline 3* | | | | -0.002<br>(0.000)*** |
| *N* | 2,754 | 2,754 | 2,754 | 2,754 |
| *Log Pseudolikelihood* | -4,094.484 | -3,328.300 | -3,408.573 | -3,058.422 |
| *Wald* $\chi^2$ | 0.05 | 160.41*** | 102.05*** | 468.19*** |

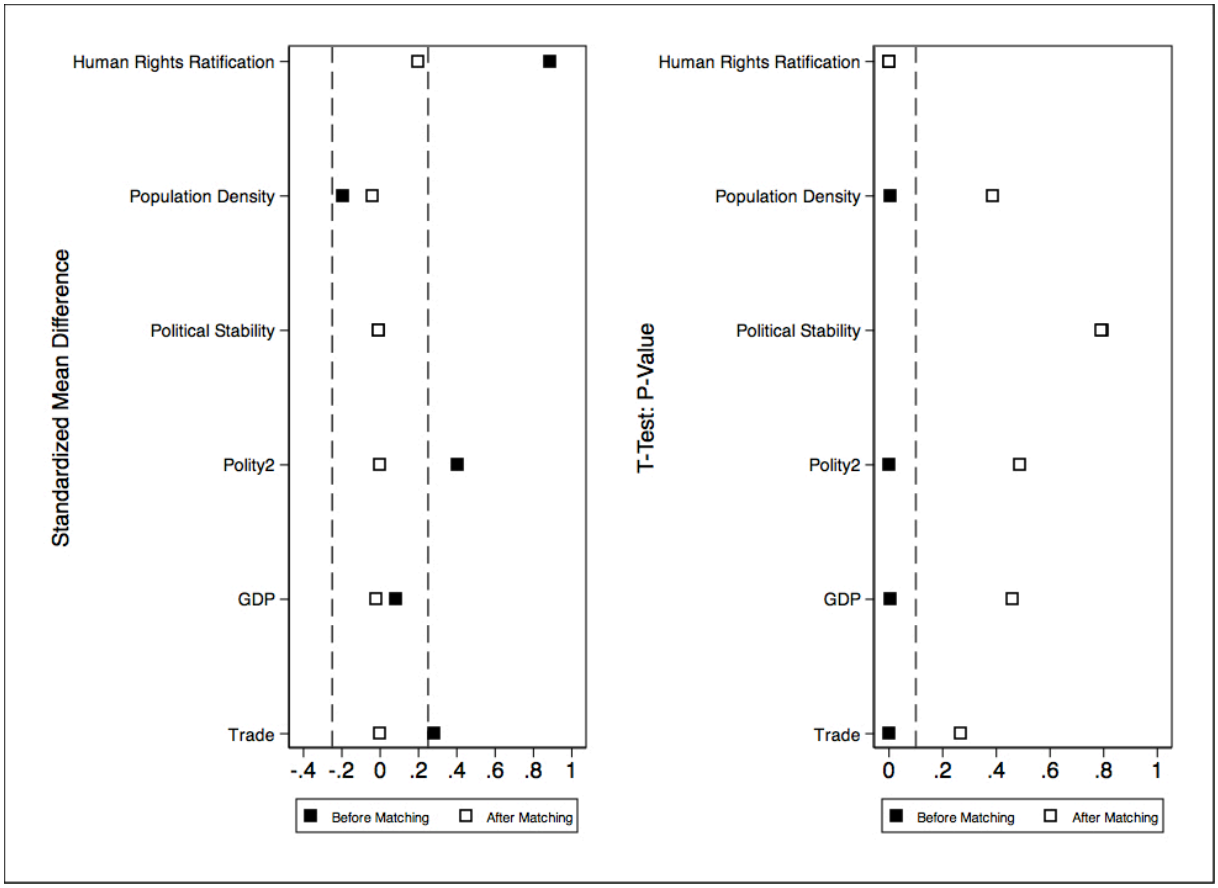
**Note**: table entries are coefficients; robust standard errors clustered on country in parentheses; * significant at 10%; ** significant at 5%; significant at 1% (two-tailed).

**Figure 1**. Matching: Balance Statistics – *PTA Law*



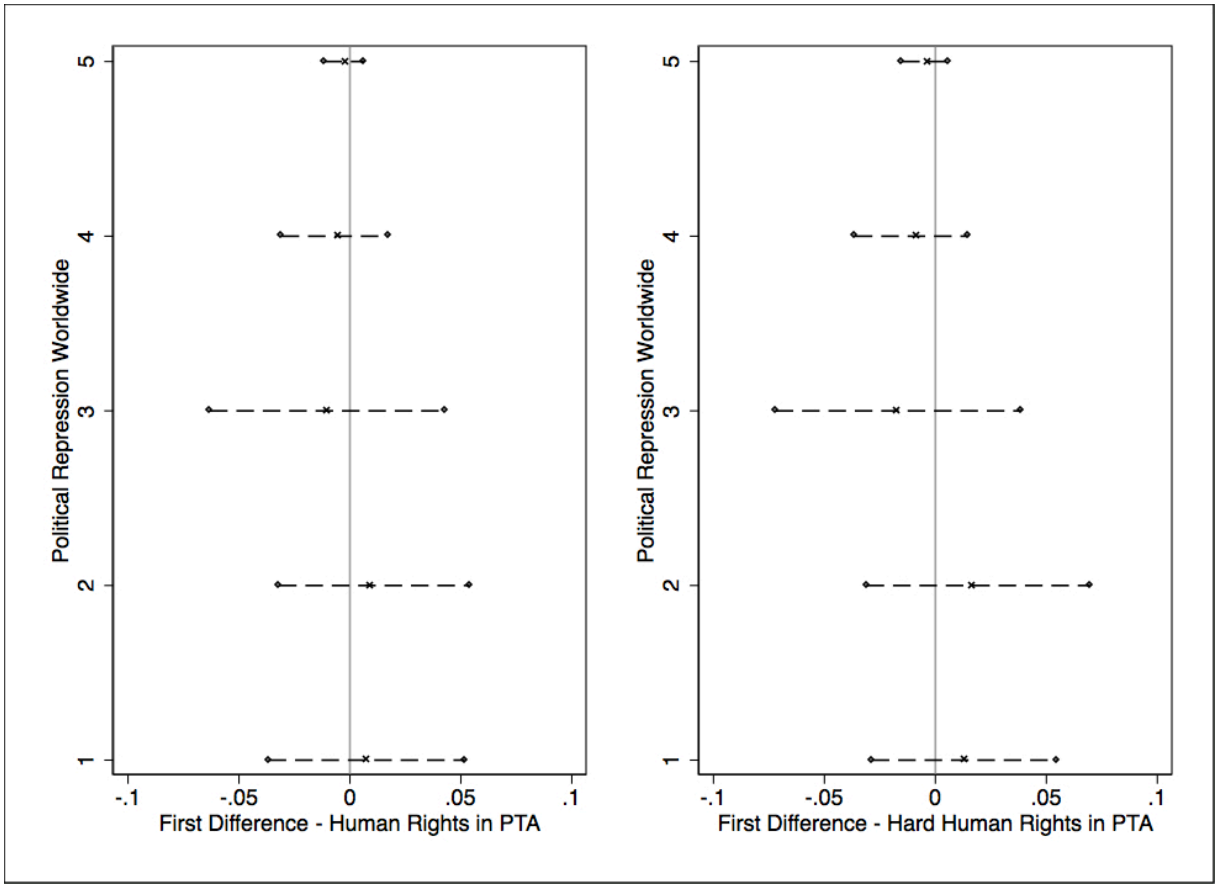

**Note**: balance statistics refer to *PTA Law* as treatment variable; dashed lines mark specific threshold levels (or intervals) for respective balance statistic; balance statistics before matching might be covered by balance statistics after matching.

**Figure 2**. Matching: Balance Statistics – *PTA Hard Law*




**Note**: balance statistics refer to *PTA Law* as treatment variable; dashed lines mark specific threshold levels (or intervals) for respective balance statistic; balance statistics before matching might be covered by balance statistics after matching.

**Figure 3**. First Difference Estimates




**Note**: *x* signifies estimate of first difference; dashed line signifies 90% confidence interval; solid line marks 0-threshold; left panel pertains to *PTA Law* as treatment variable while estimates are based on Model 2; right panel pertains *to PTA Hard Law* as treatment variable while estimates are based on Model 6.

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