Before ratification: understanding the timing of international treaty effects on domestic policies

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Abstract

When do international treaties cause domestic policy adjustments? While previous research emphasizes the consequences of treaty ratification, we argue that the need to secure entry into force can induce states to change their policies already before ratification. If a state expects benefits from a treaty, it can increase the probability of foreign ratification by implementing policies that benefit pivotal domestic players within its partner country. Accordingly, studies that focus on policy change after ratification underestimate the importance of treaties and partly misconstrue the causal connection between treaties and policies. We test the theory against data on the relationship between North-South preferential trading agreements and automobile emission standards, finding that developing countries adopt automobile emission standards between the signature and ratification of North-South preferential trading agreements.

1 Introduction

When and how do international treaties influence state behavior? While the extant literature has investigated how treaty ratification affects domestic policy (Chayes and Chayes, 1995; Downs, Rocke, and Barsoom, 1996; Hafner-Burton, 2005; Simmons, 2009; Von Stein, 2005), the exact timing of treaty effects on domestic policy has, with some exceptions (Deere and Esty, 2002; Esty, 1994; Hufbauer et al., 2000), drawn less attention. When should we expect treaties to shape domestic policy upon their entry into force, and when should treaties affect policy already prior to their ratification? Answering these questions is important for the study of treaty effects because if treaties have anticipatory effects, an exclusive focus on treaty effects following ratification may give an incomplete understanding of their importance.

We explore the possibility that treaties influence state behavior already prior to ratification. We argue that if a state expects reputational (Guzman, 2008) and/or material (Keohane, 1984) gains from a given treaty, it may implement policies that increase the likelihood that a foreign country's legislature ratifies the treaty. Treaties affect signatories' policy formation already *between* the negotiation period and actual entry into force, so studies focusing on events following entry into force underestimate the importance of international treaties and partly misconstrue the causal mechanisms that produce such effects. For example, an agreement to protect intellectual property rights can be effective even if domestic policy in member states does not change after ratification.

We test the theory by analyzing whether North-South preferential trading agreements (PTAs) induce developing countries to adopt automobile emission standards. These standards are ideal for empirical analysis because their focus on product characteristics means that enforcement problems are mitigated. While previous studies have recognized the relationship between trade agreements and environmental policy (Esty, 1994; Gallagher, 2004; Hufbauer et al., 2000), these studies focus mostly on the United States and do not offer a general theory of exactly when treaties can be expected to effectuate policy changes. However, domestic NGOs in industrialized countries have raised concerns about the environmental impacts of free trade agreements (Bechtel and Tosun, 2009; Raustiala, 1997) while import-competing businesses and labor unions have worried that lax regulatory standards in developing countries give these countries an unfair competitive advantage

(Bernauer and Caduff, 2004; Desombre, 1995; Vogel, 1995).

We hypothesize that developing countries have incentives to improve their environmental policies to facilitate the ratification of PTAs in wealthy industrialized countries, and thus secure access to large markets for their exports. If our theory is valid, we should see developing countries implementing environmental policies between PTA negotiations and ratification. To evaluate the theory, we collected data on negotiations of all North-South PTAs formed during the 1990-2007 period. Our statistical analyses of the data show that, indeed, developing countries are unusually likely to implement automobile emission standards after the negotiations but still before ratification. If a developing country has negotiated a PTA, but this PTA has yet to enter into force, the likelihood of new automobile emission standards is higher than at any other time.

We do not find evidence for treaty effects on automobile emission standards during negotiations or after ratification. If we replicate our analysis focusing on the negotiation period, we also find no effect. Similarly, there is no effect following entry into force. These results do not mean that treaties have no effects after ratification. But, they do suggest that treaties can also have effects before ratification, especially on policies that play an important role in the ratification process.

This article offers two broad contributions to international relations theory. First, we draw attention to an overlooked possibility causal mechanism through which treaties affect state behavior. In particular, the findings serve as a cautionary note against a narrow legalistic view of compliance, thus inviting scholars to consider more complex causal pathways to treaty effectiveness. Considering the wide variety of mechanisms through which treaties can influence state behavior can improve the study of international treaty compliance.

Second, we contribute to the burgeoning literature on regulatory convergence in an interdependent world. A heated debate has emerged on the environmental consequences of international trade institutions, with pessimists proclaiming a race to the bottom (Porter, 1999) and optimists highlighting the possibility of benevolent upward harmonization (Vogel, 1995). We provide evidence for the beneficial effects of international trade institutions on environmental policy, at least in the case of product standards. Future research should scrutinize the generalizability of our arguments to other policies, such as environmental process standards and various policies outside the

environmental realm.

2 Treaties and Policy: State of the Art

To set the stage for our theory, we review both the general literature on treaty effects and the particular case of economic integration and environmental policy.

2.1 Treaty Effects

The literature on treaties is characterized by a debate between optimists and pessimists. Optimists argue that states form treaties because they have a genuine interest in solving cooperation problems (Chayes and Chayes, 1995; Tallberg, 2002). According to this "managerialist" view, states usually do their best to comply with the treaties they have ratified. In this telling, treaties are inconsequential when they present unclear rules or states lack the capacity to comply with their obligations. In contrast, willful defections are rarely the reason for non-compliance.

Pessimists warn that such compliance results from screening, instead of treaty constraints on state behavior, as states design and ratify treaties if they expect compliance to be easy and convenient (Downs, Rocke, and Barsoom, 1996). According to this view, states comply with treaties because few costly adjustments are required. In a statistical sense, much of the perceived compliance with treaty obligations would result from "selection bias" (Von Stein, 2005).

The empirical evidence for both claims is decidedly mixed. Some scholars, such as Von Stein (2005), have found evidence for a selection bias, whereas others report that treaties often influence behavior, even if the actual enforcement mechanisms are not necessarily international but instead domestic (Dai, 2005; Simmons, 2009). Yet others emphasize that treaty compliance is also contingent on domestic politics in member states, such as the partisan preferences of the government (Grieco, Gelpi, and Warren, 2009). In a recent review of treaty compliance, Simmons (2010) notes that while the evidence seems to indicate that treaties have effects on state behavior, large uncertainties remain and the conditions under which they can be effective are poorly understood. This article contributes to this debate by drawing attention to previously unrecognized mechanisms of treaty effectiveness.

An important problem with the literature on treaty effects is that it focuses almost exclusively on events following ratification. If treaties influence behavior already prior to ratification, then results from analyses focusing on the consequences of ratification are biased. Specifically, this omission causes three biases. First, the effectiveness of treaties in general is underestimated because some possible causal channels are ignored.[1] Second, the effect of treaty ratification itself is possibly overestimated because those treaties that influence state behavior prior to ratification are wrongly characterized as ineffective. Finally, there is a bias against treaties that, for whatever reason, tend to influence state behavior early, already prior to ratification.

In some cases, such as accession to the European Union, the pre-accession effects are readily observable: membership candidates enact reforms to meet the formal conditions for initiating negotiations or accession itself (Mattli and Plümper, 2004). The eastward expansion of NATO also hints at prior conditions that must be met already prior to the actual act of accession (Kydd, 2001). But does this pre-accession effect generalize beyond the somewhat unusual cases of EU and NATO enlargement in the post-communist area? Is the effect limited to accession to international organizations, or does it apply more broadly?

Previous research offers few insights into these questions. Beyond the specific case of EU negotiations (Mattli and Plümper, 2004), few empirical studies focus on the effect of treaties prior to ratification. Instead, scholarship has focused on the problem of securing ratification. According to models of two-level games, executives strategically negotiate treaties that fall within the "win set" of the domestic legislature (Putnam, 1988; Milner, 1997). These models indicate that some treaties may fail due to the ratification constraint, but they are based on the premise that domestic policies remain unchanged until ratification.

A partial exception is Raustiala (1997) who argues that environmental NGOs can help governments ratify treaties that they have previously negotiated. To the extent that such NGOs help the government implement policy packages, they may cause the state to initiate policy changes already prior to ratification, perhaps as part of a package deal. Similarly, states may implement policies to mobilize NGOs to support a treaty. However, Raustiala (1997) does not provide a causal claim

[1] If the pre-ratification effect were *negative*, the effect would not be underestimated. However, there are neither theoretical nor empirical reasons to expect a negative effect.

regarding the conditions under which environmental NGOs can effect policy change. Nor does he provide a systematic empirical test of his argument.

2.2 Economic Integration and Environmental Policy

Since we examine the effect of economic integration on domestic environmental policy, this segment of the literature warrants a more detailed review. We are not the first to argue that international institutions in general, and trade agreements in particular, can produce environmental policy changes even before the actual ratification. In previous studies, the case of NAFTA is commonly cited. Esty (1994) claims that improving environmental standards included into the treaty was crucial for the NAFTA being ratified by the United States Congress. More generally, Deere and Esty (2002, 5) argue that "in the United States and in Canada advances on the trade policy front cannot be achieved without attention to related environmental issues."

This argument is echoed in other studies. For instance, Hufbauer et al. (2000, 2) argue that "'the greening' of the NAFTA produced notable results when the talks were concluded in August 1992." Similarly, Gallagher (2004, 2) notes that "in the lead-up to NAFTA, Mexico doubled spending on environmental protection and started a much-needed industrial environmental inspection program." Furthermore, some studies explore the impact of trade agreements on environmental policy changes in countries other than Mexico. Silva (1996) argues that the possible extension of NAFTA to Chile has encouraged the country to strengthen its domestic environmental standards in the 1990s, even as this is not the main focus of the study. Similarly, Vogel (2000, 270) claims "the prospect of membership in the European Union has provided an important incentive for a number of central European nations to bring their production as well as product standards into conformity with those of the EU."

While useful, these studies suffer from two shortcomings. First, although the presence of anticipatory effects of trade agreements on environmental policy is suggested, the existing studies fail to systematically analyze these anticipatory effects and to empirically test their presence against data on a large number of trade agreements. Second, in highlighting a few successful cases, the existing literature is generally skeptical about the ability of trade agreements to improve environ-

mental standards. For instance, Deere and Esty (2002, 6) claim that "at the present the FTAA process fails to meet these basic requirements" of improving environmental standards. Similarly, Vogel (2000) argues that with the exception of the EU, which improves environmental standards among its members, the impact of other trade agreements on improving regulatory standards has been modest so far.

Our study differs from the extant studies in three important respects. First, previous studies do not distinguish between the different phases of treaty formation. Indeed, the aforementioned studies do not distinguish between negotiation and ratification periods. Thus, even if anticipator effects are present, their exact timing remains subject to uncertainty. By distinguish between negotiation, ratification, and entry into force, our theory pins down exactly when reforms can be expected to occur.

Second, existing empirical studies operationalize environmental standards by looking at expenditures. However, expenditure is but one piece of the puzzle. Gallagher (2004, 2) notes that "shortly after NAFTA was signed and fiscal and financial woes set in, attention to the environment nose-dived. According to INEGI, since 1994 real spending on environmental protection declined by the equivalent of 200 million dollars, or 45%." Aside from enforcement concerns, we focus on more lasting, and thus more difficult to reverse, policy changes.

Third, the focus of previous studies is mostly on the United States. This limits the generalizability of the finding to other North-South PTAs. For instance, Hufbauer et al. (2000) stress the importance of the newly elected president Bill Clinton in improving environmental and labor standards included into the NAFTA treaty. As Hufbauer et al. (2000, 2) puts it, "he [Clinton] pledged not to implement NAFTA until a supplemental agreement has been concluded requiring each party to enforce its own environmental standards ... The North American Agreement on Environmental Cooperation (NAAEC) that was concluded in August 1993 augmented the NAFTA's environmental provisions and dispute settlement procedures, making the greenest trade accord even greener." In contrast, our argument applies to the entire universe of North-South PTAs.

3 Before Ratification: The Argument

Our theory focuses on the effects of treaties on state behavior prior to ratification. We describe the logic behind the possibility that treaties influence state behavior even if they have yet to enter into force, and especially during the ratification period. Testable hypotheses follow.

We do not focus on any particular issue area. However, the empirical analysis is limited to the relationship between PTAs and environmental policy. Throughout this section, we use regulatory policies in the context of trade cooperation as illustrative examples. In this and the following sections, we also discuss the generalizability and idiosyncracies of the environmental realm.[2]

In our theory, two or more states are negotiating a treaty that is supposed to constrain state behavior and require costly policy adjustments (Keohane, 1984). Both states expect some benefits from forming the treaty. However, the states are also worried about possible cooperation failure. Thus, each state must convince the other of its benevolent intentions and willingness to implement cooperative policies (Kydd, 2005). Without such reassurance, the foreign cooperation partner may refuse to participate in the cooperative effort.[3]

The situation may be symmetric or asymmetric. In the case of trade, most of the concerns pertain to the developing country's policies. When the two sides are highly asymmetric, the weaker side in particular may have to adjust its policies to secure cooperation with the more powerful country (Bechtel and Tosun, 2009; Drezner, 2007; Shadlen, 2005). In other cases, however, concerns about relative gains or defection at the implementation stage are mutual. For example, in arms control both sides may worry that the other will secretly invest in arms (Kydd, 2000). The key implication of symmetry is that both sides have incentives to change their policy to reassure the other.

[2]Treaties can influence state behavior in many ways. We do not argue that the causal pathway identified here is the exclusive, or even the primary, one. Instead, our purpose is to develop a theoretical framework for analyzing this particular and important causal pathway, and then provide evidence that it operates in the highly salient case of the trade-environment nexus. By doing so, we lay a foundation for future comparative studies on the importance of treaty effects on different policies before and after entry into force.

[3]These characteristics of the "situation structure" (Mitchell and Keilbach, 2001) can be thought of as analytical scope conditions. If they are met, our theoretical argument applies; otherwise alternative theoretical frameworks are needed. For example, our theoretical argument should not be applied to purely symbolic or expressive treaties or declarations, nor should it be used if states face a simple coordination problem that does not present any concerns regarding willful defections or non-compliant behavior more generally (Stein, 1982).

In the case of trade and regulatory policy, the issue of national competitiveness nicely illustrates concerns about policy implementation. Industrialized countries benefit from trade liberalization with developing countries through lower consumer prices and expanded markets, but they may worry that if such developing countries do not impose adequate regulations, such as environmental and labor standards, their industries obtain undue competitive advantages vis-á-vis their Northern competitors (Bechtel and Tosun, 2009; Bernauer and Caduff, 2004; Neumayer, 2001). Such concerns provoke domestic opposition to free trade, because the import competitors and the general public perceive lax regulatory standards in the developing country as "unfair" in the context of free trade. In such cases, a developing country may have to demonstrate its willingness and ability to protect the environment to secure market access (Prakash and Potoski, 2006). One way for a developing country to demonstrate such willingness and ability is to impose and enforce new regulations.[4]

In the treaty formation process, each state faces two strategic decisions. First, should it adjust policies already prior to treaty ratification? Such prior policy adjustment is costly and may not produce benefits if the treaty does not enter into force. Second, after each state observes the other party's policy adjustment decision, should it ratify the treaty? In deciding on ratification, the state uses information from prior policy adjustments to gauge the trustworthiness of the other side.

The *timing* of such policy adjustments plays a central role in our theory. If a state is to increase the probability of cooperation, should it implement favorable and informative policy adjustments during the negotiations, between signature and ratification, or only after ratification? We argue that the value of such signaling is maximized in the ratification stage. If states are concerned about the credibility of promises to implement new policies, they may withhold ratification until they are convinced that the foreign partner has implemented the policies in focus. By implementing salient policy adjustments during the ratification period, a state can influence the ratification debate in the foreign state to its favor. As previous reseach on economic integration and environmental policy suggests, for example, environmental issues have been a roadblock to trade treaties in the United

[4]Survey studies also suggest that people in industrialized countries see a causal connection between trade liberalization and environmental degradation. For example, Bechtel, Bernauer, and Meyer (2011) provide survey evidence from Switzerland, finding that people who worry about environmental problems also have more negative views of trade, and economic globalization more generally. If governments of industrialized countries are to form trade agreements with developing countries, they can try to appease environmentally aware voters by insisting on the prospective partner's implementing more stringent environmental regulations.

States and Canada (Deere and Esty, 2002). Worried about the lack of adequate environmental standards in their partner countries, American and Canadian legislators have at times demanded policy changes before the ratification of a trade treaty, such as NAFTA.

Additionally, the ratification stage gives domestic groups unusual leverage, and thus the importance of reassuring them is also maximized (Milner, 1997; Raustiala, 1997). The executive of a state must play a "two-level game" whereby the successful conclusion of treaty negotiations is followed by a ratification stage. At this stage, the executive's ability to influence outcomes is limited because the legislature holds the ultimate authority to accept or reject the treaty in focus. If the median legislator is less inclined toward cooperation than the executive, the ratification stage is a important obstacle to cooperation than the negotiation stage. Instances of "involuntary defection" – ratification failure in spite of an executive interested in cooperation – are common in international cooperation (Evans, Jacobson, and Putnam, 1993).

Strategically, a state's policy adjustments at the ratification stage allow the other side to update its beliefs regarding the state's trustworthiness. If a state is considered unreliable by the other side, the state can send a signal of trustworthiness by implementing costly, and potentially irreversible, policies. If it does not send this signal, the other side learns that the state is of a type that is not willing to invest in cooperation. For example, a developing country could implement environmental policies to convince an industrialized country that it is willing to forgo the competitive advantage from lax regulatory policies. If strong enough, the signal reassures the industrialized country, and domestic constituencies within it, that the developing country will not attempt to gain an unfair competitive advantage through lax or unenforced environmental policies.

The alternatives are often less attractive. Policy adjustments following ratification can certainly be useful if non-compliance is costly, either for reputational reasons (Guzman, 2008) or due to sanctions (Downs, Rocke, and Barsoom, 1996), but they do not help secure ratification. If the foreign partner worries that a state's willingness to immplement policies is limited, the treaty does not enter into force, and thus a compliance pull that would allow meaningful compliance never materializes. If the legislature of a foreign country does not believe the state in focus is trustworthy, empty promises hardly constitute a credible signal regarding the state's real intentions. For example,

it is not surprising that American Congressmen would not believe Mexico's promises to implement environmental policies following NAFTA's entry into force, instead requiring evidence of regulatory change during the ratification period.

At the even earlier negotiation stage, uncertainty may make it difficult for countries to facilitate ultimate treaty implementation. If a state is not yet sure what the content of the treaty is, how can it implement policy adjustments that increase the probability of ratification? Similarly, how can a state increase the probability of ratification without seeing exactly what is being debated at the ratification stage? Prior to treaty signature, the treaty in focus is often not very significant for domestic constituencies in the partner country – after all, the government is responsible for the negotiations (Putnam, 1988). Only in cases where the negotiation process explicitly requires policy changes, as is true of EU accession (Mattli and Plümper, 2004), would one expect policy changes already before signature.

Accordingly, the ratification stage should maximize the incentive to implement policies in an effort to secure the ratification of a previously negotiated treaty.

Hypothesis 1 (effect between signature and ratification). *If a state has negotiated a treaty, but the foreign partner's ratification is pending, the state is more likely to implement policies that accord with the provisions of the treaty and the foreign partner's preferences than at other times.*

4 Alternative Causal Mechanisms

Treaties could also influence state behavior in during and after negotiations. For completeness, we summarize the arguments for these alternatives in brief, although these arguments do not have similarly strategic foundations as our theory. These causal pathways are not mutually exclusive, but ignoring one at the expense of another may result in biased expectations. For example, a treaty that has dramatic effects already during the negotiation stage may have less dramatic effects during the ratification or implementation stages.

To begin with, we consider the possibility of implementation upon entry into force. Treaties often impose legally binding obligations on states (Goldstein et al., 2000), so they cannot operate

unless the members implement the requisite policies. Accordingly, countries could simply agree to implement policies upon ratification. If states are expected to honor their commitments, there is no need to implement policies prior to ratification.

Hypothesis 2 (ratification effect). *If a state has ratified a treaty, it is more likely to implement policies that accord with the provisions of the treaty and the foreign partner's preferences than at other times.*

Another possibility pertains to the negotiation period. A country might implement policies during the negotiations, instead of waiting for signature, if the primary audience of these signals is the executive of the partner country. In this case, it is hardly reasonable to wait until the period between signature and ratification. The act of signature itself depends on reassurance, and so the time to signal willingness to cooperate is already during the negotiations (Kydd, 2005).

Hypothesis 3 (negotiation effect). *Suppose a state is negotiating a treaty. At this time, the state is more likely to implement policies that it believes would accord with the provisions of the treaty and the foreign partner's preferences than (i) before the negotiations began and (ii) after the negotiations were finished.*

Note a subtle, though important, logical difference between this and the previous two hypotheses. During the negotiation stage, the state enacting policies obviously cannot be sure what the future treaty would contain, and thus what would be needed to enhance credibility in the eyes of the foreign partner. Thus, beliefs are critical.

5 Ratification of Trade Treaties and Environmental Policy: Research Design

To test our theory, we examine the adoption of automobile emission standards in developing countries between the signature and ratification of a North-South PTA. This empirical application is appropriate given our theoretical argument for several reasons. First, automobile emission standards are an important and costly policy adjustment. Urban air pollution is a major cause of premature deaths and serious health problems both in industrialized and developing countries (Dasgupta et al., 2006), and the transportation sector is a major source of a particularly lethal form of pollution, suspended particulate matter (Mayer, 1999). If a government imposes an automobile emission standard, both domestic and foreign producers must change the design of the vehicles they sell in these markets (Vogel, 1995). In particular, they must install advanced catalytic converters that reduce the amount of air pollution that each gallon of gasoline emits consumed in the engine. This is particularly costly for developing countries because their producers often continue to produce automobiles that do not contain such catalytic converters. Additionally, they must obtain costly licenses from Northern automobile producers with advanced technology, such as Toyota or Volkswagen (Gallagher, 2006).

Second, PTAs are among the most important economic agreements that developing countries can form with industrialized countries. Not only do they enhance market access to lucrative markets in very wealthy countries (Mansfield and Milner, 1999; Whalley, 2008), but recent research also indicates that they can increase foreign direct investment (Büthe and Milner, 2008; Manger, 2009). Thus, developing countries have very strong economic incentives to implement environmental policies that speed up the entry into force of a North-South PTA.

Finally, a natural "substantive issue linkage" (Oye, 1992; Haas, 1980) exists between automobile emission standards and PTAs. Automobiles are an economic sector for which international trade is important, and automobile emission standards have powerful effects on the competitiveness of producers on different steps of the technological ladder (Gallagher, 2006; Thun, 2006). Increasingly stringent automobile emission standards in developing countries favor producers in industrialized

countries, and thus present a dilemma for domestic producers: if foreign producers hold a technological advantage, how can the domestic producers maintain their market share at an acceptable adjustment cost?

Automobile emission standards are also important for the trade-environment nexus that has an important role to play in controversial surrounding free trade agreements (Esty, 2001). Much of the concern about the adverse environmental effects of PTAs pertain to the infamous possibility of a "race to the bottom" (Porter, 1999), and upward harmonization of key environmental standards is an effective way to prevent such a race (Drezner, 2001; Vogel, 1995).

Our unit of analysis is a country-year. While the theory could also be examined using dyad-year data, we simplify here by focusing on developing countries' policy adjustments to increase the probability of PTA ratification in industrialized countries. Since we are interested in testing the impact of North-South PTAs on adoption of automobile emission standards, we limit our analysis to developing countries. We classify a country as *developing* if it is included in the category Upper Middle Income, Lower Middle Income, or Low Income by the World Bank.[5] We focus on the years 1992-2007 for which data are available. The dataset is *balanced* since all the countries countries enter the dataset in 1992. In total, we have $1,900$ country-year observations.

We are interested in detecting the year(s) in which a country improves automobile emission standards. Put simply, our dependent variable is a binary variable that score one in the year in which such standards are increased and zero otherwise. We estimate our models using a probit model. We base our significance tests on Huber (robust) standard errors (Beck, 2008, 486). These standard errors can account for possible heteroskedasticity, serial correlation, or intra-group correlation. To control for heterogeneity among countries, we include dummy variables following a 2003 World Bank classification for eight regions.[6]

A key robustness check that we conduct is a matching analysis that ensures the similarity of countries that do adopt automobile emission standards to those that do not. In a nutshell, this

[5]We include as many countries as possible given data limitations. With 132 developing countries included, most of the missing countries are very small economies, such as island states.

[6]Sub-Saharan Africa represents the omitted reference category in the estimations. We cannot use country fixed effects because some developing countries do not improve automobile emission standards during the period under investigation. If we used country fixed effects, we would lose these observations due to collinearity. However, our main results still hold if we use country fixed effects.

matching analysis ensures that we are not comparing apples to oranges: by showing that our results are fully robust to excluding countries that are different from correctly predicted cases, we alleviate concerns about selection bias. To be sure, matching is a powerful non-parametric way of controlling for selection on observables, i.e. of dealing with omitted variable bias without imposing functional form assumptions. Matching does not help us with unobserved variables that are correlated with the treatment and the outcome variable, which induces correlation between the dependent variable and the error term.

Moreover, as mentioned previously, the majority of adoptions of automobile emission standards are adopted toward the end of our timeframe. To account for such trending, we include year fixed effects in some estimations. To further tame the serial correlation problem, we re-estimate our main models using probit with cubic polynomials, probit with cubic splines, and GEE estimation with first order autocorrelation.[7]

5.1 Dependent Variable

Our dependent variable is derived from the stringency of automobile emission standards in a developing country. All automobile emission standards in existence are basically copied from the American or European emissions standards, and a common yardstick for the stringency of such standards is to compare them to the EURO standard. Since 1992, the EU has adopted five increasingly stringent standards, from the original EURO 1 to the EURO 5 standard of 2007. These standards are, in turn, comparable to various "tiers" of standards in the United States, and thus those countries that copied the US standards can also be included. Since all standards are comparable with the unit emission restrictions contained in these five standards, it is possible to compare standards both across countries and over time. Our data are from Perkins and Neumayer (2012).

Since we are interested in the *event* of increased stringency, our dependent variable is *binary*. It is coded as 1 for country i in year t if this country increased the stringency of its standards in that year. It is otherwise coded as zero. Figure 1 below shows the distribution of our dependent

[7] In these cases, we also include year fixed effects. Polynomials and splines are given by a variable that counts the number of years from the previous improvement in standards for each country. Since only an handful of countries improve standards more than once, polynomials and splines are often not statistically significant.

variable over time.

We account for previous stringency, measured as the developing country's standard at time $t-1$ on a $0-5$ scale, with 0 indicating no standard and $1-5$ measuring the comparable EURO standard, by adding it as a control variable on the right-hand side. Among other things, this control will help us account for the possibility that some countries have a much stronger interest in environmental protection than others, and thus they gradually increase the stringency of their standards over time while others remain at zero levels. We also note here that no developing country in our dataset has adopted a standard that is as stringent as those of the EU and the US, and thus our estimates cannot be biased by the possibility that some countries could not even in principle increase the stringency of their standards. In other words, every observation in our dataset could potentially increase the stringency of its standards in all years.

Developing countries have chosen increasingly stringent standards over time, as shown in Figure 1 (right side). While most countries have not imposed any standard at all, many have now gone beyond the initial 1992 EURO 1 standard. We account for this trend by including year fixed effects in the estimations.

[Figure 1 about here.]

5.2 Main Explanatory Variable

Our main explanatory variable is a binary indicator for whether country i had a PTA with a Northern partner – in practice Australia, Canada, EFTA, EU, Japan, New Zealand, or the US – between signature and ratification in year t. For example, consider Mexico's values in the years 1990-1995. Mexico signed NAFTA in 1992 and it entered into force in 1994, so Mexico would score 0 in 1990-1992, then 1 in 1993, and again 0 from 1994 onwards. Recall that our theoretical argument leads us to expect that the time between signature and ratification should be particularly conducive to the adoption of automobile emission standards. Both signature and ratification data can be found in the World Trade Organization's PTA database, and if necessary, through other sources such as official websites. Given that we rely exclusively on official data, measurement error is minimized. This is particularly important for an event history analysis. The supplementary

appendix illustrates the data.

Our focus is on North-South PTAs because South-South PTAs rarely contain environmental regulations, with the few exceptions being at the very end of our dataset (OECD, 2007). Similarly, North-North PTAs are also not particularly interesting for us because variation in the stringency of automobile emission standards is fairly limited among OECD members. In trade-environment interaction, the action is in North-South interactions (Bechtel and Tosun, 2009; Esty, 2001).

The selection of PTAs is in line with our theoretical approach. We include into the analysis PTAs that have a wide scope and regulate not only trade issues, but also other economic issues related to trade, such as environmental regulations or more general policy harmonization. We exclude framework agreements or partial economic agreements from the analysis, since these treaties are usually quite shallow and mandate tariffs reductions in a very limited number of sectors. For instance, we do not include the Partnership and Co-operation Agreement (PCA) between the EU and Russia in 1994. This is a vague agreement that "provides a suitable framework for political dialogue and accompanies Russia transition to a market economy." Similarly, we exclude Interim Agreements between the EU and post-communist countries because they do not contain detailed provisions on economic reform. To give an example, the EU-Latvia Interim Agreement has only 16 pages whereas the Association Agreement has 239 pages. Of the two, the latter contains all the key provisions for liberalization and harmonization between Brussels and Riga.

For robustness purposes, we also consider in some models a slight variation of our current independent variable. Since some developing countries have multiple PTAs, we also consider the sum of PTAs between signature and ratification. Since our theory does not lead to precise expectations regarding the difference between one and multiple PTAs in the ratification phase, this robustness check is important.

5.3 Control Variables

We add several control variables to (i) capture alternative explanations for the adoption of automobile emission standards and (ii) guard against spurious correlations that omitted variable bias. All these variables are lagged by one year.

First, we include the current level of automobile emission standards. This control allows us to control for time dependence, and account for the possibility that the incentive to impose more stringent standards depends on the current levels. It is also a measure against selection bias because it distinguishes between countries based on their track record. Some countries may be inclined towards adopting standards for reasons that our other variables cannot capture, and accounting for their previous level of standards helps capture this possibility.

Next, we add *per capita GDP* to capture the income level of a developing country. According to a large amount of literature in environmental economics, income levels affect countries' willingness to adopt environmental regulations (Dasgupta et al., 2002; Grossman and Krueger, 1995). In the case of automobile emission standards, this issue is pronounced by the fact that income levels are also highly correlated with the number of automobiles per household. Similarly, wealthy countries are more lucrative partners for PTA formation.

We also add total GDP to measure the economic importance of a developing country. Large countries may have stronger incentives to form PTAs, and country size is also correlated with the stringency of automobile emission standards. For example, it is easier to induce foreign automobile manufacturers to transfer advanced technology to large markets (Gallagher, 2006; Thun, 2006).

As to political institutions, *Regime* is a dummy variable that scores 1 if a developing country i is a democracy at time $t-1$, as measured in Cheibub, Gandhi, and Vreeland (2010). This democracy measure relies on competitive elections as an indicator. Previous research has shown that democratic pairs of countries sign PTAs more frequently than autocratic or mixed pairs (Mansfield, Milner, and Rosendorff, 2002). Similarly, several scholars have argued that democratic countries are more willing to implement environmental policies because their governments have incentives to supply public goods (Li and Reuveny, 2006; Neumayer, 2002).

We also add a dummy that scores one if a country is a WTO member. If our general theoretical argument is valid, WTO member countries may have already implemented automobile emission standards to conclude negotiations with important economic powers. Moreover, WTO members tend to implement trade policies that differ from countries that are not part of this international organization, and non-members also have incentives to form PTAs in view of WTO negotiations

to enhance their bargaining (Mansfield and Reinhardt, 2003). Thus, developing countries that are WTO members should be more likely to conclude an agreement with industrialized countries, as they are all WTO members.

Since previous research indicates that automobile emission standards "diffuse" across borders, with developing countries enacting standards if exporter companies must comply with them in any case (Perkins and Neumayer, 2012; Vogel, 1995), we include a spatial term from Perkins and Neumayer (2012). This variable weighs a developing country's automobile exports to trade partners by the stringency these partners' standards, and thus accounts for the incentive to adopt standards due to the diffusion effect.

Furthermore, we include a measure of urban concentrations of suspended particulate matter (PM10). This is one of the key pollutants that automobile emission standards can help combat, so high levels may encourage countries to act. More generally, pollution levels are the main rationale for environmental regulations (Dasgupta et al., 2006). Another related variable that we include is the share of population living in urban areas. Since automobiles produce lethal air pollution in crowded surroundings, cities are the primary location of deaths from traffic-related air pollution (Dasgupta et al., 2006). These variables are from the World Development Indicators.

In some of the models, we also consider two additional environmental variables.[8] First, we examine the total amount of environmental aid as a percentage of GDP given to a developing country. The data are from the AidData project.[9] Since environmental aid reflects donor interest in a developing country's environmental conditions (Hicks et al., 2008), it may function as another source of international "leverage" (Levitsky and Way, 2006) on environmental policy. Second, to capture the intensity of domestic environmental activism in a developing country, we include the number of environmental NGO secretariats in a country (Murdie, 2009). We unfortunately do not have detailed global data on membership numbers, but the number of secretariats captures the potential for organized environmental lobbying.

We add two variables that capture domestic institutional features of developing countries. First,

[8]Data for these variables are missing for several developing countries, so we do not include them in the main models.

[9]See `http://www.aiddata.org`. Accessed on October 21, 2011.

we include a measure of government fractionalization from the Database of Political Institutions (DPI) (Beck et al., 2001).[10] Previous research suggests that it is difficult for fractionalized governments to implement policies (Alesina and Tabellini, 1990) and ratify treaties (Mansfield and Milner, 2010). Since we examine the relationship between treaty ratification and domestic policy formation, accounting for such difficulties is important. Second, we include a measure for government partisanship, also from the DPI. This variable scores -1 for a left-wing executive, 0 for a centrist executive, and 1 for a right-wing executive. We account for partisanship because left-wing governments are more willing to impose environmental regulations (Neumayer, 2003) while right-wing governments have stronger incentives to engage in trade cooperation (Krever, 2001). While the sign of the coefficient is difficult to predict, it is nonetheless important to verify that our result holds when we control for government partisanship.

Finally, we include year and region fixed effects.[11] The regional classification that we follow is that used by the World Bank. These fixed effects allow us to account for time dependence and broad regional differences that our variables cannot capture, respectively. Descriptive statistics are provided in Table .

[Table 1 about here.]

5.4 Testing Alternative Causal Mechanisms

Hypotheses 2 and 3 emphasize alternative ways in which treaties could influence state behavior. To test the negotiation argument, we construct an alternative measure for PTA negotiations from the underlying data. This binary variable scores 1 if the developing country in focus is currently negotiating a PTA with a Northern partner. It scores 0 otherwise. To test the effect of treaty ratification, we also construct an "in force" variable that scores 1 if country i has a North-South PTA in force in year t. Following previous research (Von Stein, 2005), this allows us to test treaty effectiveness following ratification. In some of the models, we include combinations of these and our main explanatory variable. This allows us to compare the explanatory power of the different

[10] For the most recent dataset, see `http://go.worldbank.org/2EAGGLRZ40`. Accessed on October 21, 2011.

[11] We perform a test to see if year fixed effects are needed (in STATA 11, *testparm*). The test failed to reject the null hypothesis at the 90 percent level ($p = 0.102$). Thus, we decided to include year fixed effects in only some models.

hypotheses in one statistical model.

The purpose of these tests is *not* to reject the possibility that negotiations or ratification induce other policy changes. For example, it seems entirely plausible that most of the tariff reductions that PTAs require would be implemented only after ratification. We are only trying to ensure that we are accounting for alternate causal pathways for this particular policy in focus. The total treaty effect may well cover multiple policies, and we do not intend to estimate this total effect within the confines of this article.

In addition to testing the alternative hypotheses, constructing these variables carries two additional major benefits. First, the variables allow us to guard against conflating causal effects with selection effects. A selection problem could exist if the countries forming PTAs are in many ways different from those that do not. In addition to using matching, we guard against this bias by including the negotiation and in-force measures in some of our models. By including the negotiation and in-force measures, we can check whether unobservable country characteristics are driving our results. After all, every country that forms a PTA at some point negotiates and sees the PTA come into force. If these three stages have *different* effects on domestic policy, then unobservable country characteristics should not drive our results.

Second, these variables help us account for possible non-stationarity in the data.[12] Both PTAs and automobile emisions standards are heavily trended. Such trends can cause bias in time series analysis due to non-stationarity. In addition to including year fixed effects in some of our models, the negotiation and in-force measures help us account for non-stationarity. These variables are also trended, so if non-stationarity is biasing our results, these variables should also be biased. But if these three stages have *different* effects on domestic policy, then non-stationarity should not drive our results. In sum, our econometric strategy is similar to the "event-time specification model" employed by Kuziemko and Werker (2006).

[12]When we run the Hadri Lagrange multiplier (LM) test, we are unable to reject the null hypothesis that all the panels are (trend) stationary (p=0.28). We obtain similar results by running other test for unit roots/stationarity such as the Levin-Lin-Chu test and the Im-Pesaran-Shin test.

5.5 Matching Analysis

The period between the signature and the entry into force of a PTA is not a randomly assigned variable. In other words, units receiving treatment and those excluded from treatment may differ not only in their treatment status but also in other characteristics. If these characteristics affect both the probability of signing and ratifying a PTAs, as well as the probability of improving automobile emission standards, our results are hampered by selection bias. We correct for nonrandom assignment using a matching technique. Matching deals with the crucial problem of causal inference: we can only observe each unit in either the treated or control condition, but not both. Matching overcomes this problem by finding for a treated unit a non-treated unit with similar characteristics. Then, it is possible to estimate our models using this non-treated unit as the imputed control for the treated unit.

Three main matching techniques exist: [LEO] Mahalanobis covariate matching (MDM), propensity score matching (PSM), and coarsened exact matching (CEM). Recent studies point out that the use of PSM is often not benign. In particular, King et al. (2010, 2) show that PSM "increases variance and reduces balance (between the treated and control groups) on average compared to not matching at all." Moreover, simulations show that CEM outperforms MDM in several applications related to social science and medical studies (King et al., 2010). Given these recent findings, we opt for CEM.

Matching requires a series of discretionary decisions. First, which variables should be selected for matching between the treatment group and the control group? We follow two criteria. We select variables that are (1) important drivers of PTA formation and (2) correlated with the probability of improving automobile standards. To begin with, we match on GDP and $percapitaGDP$. Previous studies show that the size and the level of development of countries affect their probability of forming a PTA (Baier and Bergstrand, 2004). The adoption of automobile emission standards is also related to economic wealth and seems to indicate a large country bias (Perkins and Neumayer, 2012).

In addition to these economic variables, we match on two important political variables. First, we use the variable *Regime* because previous research indicates that democratic countries have

stronger incentives to engage in trade cooperation with other democracies (Mansfield, Milner, and Rosendorff, 2002) and to protect the environment (Neumayer, 2002). Second, we match on WTO membership. WTO members have already previously liberalized their trade policies, so they can form PTAs at a lower cost (Baier and Bergstrand, 2004). To the extent that trade liberalization induces the diffusion of environmental standards (Vogel, 1995), they may also have a greater propensity to enact automobile emission standards.

The second discretionary decision is: the choice of coarsening for these variables? To choose the coarsening, we examined the distribution of these variables. Both $GDPpc$ and GDP are skewed on the right, i.e. there are few rich big countries. Thus, we chose the coarsening of $GDPpc$ and GDP at their mean and one standard deviation above the mean. The main advantage of doing so is that we can place outliers, i.e. rich and big countries, in the same bin. The $Regime$ and WTO variables cannot be coarsened since they are dummies. The supplementary appendix illustrates.

Third, we identify these observations that contain at least one treated and one control unit while dropping all the others.[13] As shown in the supplementary appendix, a dramatic reduction in the imbalance of each covariate is achieved. Importantly, the overall L_1 balance measure, which captures imbalance with respect to the full joint distribution, drops significantly from 0.82 to 0.62, i.e. matching reduces the imbalance of the full joint distribution by more than 20 percent.[14]

Fourth, we re-estimate the model on this subsample including all the control variables of the main model. Coarsening leaves some imbalance in the matched data, so we also include the covariates that we used to balance the treatment group and the control group (Blackwell et al., 2009, 537). Results remain unchanged. If anything, our main explanatory variable is statistically significant at 99 per cent level, whereas it was statically significant at 95 per cent level before matching. Thus, the argument that developing countries forming PTAs with industrialized countries also improve their automobile emission standards is not supported from our data and analysis. In particular, we find that there are some small developing countries whose economy is too small to be of interest for

[13]We used the CEM command in Stata. We lose roughly a quarter of the observations, indicating that matching is indeed needed. However, we do not lose any PTA negotiations.

[14]Blackwell et al. (2009, 531) note that "the L_1 value is not valuable on its own, but rather as a point of comparison between matching solutions." We also tried a parametric estimation to control for the selection bias, i.e. Heckman model. We get similar results, which are available upon request, if we use this parametric technique.

industrialized countries, yet they have improved automobile standards over the past two decades.

6 Findings and Robustness

The results from the empirical analysis without matching are provided in Table 2. Model (1) is the main model, while models (2) and (3) control for the alternative causal mechanisms of PTA in force and ongoing negotiations. Models (4) to (6) include year fixed effects, and models (5) and (6) remove the first and last two years of the data, respectively, for robustness purposes. The last model also replaces the binary indicator for being between signature and ratification with the number of North-South PTAs between signature and ratification for the country-year in focus.

In all models, the effect of the PTA-between variable is positive and statistically significant at the conventional $p < 0.05$ level. This is also true of the sum. By contrast, the PTA-negotiations variable has a negative and statistically insignificant effect while the PTA-inforce variable also has a positive effect on the adoption of automobile emission standards.

[Table 2 about here.]

Moreover, the substantive magnitude of these effects is large, as shown in Table 3. The effect of Between-PTA is larger than that of the other main covariates and comparable with GDP. The number of predicted increases in automobile standards is more than one per year for developing countries countries that are in the process of mutual ratification of a PTA with industrialized countries.[15] Thus, any empirical analysis of automobile emission standards that fails to incorporate the role of PTAs is bound to produce biased results.

These findings are notable in light of the extant literature. While some authors have noted the role of environmental policy in individual PTAs, such as NAFTA (Deere and Esty, 2002; Hufbauer et al., 2000), we show that policy changes during the ratification stage are central to understanding the trade-environmental linkage. Conversely, we find no evidence for effects during negotiations or following ratification. At least in this case, then, the conventional approach, which concentrates

[15]There are 68 country-years with an increase in regulatory stringency in our dataset. To calculate the number of predicted increases, we follow Mansfield, Milner, and Rosendorff (2002).

on domestic policy changes following ratification (Chayes and Chayes, 1995; Downs, Rocke, and Barsoom, 1996; Simmons, 2009; Von Stein, 2005), would not be useful. The evidence suggests that the ratification stage is is key to understanding North-South PTA effects on environmental policy, and this thus consistent with the notion that concerns about policy implementation are central to understanding treaty cooperation, even as we recognize that the quantitative analysis cannot directly speak to the causal mechanisms at play.

[Table 3 about here.]

The control variables contain few surprises. High income levels and large country size predict automobile emission standards, while a democratic regime and susceptibility to diffusion through trade channels also do so. Perhaps the only surprise is the negative coefficient of the air pollution variable. This may reflect the fact that air pollution generally decreases over time while standards become more stringent, or the fact that the worst offenders are countries with little interest in environmental quality.

Table 4 presents the results using matched data. The models correspond to those in the previous table, except that the matched data are used. The table shows that the results are generally stronger, indicating that any possible selection biases in the data undermine our argument. When we account for them, we observe stronger effects. Interestingly, they also flip the sign of the PTA-inforce variable, indicating that this finding may have been spurious. At any rate, the matching analysis strongly supports our theoretical argument.

[Table 4 about here.]

Table 5 shows the results of a robustness analysis. We use a different measure of regime type, namely Polity IV, with matching (models 1 and 2) and without matching (model 3). Model (4) is a probit with cubic polynomials, while model (5) includes cubic splines. Model (6) presents a generalized estimating equation (GEE). These latter three models allow us to account for time dependence. Again, the main results remain unchanged in all six models.

[Table 5 about here.]

In sum, the quantitative analysis indicates that if a developing country has recently signed a PTA, yet ratification by the foreign partner is yet to occur, the probability of adopting an automobile emission standard is much higher than in other circumstances.

Given this broad quantitative finding, we next examine actual instances of such behavior. Which country-years accord with our theoretical expectations, and can something more be learned from a qualitative analysis of these cases? Table 6 provides a list of correctly predicted cases. The row indicates the developing country, the column gives the Northern partner, and each entry indicates the year in which they adopted standards while a PTA was between signature and ratification. For example, the table indicates that Chile and New Zealand had a PTA between signature and ratification in 2005, and Chile also adopted an automobile emission standard in this year.

[Table 6 about here.]

Of these cases, perhaps the most politically salient is the case of NAFTA (Mexico, 1993). In 1990, Mexico officially began negotiating a trade agreement with the United States and Canada. This was the first PTA that the United States negotiated with a developing country, and due to the importance of trade in manufactured goods, environmental issues loomed large (Cameron, 1997; Raustiala, 1997). As Audley (1997, 2) puts it, "[f]rom the start of preliminary trade talks between Mexico and the United States ... local and national environmental organizations expressed concern that a trade agreement with no regard for the environment would be disastrous for the long-term ecologically sustainable goals." These concerns were duly noted in Mexico because the country was "in a vulnerable position during these negotiations – failure to supplemental accords [on the environment] could derail NAFTA, and Mexico needed NAFTA more than ever as economic growth began to falter in 1993 and the date for presidential elections approached" (Cameron, 1997, 124). The negotiations were concluded by 1992, and NAFTA entered into force in 1994 after a contentious ratification debate in the United States finally ended in November 1993.

If our theory is valid, we should see evidence of Mexico improving its environmental regulations – automobile emission standards and others – especially between signature in 1992 and entry into force in 1994. The case thus allows a qualitative illustration of our argument. In particular, we can look beyond automobile emission standards for broader policy developments. In what follows,

we examine how the shadow of the US Congress, and the threat of NAFTA ratification failure in particular, changed the logic of environmental policy in Mexico.

NAFTA does not explicitly force Mexico to enact automobile emission standards, but the treaty text of the associated North American Agreement on Environmental Cooperation does note sustainable development as a goal and encourage the partners to combat a race to the bottom in environmental regulations. Notably, it also proscribes environmental deregulation as a strategy for increasing foreign direct investment.

Given these environmental goals, and their salience in the US political debate, did Mexico enhance its environmental standards to increase the probability of ratification by the United States? In addition to imposing automobile emission standards in 1993, Mexico expanded the coverage of industrial effluent standards to cover 11 new sectors. According to Knill, Tosun, and Heichel (2008, 1029-1030), "the revision of effluent standards as a constituent of the general environmental reform package can be judged to have been triggered by conditionality." It was not legal conditionality, though, because NAFTA had yet to enter into force. Conventional statistical analyses of treaty effectiveness would have missed these effects, even if adequate data on environmental policy had been available.

Mexico also invested in enforcement of environmental regulations, especially in the particularly sensitive border region (Husted and Logsdon, 1997, 35). In 1991, Mexico implemented altogether 3,119 environmental inspections in manufacturing plants, and in 1992 this number had increased to 3,713. The real boom came in 1993, however, with an increase to 14,387. As soon as NAFTA was ratified, though, the number decreased to 8,187. Similarly, the number of regulatory enforcement activities related to the environment increased from 3,713 (1992) to 21,685 (January 1993 – June 1994) (Husted and Logsdon, 1997, 34). In addition to improved enforcement, Mexico invested in institutional capacity. In May 1992, towards the end of the negotiations, Mexico established a new agency with ministerial competencies, the Secretariat of Social Development (Husted and Logsdon, 1997, 33).

Were these and other activities noted in the United States? President Clinton, the pre-eminent advocate of NAFTA, publicly praised Mexico's progress in environmental protection. As Mall

(1998, 162) writes, President Clinton noted, in a Department of State dispatch, a quadrupling in the number of environmental inspectors employed by the Mexican state and Mexico's willingness to participate in a billion dollar cooperation program for environmental improvement.

Slovenia and Croatia feature similar dynamics. Each country sought a PTA with the EU in view of the ultimate goal of full membership, and such membership requires harmonization of standards with the EU (Mattli and Plümper, 2004). In the EU, automobile standards are harmonized and no member state is allowed to deviate from the commonly chosen level (Arp, 1993). Thus, both Slovenia and Croatia would ultimately have to adopt standards. An EU PTA, or an Association Agreement, is a key step toward full accession, as it is the first formal contract between the union and the candidate that prescribes major policy harmonization with the European *acquis communautaire*. Without the entry into force of the PTA, the candidate cannot initiate formal accession negotiations. It thus makes a lot of sense for them to pass environmental policies between signature and ratification, so as to facilitate the entry into force of the PTA, and thus speed up the intiation of formal accession negotiations.

The Peruvian case also accords with these expectations. Peru's negotiations on a PTA with the US were controversial, and environmental NGOs blamed the Peruvian government for failing to protect the local environment. As a result, Peru's adoption of a key environmental policy seem suited for reducing concerns about environmental protection in the Congress – even if the NGOs themselves ultimately remained in opposition to the US-Peru PTA.

6.1 Placebo Test

As said above, both selection bias and serial correlation present challenges to our identification strategy. A skeptical reader might note that both the formation of North-South PTAs and improvements in automobile standards occur during the same time period, i.e. during the last two decades, and that developing countries that form North-South PTAs are also more likely to implement environmental regulations. A placebo test with other international institutions allow us to further test our argument.

Specifically, we explore the effect of bilateral investment treaty (BIT) formation and member-

ship in the WTO on improving automobile standards. Both BITs and WTO membership share characteristics with North-South PTAs. First, BITs are usually formed between North-South countries (Elkins, Guzman, and Simmons, 2006). Similarly, only developing countries joined the WTO in the period under investigation (Pelc, 2011). Second, the countries that formed BITs or joined the WTO are also the best candidates for improving environmental standards due to good governance. Third, WTO membership and formation of BITs are also trend variables, i.e. they increase over time.

However, BITs and the WTO differ from North-South PTAs in a crucial aspect. They do not require developing countries to improve environmental standards, which are neither part of the BIT treaty nor part of the WTO treaty. As such, we expect that BITs and the WTO have *no effect* in improving automobile standards. If we do find an effect, that would raise concerns that selection bias and/or serial correlation are responsible for the previous findings.

To implement the placebo test, we first build a variable, labeled Pre-WTO Accession, that scores "1" for the five years that precede the accession to the WTO for each country in the dataset, and "0" otherwise. Notably, this variable scores "0" even if a country joined the GATT before 1990. Similarly, we build a variable, labeled Post-WTO Accession, that scores "1" for the five years that follow the accession to the WTO for each country in the dataset, and "0" otherwise.

Regarding BITs, we follow our operationalization for North-South PTAs. Specifically, we build a variable, labeled Signature-BIT, that scores "1" if a North-South BIT is formed. Moreover, we build a variable, labeled In Force-BIT, that scores "1" if a North-South BIT enters into force. Finally, we build a variable, labeled Between-BIT, that scores "1" in the years between the signature and the ratification of a BIT. The data on BITs come from Allee and Peinhardt (2010).

The results are reported in the supplementary appendix. They show that neither Pre-WTO Accession nor Post-WTO Accession are statistically significant at the conventional levels, though their coefficients are positive.[16] Similarly, no BIT variable is statistically significant at the conventional level. Interestingly, Between-BIT has a *negative* coefficient in both estimations, suggesting the

[16]The correlation between the PTA variables and the WTO variables is never higher than 0.1 according to the tetrachoric correlation test.

opposite effect compare to Between-PTA.[17] These placebo tests suggests that North-South PTAs are the driving force behind environmental regulations.

6.2 Low-Hanging Fruit?

Our theory is premised on the notion that countries implement substantively meaningful policies during the ratification stage. A possible alternative explanation maintains that these policies are mostly symbolic "low-hanging fruit," or policies that the developing countries would have implemented in any case (Hoffman, 2000; Vandenbergh, Barkenbus, and Gilligan, 2008). To rule out this alternative mechanism, we interact our main explanatory variable, Between-PTA, with a variable capturing the presence of fair and regular elections. If standards are but low-hanging fruit, one would expect countries with fair and regular elections to implement them the most readily, because democratic political systems give access to environmental groups. Data on electoral accountability come from the Worldwide Governance Indicators (Kaufmann, Kraay, and Mastruzzi, 2008). The variable ranges between -2.5 (low accountability) and 2.5 (high accountability).

A graphical representation of the interactive effect is provided in the supplementary appendix. There is little evidence for the low-hanging fruit argument. If anything, the marginal effect of a PTA ratification period *decreases* as electoral accountability increases. This is exactly the opposite of what one would expect if the standards were low-hanging fruit.

7 Conclusion

International treaties can influence state behavior in many ways. This article has shown that in some cases they do so between signature and ratification: states seek to secure the support of pivotal players in foreign ratification debates. The strong and statistically robust effect of North-South PTAs between signature and ratification on the probability that a developing country adopts an automobile emission standard provides evidence for this argument. In this case, the other causal pathways of treaty effects – during negotiations and after ratification – do not have similar effects.

[17] The correlation between the PTA variables and the BIT variables is never higher than 0.5 according to the tetrachoric correlation test.

Although this article focuses on environmental standards, and automobile standards in particular, the results may generalize to other policy areas. Olga Lucia Lozano, a Colombian lead trade negotiator, noted that the trade agreement with the United States forced Colombia to substantially improve labor and social protections and health standards during the ratification perod.[18] The agreement was signed on November 22, 2006 and ratified by president Barack Obama on October 12, 2011. According to Lozano, Colombia had to create a health register for which the Instituto Nacional de Vigilancia de Medicamentos y Alimentos (INVIMA), a regulatory agency under the Ministerio de Proteccion Social (Social Protection Ministry), was responsible. Such a register handles, for instance, sanitary aspects related to processed food trade, including monitoring of domestic beef slaughtering and processing plants. Similarly, the agreement required the Colombian Agricultural Institute (ICA) to issue sanitary import permits for animal products, vegetables, fruits, and grains. Given the long period between signature and ratification, this agreement exemplifies nicely the importance of the mechanism explored in this article.

These findings have important implications for the study of international law. The results underscore the multidimensional nature of treaty effects. A large body of literature on treaty compliance and consequences has emerged (Simmons, 2010; Von Stein, 2005), but this literature remains narrowly focused on events following ratification. We have shown here that this narrow focus partly misconstrues the channels through which treaties influence state behavior, for it adopts an overly legalistic view of international treaties.

Based on these results, we recommend that scholars of international law heed to differences among policies that treaties could affect. Our theory can be applied to those that play a role in ratification debates, such as environmental regulations in the case of PTA formation. For policies that do not promote ratification, such as costly adjustments with little, if any, domestic political support, our hypotheses probably apply to a much lesser extent. Future studies of treaty consequences should develop contingent arguments that allow the type of policy to modify the expected effects of treaty negotiations, ratification, and entry into force.

The findings can also guide policy. One of the most daunting challenges in managing globaliza-

[18]Interview conducted by the authors in Princeton, NJ, on March 2, 2012.

tion is the difficulty of ensuring that international law, and economic agreements in particular, help developing and industrialized countries realize joint gains and avoid destructive zero-sum games based on antagonistic rivalry. If policy adjustments between signature and ratification are key to securing market access, governments in developing countries would benefit from considering such adjustments and publicly marketing them to concerned constituencies in industrialized countries. To facilitate such efforts, treaties themselves could make increased use of interim commitments or prior actions.

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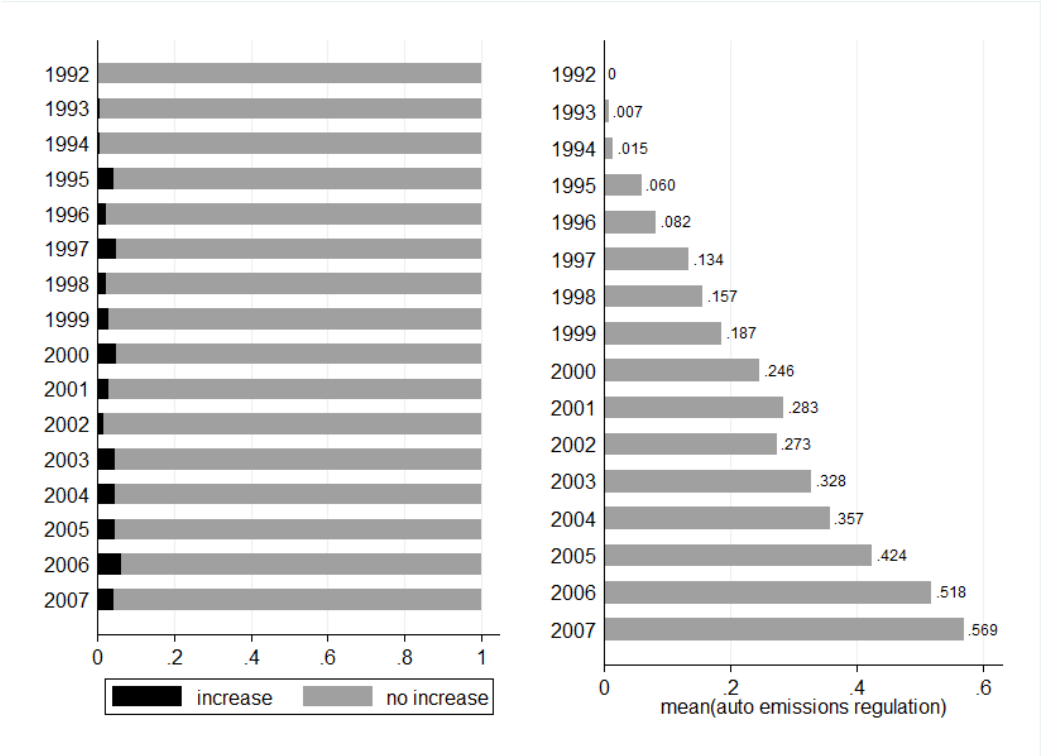

Figure 1: Left panel: Distribution of the binary dependent variable. Right panel: Mean stringency of automobile emission standards in the dataset on a $0-5$ scale.

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**Obs.**	**Source**
DV	0.03	0.16	0	1	2440	Perkins-Neumayer (2011)
PTA (btw)	0.05	0.23	0	1	2440	Authors (2009)
PTA (neg)	0.13	0.34	0	1	2440	Authors (2009)
PTA (force)	0.09	0.29	0	1	2440	Authors (2009)
Regime	0.43	0.50	0	1	2392	Cheibub et al (2010)
GDPpc	2.80	4.55	0.03	44.12	2440	IMF 2009
GDP (log)	2.41	1.57	0.10	7.89	2440	WDI 2009
WTO	0.64	0.48	0	1	2392	Authors
Diffusion	0.12	0.93	0	19.43	2066	Perkins-Neumayer (2011)
Air Pollution	71.27	48.84	6.12	480.56	2167	WDI 2009
Urban Population	48.51	22.45	5.4	100	2217	WDI 2009
Previous Auto Reg.	0.22	0.60	0	4	2066	Perkins-Neumayer (2011)
Env. Aid	4.53	9.75	0	155.7	1894	AidData
Env. NGO	1.61	3.15	0	21	2440	Murdie (2009)
Gov. Frac	0.20	0.28	0	1	2018	DPI
Partisanship	-0.17	0.93	-1	1	1238	DPI

Table 1: Descriptive statistics for variables used in the main analysis.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Between-PTA (max)	0.37**	0.38**	0.37**	0.44***	0.33*	0.36**	0.37*	0.55**
	(0.17)	(0.18)	(0.18)	(0.17)	(0.20)	(0.17)	(0.20)	(0.22)
Negotiation-PTA (max)		-0.02						
		(0.17)						
In Force-PTA (max)			0.39*					
			(0.20)					
Regime	0.30**	0.30**	0.29**	0.33**	0.37**	0.37**	0.32**	0.50*
	(0.14)	(0.14)	(0.13)	(0.15)	(0.16)	(0.15)	(0.14)	(0.29)
ln(GDP)	0.31***	0.31***	0.31***	0.33***	0.35***	0.31***	0.30***	0.32***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.06)	(0.05)	(0.06)	(0.07)
GDP p.c.	-0.01	-0.01	-0.01	-0.01	0.02	-0.01	-0.02	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
WTO	0.46**	0.46**	0.34*	0.39*	0.41*	0.40*	0.40*	0.59**
	(0.20)	(0.20)	(0.19)	(0.22)	(0.21)	(0.20)	(0.21)	(0.26)
Diffusion	0.06***	0.06***	0.04*	0.06**	0.04	0.07***	0.06**	0.05**
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
Air Pollution	-0.01***	-0.01***	-0.01***	-0.01**	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Urban Pop.	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Previous Auto Reg.	-0.20**	-0.20**	-0.23***	-0.31***	-0.31***	-0.23***	-0.17*	-0.31***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)	(0.10)	(0.11)
Env. Aid							0.02**	0.01
							(0.01)	(0.03)
NGO Aid							0.01	0.04*
							(0.02)	(0.03)
Gov. Frac								-0.89**
								(0.41)
Partisanship								-0.02
								(0.11)
Constant	-3.99***	-3.99***	-3.96***	-4.04***	-3.99***	-4.01***	-4.07***	-4.39***
	(0.39)	(0.38)	(0.35)	(0.43)	(0.39)	(0.40)	(0.39)	(0.54)
Year fixed effects	no	no	no	yes	no	no	no	no
Regional fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1,900	1,900	1,900	1,900	1,685	1,769	1,636	827

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Empirical results without matching.

Variable	Δ Probability	Δ Improvements
Bewteen-PTA (max)	0.01	1.2
Regime	0.002	0.24
WTO	0.002	0.24
Diffusion	0.001	0.12
ln(GDP)	0.01	1.2

Table 3: Effect of changes in the values of main covariates on the predicted probability of improved automobile emission standards and the predicted number of improvements per year. The estimates are based on Model 1. For continuous variables, the changes in probabilities are calculated for moving from a standard deviation below the mean to a standard deviation above the mean. For dummy variables, the probabilities are calculated moving from zero to one. The predicted number of improvements per year is calculated by multiplying the predicted probabilities by the number of observations (1900) and dividing by the number of years (16).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Between-PTA (max)	0.44**	0.54**	0.46**	0.51**	0.49**	0.43**	0.43*	0.57**
	(0.20)	(0.21)	(0.20)	(0.22)	(0.24)	(0.21)	(0.23)	(0.24)
Negotiation-PTA		-0.26						
		(0.20)						
In Force-PTA			0.14					
			(0.25)					
Regime	0.32**	0.31**	0.32**	0.31*	0.38**	0.38**	0.25*	0.80***
	(0.15)	(0.15)	(0.14)	(0.16)	(0.16)	(0.17)	(0.15)	(0.29)
ln(GDP)	0.28***	0.29***	0.28***	0.32***	0.28***	0.28***	0.23***	0.27***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)	(0.10)
GDP p.c.	-0.05**	-0.05**	-0.05**	-0.05**	-0.04	-0.06**	-0.09***	-0.09*
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
WTO	0.81***	0.80***	0.76***	0.86***	0.76**	0.78**	1.01***	0.96***
	(0.30)	(0.29)	(0.27)	(0.33)	(0.31)	(0.32)	(0.32)	(0.33)
Diffusion	0.07***	0.07***	0.06***	0.06***	0.05	0.07***	0.07***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.02)	(0.02)	(0.02)
Air Pollution	-0.01**	-0.01**	-0.01**	-0.01**	-0.01***	-0.01**	-0.01**	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Urban Pop.	0.01*	0.01*	0.01*	0.01**	0.01	0.02*	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Previous Auto Reg.	-0.27**	-0.25**	-0.27**	-0.38***	-0.38***	-0.30***	-0.19	-0.38***
	(0.11)	(0.10)	(0.11)	(0.12)	(0.10)	(0.11)	(0.12)	(0.13)
Env. Aid							0.01	0.01
							(0.02)	(0.05)
Env. NGO							0.01	0.06
							(0.02)	(0.04)
Gov. Frac.								-1.11**
								(0.55)
Partisanship								-0.09
								(0.09)
Constant	-4.26***	-4.18***	-4.26***	-4.30***	-4.09***	-4.31***	-4.31***	-4.57***
	(0.57)	(0.56)	(0.57)	(0.57)	(0.58)	(0.60)	(0.58)	(0.62)
Year fixed effects	no	no	no	yes	no	no	no	no
Regional fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Observations	1,773	1,773	1,773	1,773	1,573	1,648	1,555	777

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Empirical results with matching.

	(1)	(2)	(3)	(4)	(5)	(6)
Between-PTA (max)	0.36**		0.44**	0.36**	0.38**	0.43**
	(0.17)		(0.20)	(0.18)	(0.18)	(0.21)
Between-PTA (sum)		0.30**				
		(0.13)				
Polity	0.02	0.02	-0.02			
	(0.01)	(0.01)	(0.03)			
Regime				0.34**	0.35**	0.32*
				(0.15)	(0.15)	(0.18)
ln(GDP)	0.29***	0.29***	0.27***	0.35***	0.35***	0.33***
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)
GDP p.c.	-0.01	-0.01	-0.05**	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
WTO	0.48**	0.47**	0.81***	0.46*	0.44*	0.38*
	(0.20)	(0.20)	(0.28)	(0.24)	(0.24)	(0.21)
Diffusion	0.07***	0.07***	0.07***	0.05**	0.06***	0.06
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.04)
Air Pollution	-0.01**	-0.01**	-0.00	-0.01***	-0.01***	-0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Urban Pop.	0.00	0.01	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Previous Auto Reg.	-0.20**	-0.20**	-0.26**	-0.22	-0.22	-0.28**
	(0.09)	(0.09)	(0.11)	(0.22)	(0.23)	(0.11)
Constant	-3.92***	-3.93***	-4.22***	-4.53***	-4.68***	-4.02***
	(0.37)	(0.37)	(0.60)	(0.79)	(0.82)	(0.50)
Year fixed effects	no	no	no	yes	yes	yes
Regional fixed effects	yes	yes	yes	yes	yes	yes
Observations	1,810	1,810	1,749	1,900	1,900	1,900

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Robustness of empirical results.

Country	AUS	CAN	EFTA	EU	JPN	NZL	USA
Chile	0	0	0	0	0	2005	0
Croatia	0	0	2005	2000	0	0	0
Egypt	0	0	0	2003	0	0	0
Mexico	0	0	0	0	2004	0	1993
Peru	0	0	0	0	0	0	2007
Singapore	0	0	2001	0	0	0	0
Slovenia	0	0	1996	1996	0	0	0
South Africa	0	0	2006	0	0	0	0

Table 6: Country-years that adopted automobile emission standards between PTA signature and ratification. The first column indicates the developing country and the first row indicates the Northern partner. Unless zero, the cell indicates the year of the positive observation.