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Making Anarchy Work:  
International Legal Institutions and Trade Cooperation

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## Abstract

Do international institutions matter? If so, why? International relations theory offers three broad answers to these questions. First, institutions do not matter because the international system is anarchic. Second, institutions matter because they supplant anarchy with hierarchy. Third, institutions matter because they remedy market failures that impede cooperation under anarchy. I evaluate these rival claims by examining the impact of international dispute settlement mechanisms (DSMs) on preferential trade liberalization. I find that, although the existence of dispute settlement panels promotes trade liberalization, more legalistic DSM features do not. This suggests that, while institutions matter, they do not formally constrain states. Rather, they promote cooperation by facilitating governments' reciprocal strategies and raising the reputational costs of noncompliance. In other words, institutions do not change the anarchic nature of international politics but rather make the anarchic system work better.

In March 2002, the U.S. government imposed 30 percent tariffs on steel imports, defending them as an emergency safeguard measure against an import surge. The U.S.'s trading partners immediately denounced the tariffs as a violation of World Trade Organization (WTO) rules and initiated dispute settlement proceedings within the WTO. In November 2003, despite a U.S. appeal, the WTO ruled the tariffs illegal, and China, the European Union, and Japan announced their intent to levy retaliatory sanctions if the U.S. did not comply with this ruling. One month later, the U.S. dropped the tariffs.<sup>1</sup>

Did the presence of an international institution—the WTO Dispute Settlement Body—influence the U.S. decision to reverse its tariff policy? The answer is unclear. Perhaps the U.S. reversal was a response to retaliatory threats that its partners would have made even in the WTO's absence. Perhaps, conversely, U.S. policymakers changed course because they felt legally bound by the WTO's decision. Or perhaps the WTO played a facilitating role, coordinating enforcement from complainant states but providing none on its own. The facts of the case are consistent with all three stories.

These differing accounts of the steel dispute illustrate three common views on the role of international institutions in a supposedly anarchic international system. First, institutions may not matter because, under anarchy, they cannot constrain government behavior (Mearsheimer 1994-95). Second, institutions may transform the anarchic system into a hierarchical one by constraining governments to act even against their own self-interest (Chayes and Chayes 1993; Burley and Mattli 1993). Third, institutions may affect outcomes without subverting anarchy by remedying market failures to which the anarchic system gives rise (Keohane 1984). International institutions may thus be rendered irrelevant by anarchy; they may subvert anarchy; or they may make anarchy work better.

As the above example illustrates, it is difficult to refute any of these propositions. Although some studies identify systematic relationships between institutions and policy outcomes (Simmons 2000; Davis 2004), others are quick to point out that such relationships may reflect the endogeneity of institutions to government preferences (Downs, Rocke and Barsoom 1996; von Stein 2005). Moreover, even if we accept that institutions matter, it is not always clear why. Most institutions serve multiple functions: for example, the WTO dispute settlement system generates both legally binding decisions and reputational concerns. Because these functions are bundled together, it is difficult to say whether compliance with WTO decisions reflects their legal status, reputational concerns, or both. Disentangling these effects is important, however, because they imply very different causal mechanisms: the legal status of decisions matters only in hierarchical systems, while reputational concerns are most salient in a system of decentralized enforcement. Identifying which of these factors is most important would thus allow us to say whether the WTO erodes anarchy or merely mitigates its adverse effects.

I examine the impact of dispute settlement mechanisms (DSMs) on preferential trade liberalization. For several reasons, preferential trade arrangements (PTAs) provide a unique opportunity to assess both whether and how international institutions affect state behavior. First, PTAs have comparable goals: nearly all purport to liberalize trade among members. Second, some PTAs have been much more successful than others at achieving these goals. Third, PTAs vary greatly in their DSM design. Some have no formal DSMs at all; others have minimally legalistic DSMs that provide only non-binding third-party review; and still others have highly legalistic features such as standing tribunals, direct effect in national courts, and the ability to issue legally binding rulings. Because PTAs have similar goals but vary in both their DSM design and intra-bloc liberalization, they provide an ideal opportunity to examine the relationship

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between the former and the latter. Moreover, variation in DSM design allows us to identify which, if any, DSM characteristics promote trade liberalization. This permits us to say, not only whether international institutions matter, but also how.

I find that DSMs have significant positive effects on intra-PTA trade liberalization. These effects stem wholly from the creation of third-party tribunals that can issue authoritative rulings: more legalistic DSM features, such as legally binding decisions and direct effect, have no significant additional effects. I thus conclude that institutions affect state behavior, not by formally constraining states, but by facilitating governments' reciprocal strategies and raising the reputational costs of noncompliance. In other words, institutions do not subvert anarchy but rather make it work better. This result has important implications, both for international relations theory and for current proposals to reform international institutions such as the WTO's dispute settlement system.

### Dispute Settlement and Trade Cooperation

The term "dispute settlement mechanism" encompasses a wide range of international legal institutions. Smith's (2000) study on the determinants of DSM design ranks DSMs from non-legalistic to highly legalistic on the basis of five characteristics: (1) whether disputants have a right to third-party review by a dispute settlement panel, (2) whether panel rulings are legally binding under international law, (3) whether panel judges are chosen on an *ad hoc* basis or constitute a standing tribunal, (4) whether standing to bring cases is limited to national governments or, alternatively, extends to private actors, and (5) whether panel decisions have direct effect in member states, or, alternatively, must be enforced by complainant sanctions. These ranking criteria, their relationship to DSM legalism, and examples of PTAs with each set of characteristics are presented in Table 1.

Table 1 about here

DSMs that provide no opportunity for third-party review are non-legalistic, in that disputes are settled entirely by state-to-state bargaining. Those that provide a right to third-party review, but nothing more, exhibit low legalism. DSMs that can issue legally binding rulings have medium legalism; those with standing tribunals have high legalism; and those that provide standing for individuals and direct effect exhibit very high legalism. Note that third-party review is a prerequisite for the other conditions; and, while the others are independent in principle, they tend to be cumulative in practice. Hence, DSMs with standing tribunals also issue legally binding rulings; DSMs with direct effect also have standing tribunals, and so on. The cumulative nature of these DSM traits justifies Smith's ordinal approach. Note also that, while private standing and direct effect are distinct DSM features in principle, they are almost perfectly correlated in practice. Highly legalistic DSMs thus exhibit both characteristics.

Different DSM characteristics should, theoretically, promote trade in different ways. I first discuss DSM functions that may facilitate cooperation under anarchy, then turn to functions that may alter more fundamentally the nature of international politics.

DSMs that provide *third-party review* could promote trade cooperation even in the absence of supranational enforcement. The argument begins from two widely accepted premises: (1) governments often have Prisoner's Dilemma (PD) preferences, and (2) cooperation can emerge from an iterated PD if governments pursue tit-for-tat strategies that reciprocate both cooperation and defection (Axelrod 1984). If these premises are true, if interstate relations are iterated, and if information is perfect, then cooperation should emerge even in the absence of institutions because all defections will be detected and punished.<sup>2</sup>

However, in a world of imperfect information, institutions may promote cooperation by providing information that facilitates reciprocal strategies (Keohane 1984).

Reciprocity can be “specific” or “diffuse” (Keohane 1986). Specific reciprocity involves targeted responses to particular actions, such as the European Union’s threat to levy \$2.2 billion worth of retaliatory sanctions in response to U.S. steel tariffs. In contrast, diffuse reciprocity involves more general responses to long-term patterns of behavior. For example, the reluctance of many countries to admit Russia to the WTO stems not from any particular Russian misdeed but from a broader perception that Russia is an unreliable trading partner. To practice either form of reciprocity, governments need information. To punish specific treaty violations, they must first know whether a violation has occurred. To respond to more general behavioral tendencies, they must know something about a country’s past history of cooperation—or, put differently, they must know the country’s reputation. The reason reciprocity may be difficult to practice is that reliable information of both kinds can be difficult to obtain.

Two problems account for these informational deficiencies. First, contracting is always incomplete: all treaties leave room for interpretation (Chayes and Chayes 1993). For example, WTO members often disagree about what constitutes an illegal export subsidy or what circumstances justify the use of an escape clause. Such ambiguity may allow treaty violators to claim that they are in compliance. Although the violator’s partners can decide for themselves whether a violation has occurred, their own interest in the outcome reduces the credibility of their claims. If they cannot prove unambiguously that the violator is guilty, retaliation could itself be reputationally costly and could lead to a trade war with the target state. Treaty ambiguity thus raises the costs of practicing specific reciprocity. Second, in the absence of international arbitration, disputes tend to be settled (or not) through relatively quiet bilateral

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bargaining. This process generates information for disputants about each other's past behavior, but this information is rarely shared with other potential trading partners. The private nature of bilateral dispute settlement thus makes it hard for disputants to develop reputations with non-disputants and impedes diffuse reciprocity.

Third-party dispute settlement may ameliorate these problems in two ways. First, relatively impartial international tribunals may be able to determine more credibly than disputants whether violations have occurred (Smith 2000). If such tribunals rule for defendants, then complainants cannot retaliate without risking reputational costs. Positive rulings thus reduce the likelihood of unwarranted retaliation and trade wars. Conversely, if tribunals rule against defendants, then complainants can retaliate at little reputational cost. Negative rulings thus raise the likelihood of warranted retaliation. Decisions by DSM judges thus make it easier for governments to reward cooperation and to punish defection in specific instances. Second, disputes that reach international courts are much more public than ones resolved through bilateral bargaining. By publicizing disputes, DSMs increase the reputational consequences of state behavior and facilitate diffuse reciprocity. Hence, by clarifying obligations and publicizing disputes, DSMs increase both the immediate and the long-term costs of treaty violations. This should encourage treaty compliance and promote trade cooperation.

Note that this argument does not require that DSMs possess sanctioning powers, direct effect, or other enforcement mechanisms that would alter the anarchic nature of international politics. As Garrett and Smith (2002: 5) observe, "Simply painting scarlet letters on violators may be sufficient for tit-for-tat retaliation...to produce free trade." In other words, the mere provision of third-party review should promote trade cooperation.

Two other DSM features could similarly promote cooperation via reputational mechanisms. First, *standing tribunals* may be perceived as more independent and impartial than ad hoc panels, which are formed on a case-by-case basis via a possibly politicized selection process. If rulings by standing tribunals are viewed as less politicized and more legitimate, then defying such rulings may involve greater reputational costs. DSMs with standing tribunals could thus produce greater compliance with trade agreements. Second, *private standing*—which enables private parties to bring cases to court—should, by eliminating the gatekeeping role of governments, increase the number of cases heard by DSM tribunals. This should increase the likelihood that violations will be condemned and publicized, thus boosting the reputational costs of noncompliance. DSMs that provide private standing may thus also promote compliance with trade agreements.

Although the above arguments maintain that international institutions matter, they do not challenge the view that the international system is anarchic. Other arguments do, however. For example, some scholars claim that the normative force of international law can constrain governments to follow even rules that conflict with their self-interest. Chayes and Chayes (1993: 185) exemplify this approach, stating that “Treaties are acknowledged to be legally binding on the states that ratify them. In common experience, people, whether as a result of socialization or otherwise, accept that they are obligated to obey the law. So it is with states.” In other words, governments honor legally binding commitments because they are legally binding. If this is true, then DSMs that can issue *legally binding rulings* should elicit greater compliance with PTA rules than ones whose rulings are mere recommendations.

Other scholars argue that DSM rulings with *direct effect* can constrain governments against their wishes (Burley and Mattli 1993). In this case, the mechanism is not normative but

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juridical: rulings with direct effect can be invoked and enforced in domestic courts. Direct effect thus enables private parties to sue governments for treaty violations in domestic courts. If these courts side with DSMs against their governments, and if the latter are bound by domestic court decisions, then DSMs can constrain even recalcitrant governments. As this constitutes a potentially powerful mechanism for enforcing trade agreements, DSMs with direct effect should also promote intra-PTA trade liberalization.

In sum, different DSM characteristics may promote cooperation in different ways. Third-party review, standing tribunals, and private standing should affect behavior primarily by raising the reputational costs of treaty violations. Evidence that these DSM features matter would thus support the claim that institutions facilitate cooperation under anarchy but not the claim that they subvert anarchy. In contrast, to the extent that legally binding rulings and direct effect influence state behavior, they do so by legally constraining it. Evidence that these features matter would thus indicate that institutions can, to some extent, supplant anarchy with hierarchy.

Whether any of these DSM features in fact promote cooperation is unclear. A growing body of research examines the determinants of compliance with WTO panel decisions (Hudec 1993; Busch and Reinhardt 2000, 2003). However, while this research is useful in its own right, it does not directly address the question of whether DSMs promote trade cooperation because compliance with panel rulings is not the same as compliance with a treaty agreement to liberalize trade. If DSMs function properly, they will deter many potential treaty violations. Hence, in evaluating the performance of DSMs, the violations that do not occur are at least as important as responses to judicial decisions concerning the ones that do. Although it is difficult to measure non-events, we can measure the degree to which treaties accomplish their stated goal of trade

liberalization. We can then determine which, if any, DSM characteristics are associated with greater liberalization. The next two sections perform these tasks.

### Measuring Variation in Intra-PTA Liberalization

Because I employ Smith's (2000) measure of legalism, my sample of PTAs is similar to his and employs similar selection criteria. First, PTAs must be reciprocal, since arguments about DSMs are based on a logic of reciprocity. This excludes e.g. the EC's preferential arrangements with former colonies. Second, because I wish the PTAs to be comparable, the proposed scope of liberalization must be comprehensive rather than narrowly sectoral. This excludes e.g. the European Coal and Steel Community, which was designed to manage trade in these sectors. Third, I include only trade agreements and exclude ones, e.g. the Southern African Development Community, designed solely to promote joint infrastructural and other non-trade objectives. Fourth, I exclude framework agreements, e.g. the Latin American Integration Association, that are meant to facilitate trade negotiations but have no explicit policy targets. Finally, because I wish to examine trends in trade for a number of years before and after PTA formation—and because my data extend from 1950-2000—I include only PTAs that went into effect between 1958 and 1995. My sample, shown in Table 1, includes all PTAs that meet these criteria and for which data were available.<sup>3</sup>

I measure intra-PTA trade liberalization using a modified gravity model, the standard framework for assessing the effects of PTAs on trade. Gravity models predict dyadic trade on the basis of trading partners' GDPs, populations, geographic distance, adjacency, and other controls. Dummies indicating PTA membership are then included to measure the impact of PTAs. If the actual trade of PTA members differs from predicted trade, this difference is

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ascribed to the PTA. I employ dyadic imports rather than dyadic trade (exports + imports) as my dependent variable because the former better proxy the home country's trade policies.

To reduce omitted-variable bias, I estimate bilateral import shares rather than absolute values of bilateral imports. That is, instead of estimating  $\text{Imports}_{ijt}$  (country  $i$ 's imports from country  $j$  in year  $t$ ), I estimate  $\text{Imports}_{ijt} \div \text{Imports}_{it}$ , ( $j$ 's share of  $i$ 's total imports in year  $t$ ). This model specification reduces omitted-variable bias because  $i$ 's total imports control more accurately for  $i$ 's overall propensity to import than do variables such as  $i$ 's GDP and population (Haveman and Hummels 1998). This specification also eliminates the need to include  $i$ 's GDP, population, and other purely monadic home-country characteristics in the model.

I estimate an error-correction model of the following form:

$$\begin{aligned} \Delta \ln(\text{Import Share}_{ijt}) = & \beta_0 + \beta_1 \ln(\text{Import Share}_{ijt-1}) + \sum_{ijt} \beta_{ijt} \text{PTA}_{ijt} + \sum_{ijt} \beta_{ijt} \text{Pre-PTA}_{ijt} \\ & + \gamma \Delta \text{CONTROLS}_t + \lambda \text{CONTROLS}_{t-1} + \varepsilon_{ijt}, \end{aligned}$$

where  $\Delta \ln(\text{Import Share}_{ijt})$  is the annual change in logged import shares,  $\ln(\text{Import Share}_{ijt-1})$  is the one-year lag of logged import shares,  $\Delta \text{CONTROLS}_t$  is a vector of annual changes in control variables,  $\text{CONTROLS}_{t-1}$  is a vector of one-year lags of control variables, and  $\gamma$  and  $\lambda$  are vectors of coefficients for the first-differenced and lagged controls, respectively. Importing country  $i$  is always a PTA member, whereas exporting countries  $j$  comprise the universe of  $i$ 's PTA and non-PTA trading partners. Import shares cover no less than six years and no more than ten years before and after PTA formation. The resulting sample contains at least 16 and no more than 20 years for each PTA.<sup>4</sup>

I employ a first-differenced dependent variable both to guard against potential unit-root problems (Beck and Katz 2004)<sup>5</sup> and because my research question requires a focus on changes:

to know whether PTAs induce intra-bloc liberalization, we need to determine, not whether PTA members trade more with each other than with non-members, but whether the formation of a PTA causes members to trade more with each other than they did before. I employ an error-correction model because it imposes fewer restrictive assumptions than other time-series models and allows me to control for both the short-term and long-term effects of all controls (De Boef and Keele 2006). To estimate these effects, I include the lagged level of import shares as well as changes and lags of all controls.<sup>6</sup> I include standard gravity model controls—partner GDP and population, dyadic distance, and dummies for contiguity and common language—as well as political controls that previous research has shown to influence trade: shared alliance membership, militarized interstate disputes, shared GATT membership, and joint democracy.<sup>7</sup> I also include year dummies to control for unobserved time-specific influences on all countries.

To measure the impact of PTAs I include  $PTA_{ijt}$  dummies, which are coded 1 if  $i$  and  $j$  belonged to a PTA at time  $t$  and 0 otherwise. They are thus coded 1 for PTA partner dyads following PTA formation but 0 for these same dyads prior to that time and for all dyads containing non-PTA partners. Because I wish to measure variation across PTAs, I include a separate dummy for each arrangement. In separate analyses below, I define PTAs both regionally (e.g. NAFTA) and dyadically (e.g. US-Mexico). Because PTAs should boost trade among members, these dummies should be positively signed.

It is often noted that studies of PTA effects face endogeneity problems because states that trade more with one another—or liberalize trade faster—may be more likely to form PTAs. A positive PTA coefficient may thus reflect a higher propensity to trade rather than the effects of the PTA *per se*. The same problem arises when studying DSM effects: if DSM design is endogenous to prior trade policy preferences, then legalistic DSMs may be associated with

rapid trade liberalization even if the latter is merely a continuation of pre-DSM trends. To address this problem, and to assess the causal impact of DSMs, we need a dependent variable that captures the *additional* growth in trade that occurs after PTA formation.<sup>8</sup>

To obtain such a measure, I include another set of PTA dummies,  $Pre-PTA_{ijt}$ , which are coded 1 for eventual PTA partners *prior* to PTA formation and 0 otherwise. In other words, they are coded 0 for all non-PTA dyads, 0 for PTA partner dyads following PTA formation, and 1 for the latter dyads prior to PTA formation. If an endogeneity problem exists, these dummies will be positively signed.

To explicate my approach to measuring PTA effects, I first examine variation in the aggregate effects of trade blocs using regional PTA dummies. I correct for possible serial correlation by using robust-cluster estimators that correct for within-dyad correlation of residuals. Results are shown in Table 2.

Table 2 about here

Because the control variable results and model statistics are not of primary interest, I present them in the appendix. Suffice it to say that all controls are signed as expected and nearly all are highly significant. Turning to the variables of interest, column 1 presents results for the PTA dummies. 14 of the 34 dummies have significant positive coefficients, suggesting that many PTAs liberalize trade among members. However, column 2, which presents the pre-PTA coefficients, suggests a need for caution in interpreting this result. Of the 14 arrangements with significant positive PTA coefficients, 7 also have significant positive pre-PTA coefficients. Many blocs that liberalized intra-PTA trade were thus already liberalizing rapidly prior to PTA

formation. This suggests that endogeneity is a concern but does not preclude the possibility of marginal PTA effects, as many PTA coefficients are larger than pre-PTA ones.

To calculate the net effect of each PTA, I subtract the pre-PTA from the post-PTA coefficients. Results are shown in column 3. Note that the post-PTA estimates alone typically overstate (e.g. the EC) but sometimes understate (e.g. the EC-Turkey Association) the true impact of PTA formation. Both examples underscore the importance of examining marginal, rather than total, intra-PTA liberalization when assessing the effects of PTAs. To determine whether these marginal effects were significant, I performed Wald tests of the hypothesis that the PTA and pre-PTA coefficients are identical. Results are shown in column 4. In 13 cases, the tests indicate that bloc members increased trade with each other significantly faster following PTA formation. Hence, even after controlling for prior trade trends, about 40 percent of the PTAs led to significantly faster growth in intra-bloc trade. In 3 cases, PTA formation was associated with significantly slower trade growth. In the remaining 18 cases, PTAs had no significant effects. My results thus reveal considerable variation in the effects of PTAs.

Although the regional dummies provide a useful first cut at examining the effects of PTAs, I employ dyadic measures in my analysis of DSM effects because research shows that intra-PTA liberalization varies across dyads and is influenced by dyadic characteristics such as shared alliance membership (Mansfield and Bronson 1997). A dyadic analysis allows us to control for such influences. To measure dyadic variation in the effects of PTAs, I simply repeat the above analysis using dyadic dummies. That is, I generate pre- and post-PTA dummies for each intra-PTA dyad, then subtract pre- from post-PTA coefficients to obtain estimates of PTA effects on each dyad's trade. These estimates constitute a measure of dyadic PTA effects, *PTA Effect<sub>ij</sub>*, that I employ as my dependent variable in the next section. Because this procedure

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generates many coefficients (2560), I do not present them here. However, dyadic liberalization, like regional liberalization, varies considerably. I now examine the extent to which DSM design contributes to this variation.

### The Effects of Dispute Settlement Design

I examine the effects of DSM design on intra-PTA liberalization by regressing my dyadic measure of PTA effects against variables measuring DSM design. The latter are based on the DSM characteristics described earlier and presented in Table 1. Although Smith (2000) ranks DSM legalism ordinally, I do not employ an ordinal measure because (1) the relationship between DSM legalism and liberalization may not be linear, and (2) an ordinal measure would not tell us which aspects of DSM design promote trade liberalization. Instead, I employ a series of dummy variables based on the criteria in Table 1.  $DSM_k$  indicates whether PTA  $k$  has a legalistic DSM and is coded 1 for all DSMs that provide third-party review (legalism > “none”). When included on its own, DSM measures the effects of having any kind of minimally legalistic DSM.  $Binding_k$ ,  $Standing Tribunal_k$ , and  $Private Standing_k / Direct Effect_k$  indicate more legalistic DSM features. Binding is coded 1 for all DSMs that can issue legally binding rulings (legalism > “low”); standing tribunal is coded 1 for all DSMs with standing tribunals (legalism > “medium”); and private standing / direct effect is coded 1 for all DSMs that provide standing for private actors or direct effect (legalism = “very high”). I employ the “>” criterion because, as noted earlier, DSM characteristics are cumulative. Including all dummies simultaneously allows us to examine the impact of each DSM characteristic when all other characteristics have been controlled for. Hence, the DSM dummy indicates the effect of establishing third-party review; the binding dummy indicates the effect of legally binding decisions, and so on.

*Control Variables*

Research on PTAs identifies a number of factors that may affect intra-PTA liberalization. Because some of them may also influence DSM design, it is important to include them on the right-hand side to control for the endogeneity of DSMs to these factors. I first discuss controls that might influence DSM design, then discuss other controls.

Mattli (1999: 56) argues that PTAs are more likely to succeed when there is a dominant regional power that “serves as a focal point in the coordination of rules...[and] helps to ease distributional tensions through, for example, side-payments.” Regional economic asymmetries should thus, according to Mattli, promote intra-PTA liberalization. Others have argued, in contrast, that regional asymmetries may impede liberalization because disproportionate gains to the largest state may cause smaller states to oppose liberalization (Balassa 1961).

Regional economic asymmetries may also affect DSM design because dominant powers that can achieve their goals through power-based bargaining are likely to oppose legalistic DSMs (Smith 2000). Because regional asymmetries may affect both DSM design and intra-PTA trade, the omission of this variable could produce a spurious relationship between DSMs and trade liberalization. I thus include  $Asymmetry_k$ , which measures the asymmetry of member GDPs in PTA  $k$ . I employ Smith’s (2000) adjusted proportional asymmetry index:  $Asymmetry_k = (\sum_i x_i^2 - 1/N_k) / (1 - 1/N_k)$ , where  $x_i$  is the GDP share of PTA member  $i$  at the time of PTA formation and  $N_k$  is the number of members in PTA  $k$ . Following Smith, I treat the EC as a single actor in association agreements between it and other countries. High values indicate asymmetric GDP distributions, while low values indicate more equal distributions. As the effects of asymmetry on regional integration are disputed, this variable could be either positively or negatively signed.

Integrationist aspirations should also affect intra-bloc liberalization, albeit in possibly contradictory ways. On the one hand, governments that seek deeper integration may achieve it, in which case PTAs with ambitious objectives will exhibit more trade liberalization. On the other hand, deeper integration is multidimensional: it requires governments to liberalize not only trade but also capital and immigration flows, and sometimes to coordinate other policies as well. Such multidimensional integration may be politically more difficult than unidimensional trade liberalization. If so, then ambitious integration agendas will actually hinder trade liberalization.

Smith (2000) finds that integrationist aspirations also influence DSM design: because deeper integration tends to generate more disputes, governments seeking deep integration form legalistic DSMs to manage these disputes. Because integrationist goals may influence both DSM design and intra-bloc liberalization, the omission of these goals could produce a spurious relationship between DSM design and liberalization. I thus include Smith's (2000) measure of *Proposed Integration<sub>k</sub>*, which describes each PTA's stated goals (rather than actual success at achieving them). Proposed integration is coded 0 (low) if governments propose an FTA or customs union and 1 (high) if they propose an economic union or common market. Although one must be cautious about inferring government preferences from their words, the very explicit, formal, and public nature of PTA commitments makes them a reasonably credible proxy for government intentions (Mansfield, Milner and Rosendorff 2002). Because the effects of proposed integration are theoretically ambiguous, this variable could also be either positively or negatively signed.

Some scholars have argued that cooperation is harder in larger groups, both because the increased cost of monitoring and enforcement makes it more difficult for governments to pursue reciprocal strategies (Oye 1986) and because the increased heterogeneity of member preferences

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makes it more difficult to reach agreements (Haggard 1997). I thus include *Group Size<sub>k</sub>*, which measures the number of independent trade-policy actors in PTA *k*. For most PTAs, group size is simply the number of countries, but for the EC association agreements the EC is treated as a single actor. If the above arguments are correct, group size will be negatively signed.

Mattli (1999) argues that large potential gains from trade generate political support for regional integration. One major determinant of gains from trade is market size: larger markets create more demand for PTA members' exports and may allow members to exploit economies of scale. I thus include *Partner Export Share<sub>ij</sub>*, the log of PTA member *j*'s share of member *i*'s exports one year prior to PTA formation, to control for the importance of *j*'s export market to *i*. If larger markets promote integration, partner export shares will be positively signed.

I omit most variables from the gravity-model regressions (e.g. dyadic democracy, MIDs) because the first-stage regressions have purged my dependent variable of their effects. As expected, these variables are insignificant when included and do not affect my results. The one exception is alliances, which previous research has found to have larger positive effects on trade within intra-PTA than non-PTA dyads (Mansfield and Bronson 1997). Because the effects of alliances are smaller outside PTAs, the first-stage analysis (which combined PTA and non-PTA dyads) may not have adequately controlled for the effects of alliances within PTAs. I thus include dyadic alliance dummies in the second-stage regressions.

### *Analysis*

To control for the non-independence of intra-PTA dyadic observations, I employ robust standard errors clustered by regional PTA.<sup>9</sup> Results are shown in Table 3.

Table 3 about here

Model 1, which employs ordinary least-squares (OLS), examines whether DSMs of any kind promote trade liberalization by including only the DSM dummy and the controls. DSM is significant and positive, indicating that DSMs promote intra-PTA liberalization. Model 2 includes the dummies for additional DSM characteristics. The results indicate that, while third-party review promotes trade liberalization, additional DSM features do not. The DSM coefficient—which now measures the effects of third-party review—is very similar to that in model 1, indicating that the DSM effects in model 1 are almost wholly attributable to the existence of third-party dispute settlement panels. The coefficients for legally binding decisions, standing tribunals, standing for individuals and direct effect reinforce this point: none of these DSM characteristics has significant marginal effects on intra-PTA liberalization. Model 2 thus indicates that it is third-party review—and only third-party review—that matters. For this reason, subsequent tests employ only the DSM dummy.

The analysis already controls for the possible endogeneity of DSMs to prior trade-policy trends, regional economic asymmetries, and integrationist goals: the first has been subtracted from my dependent variable, while the latter two are included on the right-hand side. It remains possible, however, that both liberalization and DSMs are endogenous to some other variable(s) omitted from the model. This does not appear to be a concern, as the DSM dummy is completely uncorrelated with the error term ( $r = .00$ ). However, as a robustness check, model 3 performs an instrumental variables (IV) regression that treats DSM as an endogenous variable.<sup>10</sup> I instrument DSM with variables indicating the legal traditions of PTA members: specifically, my instruments measure the percentage of each PTA's members that have a British common law tradition, a French civil law tradition, a German civil law tradition, a Scandinavian civil law tradition, and a Socialist legal tradition.<sup>11</sup> These variables meet standard criteria for good instruments: they are

significant predictors of DSM but are not significantly related to either intra-PTA liberalization or the error terms. As column 3 shows, the IV procedure does not alter my results.

It is evident from Table 2 that many PTAs involve the EC and EFTA: in addition to the original EC-6 and EFTA, my sample includes four EC enlargements, six EC association agreements, six EFTA associations, and the EC-EFTA FTA. Readers may thus be concerned that my results are driven by the EC and EFTA cases. To address this concern, model 4 drops all of the EC enlargements, the EC associations, the EFTA associations, and the EC-EFTA FTA. This reduces the sample by 32 percent, and this sample change is, of course, non-random. It is thus striking that the DSM result remains almost identical to that from the baseline model 1.<sup>12</sup> Hence, although the European cases are numerous, they do not drive my results.

As a final robustness check, I examine the relationship between DSMs and trade liberalization both after and before PTA formation. If DSMs cause liberalization, then they should be associated with greater liberalization after but not before they are formed. If, on the other hand, the DSM results are spurious—caused by omitted variables that affect both DSM design and long-term trends in intra-bloc trade—then DSMs should be associated with greater liberalization both after and before DSM formation. Model 5 examines the “after” relationship by using the post-PTA coefficients as the dependent variable. This analysis reinforces my earlier results, although, not surprisingly, not controlling for pre-PTA trends leads to a larger estimate of DSM effects. Model 6 examines the “before” relationship by using the pre-PTA coefficients as a dependent variable. As expected, DSMs are not significantly related to pre-PTA liberalization. The fact that DSMs are associated with intra-bloc liberalization only after they are formed strongly suggests that they *affect* intra-bloc trade.<sup>13</sup>

I calculate the substantive effects of DSMs using the model-1 OLS coefficients because a Hausman test indicates that these estimates are consistent. The DSM coefficient of .021 indicates that the formation of a legalistic DSM raises annual growth in intra-PTA trade by  $\exp(.021)$ : roughly 2.1 percent per year or 23 percent over ten years. This is approximately the difference between the first ten years of the EC and ECOWAS and represents a substantial boost to intra-bloc trade. DSMs thus have substantively important as well as statistically significant effects on intra-PTA trade.

This result holds up to numerous other robustness checks: the addition and subtraction of other controls (including region and decade dummies), the use of trade shares instead of import shares as a dependent variable, the use of original IMF *Direction of Trade Statistics* data instead of Gleditsch's (2002) augmented dataset, the use of defense pacts instead of all alliances as a control, the use of an ordinal DSM measure, the expansion of the sample to include additional PTAs with more limited longitudinal data, and tests that ensure that the effects of DSMs do not depend on domestic regime type. All of these robustness checks are presented in the appendix. It is also worth noting that multicollinearity is low: only two variables (asymmetry and group size) are strongly correlated with one another, but their variance inflation factors (VIFs) are low (3.13 and 2.51) and my results are robust to the exclusion of these variables. VIFs for the other variables range from 1.24 to 1.61, indicating that multicollinearity is not a problem.

## Discussion

Although international DSMs matter, most DSM characteristics do not. Minimally legalistic DSMs that provide only third-party review promote trade to about the same extent as more legalistic ones that provide standing tribunals, legally binding decisions, private standing, and direct effect. These findings permit some informed speculation about why DSMs do and do

not promote trade. The insignificance of legally binding decisions and direct effect implies that DSM rulings do not constrain governments in the way that domestic court decisions bind domestic actors. Rather, DSMs promote trade primarily by facilitating decentralized enforcement of trade agreements. They do this by issuing authoritative public rulings on treaty compliance that (1) legitimize, and hence encourage, warranted retaliation against treaty violations, (2) stigmatize, and hence discourage, unwarranted retaliation (and the trade wars it could produce), and (3) raise the reputational costs of noncompliance. These conclusions accord with previous theoretical work on international institutions (Keohane 1984) as well as more recent empirical research that highlights the importance of reputation (Simmons 2000). They also concord with Busch and Reinhardt's (2000, 2003) claim that, while the convening of a WTO panel affects the level of concessions made in WTO disputes, the more legalistic rules adopted during the Uruguay Round have not. Hence, although DSMs facilitate enforcement, the enforcement itself appears to remain firmly in the hands of national governments.

Even if this is true, the insignificance of standing tribunals and private standing is puzzling. These DSM features, like third-party review, operate through a reputational mechanism. We thus need to ask why this mechanism works for third-party review but not for other reputation-based DSM features.

Standing tribunals will be more effective than ad hoc ones only if they are perceived as less politicized and more impartial. My results suggest that this is not the case, either because standing tribunals are more politicized, or because ad hoc ones are less politicized, than is often claimed. This does not mean that these tribunals are irrelevant: because they provide third-party review, standing tribunals—like ad hoc ones—promote trade cooperation. What is irrelevant is their standing as opposed to ad hoc status.

Private standing should promote treaty compliance only if it increases the number of cases brought to court. To understand why this might not occur, we must consider the politics of DSM enforcement. DSM rulings are typically enforced by sanctions from complainant states. They are thus enforced when complainant governments care enough about violations to bear the costs of imposing sanctions. This is presumably the case when domestic groups injured by violations (e.g. exporters) are politically influential. However, if these groups are influential, they should be able to pressure their governments to initiate cases even in the absence of private standing. Politically powerful groups should thus be equally represented with or without private standing. Conversely, if a group lacks the political influence to pressure its government to initiate a case, it should also be unable to persuade its government to employ sanctions on its behalf after it initiates and wins a case on its own. Politically weak groups should thus be equally *un*represented with or without private standing. This has implications not only for the enforcement but also for the initiation of cases. If weak groups rationally anticipate that their governments will not use sanctions on their behalf, they should not bother bringing cases to court at all. Hence, because DSM enforcement is a political rather than a technical decision, the legal enfranchisement of politically weak groups may not help these groups obtain greater concessions from trading partners and may not even increase the number of cases brought to court.

The insignificance of legally binding rulings and direct effect is easier to explain: the conditions that would allow these DSM characteristics to matter are simply not met. The legal status of DSM rulings should matter only if governments view legally binding rulings as more constraining or more legitimate than non-binding ones. This appears not to be the case. Likewise, direct effect will bind governments only if (1) national courts cooperate with international ones and (2) governments respect national court decisions. My results indicate that

at least one of these conditions is, in practice, not met. This is not wholly surprising. Even in the EC, national courts initially resisted the doctrine of direct effect before eventually participating in the enforcement of EC law. And, although we know little about the behavior of national courts in other PTAs with direct effect, it is certainly possible that courts in these arrangements have accorded primacy to domestic rather than PTA law. It is also possible that some member governments have simply disregarded unwelcome decisions by domestic courts. This does not mean that direct effect *cannot* promote treaty compliance: it can if conditions (1) and (2) are met. Understanding when this occurs, however, requires that we shift our focus away from international treaty provisions and toward the incentives of domestic courts and politicians to cooperate with international courts and one another.

### Conclusion

Do international institutions matter? If so, how? Although these questions are central to the study of international institutions, we still lack generally accepted answers to both. The evidence presented here indicates that some institutions matter more than others: third-party review promotes trade liberalization but more legalistic DSM features do not. This implies that institutions do not alter the state of anarchy but rather make the anarchic system work better.

This result has important implications, not only for international relations theory, but also for policy debates about international institutional design. For example, many governments seek to make the WTO's dispute settlement system (1) more effective at inducing compliance, and (2) more equitable for small, poor countries that cannot enforce WTO rulings on their own.

Although a review of reform proposals is beyond the scope of this paper, my results imply that some types of reform are more likely to succeed than others. The purely legalistic reforms of the recent Dispute Settlement Understanding (DSU)—such as the automatic adoption of legally

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binding panel rulings—are unlikely to improve either the efficacy or the equitability of WTO rulings. Recent studies on compliance with WTO rulings (Busch and Reinhardt 2000, 2003) and developing-country participation in the DSU (Bown 2005) support this point. On the other hand, reforms that build upon the decentralized nature of WTO enforcement—such as Mexico’s proposal to allow weak plaintiffs to auction off their retaliation rights to stronger countries—are more likely to produce the desired results (Bagwell, Mavroidis, and Staiger 2004). More generally, my finding that not all institutions matter implies that institutional design matters greatly. The most successful institutions are likely to be those that work with, rather than against, the anarchic nature of international politics.

## Notes

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<sup>1</sup> For further discussion of the steel dispute see Perdakis and Read (2005).

<sup>2</sup> This statement assumes that the good being provided is private (i.e. excludable), which is a reasonable assumption if the good is trade liberalization.

<sup>3</sup> Further discussion of the sample and research design are provided in the online appendix (<http://journalofpolitics.org>).

<sup>4</sup> Import, GDP, and population data are from Gleditsch's (2002) Expanded Trade and GDP Data Set. I truncate each PTA's temporal scope to obtain a balanced data set in which all PTAs are equally represented before and after PTA formation. For further discussion of this issue, see the appendix.

<sup>5</sup> When import share levels are regressed against lagged levels, the coefficient on the lagged variable is about .97. This raises unit-root concerns and suggests that a first-differenced model is most appropriate. Nonetheless, I obtain very similar results using an undifferenced model with a lagged dependent variable.

<sup>6</sup> Because distance, contiguity, and common language do not change over time, these controls are entered only as levels. Also, because my analysis of PTA effects involves a comparison of pre- and post-PTA trade—and because the PTA dummies are constant within each period—the PTA dummies are entered only as levels.

<sup>7</sup> For further discussion of these variables, see the appendix. Alliance data are from the COW2 dyadic alliance data set. Democracy scores are from the Polity IV data set. MID data from 1948-92 are from Russett and Oneal (2001), while data from 1993-2000 are from Ghosn and Bennett (2003). GATT membership data are from Reinhardt (1999).

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<sup>8</sup> And not just the additional trade. A before-after comparison of trade levels could generate the same spurious conclusions as a purely “after” focus on trade changes because an increase (decrease) in the level of trade could simply reflect a secular liberalizing (protectionist) trend that preceded, and was not caused by, the PTA. It is thus essential to focus on the change in the growth rate of trade.

<sup>9</sup> An alternative approach would be to employ a hierarchical model in which the level-1 variables are dyadic and the level-2 variables are regional. However, Kam and Franzese (2007) find that, while both approaches generate better standard error estimates than conventional ordinary least-squares, they do not differ appreciably from one another.

<sup>10</sup> The IV procedure first regresses DSM against all other regressors and exogenous instruments, then generates predicted values of DSM on the basis of this first-stage regression. It then regresses intra-PTA liberalization against predicted DSM and all other independent variables. Because predicted DSM is, by construction, uncorrelated with the residuals from the second-stage regression, the IV procedure should eliminate spurious relationships caused by omitted variables that influence both DSM and liberalization.

<sup>11</sup> Legal tradition data are from La Porta *et al* (1999).

<sup>12</sup> This remains true if I employ OLS rather than IV regression. I also obtain very similar results if I dummy the EC and EFTA cases rather than omitting them. Results of doing so are shown in the appendix.

<sup>13</sup> Note that model 6 directly estimates how much of the model-5 result is spuriously caused by factors that influence long-term trade trends, and that the model-6 coefficient is equal to the difference between the model-3 and model-5 estimates.

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Table 1. Legalism in Dispute Settlement Institutions						
Level of Legalism	Third-Party Review?	Legally Binding?	Standing Tribunal?	Private Standing?	Direct Effect?	Agreement and Date of Entry into Force
None	No					Australia-New Zealand Closer Economic Relations Trade Agreement (ANZCERTA, 1983) European Community-European Free Trade Association Free Trade Agreement (EC-EFTA FTA, 1973) EFTA Association Agreements (various dates) European Economic Area (1994) Mano River Union (1973)
Low	Yes	No				Association of Southeast Asian Nations Free Trade Agreement (AFTA, 1992) Caribbean Community (CARICOM, 1973) European Free Trade Association (EFTA, 1960) Gulf Cooperation Council (GCC, 1981) US-Israel Free Trade Agreement (1985)
Medium	Yes	Yes	No			EC Association Agreements (various dates) Southern Common Market (MERCOSUR, 1991) North American Free Trade Agreement (NAFTA, 1994)
High	Yes	Yes	Yes	No		East African Community (EAC, 1967) Economic Community of West African States (ECOWAS, 1975) West African Economic Community (CEAO, 1973)
Very High	Yes	Yes	Yes	Yes	Yes	Andean Pact (1969) Central American Common Market (CACM, 1961) Common Market for Eastern and Southern Africa (COMESA, 1995) European Community (EC, 1958) and successive enlargements (1973, 1981, 1986, 1995)

Table 2. Effects of Regional PTAs				
Name of Arrangement	(1) PTA <sub>ijt</sub>	(2) Pre-PTA <sub>ijt</sub>	(3) Net PTA Effect <sub>ijt</sub> (PTA <sub>ijt</sub> - Pre-PTA <sub>ijt</sub> )	(4) Wald Test H <sub>0</sub> : PTA <sub>ijt</sub> = Pre-PTA <sub>ijt</sub> P > F =
AFTA	.069 (.007)*	.034 (.011)*	.035*	0.0010
Andean Pact	.020 (.007)*	-.004 (.007)	.024*	0.0213
ANZCERTA	.090 (.011)*	.059 (.017)*	.031*	0.0000
CACM	.091 (.019)*	.015 (.010)	.076*	0.0002
CARICOM	.020 (.021)	-.015 (.017)	.035	0.2887
CEAO	.004 (.007)	.025 (.013)	-.021	0.0844
COMESA	.009 (.003)*	.005 (.002)*	.004	0.3020
EAC	.121 (.068)	.058 (.042)	.063	0.4702
EC6	.058 (.006)*	.034 (.010)*	.024*	0.0153
EC-Bulgaria	.013 (.007)	-.001 (.009)	.014*	0.0221
EC-Cyprus & Malta	.009 (.007)	.009 (.007)	.000	0.9998
EC-Hungary	.010 (.007)	-.003 (.008)	.013	0.0566
EC-Poland	.009 (.009)	.017 (.011)	-.008	0.3706
EC-Romania	.022 (.011)*	-.012 (.006)	.034*	0.0000
EC-Turkey	.009 (.014)	-.032 (.011)*	.041*	0.0121
EC-EFTA	.014 (.004)*	.002 (.004)	.012*	0.0000
EC Enlarge 1	.026 (.005)*	.001 (.004)	.025*	0.0000
EC Enlarge 2	.011 (.011)	-.005 (.007)	.016*	0.0144
EC Enlarge 3	.026 (.007)*	.004 (.005)	.022*	0.0000
EC Enlarge 4	.000 (.006)	.022 (.003)*	-.022*	0.0000
ECOWAS	.000 (.004)	.003 (.004)	-.003	0.5294
EEA	-.006 (.005)	.007 (.005)	-.013*	0.0032
EFTA	.032 (.004)*	.006 (.004)	.026*	0.0000
EFTA-Bulgaria	-.021 (.004)*	-.009 (.012)	-.012	0.4056
EFTA-Hungary	-.025 (.008)*	-.011 (.011)	-.014	0.4622
EFTA-Israel	-.012 (.009)	.006 (.013)	-.018*	0.0425
EFTA-Poland	-.007 (.016)	.003 (.014)	-.010	0.7150
EFTA-Romania	-.015 (.005)*	-.015 (.011)	.000	0.9824
EFTA-Turkey	-.022 (.002)*	-.016 (.005)*	-.006	0.3425
GCC	.002 (.013)	-.010 (.014)	.012	0.5563
Mano	-.001 (.010)	-.007 (.011)	.006	0.6358
MERCOSUR	.049 (.022)*	.050 (.015)*	-.001	0.9923
NAFTA	.083 (.016)*	.063 (.022)*	.020	0.0896
US-Israel	.047 (.019)*	.073 (.034)*	-.026	0.0798

Dependent Variable:  $\Delta \ln(\text{Import Share}_{ijt})$  \*p<.05 Robust (dyad-clustered) standard errors in parentheses

Table 3. DSM Legalism and Intra-PTA Trade Liberalization

Explanatory Variable	(1) OLS	(2) OLS	(3) IV	(4) IV, No EC/EFTA	(5) IV, Post-PTA	(6) IV, Pre-PTA
DSM <sub>k</sub>	.021* (.005)	.017* (.006)	.027* (.009)	.021* (.008)	.037* (.018)	.010 (.013)
Binding <sub>k</sub>		-.000 (.009)				
Standing Tribunal <sub>k</sub>		.009 (.010)				
Private Standing <sub>k</sub> / Direct Effect <sub>k</sub>		.003 (.008)				
Asymmetry <sub>k</sub>	-.023 (.012)	-.022 (.016)	-.021 (.012)	-.025 (.017)	-.035* (.014)	-.014 (.007)
Proposed Integration <sub>k</sub>	-.026* (.005)	-.034* (.006)	-.028* (.005)	-.026* (.010)	-.021* (.007)	.007 (.005)
Group Size <sub>k</sub>	-.000 (.001)	-.000 (.001)	-.000 (.001)	-.000 (.001)	-.000 (.001)	.000 (.000)
ln(Partner Export Share <sub>ij</sub> )	-.001 (.004)	-.001 (.004)	-.001 (.004)	-.004 (.007)	.033* (.003)	.034* (.003)
Alliance <sub>ij</sub>	.025* (.009)	.026* (.009)	.027* (.008)	.028* (.013)	.009 (.008)	-.017* (.005)
Constant	.014 (.016)	.017 (.018)	.009 (.016)	.014 (.018)	-.009 (.023)	-.018 (.015)
Observations	1280	1280	1280	868	1280	1280
p > F	.0000	.0000	.0000	.0000	.0000	.0000
R-Squared	.05	.05	.05	.05	.26	.32
Instrumented (Models 3-6): DSM <sub>k</sub>						
Instruments (Models 3-6): Common Law <sub>k</sub> , French Law <sub>k</sub> , German Law <sub>k</sub> , Scandinavian Law <sub>k</sub> , Socialist Law <sub>k</sub> , Other Explanatory Variables						
Dependent Variable: PTA Effect <sub>ij</sub> (Models 1-4), Post-PTA <sub>ij</sub> (Model 5), Pre-PTA <sub>ij</sub> (Model 6)						
*p<.05 Robust (PTA-clustered) standard errors in parentheses						