How Important is State Enforcement for Trade?

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According to conventional wisdom, state-provided contract enforcement is critical to an expansive, growing trade. This paper estimates state enforcement's impact on international trade for one hundred and fifty-seven countries over the last half a century. I find that state enforcement increases trade between nations by about fifteen to thirty-eight percent. This effect is significant though modest compared to intuition about the importance of government enforcement, the long-run growth of trade, and the estimated effect of trade's other determinants. Thus, while state enforcement appears to enhance trade, it does so less impressively than its status as essential for flourishing trade tends to suggest. (*JEL* F10, F53, F55)

Commerce . . . can seldom flourish . . . [where] the faith of contracts is not supported by the law, and . . . [where] the state is not supposed to be regularly employed in enforcing the payment of debts from all those who are able to pay.

—Adam Smith (1976 [1776], p. 910).

1. Introduction

Is state-provided contract enforcement important for trade? Most economists certainly think so. Many would go as far as to say that a high volume of growing trade *requires* state enforcement. The rationale underlying this conventional wisdom is highly sensible. Formal enforcement pulls individuals out of anarchy. In doing so, it gives anonymous and distantly located strangers security to contract major transactions without fear of fraud.[1] The importance of state enforcement seems so sensible as to nearly place it beyond the realm of propositions deserving empirical investigation. This likely explains why no one has econometrically examined the effect of state-provided contract enforcement on trade. But do we really know that state enforcement is so important for trade?

The international arena provides an excellent ground to investigate this question. With the exception of a multinational treaty known as the New York Convention, international commerce is conducted in the absence of formal contract enforcement. Private international arbitration associations govern commercial disputes between international traders.[2] No supranational authority exists for this purpose.[3] In fact, there is not even a formal, universal body of international commercial law such an authority could use to adjudicate transnational commercial agreements if one existed (see, for instance, Oye, 1986, p. 1; Plantey, 199, p. 69). Despite the lack of formal global governance, international trade is large and growing rapidly. Today, it accounts for some twenty-five percent of global economic activity. Since 1960, the real value of global exports has increased thirteen-fold; and

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1. A burgeoning literature finds that self-enforcing mechanisms such as reputation can support *low* volumes of trade between relatively small populations but concludes that such arrangements cannot support growing, high volumes exchange among large, diverse populations. See for instance, Greif (2002). Against this view, see leeson (forthcoming).

2. Casella (1996) considers arbitration's connection to the growth in international trade, while Mattli (2001) examines various forms of arbitration in response to differing needs of international traders.

3. The United Nations International Court of Justice settles disputes (for instance, regarding the interpretation of treaties) between states, not private individuals. Likewise, the European Court of Justice, which applies only to members of the European Union, is designed to adjudicate disputes between member countries concerning "European Community law." The United Nations International Criminal Court applies to private individuals but deals only with international criminal matters—not commercial ones.

in 2003, world exports of merchandise and commercial services exceeded $9.5 trillion (World Bank, 2004).

In 1958, members of the global community introduced a multinational treaty called the United Nations New York Convention on the Recognition and Enforcement of Foreign Arbitral Awards, creating state enforcement for private commercial agreements in the international arena. International arbitration and the New York Convention (NYC) are connected in the following way. Private parties to international commercial contracts agree to have their disputes settled by arbitration associations. Since these associations are private, they cannot formally compel losers to comply with their decisions. However, under the terms of the NYC, winners can have their arbitral decisions enforced by losers' governments if these governments are members of the convention.

A simple example illustrates how the NYC provides state enforcement for international traders. Suppose a Bulgarian importer contracts with an Argentinian exporter for a shipment of grade A quality leather. When the shipment arrives, the Bulgarian finds that the leather is only of B quality, though his trade partner insists it is A. Before 1958 these traders would have privately settled their dispute through an international arbitration association. If the arbitrator decided the Argentinian did not fulfill his end of the contract and ordered him to pay, the Bulgarian had no means of compelling payment should the Argentinian refuse. However, the introduction of the NYC in 1958 changed this. Traders still use private arbitration to settle disagreements. But now, under the NYC, if the Argentinian refuses to pay, the Bulgarian can call on the Argentinian government, which has signed the NYC, to enforce his arbitral award.

The NYC provides a straightforward way to empirically evaluate the impact of state enforcement on trade. I use a gravity model to examine the bilateral trade flows of one hundred and fifty-seven countries over the last half a century. If state enforcement increases trade, I expect members of the NYC to have higher trade than non-members. As it turns out, they do—but less dramatically than the wisdom that state enforcement is essential for trade to flourish suggests. I find that state enforcement increases trade between nations by about fifteen to thirty-eight percent. This effect is significant, but modest compared to intuition about the importance of government enforcement, the long-run growth of trade, and the estimated effect of trade's other determinants.

This paper is most closely connected with important work by Anderson and Marcoullier (2002), which considers how the quality of countries' domestic institutions, and in particular of their courts, impacts international commercial contract enforcement. These authors do not mention the NYC. But in suggesting the importance of domestic court quality for private international contract enforcement, their analysis implicitly assumes that domestic courts are indeed capable of enforcing international commercial agreements. My paper complements this research by examining the explicit institutional mechanism through which this is made possible: the NYC.

2. International Arbitration and the NYC

Private international arbitration is the dominant means of settling disputes arising from international transactions (see, for instance, Schultsz and van den Berg, 1982; Mentschikoff, 1961; Craig et al., 2000; Salacuse, 1991).[4] An estimated ninety percent of all international commercial contracts include arbitration clauses (see, for instance, Volckart and Mangels, 1999; Casella, 1996). As one leading international practitioner put it, " in today's world the dispute resolution mechanism will invariably be arbitration" (Aksen, 1990, p. 287).

International traders use arbitration to settle disputes for several reasons. First, they are interested in avoiding the home court of the other party. Parties fear being subjected to unknown laws, having a decision rendered in an unknown language via unknown procedure, being subjected to law or procedure they disagree with, or they fear that a state court will favor their adversary if he is a citizen of that nation.[5] Second, there is an important question as to which state court, if either, has jurisdiction in the matter of a dispute. Competing claims to jurisdiction are problematic.[6] But equally

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4. For classic treatments of international arbitration within the legal literature, see, David (1985) and Trakman (1983).

5. Issues of conflicting law may be especially problematic when one of the parties involved comes from a common law legal system and the other from a civil law system.

6. As Rusk (1984, p. 19) has pointed out, even in some cases where jurisdiction seems clear, "some countries are strongly committed to the idea that such disputes should be settled within the jurisdiction of their own national court". Private international law contains conflict of law principles meant to deal with questions of jurisdiction. However, it consists merely of differing national laws regarding declarations of jurisdiction in certain

troublesome is the unwillingness of either state court to decide a dispute when neither feels equipped to adjudicate an international matter. Third, the decisions of state courts regarding matters of international commerce are difficult to enforce (Dezalay and Garth, 1996, p. 6). In some cases, state courts do not recognize foreign judgments. Even when they do, it is difficult to seize the assets of the loser if he is not from the country where the court's decision is made.[7]

International arbitration overcomes these problems by "delocalizing" dispute resolution.[8] Under arbitration, parties may choose the variables concerning the adjudication of their disputes. These variables include the site of dispute resolution and the law that will govern their dispute, which ranges from any national law to the evolved customs called the *lex mercatoria* (law merchant) that through common practice and usage have come to govern international commerce.[9] Parties may also select the number of arbitrators who will decide their dispute, the identity of these arbitrators, or the process by which they are appointed. If parties do not agree on one or more of these

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cases, which may come into conflict with the competing claim of another nation to have right of jurisdiction in that case. The Hague Conference on Private International Law and more recently UNCITRAL have contributed to the harmonization of conflict rules in an effort to mitigate this problem.

7. Two other benefits of international arbitration are its speed, enabled by an extremely limited capacity to appeal, and the privacy it affords. Arbitration institutions pride themselves on keeping both disputes brought to their attention, as well as the decisions in such disputes, private. Indeed, this is part of the problem in obtaining specific data regarding international arbitration. A concern for privacy in the process of dispute resolution is especially important to firms that keep closely guarded trade secrets they do not wish to be made public.

8. This useful terminology comes from Cutler (2003).

9. For discussions of the modern law merchant within the legal literature, see, among others, Berman and Kaufman (1978), Cremades and Plehn (1983–1984), Carbonneau (1984), and Schmitthoff (1961). Leeson (2006), Benson (1989), and Volckart and Mangles (1999) consider the historical roots of modern international arbitration in the medieval *lex mercatoria*. For a game-theoretic treatment of how international traders secured cooperation in the context of the medieval law merchant, see, Milgrom, North, and Weingast (1990). On the role that merchant guilds played in the expansion of international trade within the medieval law merchant system, see, Greif, Milgrom, and Weingast (1994). Bernstein (1992, 2001), and Benson (1995) examine the use domestic arbitration within the United States. Dixit (2003) considers the general role of arbitration in providing improved information, though he is not concerned with international arbitration. Also, for an analysis of the market's ability to provide the optimal level of adjudication, see Landes and Posner (1979).

variables, they may stipulate that a neutral third party—the arbitrators of their case, for instance—decide these items for them.[10]

There are hundreds of international arbitration forums globally (Graving, 1989, p. 328). The largest of these include the International Chamber of Commerce's (ICC) International Court of Arbitration, the London Court of International Arbitration (LCIA), the American Arbitration Association's International Center for Dispute Resolution (ICDR), and the Arbitration Institute of the Stockholm Chamber of Commerce. The biggest and most significant of these is the ICC.[11]

The community of international arbitration users is large and diverse. Between 1923 and 1976, three thousand requests for international arbitration were submitted to the ICC—an average of about fifty-seven cases per year over the period. Between 1976 and 1998, the ICC received its ten thousandth case—an average of over three hundred and eighteen cases per year over the period (Craig et al., 2000, p. 2). In 2000, the ICC arbitrated a caseload involving nearly fifteen hundred parties from close to one hundred and twenty countries worldwide (ICC Bulletin, 2002).[12] The sums at stake between these parties are substantial. Table 1 identifies the amounts in dispute in international arbitration through the ICC from 1988–1998 and 2001.

The sums in contention typically rise throughout the arbitration process, so this table tends to understate the value of these disputes. Furthermore, the cases that come before international arbitration forums without specified

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10. In recent years, both UNCITRAL and UNIDROIT have contributed to the harmonization of international commercial law and arbitration practices by encouraging inter state cooperation toward this end, drafting model laws regarding international commerce that states may adopt, drafting model arbitration clauses that may be used by parties to arbitration, drafting arbitration rules that may be used in *ad hoc* arbitration procedures, and other such efforts.

11. In addition to institutional arbitration conducted by such forums, parties to international trade may also use *ad hoc* arbitration, which is based on the same general principles as institutional arbitration, but is generally more open ended with respect to procedure. *Ad hoc* arbitration is organized and administered by individuals independent of any institutional arbitration forum. Because of its nature, data regarding *ad hoc* arbitration and specific information regarding the details of its operation and outcomes are unavailable.

12. Although I do not discuss it here, governments and government entities may also resolve disputes via international arbitration. These cases, however, comprise only a very small percentage of international arbitration users. In 2000, for instance, only 5% of all parties to international arbitration through the ICC, and 12% of its cases, involved state or parastatal entities (ICC Bulletin, 2001).

Table 1. Amounts in Dispute Through the ICC

	1988–1991 (%)	1992–1995 (%)	1996–1998 (%)	2001 (%)
<$50K	4.9	4.5	3.1	1.1
$50K–$200K	13.1	11.1	12.1	9.8
$200K–$1M	25.3	24.0	23.1	22.0
$1M–$10M	33.1	36.7	34.6	31.4
>$10M	11.3	14.7	16.0	22.6
Amount not indicated	12.3	9.1	11.0	13.1

Notes: Average per period. Source: Craig et al., 2000; ICC Bulletin, 2002.

amounts are often the largest, some in excess of $1 billion.[13] Although the typical case brought before international arbitration involves a substantial sum of money, the value of trade arbitrated relative to the total value of international trade is very small since only a small percentage of trades results in disagreement.

The NYC makes private international arbitral awards enforceable in state courts and for this reason is considered the "cornerstone of current international commercial arbitration" (van den Berg, 1981, p. 1). Between 1959 and 2003, one hundred and thirty-four nations signed this treaty. Its terms are simple and stipulate that signing nations agree to recognize and enforce international arbitral decisions brought to them for enforcement by parties to international arbitration.[14] If the losing party to arbitration does not comply with the arbitrators' decision, the winning party may have this decision enforced by the loser's state court if the loser's state has signed the NYC. The NYC provides the formal teeth to the otherwise private, informal process of commercial contract dispute resolution in the international sphere.

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13. This pattern holds for the other major international arbitration institutions as well. For instance, the ICDR, a much smaller international arbitration forum than the ICC or the LCIA, arbitrated a caseload worth more than $10 billion involving parties from 63 countries across the globe (ICDR, 2002). See also, LCIA (1998, 1999, 2000, 2001, 2002).

14. The NYC allows nations to sign subject to two reservation conditions: (1) The reciprocity condition—states are not required to enforce arbitral awards rendered in nations that are not also signatories of the treaty (Article 1(3)). Sixty-eight nations have signed subject to this condition. (2) The commercial reservation—states are not required to enforce arbitral awards related to noncommercial matters, with the commerciality of a matter being defined by the state's national law (Article 1(3)). Forty-three nations have signed subject to this condition.

The ICC estimates that ninety percent of its arbitral awards are complied with voluntarily (Craig et al., 2000, p. 404). This provides an indirect estimate ($\approx 10\%$) of the number of cases that seek enforcement under the NYC, but cannot be taken as a measure of the (un)importance of state enforcement for trade. The overwhelming extent of voluntary compliance reported by the ICC may simply be evidence that formal enforcement provided by the NYC is working precisely as it was designed to. Traders' knowledge that refusal to comply with an arbitral award will result in state enforcement under the NYC compels them to voluntarily comply at the arbitration stage. In other words, as a result of the NYC, voluntary compliance always occurs in the "shadow of the state." Establishing the importance of state enforcement therefore requires an approach that econometrically isolates the impact of formal enforcement on trade.

3. Empirical Strategy

To investigate the importance of state enforcement for trade, I use the most conventional and widely accepted empirical approach (and data, as I discuss below) for determining the impact of various factors on international trade. I follow Rose (2004a) who employs a gravity model of bilateral trade, which explains trade using the distance between countries and their joint income. I want to control for as many factors affecting trade as possible, both "natural" and "man-made," so I augment the basic gravity equation with additional variables. These variables include: culture (e.g., if a pair of countries share the same language), geography (e.g., whether either country is landlocked), history (e.g., whether one colonized the other, whether both were colonized by the same country, etc.), and membership in trade agreements (e.g., if the two countries are members of the same regional trade agreement, if one or both are members of the WTO, or one country was a GSP beneficiary of another country and vice versa), which might be important in accounting for the volume and pattern of exchange. Appendix B describes my variables comprehensively.

I estimate the augmented gravity equation:

$$\log(X_{ijt}) = \alpha + \beta_1 BothinNYC_{ijt} + \beta_2 OneinNYC_{ijt} + \gamma \mathbf{Z}_{ijt} + \varepsilon_{ijt} \quad (1)$$

where X_{ijt} is the average value of real bilateral trade between i and j at time t, β_1, and β_2 are my parameters of interest, and ε_{ijt} is a random

error term. $BothinNYC_{ijt}$ is a binary variable that is 1 if both i and j are members of the NYC at time t, and 0 otherwise. $OneinNYC_{ijt}$ is a binary variable that is 1 if either i or j is a member of the NYC at time t, and 0 otherwise. β_1 measures the effect of the NYC on trade when both trading partners are members of the convention and β_2 measures the effect of the NYC on trade when one country is a member and the other is not. I search for the effect of state enforcement using variation across countries, since not all countries are members of the NYC, and across time, since membership grows over the sample. If state enforcement is highly important for trade, β_1 and β_2 should be positive and large relative to the nuisance coefficients γ on the variables I use to condition the gravity model. These variables are given by the vector of controls $\mathbf{Z}_{ijt}$.

I use ordinary least-squares (OLS) with standard errors that are robust to clustering by country-pairs to estimate the gravity model. I also use year-specific fixed effects to account for factors that are constant across countries but vary across time, such as oil shocks, the global business cycle, etc.[15] Anderson and van Wincoop (2003) suggest that it is important to include country fixed effects to account for multilateral trade resistance between countries, which, if left unaccounted for, may bias gravity model estimates. Thus, in addition to year fixed effects, I also add country fixed effects to account for multilateral trade resistance between countries and to capture any other unobservable features of countries that might affect trade. I use a single set of country fixed effects for this purpose instead of separate importer and exporter effects. In doing this, I follow Rose and van Wincoop (2001) and Rose (2004a) who employ the gravity model for purposes similar to my own.[16] Finally, since multilateral resistance may vary over time for each country, I also try estimating a specification that includes time-varying country fixed effects. My full panel covers 50 time periods for more than 150 countries. Comprehensive time-varying fixed effects would thus require about 8000 new dummy variables. To avoid the computational difficulties of this, I define only five time periods for this specification: 1950–1959, 1960–1969, 1970–1979, 1980–1989, and 1990–1999. Sections 7 and 8 perform sensitivity analyses for my benchmark regression. These

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15. The Hausman test rejects the use of random effects. $\chi^2 = 6607.82$.

16. For a discussion of the appropriateness of a single set of country fixed effects vs. separate importer and exporter fixed effects, see, Rose (2004b).

examine the potential for a lagged effect of NYC membership, address the is-
sue of intrafirm trade, investigate whether the NYC's impact on trade may be
development-dependent, and include a number of other robustness checks.

4. Data

Data for my regressand (the natural logarithm of trade) are from Rose
(2004a) who uses the IMF's "Direction of Trade" data set. My sample covers
bilateral merchandise trade for one hundred and fifty-seven countries over
fifty years between 1950 and 1999. A list of these countries is presented in
Appendix A. Using these data, Rose creates an average value of bilateral
trade between a country pair by averaging the four available measures (ex-
ports from country 1 to country 2, imports into country 2 from country 1,
etc.). These values are deflated by the American CPI for all urban consumers
(1982–1984 = 100).

My data for real GDP and GDP per capita (in constant US dollars) are
from the Penn World Table v. 6.1 and cover the years from 1950 through
1999. Data for my variable of interest—membership in the NYC—are from
the Stockholm Chamber of Commerce (2004), which reports the list of
109 member countries from the convention's first effective year in 1959
through 1999. Six countries joined the NYC in its first year: Egypt, France,
Israel, Morocco, Syria, and Thailand. By 1965, twenty-nine sample countries
had signed the NYC, including Finland, Germany, France, India, Japan,
the Netherlands, Norway, Russia, and Switzerland. Over the next decade,
fifteen additional countries joined, including some "big players," such as
the United Kingdom (in 1970) and the United States (in 1975), bringing
the total number of nations covered by the NYC to 44. By 1985, fifty-nine
nations were members of the convention; and between 1986 and 1995, thirty-
eight new countries joined (including Canada)—the largest number of new
additions in the NYC's history. In 1999, the total number of countries that
had ratified the convention stood at one hundred and nine. A complete list
of NYC signatories and the years they joined is presented in Appendix A.

Data for my remaining regressors are from Rose (2004a) who draws on a
number of standard sources to construct these variables. Data from the CIA
World Factbook are used to create controls relating to land area, landlocked
and island status, shared border, language, and colonization. Data regarding
whether a pair of countries was part of a currency union are from Glick and
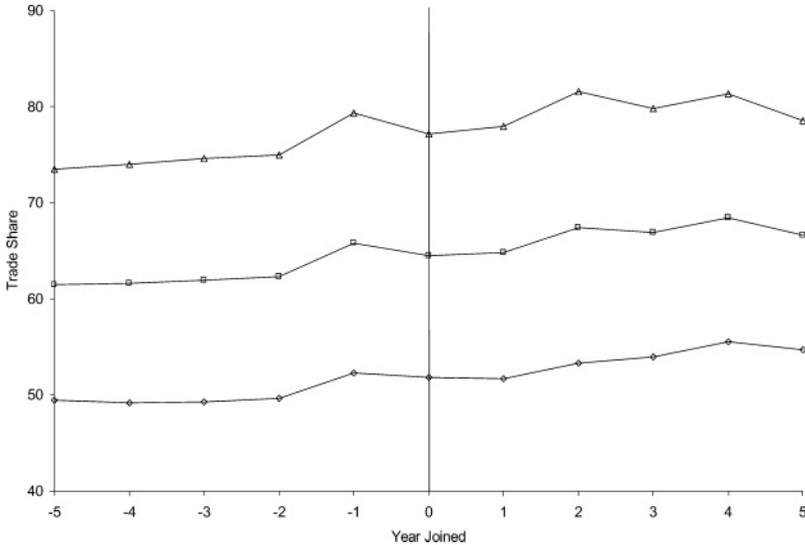Rose (2002). Data used to create an indicator of regional trade agreements

Figure 1. Trade Share within ±5 Years of Joining the New York Convention.
Notes: Middle line: simple average of 50 NYC members' trade shares
([exports+imports]/GDP) in the 10 years surrounding the dates they joined. Top
line: confidence interval of +2 SD. Bottom line: confidence interval of –2 SD. The
vertical line separates the five years before NYC ratification from the five years
after NYC ratification. Trade share data cover 1960–1998 (i.e., NYC members that
joined between 1965 and 1993).

come from the WTO and include: ASEAN, EEC/EC/EU; US-Israel FTA;
NAFTA; CARICOM; PATCRA; ANZ-CERTA; CACM, SPARTECA, and
Mercosur. Finally, data from the WTO website and the UN's publication,
the *Operation and Effects of the Generalized System of Preferences* (1974,
1979, 1984), are used to construct variables for membership in GATT/WTO
and the GSP, respectively.

5. The NYC and Trade at a Glance

A casual look at the data suggests that state enforcement has had a
positive, but small, impact on trade. Figure 1 presents a graphical 'event
study' that examines the trade share ([exports + imports]/GDP) of fifty
countries that joined the NYC five years before and after they joined the
convention. I use trade share data from *World Development Indicators* (2004)
for the years 1960 through 1998, and look only at those countries for which
trade share data are available for all ten years surrounding the date of NYC

ratification. The middle line depicts the average trade share in each year for these countries. The horizontal lines above and below show a confidence interval of ± 2 SD. The vertical line at the center of the graph separates the five years before countries joined the NYC from the five years after they joined. Average trade shares in the half-decade after countries join the NYC are slightly higher than in the half-decade before they join. Figure 1 suggests that state enforcement has enhanced trade, but very modestly.

6. Benchmark Results

Table 2 presents the results of my regressions that attempt to econometrically isolate state enforcement's impact on trade. My benchmark specification is the augmented gravity model estimated using ordinary least-squares with country and year fixed effects and robust standard errors over the entire sample. Column 1 reports these findings. My results for the variables that Rose (2004a) uses are similar to those he finds and those found elsewhere in the literature. Economically larger and richer countries trade more, while those that are further apart trade less. Additionally, countries that are members of the same regional trade agreement trade more, as do countries that have a common language, share a border, share a currency, or share colonial history. Also, like Rose, I find that membership in the Generalized System of Preferences (GSP) has a large positive effect on trade and that membership in the WTO/GATT has an economically weak impact.

What about the NYC? In my benchmark specification, the NYC increases trade $(e^{0.325} - 1 \approx)$ 38 percent when both countries are members and 15 percent when only one is. The lower bound of the 95 percent confidence interval for these estimates suggests that the NYC increases trade 28% and 8%, respectively. To put this in perspective, the NYC has roughly the same impact on trade as sharing a common language. This effect is significant, but modest compared to intuition about the importance of state courts, the long-term growth of trade, and the effect of many of the other variables impacting trade. Membership in a regional trade agreement, for example, increases trade 164 percent. Similarly, sharing a currency increases trade nearly 200 percent. My estimations evidently can deliver positive, economically large effects on trade. But state enforcement is not one of them.

When I incorporate time-varying country fixed effects in column 2, the NYC's impact on trade becomes small and negative. One explanation for this is that NYC membership is endogenous. In an effort to jump-start their

Table 2. The Effect of the New York Convention on Trade

	1 Country and Year Fixed Effects	2 Time-Varying Country Fixed Effects
Both in NYC	0.33	−0.26
	(0.04)	(0.03)
One in NYC	0.14	−0.14
	(0.03)	(0.03)
Both in GATT/WTO	0.13	−0.11
	(0.05)	(0.05)
One in GATT/WTO	0.03	−0.12
	(0.05)	(0.05)
GSP	0.68	0.67
	(0.03)	(0.04)
Log distance	−1.28	−1.28
	(0.02)	(0.02)
Log product real GDP	0.28	0.76
	(0.06)	(0.01)
Log product real GDP p/c	0.92	0.32
	(0.06)	(0.02)
Regional FTA	0.97	1.01
	(0.14)	(0.15)
Currency union	1.09	0.97
	(0.14)	(0.13)
Common language	0.32	0.33
	(0.05)	(0.05)
Land border	0.36	0.39
	(0.12)	(0.12)
Number landlocked	0.22	−0.67
	(0.45)	(0.04)
Number islands	0.96	−0.04
	(0.31)	(0.05)
Log product land area	0.32	0.03
	(0.04)	(0.01)
Common colonizer	0.65	0.67
	(0.07)	(0.07)
Currently colonized	0.69	0.45
	(0.34)	(0.33)
Ever colony	1.27	1.32
	(0.12)	(0.12)
Common country	−0.54	−0.21
	(0.40)	(0.38)
Observations	175,508	175,508
R^2	0.73	0.74
RMSE	1.69	1.66

Notes: Regressand: log real trade. OLS with country and year effects (column 1) and time-varying country effects (column 2) (intercepts not reported). Robust standard errors (clustering by country-pairs) in parentheses.

trade, countries that are experiencing trade difficulties are more likely to join the NYC than countries that are not. Baier and Bergstrand (2004), for instance, argue that membership in free-trade agreements is endogenous. This may also apply to the NYC.

In principle, it is possible to correct for endogeneity using instrumental variables. Like Rose (2004a), however, in practice I find it difficult to find variables that are reasonably well correlated with NYC membership but are not also highly correlated with trade. I have experimented with the same instrumental variables as did Rose—measures of democracy and freedom, as well as some of my own, including legal origin and distance from Paris (where the ICC is located)—but confront the same problem that he did: poorness of fit in the first stage. My instrumental variables are not well correlated with NYC membership.

Fortunately, I can still address the question of how potential endogeneity may be influencing my results by examining how economic and trade considerations may impact countries' decisions to join the NYC. To do this, I consider the relationship between whether or not a country joins the NYC during a 5-year period and its (log) GDP per capita, GDP per capita growth, (log) trade share, and trade share growth during this period. Table 3 presents the results of these estimations.

The most striking feature of table 3 is the third column, which reports the relationship between nations' (log) trade shares during a 5-year period and whether or not they joined the NYC during this period. In six of the eight time periods considered here, the relationship between trade share and NYC membership is negative; and in every case where this relationship is significant, it is negative. Consistent with the "jump-start hypothesis" mentioned above, this implies that countries struggling with trade are more likely to join the NYC than countries that are not. Since it is not possible to correct for endogeneity, it is important to bear this in mind when interpreting my results.

7. Sensitivity Analysis

Countries that are at different stages of development may experience differential benefits from having state enforcement for international commercial contracts. For instance, contractual violations might be less frequent in exchange relationships involving individuals from more developed coun-

Table 3. The Determinants of New York Convention Membership

	Log GDP p/c	GDP p/c Growth	Log Trade Share	Trade Share Growth	Observations	R^2
Join 1959–1964	0.05	−0.02	−0.08	0.03	4100	0.03
	(0.01)	(0.08)	(0.01)	(0.03)		
Join 1965–1969	0.02	−0.00	−0.01	0.00	4100	0.01
	(0.00)	(0.04)	(0.00)	(0.02)		
Join 1970–1974	0.03	0.02	−0.06	0.03	4100	0.03
	(0.00)	(0.05)	(0.01)	(0.02)		
Join 1975–1979	0.06	−0.02	−0.05	0.01	4100	0.06
	(0.00)	(0.04)	(0.01)	(0.01)		
Join 1980–1984	0.02	0.11	0.01	0.02	4100	0.01
	(0.00)	(0.05)	(0.01)	(0.03)		
Join 1985–1989	−0.01	0.08	−0.02	−0.00	4100	0.00
	(0.00)	(0.07)	(0.01)	(0.02)		
Join 1990–1994	−0.02	−0.00	−0.00	0.01	4100	0.01
	(0.00)	(0.06)	(0.01)	(0.03)		
Join 1995–1999	−0.01	−0.01	0.00	−0.03	4100	0.01
	(0.00)	(0.07)	(0.01)	(0.02)		

Notes: Regressand: A binary variable that is unity if a country joins the New York Convention in a 5-year period and zero otherwise. OLS with year effects (intercepts not reported). Robust standard errors in parentheses.

tries, where institutions are of better quality and people may exhibit a higher level of commercial honesty. In this case, we should expect the benefit of formally provided contract enforcement to be greater for poorer countries. On the other hand, richer countries may benefit more from the NYC since domestic courts, which ultimately do the enforcing under the NYC, tend to be of higher quality in these places. To determine if there are development-dependent effects of state enforcement, I break my sample into four income groupings.

The results of these regressions, presented in table 4, support the latter in tuition. Trading pairs with at least one high- or middle-income country experience about the same or slightly more gains from state enforcement than the sample as a whole, while trading pairs with at least one low-income or least-developed country experience slightly smaller gains than the sample as a whole. In the same table, I examine how the effect of state enforcement may be geographically dependent. Trading pairs with at least one country from South Asia do the worst, while those with at least one country from Latin America and the Caribbean do the best. Overall, however, the results in table 4 support the modest trade-enhancing effect of state enforcement identified in the benchmark regression.

Table 4. New York Convention Impact by Income and Region

	Both in NYC	One in NYC
Full sample	0.33	0.14
	(0.04)	(0.03)
High income	0.32	0.14
	(0.04)	(0.03)
Middle income	0.37	0.22
	—	—
Low income	0.29	0.09
	(0.06)	(0.05)
Least developed	0.20	0.09
	—	—
South Asia	−0.27	−0.37
	(0.11)	(0.09)
East Asia	0.21	0.08
	(0.12)	(0.09)
Sub-Saharan Africa	0.32	0.08
	(0.07)	(0.05)
Middle-East or North Africa	0.28	0.23
	(0.12)	(0.09)
Latin America or Caribbean	0.34	0.15
	(0.06)	(0.05)

Notes: Regressand: log real trade. OLS with country and year effects (intercepts not reported). Robust standard errors (clustering by country pairs) in parentheses. Regressors included but with unreported coefficients: both in WTO; one in WTO; GSP; log distance; log product real GDP; log product real GDP per capita; regional FTA; currency union; common language; land border; number landlocked; number islands; log product land area; common colonizer; currently colonized; ever colony; common country.

Anderson and Marcoullier (2002) consider a measure of countries' domestic court quality for 1996 using survey data from the World Economic Report's "Executive Opinion Survey." Interacting this measure with NYC membership would help us to shed light on how the NYC's effect might depend on the signatory's court quality. On the one hand, higher domestic court quality might enhance the importance of the NYC since, as table 4 suggests, well-functioning domestic courts are important to receive the full benefit of the NYC. On the other hand, domestic courts and international arbitration might be substitutes. If this is the case, when the quality of domestic courts rises, the importance of international arbitration, and thus of the NYC, which makes international arbitral awards enforceable in state courts, should fall.

The survey data Anderson and Marcoullier (2002) consider are available for only a fraction of the years covered by my panel, and even then, in

Table 5. Interaction

Both in NYC	−0.78
	(0.47)
One in NYC	−1.92
	(0.40)
Both in NYC × log product GDP p/c	0.07
	(0.03)
One in NYC × log product GDP p/c	0.13
	(0.02)

Notes: Regressand: log real trade. OLS with county and year effects (intercepts not reported). Robust standard errors (clustering by country pairs) in parentheses. Regressors included but with unreported coefficients: both in WTO; one in WTO; GSP; log distance; log product real GDP; log product real GDP per capita; regional FTA; currency union; common language; land border; number landlocked; number islands; log product land area; common colonizer; currently colonized; ever colony; common country.

most years, for only a small number of the countries my panel considers. However, since institutional quality and income are highly correlated, it is possible to examine the question of how domestic court quality influences the importance of the NYC for my entire panel, albeit somewhat more indirectly, by interacting NYC membership with the (log) product of trading partners' real GDP per capita.

Table 5 presents these results. The coefficients on both interaction terms are small but positive and significant, confirming the results in table 4. Richer trading pairs, which tend to have higher quality courts, receive a bigger boost from NYC membership than poorer trading pairs, which have lower quality courts. Equally important is the fact that the impact of the NYC on trade remains modest. When the interaction terms are added, the coefficient on joint NYC membership drops to –0.78 and on singular NYC membership drops to –1.92. Since the sample mean of log product real GDP per capita is 16.81, this implies that the net average effect of state enforcement is $(0.07 \times 16.81) - 0.78 \approx 0.40$ and $(0.13 \times 16.81) - 1.92 \approx 0.27$, respectively.

Perhaps nations realize greater benefits of state enforcement, but only some years after joining the NYC. If there is such a lag in the effect of joining the NYC, countries that joined earlier should exhibit a larger positive impact of NYC membership than those that joined later. Table 6 examines this possibility. I create four new "dummy" variables equal to one if either country in a trading pair joined the NYC 5, 10, 15 or 20 years ago.

The coefficient on joint NYC membership remains approximately of the same size as in the benchmark specification, though the coefficient on

Table 6. Dynamic Analysis

	OLS	Prais–Winsten	Prais–Winsten	Random effects	Random effects	Random effects	Lagged dependent variable
Residual autocorrelation coefficient	—	0.78	0.78	—	0.59	0.59	—
Both in NYC	0.31	0.16	0.15	0.34	0.33	0.33	0.07
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	
One in NYC	0.04	0.07	0.06	0.14	0.16	0.14	—
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	0.04
Join 5 years ago	0.25	—	0.14	0.12	—	0.11	—
	(0.02)		(0.01)	(0.01)		(0.01)	
Join 10 years ago	0.13	—	0.14	0.05	—	0.05	—
	(0.02)		(0.01)	(0.01)		(0.01)	
Join 15 years ago	0.14	—	0.12	0.07	—	0.05	—
	(0.02)		(0.01)	(0.02)		(0.02)	
Join 20 years ago	0.15	—	0.11	0.01	—	−0.02	—
	(0.02)		(0.01)	(0.02)		(0.02)	

Notes: Regressand: log real trade. OLS, Prais–Winsten, and random effects estimator with country and year effects (intercepts not reported). Standard errors (robust for OLS and Prais–Winsten, clustering by country pairs) in parentheses. Regressors included but with unreported coefficients: both in WTO; one in WTO; GSP; log distance; log product real GDP; log product real GDP per capita; regional FTA; currency union; common language; land border; number landlocked; number islands; log product land area; common colonizer; currently colonized; ever colony; common country.

single NYC membership falls substantially. The coefficients on the joined 5, 10, 15, or 20 years ago dummies are positive and significant but small, suggesting there is no substantial delayed benefit of NYC membership. In the next column I use a Prais–Winsten estimator, which delivers similar, albeit somewhat smaller, coefficients. As a robustness check, in columns 4–6 I also use a country-pair random effects estimator, which again produces similar results. As the final robustness test for the potential for a delayed effect of state enforcement, the last column of table 6 performs an OLS estimate including a lagged dependent variable. Here, the impact of state enforcement becomes economically negligible.

8. More Technical Concerns

One potential concern is the extent to which my findings are influenced by "intrafirm trade"—trade between affiliates of large multinationals located in different countries. If a large proportion of bilateral trade is between arms of the same firm in different nations, the NYC's effect on trade will be understated. The reason for this is straightforward: intrafirm trade does not face the same kinds of contract enforcement concerns that interfirm trade does.

The creation of the NYC in 1958 and its reputation as "the cornerstone of modern international trade" among scholars of international trade law strongly suggests that a substantial portion of international trade is of the interfirm variety. So does the fact that an estimated 90 percent of all international commercial contracts contain arbitration clauses to provide for the possibility of dispute. Still, it is possible that intrafirm trade is an important consideration in evaluating the impact of state enforcement on trade.

Ideally, I would like to "net out" intrafirm trade from yearly bilateral trade flows before estimating the gravity model. Unfortunately, data on intrafirm trade is available for only a few countries (the United States, Japan, Canada and Sweden) in sporadic years.[17] Nevertheless, since the rise of multinationals and intrafirm trade is a relatively recent phenomenon of globalization, I can address this issue by looking only at bilateral trade before intrafirm exchange started to become prominent in international trade. The

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17. See, for example, Bonturi and Fukasaku (1993). See also, Zeile (1997) and Rangan (2001).

Table 7. New York Convention Impact on Pre-Intrafirm Trade

	Both in NYC	One in NYC
Full sample	0.33	0.14
	(0.04)	(0.03)
Pre-1965	0.07	−0.01
	(0.09)	(0.04)
Pre-1970	−0.04	−0.01
	(0.07)	(0.04)
Pre-1975	0.05	0.05
	(0.06)	(0.04)
Pre-1980	0.17	0.08
	(0.05)	(0.03)
Pre-1985	0.24	0.10
	(0.05)	(0.03)

Notes: Regressand: log real trade. OLS with country and year effects (intercepts not reported). Robust standard errors (clustering by country pairs) in parentheses. Regressors included but with unreported coefficients: both in WTO; one in WTO; GSP; log distance; log product real GDP; log product real GDP per capita; regional FTA; currency union; common language; land border; number landlocked; number islands; log product land area; common colonizer; currently colonized; ever colony; common country.

cutoff I establish for this purpose is 1970. This date provides a conservative cutoff point since this is around the time that intrafirm trade began growing in USA, and USA led the growth of multinational firms globally.

If intrafirm trade is biasing my coefficients of interest downwards, when I re-estimate looking only at years before 1970, the NYC variables should exhibit a substantially larger effect on trade than they do when my panel covers all years. Table 7 reports the results of this estimation. My coefficients of interest are substantially *smaller* pre-1970 than they are for the entire period between 1950 and 1999. The NYC's impact on trade is negligible ($\approx 4\%$), negative, and statistically insignificant. I check the sensitivity of this result to several other cutoff dates, both before and after 1970, and continue to find that state enforcement's impact is smaller than it is for the entire period.

Following the NYC, a few, much smaller, multinational agreements were also created to provide state enforcement for international arbitral awards. These include the EU Convention, created in 2003; the Panama Convention, created in 1975; the Brussels/Lugano Convention, created in 1968; and the UN Convention on the Carriage of Goods by Sea, created in 1978. Nearly every member of each of these conventions is also a member of, and thus covered by, the NYC—the "grand-daddy" of multinational treaties

concerning the recognition and enforcement of international arbitral awards. One member of the Panama Convention—Nicaragua—is not a member of the New York Convention, and five members of the UN Convention on the Carriage of Goods by Sea are not members of the NYC—Democratic Republic of Congo, Gambia, Malawi, Pakistan, and Sierra Leone.

Since NYC membership varies over time, it is possible that the effect of state enforcement is understated if in some years of the sample some countries are not yet members of the NYC but are members of one of these other treaties with the same purpose as the NYC.[18] This seems unlikely for two reasons. First, each of these conventions covers only a small group of countries, while the NYC covers more than one hundred. Furthermore, most were already members of the NYC at the time they joined these other treaties. Still, since in principle this could affect my estimates, I compare state enforcement's effect on trade considering only the NYC with state enforcement's impact on trade considering membership in *any* agreement with the purpose of creating formal enforcement for international arbitral awards. To do this, I create a new binary variable that is equal to unity if a country is a member of *any* agreement with this purpose and zero otherwise. I again construct separate variables for when both countries in a trading pair are members of such a treaty and when only one is.[19]

Table 8 presents the results of this regression. The coefficients on my variables of interest are similar to those in the benchmark regression. State enforcement, measured as membership in any treaty with the end of providing formal enforcement for international arbitral awards, increases trade ($e^{0.29} - 1 \approx$) 34 percent when both countries are members and 15 percent when only one is. The additional NYC-inspired treaties evidently do not bias my coefficients of interest when only the NYC is used to measure state enforcement.

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18. The EU Convention poses no potential problem for my estimates since it was not created until 4 years after my sample ends.

19. To construct these variables, I use data on Panama Convention membership available at: http://www.sela.org and Brussels/Lugano Convention membership available at: http://www.fco.gov.uk/Files/kfile/statusbrussels,0.pdf.

Table 8. The Impact of Any Treaty of State Enforcement on Trade

Both in any treaty	0.29
	(0.04)
One in any treaty	0.14
	(0.03)
Both in GATT/WTO	0.13
	(0.05)
One in GATT/WTO	0.03
	(0.05)
GSP	0.70
	(0.03)
Log distance	−1.29
	(0.02)
Log product real GDP	0.25
	(0.06)
Log product real GDP p/c	0.95
	(0.06)
Regional FTA	0.96
	(0.13)
Currency union	1.09
	(0.14)
Common language	0.32
	(0.05)
Land border	0.36
	(0.12)
Number landlocked	0.22
	(0.45)
Number islands	0.96
	(0.31)
Log product land area	0.32
	(0.05)
Common colonizer	0.65
	(0.07)
Currently colonized	0.70
	(0.34)
Ever colony	1.27
	(0.12)
Common country	−0.54
	(0.40)
Observations	175,508
R^2	0.73
RMSE	1.69

Notes: Regressand: log real trade. OLS with country and year effects (intercepts not reported). Robust standard errors (clustering by country-pairs) in parentheses.

9. Concluding Remarks

The evidence suggests that the source of state contract enforcement in international trade has enhanced this trade—though not in the impressive way one would expect from a function considered essential for trade to flourish. The modest impact of formal enforcement in conjunction with international trade's considerable success strongly suggests that, in addition to formal enforcement, some private mechanisms of enforcement are also at work supporting international trade. Important research by Gould (1994), Rauch (2001), Casella and Rauch (2002), and Rauch and Trindade (2002), for example, demonstrates the significance of coethnic networks in creating private enforcement for international commercial agreements. Another private mechanism likely lessening the importance of state enforcement for trade is the use of *ex ante* arrangements, such as letters of credit, and other forms of third-party intermediation that mitigate the need for *ex post* enforcement.

Dixit's (2003) important theoretical work provides some additional conceptual underpinning for this paper's finding. His research suggests not only how formal enforcement may fail to add to trade in some cases, but how it might actually *reduce* trade by "crowding out" private enforcement mechanisms like those discussed above.

Ironically, another potential factor that may be contributing to the NYC's modest effect is the inability to formally enforce the terms of the NYC itself. Like all multinational treaties, for the NYC as well, there is no formal supranational agency of authority to compel states that have joined it to abide by its terms. This leaves the enforcement of the NYC to informal mechanisms, such as reputation, and the interstate equivalent of international arbitration through such organizations as the UN. Unfortunately, data on the frequency with which NYC member states default on the terms of their agreement, which could shed light on this issue, are not available.

Finally, as Helpman, Melitz, and Rubinstein (forthcoming) point out, traditional gravity estimations ignore countries that do not trade with each other and in doing so may generate biased estimates. To determine how accounting for such bias might affect state enforcement's impact on trade, future research should consider the NYC's effect in the context of the new model Helpman, Melitz, and Rubinstein have designed to address this problem.

Appendix A.   Trading Countries in Sample

Albania	Congo, Dem. Rep. of	Iceland	Mozambique (1998)
Algeria (1989)	Congo, Rep. of	India (1960)	Namibia
Angola	Costa Rica (1987)	Indonesia (1981)	Nepal (1998)
Antigua and Barbuda (1989)	Cote d'Ivoire (1991)	Iran	Netherlands (1964)
Argentina (1989)	Croatia (1993)	Ireland (1981)	New Zealand (1983)
Armenia (1997)	Cyprus (1980)	Israel (1959)	Nicaragua
Australia (1975)	Czech Republic (1993)	Italy (1969)	Niger (1964)
Austria (1961)	Denmark (1972)	Jamaica	Nigeria (1970)
Azerbaijan	Dominica (1988)	Japan (1961)	Norway (1961)
Bahrain (1988)	Dominican Republic	Jordan (1979)	Oman (1999)
Bangladesh (1992)	Ecuador (1962)	Kazakhstan (1995)	Pakistan
Barbados (1993)	Egypt (1959)	Kenya (1989)	Panama (1984)
Belarus (1960)	El Salvador (1998)	Kuwait (1978)	Papua N. Guinea
Belgium (1975)	Equatorial Guinea	Kyrgyz Republic (1996)	Paraguay (1997)
Belize	Estonia (1993)	Lao People's Dem. Rep. (1998)	Peru (1988)
Benin (1974)	Ethiopia	Latvia (1992)	Philippines (1967)
Bermuda	Fiji	Lebanon (1998)	Poland (1961)
Bhutan	Finland (1962)	Lesotho (1989)	Portugal (1994)
Bolivia (1995)	France (1959)	Liberia	Qatar
Botswana (1971)	Gabon	Libya	Romania (1961)
Brazil	Gambia	Lithuania (1995)	Russia (1960)
Bulgaria (1961)	Georgia (1994)	Luxembourg (1983)	Rwanda
Burkina Faso (1987)	Germany (1961)	Macedonia (1994)	Samoa
Burundi	Ghana (1968)	Madagascar (1962)	Sao Tome & Principe
Cambodia (1960)	Greece (1962)	Malawi	Saudi Arabia (1994)
Cameroon (1988)	Grenada	Malaysia (1985)	Senegal (1994)
Canada (1986)	Guatemala (1984)	Mali (1994)	Seychelles
Cape Verde	Guinea (1991)	Malta	Sierra Leone
Central African Rep. (1962)	Guinea-Bissau	Mauritania (1997)	Singapore (1986)
Chad	Guyana	Mauritius (1996)	Slovakia (1993)
Chile (1975)	Haiti (1983)	Mexico (1971)	Slovenia (1992)
China (1987)	Honduras	Moldova (1998)	South Africa (1976)
Colombia (1979)	Hong Kong	Mongolia (1994)	Spain (1977)
Comoros	Hungary (1962)	Morocco (1959)	Sri Lanka (1962)
St. Kitts & Nevis	Syria (1959)	Turkey (1992)	Vietnam (1995)
St. Lucia	Tajikistan	Uganda (1992)	Yemen, Rep. of

(*continued overleaf*)

Appendix A. (*Continued*)

St. Vincent & Gren.	Tanzania (1964)	Ukraine (1960)	Zambia
Sudan	Thailand (1959)	United Kingdom (1975)	Zimbabwe (1994)
Swaziland	Togo	United States (1970)	
Sweden (1972)	Trinidad and Tobago (1966)	Uruguay (1983)	
Switzerland (1965)	Tunisia (1967)	Venezuela (1995)	

Notes: Countries with years beside them are members of the New York Convention. The year refers to when they joined.

Appendix B. Variable Descriptions

Independent variable	Description
Both in NYC	A binary variable that is unity if a country pair belongs to the New York Convention at time *t* and zero otherwise. Source: Stockholm Chamber of Commerce (2004).
One in NYC	A binary variable that is unity if either country in a country pair, but not the other, belongs to the New York Convention in time *t* and zero otherwise. Source: Stockholm Chamber of Commerce (2004).
Both in GATT/WTO	A binary variable that is unity if a county pair are GATT/WTO members at time *t* and zero otherwise. Source: Rose (2004a).
One in GATT/WTO	A binary variable that is unity if either country in a country pair, but not the other, is a GATT/WTO member at time *t* and zero otherwise. Source: Rose (2004a).
GSP	A binary variable that is unity if either country in a country pair was a GSP beneficiary of the other at time *t* and zero otherwise. Source: Rose (2004a).
Log distance	The log of the distance between a pair of countries. Source: Rose (2004a).
Log product real GDP	The log of the product of the real GDP of each country in a country pair in time *t*. Source: Penn World Table v. 6.1 (2002).
Log product real GDP p/c	The log of the product of real GDP per capita of each country in a country pair in time *t*. Source: Penn World Table v. 6.1 (2002).
Regional FTA	A binary variable that is unity if a country pair belongs to the same regional trade agreement at time *t* and zero otherwise. Source: Rose (2004a).
Currency union	A binary variable that is unity if a country pair uses the same currency at time *t* and zero otherwise. Source: Rose (2004a).

Appendix B. (*Continued*)

Common language	A binary variable that is unity if a country pair has the same language and zero otherwise. Source: Rose (2004a).
Land border	A binary variable that is unity if a country pair shares a land border and zero otherwise. Source: Rose (2004a).
Number landlocked	The number of landlocked countries in the country pair (0, 1, or 2). Source: Rose (2004a).
Number islands	The number of island nations in the country pair (0, 1, or 2). Source: Rose (2004a).
Log product land area	The log product of the land areas of two countries in a country pair (in square kilometers). Source: Rose (2004a).
Common colonizer	A binary variable that is unity if a country pair were ever colonies post-1945 with the same colonizer and zero otherwise. Source: Rose (2004a).
Currently colonized	A binary variable that is unity if one country in a country pair is a colony of the other at time *t* and zero otherwise. Source: Rose (2004a).
Ever colony	A binary variable that is unity if one country in a country pair ever colonized the other and zero otherwise. Source: Rose (2004a).
Common country	A binary variable that is unity if a country pair remained part of the same nation during the sample and zero otherwise. Source: Rose (2004a).

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