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How Important is State Enforcement for Trade?

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Abstract

This paper investigates the effect of state contract enforcement on international trade. Following Rose (2004a), I estimate a gravity model of bilateral trade using panel data that covers 157 countries over the last 50 years. I find that state enforcement increases trade between nations— but less impressively than its status as essential for flourishing trade suggests. My analysis provides the first direct evidence of state enforcement's impact on trade in general and international trade in particular. (JEL Codes: K33, F10)

The ideas presented in this research are the author's and do not represent official positions of the Mercatus Center at George Mason University.

Commerce . . . can seldom flourish . . . [where] the faith of contracts is not supported by the law, and . . . [where] the authority of 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, *Wealth of Nations*, 1976[1776], vol. 2: 445.

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 and in doing so 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 test this claim. 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 on the basis of which such an authority could adjudicate transnational commercial agreements if one existed (see for instance, Oye 1986: 1; Plantey 1993: 69).

Despite the lack of formal global governance, international trade is large and growing rapidly. Today it accounts for some 25 percent of global economic activity.[4] Since 1960 the value of global exports has increased more than 44 fold, and in 2003, world exports of merchandise and commercial services exceeded $9 trillion (WTO 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 helps to illustrate 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 of only 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. The introduction of the NYC in 1958, however, changed all 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

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enforce his arbitral award.

The NYC provides a straightforward way to empirically investigate the impact of state enforcement on trade. I use an augmented gravity model of bilateral trade similar to that used by Rose (2004a) to determine the impact of the WTO on trade between nations. 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 suggested by the wisdom that state enforcement is essential for trade to flourish. State enforcement moderately enhances trade once standard factors contributing to trade have been accounted for. These results provide the first direct evidence of state enforcement's impact on trade in general and international trade in particular.

Recent research by Gould (1994), Rauch (2001), Casella and Rauch (2002), and Rauch and Trindade (2002) demonstrates the importance of coethnic networks in creating private enforcement for international commercial agreements. The work of Rauch and Trindade (2002), which estimates the effect of Chinese networks on bilateral trade, has been especially useful in this regard. Building on their work, it would be interesting to run a 'horse race' between state and private enforcement mechanisms to determine which matters more for international trade. Unfortunately, data limitations prevent this. Data on ethnic Chinese populations are available for only two years (1980 and 1990) and fewer than half the countries I consider. Further, this information covers only one major ethnic network in international trade. To capture the effect of private enforcement on trade more generally, population data for other ethnic networks, which are also unavailable, are needed as well. Additionally, to make the race a fair one, we would need to account for other private enforcement mechanisms also supporting international trade, such as letters of credit. Again, in light of data availability, there is no clear way of accomplishing this. Thus, my sole purpose in this paper is to establish the effect of state enforcement on trade, which

takes advantage of a sizeable panel that includes 157 countries over the last half century.

Section 2 describes the status of dispute resolution for international commercial contracts. Sections 3 and 4 discuss my empirical strategy and data. Section 5 examines the NYC's impact on trade at a glance. Section 6 presents my main results and Sections 7 and 8 examine their robustness. I conclude with the implications of my analysis in Section 9.

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).[5] Today, 90 percent of all international commercial contracts include arbitration clauses (see for instance, Volckart and Mangles 1999 and Casella 1996). As one leading international practitioner put it: "in today's world the dispute resolution mechanism will invariably be arbitration" (Aksen 1990: 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 feel is inappropriate for their case,[6] or they fear that a state court will favor their adversary if he is a citizen of that nation. 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.[7] But equally troublesome is the unwillingness of either state court to decide the dispute because neither feels that it is equipped to

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adjudicate an international matter. Third, the decisions of state courts regarding matters of international commerce are difficult to enforce (Dezalay and Garth 1996: 6). In many 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.[8]

International arbitration overcomes these problems by "delocalizing"[9] dispute resolution. Under arbitration, parties may choose with respect to the variables concerning adjudication of their dispute. 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)[10] that through common practice and usage have come to govern international commerce.[11] 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 cannot agree to one or more of these variables they may stipulate that a neutral third party—the arbitrators of their case, for instance—decide these items for them.[12]

There are hundreds of international arbitration forums globally (Graving 1989: 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.[13]

Evidence from the world's largest international arbitration associations suggests that the community of international arbitration users is large and diverse. Between 1923 and 1976, 3,000 requests for international arbitration were submitted to the ICC—an average of about 57 cases per year over the period. Between 1976 and 1998 the ICC received its 10,000th case—an average of over 318 cases per year over the

period (Craig et al 2000: 2). In 2000 the ICC alone arbitrated a caseload involving nearly 1,500 parties from close to 120 countries worldwide (ICC Bulletin 2002).[14] 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 amounts in dispute are often the largest, some in excess of $1 billion.[15] 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 tiny percentage of all trades result in disagreement.

Under the terms of a relatively recent multinational treaty—the 1958 United Nations New York Convention on the Recognition and Enforcement of Foreign Arbitral Awards (NYC)—private international arbitral awards are enforceable in state courts. For this reason the NYC is considered the "cornerstone of current international commercial arbitration" (van den Berg 1981: 1). Between 1959 and 2003, 134 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.[16] If the losing party to arbitration does not comply with the arbitrators' decision, the winning party may take this decision and have it enforced by the loser's state court if it 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.

Unfortunately, data is not available regarding how many of the cases brought before international arbitration, or what percentage of the sums identified in Table 1, are actually enforced by state courts under the NYC. The ICC estimates that 90

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percent of its arbitral awards are complied with voluntarily (Craig et al 2000: 404). This provides an indirect estimate ($\approx$10 percent) 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 formal enforcement on trade.

3 Empirical Strategy

To determine 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 the standard gravity model of bilateral trade that explains trade with 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 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

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and vice versa), which might be important in accounting for the volume and pattern of exchange. Table A2 comprehensively describes the variables I use and Table A3 provides descriptive statistics. My variables of interest are not highly correlated with the standard gravity variables or the factors I condition them on so multicollinearity is not a concern.

I estimate the augmented gravity equation:

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

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 one if both i and j are members of the NYC at time t and zero otherwise. $OneinNYC_{ijt}$ is a binary variable that is one if either i or j is a member of the NYC at time t and zero 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 along the lines suggested by conventional wisdom, 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 a comprehensive set of 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.[17] Finally,

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I combine country and year fixed effects to account for year-specific concerns and to capture any permanent differences across countries that might affect trade. The literature that discusses the gravity model is undecided about the appropriate type of country effects to use when estimating the gravity equation (see for instance, Rose 2000b, 2000c; Anderson and van Wincoop 2003; Tomz, Goldstein and Rivers 2005; Subramanian and Wei 2003). Standard country fixed effects capture permanent national differences across countries that affect trade, whereas country-pair fixed effects capture permanent differences that influence trade between a pair of countries. As a robustness check, I estimate the gravity model using both types of country effects to ensure that my findings are not the result of choosing one type over the other.

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 157 countries over 50 years between 1950 and 1999. A list of these countries is available in Table A1. Using this data Rose creates an average value of bilateral trade between a country pair by averaging the four available measures (exports 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 ($1982 - 1984 = 100$).

I use the most updated data for real GDP and GDP per capita (in constant U.S. dollars) available. These data come from the Penn World Table v. 6.1 and in my sample cover the years from 1950 through 1999. I obtained data for my variable of interest—membership in the NYC—from the Stockholm Chamber of Commerce (2004), which contains 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:

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Egypt, France, Israel, Morocco, Syria, and Thailand. By 1965, 29 sample countries had signed the NYC including Finland, Germany, France, India, Japan, the Netherlands, Norway, Russia and Switzerland. Over the next decade 15 additional countries joined convention 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, 59 nations were members of the convention and between 1986 and 1995 38 new countries joined (including Canada)—the largest number of new additions in the NYC's history. By 1999 the total number of countries that had ratified the convention stood at 109. A complete list of NYC signatories and the years they joined is available in Table A1.

Data for the remainder of my regressors come from Rose (2004a) who draws on a number of standard sources to construct the remaining variables. He uses the CIA *World Factbook* to create country-specific controls including: latitude and longitude, land area, landlocked and island status, shared border, language, colonizers, and dates of independence. 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 come from the WTO and include: ASEAN, EEC/EC/EU; US-Israel FTA; NAFTA; CARICOM; PATCRA; ANZ-CERTA; CACM, SPARTECA, and Mercosur. Finally, Rose uses data from the WTO website and the UN's publication, the *Operation and Effects of the Generalized System of Preferences* (1974, 1979, 1984), 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 negligible impact on trade. Figure 1 presents a graphical 'event study' that examines total trade share

around the time countries join the NYC. It depicts the average trade share ([exports + imports]/GDP) for 50 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 is available for all ten years surrounding the date of NYC ratification. The middle line connected by circles depicts average trade share in each year for these countries. The horizontal lines above and below show a confidence interval of plus and minus one standard deviation. The vertical line in 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 before and after countries join the NYC are nearly the same. The event study provides little evidence to suggest that the introduction of state contract enforcement has mattered much for trade.

The preliminary impression created here may be overturned by a more in-depth consideration of the NYC's impact on trade—one that econometrically isolates the effect of state enforcement. As it turns out, doing so improves the case for the importance of state enforcement, but not spectacularly. Although my estimation of the augmented gravity model finds a significant positive impact of state enforcement on trade, this effect is modest relative to the size of other factors contributing to trade and the common perception about the necessity of government enforcement for trade to flourish.

6   Benchmark Results

Table 2 presents the results of my regressions analyzing the impact of the NYC on trade. My default specification is the augmented gravity model estimated using ordinary least-squares with year fixed effects and robust standard errors over the

entire sample. Column 1 reports these findings. My results on the variables used by Rose (2004a) are very close to those he finds in both size and direction, similar to those found elsewhere in the literature, and are highly sensible. Economically larger and richer countries trade more, while those that are further apart, landlocked, and physically larger 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 membership in the WTO/GATT has an economically weak impact.

What about the NYC? In my default specification the impact of NYC membership is small, negative, and insignificant. That is, state enforcement does not appear to increase trade. The effect of state enforcement improves substantially when country fixed effects are added in column 2, but is still not all that impressive. 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 percent and 8 percent respectively. The results are virtually identical when dyadic fixed effects are substituted for country fixed effects. This effect is significant in one sense, but modest when compared to the effect of the other variables impacting trade, intuition about the importance of state courts, and the long-term growth of trade.

To put this in perspective, the NYC has roughly the same impact on trade as sharing a common language. Compare the size of this effect with the effect of membership in a regional trade agreement which is 163 percent and far from the largest coefficient in Table 2. Or consider the NYC's impact on trade relative to the impact of having a shared currency, which increases trade nearly 200 percent. My estimations evidently can deliver positive, economically large effects on trade. But state enforcement is not

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one of them.

7 Sensitivity Analysis

Countries that are in 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 countries 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 in 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 higher quality in these places. To determine if there are development-dependent effects of state enforcement, I break the sample into four income groupings.

The results of these regressions, presented in Table 3, support the latter intuition. Countries in the top half of the sample income distribution experience slightly more gains from state enforcement than the sample as a whole, while countries in the poorer half of the sample do considerably worse than the sample as a whole. Importantly, cutting the sample by income class this way still yields little evidence that state enforcement appreciably increases trade for countries at any level of development.

In the same table I look at how state enforcement differentially impacts geographical regions. Sub-Saharan Africa and East Asia do the worst, while Latin America and the Caribbean do the best. But there are again no positive coefficients on NYC membership of statistical or economic significance. Even the moderate boost trade receives from state enforcement in Table 2 is absent here.

Interacting the NYC variables with (log) real GDP per capita, common lan-

guage, and (log) distance yields similar results. Only the coefficient on real GDP per capita is significant and yields a coefficient of 0.34 with a robust standard error of 0.03. However, when the interaction terms are included, the coefficient on joint NYC membership drops to -5.51. Since the sample mean of (log) real GDP per capita is 16.81, this implies that the net average effect of state enforcement is only $(0.34 * 16.81) - 5.51 = 0.21$.

Perhaps state enforcement enhances trade, but the benefits are only realized 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 4 examines this possibility. I create four new 'dummy' variables equal to one if either country in a trading pair joined the NYC five, ten, fifteen or twenty years ago.

The coefficients on these variables are positive, significant, and moderately sized. The dynamic analysis suggests that there may be a delayed effect of joining the NYC on trade. In the next column I use a Prais-Winsten estimator to check if this is the result of significant serial correlation, which it largely is. Consistent with my benchmark findings, the Prais-Winsten estimates in columns 2 and 3 indicate a small, positive impact of state enforcement on trade, with and without accounting for membership lags. As a robustness check, in columns 4-6 I also use a country-pair random effects estimator, which delivers slightly larger, but consistent findings both when membership lags are included and when they are not. As one final robustness test on the potential for a delayed effect of state enforcement, the last column of Table 4 performs an OLS estimate with year effects including a lagged dependent variable. I again find similar results. Dynamic considerations do not seem to provide evidence that state enforcement has substantially increased trade.

8   More Technical Concerns

One potential concern is the extent to which my findings are influenced by "intra-firm 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. Presumably intra-firm trade does not face the same kinds of contract enforcement concerns that inter-firm 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 inter-firm 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 intra-firm trade is an important consideration in evaluating the impact of state enforcement on trade.

Ideally I would like to 'net out' intra-firm trade from yearly bilateral trade flows before estimating the gravity model. Unfortunately, data on intra-firm trade is available for only a few countries (the United States, Japan, Canada and Sweden) in sporadic years.[18] Nevertheless, since the rise of multinationals and intra-firm trade is a relatively recent phenomenon of globalization, I can address this issue by looking only at bilateral trade before intra-firm exchange started to become prominent in international trade. The cutoff I establish for this purpose is 1970. This date provides a conservative cutoff point since this is around the time that intra-firm trade began growing in the U.S., and globally the U.S. led the growth of multinational firms.

If intra-firm trade is biasing my coefficients of interest downwards, when I re-estimate looking only at years before 1970, the NYC variables should exhibit a sub-

stantially larger effect on trade than they do when my panel covers all years. The results of this estimation, which include year and country fixed effects, are reported in Table 5. 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 (≈ 4 percent), negative and statistically insignificant. I check the sensitivity of this result to several other cut-off dates, both before and after 1970, and find essentially the same results. The effect of the NYC remains small, lower than it is for the entire period and, with one exception, insignificant.

Additionally, I consider the NYC's impact on trade in individual years at five-year intervals from 1960-1995. By exploiting cross-sectional variation with the gravity model I can gauge how state enforcement's impact varies over time. The further back one goes, the less intra-firm trade should be contributing to international trade. So, if intra-firm trade is influencing my result, the effect of the NYC should be consistently falling over time. The results of the cross-sectional analysis are reported in Table 6.

There is no pattern to how the NYC affects trade over time. Membership has a small but increasingly positive effect on trade in the late 1960's and early 1970's. This effect turns negative in 1980 and falls until 1990 when it begins to tick upwards, and then falls again in 1995, although not to its level in the mid-1980s. The coefficients are unstable over time in terms of sign and size and are usually insignificant. The cross-sectional analysis provides no evidence that intra-firm trade is a problem for my estimates. It does, however, cast further doubt on the positive effect of state enforcement. Only for 1970 do the results indicate positive and significant effects on trade, whereas those for 1980, 1985, and one of the coefficients for 1995 ('Both in NYC') suggest that state enforcement has had a significant, *negative* impact on trade.

Measurement error is not a problem concerning the date of a country's entrance

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into the NYC. Similarly, 'informal membership' is not an obstacle for measuring the impact of the NYC on trade since, unlike some other multinational agreements such as the WTO, this agreement makes no provisions for non-explicit (i.e., *de facto* or provisional) membership (see Tomz, Goldstein and Rivers 2005). Reverse causality, however, may be an issue. Countries may join the NYC in 'bad times' in an effort to boost their trade. If this is the case, the estimated effect of the NYC on trade will be understated. In principle it is possible to correct for endogeneity by 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 Rose—measures of democracy and freedom, as well as some of my own, including legal origin and distance from Paris, France (where the ICC is located)— but confront the same problem that Rose does: poorness of fit in the first stage. My instrumental variables are not well correlated with NYC membership.

Fortunately, despite the absence of valid instruments I can address the endogeneity question raised above by exploiting variation in *when* countries join the NYC. This does not substitute for formal endogeneity testing with valid instruments. But if it turns out that countries do not join the NYC in bad times, a plausible story about endogeneity becomes considerably more difficult to tell.

To determine this I examine how things are going (a) economically and (b) trade-wise for a country near the time it joins the NYC. I use only those 109 countries that join the NYC at some point between 1959 and 1999. I consider the relationship between when a country joins the convention and its (log) GDP per capita, GDP per capita growth, (log) trade share, and trade share growth around this time. I construct a five-year panel in which my regressand is a binary variable that is unity when a country joins in a five year period and zero otherwise. Table 7 contains the results of

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this estimation.

Most of the effects in Table 7 are positive, all of them are small, and many are insignificant. When the coefficient for one independent variable in a 'set' (GDP per capita and GDP growth or trade share and trade share growth) is negative, the coefficient on the other variable *in the same set* is frequently positive—not what one would expect if the endogeneity story pointed to above were correct. There is no five year period in which the effect of the potential economic determinants I consider is always negative. Though in one period (1980-1984) they are always positive. As the R^2's display, income and trade share explain very little of the variation in when countries join the NYC. Economic factors do not seem to exert a significant impact on the timing of convention membership and the results are inconsistent with the 'join in bad times' story discussed above.

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

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same purpose as the NYC.[19] This seems unlikely for two reasons. First, each of these conventions covers only a small group of countries, while the NYC covers more than 100. 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.[20]

Table 8 presents the results of these regressions. Column 1 contains the default specification, which again includes year effects, and column 2 adds country fixed effects. The results for all the variables, including state enforcement, are nearly identical to those in Table 2. In the default specification, state enforcement, measured as membership in any treaty with the end of providing formal enforcement for international arbitral awards, is economically small and negative. In the specification that includes country fixed effects, membership in any such treaty increases trade $(e^{0.29} - 1 \approx)$ 34 percent when both countries are members and 15 percent when only one is. Substituting dyadic fixed effects again leaves this result virtually unchanged. The additional NYC-inspired treaties evidently do not bias my coefficients of interest when only the NYC is used to measure state enforcement.

A final concern, which has been raised in the literature discussing the gravity model, is that trade cannot be negative. To see if this is influencing my results, I use a Tobit estimator, replacing the smallest 5 percent of the sample trade observations with zero. This robustness check does nothing to improve the effect of state enforcement.

When Tobit is used, the coefficients of interest fall substantially.[21] Modifying the threshold below which sample trade observations are replaced with zero delivers the same result.

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. This finding corroborates the work of Rauch and Trindade (2002), which indicates that private enforcement achieved through coethnic networks plays an important role in this capacity. 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. To get at the issue of which matters more for trade—public mechanisms of enforcement or private ones—additional data regarding the latter are needed. As already noted, future work that runs a 'race' between the two could be particularly interesting.

In addition to raising questions about what private mechanisms secure cooperation in the international arena, my analysis raises several others that deal more specifically with the NYC itself. For instance, bad economic times do not drive when countries join the convention, but what else can be said about the different timing of convention ratification by different countries? Furthermore, what impact does the NYC have on other important international variables?

Finally, the results of my investigation raise questions about the importance of state enforcement in enhancing trade more generally. What is the effect of state contract enforcement on trade domestically? How important is it to establish state contract enforcement in developing countries that lack such regimes relative to other priorities, such as joining regional trade agreements? The findings presented here pose some provocative answers. But these and other potentially important questions await future inquiry.

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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).

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

	1	2	3
	Default	Country fixed effects	Country-pair fixed effects
Both in NYC	-0.11	0.33	0.33
	(0.05)	(0.04)	(0.04)
One in NYC	-0.10	0.14	0.15
	(0.04)	(0.03)	(0.03)
Both in GATT/WTO	0.12	0.13	0.13
	(0.06)	(0.05)	(0.05)
One in GATT/WTO	-0.02	0.03	0.06
	(0.06)	(0.05)	(0.05)
GSP	0.91	0.68	0.14
	(0.03)	(0.03)	(0.03)
Log distance	-1.10	-1.28	—
	(0.03)	(0.02)	—
Log product real GDP	0.97	0.28	0.43
	(0.01)	(0.06)	(0.06)
Log product real GDP p/c	0.36	0.92	0.66
	(0.02)	(0.06)	(0.06)
Regional FTA	1.14	0.97	0.90
	(0.11)	(0.14)	(0.08)
Currency union	0.94	1.09	0.60
	(0.13)	(0.14)	(0.13)
Common language	0.35	0.32	—
	(0.05)	(0.05)	—
Land border	0.67	0.36	—
	(0.13)	(0.12)	—
Number landlocked	-0.34	0.22	—
	(0.03)	(0.45)	—
Number islands	-0.03	0.96	—
	(0.04)	(0.31)	—
Log product land area	-0.11	0.32	—
	(0.01)	(0.04)	—
Common colonizer	0.90	0.65	—
	(0.08)	(0.07)	—
Currently colonized	0.99	0.69	0.27
	(0.29)	(0.34)	(0.20)

Table 2 (Cont.)

	1	2	3
	Default	Country fixed effects	Country-pair fixed effects
Ever colony	1.12	1.27	—
	(0.13)	(0.12)	—
Common country	-1.19	-0.54	—
	(0.29)	(0.40)	—
Observations	175,508	175,508	175,508
R^2	0.68	0.73	0.86
RMSE	1.84	1.69	1.23

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

Table 3–New York Convention Impact by Income and Region

	Both in NYC	One in NYC
Default	-0.11	-0.10
	(0.05)	(0.04)
High income	-0.02	-0.06
	(0.06)	(0.05)
Middle income	0.12	0.08
	(0.06)	(0.05)
Low income	-0.38	-0.20
	(0.07)	(0.06)
Least developed	-0.56	-0.21
	(0.10)	(0.07)
South Asia	-0.46	-0.58
	(0.16)	(0.11)
East Asia	-0.02	-0.07
	(0.14)	(0.12)
Sub-Saharan Africa	-0.40	-0.21
	(0.08)	(0.06)
Middle-East or North Africa	-0.14	-0.05
	(0.15)	(0.12)
Latin America or Caribbean	0.06	0.04
	(0.08)	(0.06)

Notes: Regressand: log real trade. OLS with 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.

Table 4–Dynamic Analysis

	OLS	Prais-Winsten	Prais-Winsten	Random effects	Random effects	Random effects	Lagged dependent variable
Residual autocorrelation coefficient	—	0.81	0.81	—	0.65	0.65	—
Both in NYC	-0.08	0.08	0.08	0.34	0.25	0.25	0.00
	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
One in NYC	-0.22	0.03	0.02	0.14	0.10	0.10	0.01
	(0.04)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Join 5 years ago	0.34	—	0.17	0.12	—	0.11	—
	(0.03)	—	(0.01)	(0.01)	—	(0.01)	—
Join 10 years ago	0.18	—	0.18	0.05	—	0.09	—
	(0.02)	—	(0.01)	(0.01)	—	(0.02)	—
Join 15 years ago	0.19	—	0.18	0.07	—	0.08	—
	(0.02)	—	(0.01)	(0.02)	—	(0.02)	—
Join 20 years ago	0.23	—	0.17	0.01	—	0.07	—
	(0.03)	—	(0.01)	(0.02)	—	(0.02)	—

Notes: Regressand: log real trade. OLS, Prais-Winsten, and Random effects estimator with 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.

Table 5–New York Convention Impact on Pre- Intra-Firm Trade

	Both in NYC	One in NYC
Default	-0.11	-0.10
	(0.05)	(0.04)
Pre-1970	-0.04	-0.01
	(0.07)	(0.04)
Pre-1965	0.07	-0.01
	(0.09)	(0.04)
Pre-1975	0.05	0.05
	(0.06)	(0.04)
Pre-1980	0.17	0.08
	(0.05)	(0.03)

Notes: Regressand: log real trade. OLS with year and country fixed 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.

Table 6–Cross-Sectional Analysis

	Both in NYC	One in NYC
1960	0.03	0.06
	(0.44)	(0.08)
1965	−0.04	−0.06
	(0.10)	(0.06)
1970	0.21	0.18
	(0.10)	(0.07)
1975	0.06	0.03
	(0.09)	(0.07)
1980	-0.24	-0.21
	(0.10)	(0.08)
1985	-0.46	-0.27
	(0.11)	(0.11)
1990	-0.11	-0.08
	(0.12)	(0.13)
1995	-0.39	−0.23
	(0.16)	(0.16)

Notes: Regressand: log real trade. OLS (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.

Table 7–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	0.02	-0.11	-0.04	0.02	2,924	0.004
	(0.01)	(0.15)	(0.01)	(0.06)		
Join 1965-1969	0.02	0.01	0.00	-0.00	2,924	0.01
	(0.00)	(0.09)	(0.01)	(0.03)		
Join 1970-1974	0.02	0.03	-0.06	0.03	2,924	0.02
	(0.01)	(0.09)	(0.01)	(0.03)		
Join 1975-1979	0.07	-0.01	-0.05	-0.01	2,924	0.05
	(0.00)	(0.07)	(0.01)	(0.02)		
Join 1980-1984	0.01	0.24	0.05	0.02	2,924	0.02
	(0.00)	(0.10)	(0.01)	(0.05)		
Join 1985-1989	-0.05	0.14	0.02	-0.01	2,924	0.02
	(0.01)	(0.13)	(0.01)	(0.04)		
Join 1990-1994	-0.06	-0.05	0.04	0.00	2,924	0.04
	(0.01)	(0.12)	(0.01)	(0.05)		
Join 1995-1999	-0.04	-0.20	0.04	-0.06	2,924	0.03
	(0.00)	(0.13)	(0.01)	(0.04)		

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

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

	1	2	3
	Default	Country fixed effects	Country-pair fixed effects
Both in any treaty	-0.21	0.29	0.33
	(0.05)	(0.04)	(0.04)
One in any treaty	-0.14	0.14	0.15
	(0.04)	(0.03)	(0.03)
Both in GATT/WTO	0.12	0.13	0.13
	(0.06)	(0.05)	(0.05)
One in GATT/WTO	-0.02	0.03	0.06
	(0.06)	(0.05)	(0.05)
GSP	0.90	0.70	0.14
	(0.03)	(0.03)	(0.03)
Log distance	-1.10	-1.29	—
	(0.03)	(0.02)	—
Log product real GDP	0.97	0.25	0.43
	(0.01)	(0.06)	(0.06)
Log product real GDP p/c	0.36	0.95	0.66
	(0.02)	(0.06)	(0.06)
Regional FTA	1.15	0.96	0.90
	(0.11)	(0.13)	(0.08)
Currency union	0.93	1.09	0.60
	(0.13)	(0.14)	(0.13)
Common language	0.35	0.32	—
	(0.05)	(0.05)	—
Land border	0.67	0.36	—
	(0.13)	(0.12)	—
Number landlocked	-0.35	0.22	—
	(0.03)	(0.45)	—
Number islands	−0.03	0.96	—
	(0.04)	(0.31)	—
Log product land area	-0.11	0.32	—
	(0.01)	(0.05)	—
Common colonizer	0.90	0.65	—
	(0.08)	(0.07)	—
Currently colonized	0.99	0.70	0.27
	(0.29)	(0.34)	(0.20)

Table 8 (Cont.)

	1	2	3
	Default	Country fixed effects	Country-pair fixed effects
Ever colony	1.11	1.27	—
	(0.13)	(0.12)	—
Common country	-1.23	-0.54	—
	(0.29)	(0.40)	—
Observations	175,508	175,508	175,508
R^2	0.68	0.73	0.86
RMSE	1.84	1.69	1.23

Notes: Regressand: log real trade. OLS with year effects (intercepts not reported). Robust standard errors (clustering by country-pairs) are in parentheses.
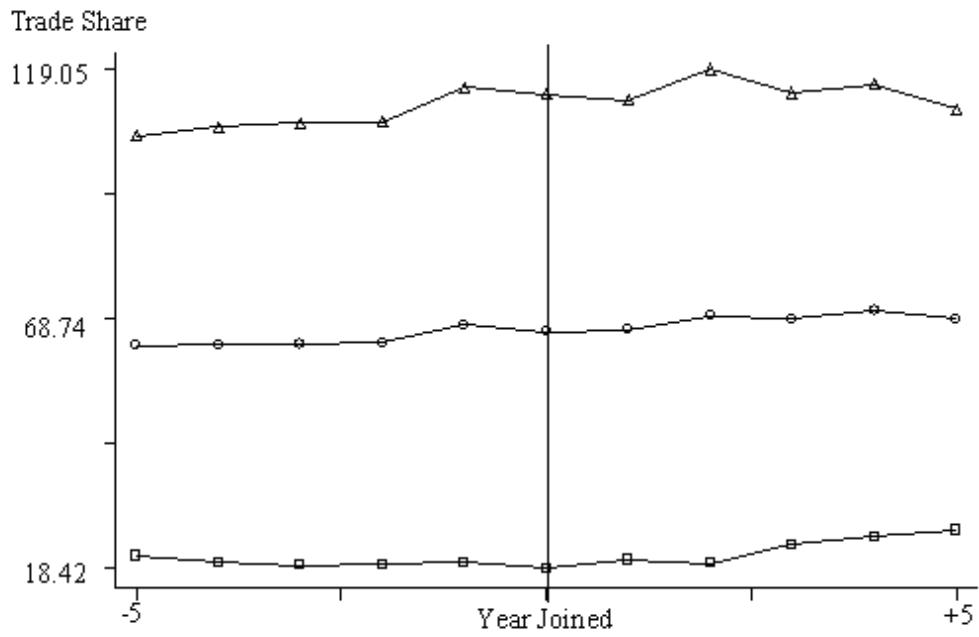
Figure 1–Trade Share within +/-5 Years of Joining the New York Convention

A   Appendix

Table A1–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)

Table A1 (Cont.)

St. Kitts & Nevis	Syria (1959)	Turkey (1992)	Vietnam (1995)
St. Lucia	Tajikistan	Uganda (1992)	Yemen, Rep. of
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.

B Appendix

Table A2–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).
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).

Table A2 (Cont.)

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).

C   Appendix

Table A3–Descriptive Statistics

	Mean	Standard deviation	Correlation with both in NYC	Correlation with one in NYC
Log real trade	10.20	3.27	0.22	-0.10
Both in NYC	0.27	0.45	1.00	-0.54
One in NYC	0.43	0.50	-0.54	1.00
Both in GATT/WTO	0.55	0.50	0.19	0.02
one in GATT/WTO	0.38	0.48	-0.14	0.01
GSP	0.25	0.43	0.17	0.12
Regional FTA	0.02	0.13	0.05	-0.05
Currency union	0.02	0.13	-0.05	-0.02
Log distance	8.19	0.81	-0.05	0.08
Log product real GDP	35.17	2.58	0.35	-0.05
Log product real GDP p/c	16.81	1.44	0.27	-0.02
Common language	0.22	0.42	-0.07	-0.02
Shared border	0.03	0.17	-0.01	-0.07
Number landlocked	0.27	0.49	0.003	0.04
Number islands	0.31	0.52	-0.03	0.03
Log product land area	24.54	3.06	0.07	-0.07
Common colonizer	0.09	0.29	-0.07	-0.001
Currently colonized	0.002	0.04	-0.02	-0.02
Ever colony	0.02	0.15	0.003	-0.01
Common country	0.0003	0.002	-0.002	0.003

Observations: 175,508

Notes

[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).

[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.

[4]Figure calculated using 2003 world merchandise and commercial services export data from the WTO (2004) and 2003 world GDP data from the World Bank (2004) available at: http://www.worldbank.org/data/databytopic/GDP.pdf.

[5]For classic treatments of international arbitration within the legal literature see: David (1985) and Trakman (1983).

[6]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.

[7]As Rusk 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" (1984: 19). 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 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.

[8]Two other benefits of international arbitration are its speed, enabled by an extremely limited capacity to appeal, and the privacy if 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 much specific data regarding international arbitration. A concern for privacy in the process of dispute is especially important to firms that keep closely-guarded trade secrets they do not wish to be made public.

[9]This useful terminology comes from Cutler (2003).

[10]For discussions of the modern law merchant within the legal literature, among others, see: Berman and Kaufman (1978); Cremades and Plehn (1983-1984); Carbonneau (1984); Schmitthoff (1961).

[11]Leeson (2004), 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). Also, 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) examines 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).

[12]In recent years, UNCITRAL and UNIDROIT have both 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.

[13]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 generally more open ended with respect to procedure. *Ad hoc* arbitration is organized and administrated 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 is unavailable.

[14]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 percent of all parties to international arbitration through the ICC and 12 percent of its cases included state or parastatal entities (ICC Bulletin 2001).

[15]This pattern holds for the other major international arbitration institutions as well. For instance, the ICDR, a much smaller international arbitration forum than say 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).

[16]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 who are not also signatories of the treaty (Article 1(3)). 68 nations have signed subject to this condition. (2) The commercial reservation—states are not required to enforce arbitral awards related to non-commercial matters, with the commerciality of a matter being defined by the state's national law (Article 1(3)). 43 nations have signed subject to this condition.

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

[18]See Bonturi and Fukasaku (1993). See also Zeile (1997) and Rangan (2001).

[19]The EU Convention poses no potential problem for my estimates since it was not created until four years after my sample ends.

[20]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.

[21]'Both in NYC' has a coefficient of -1.45 with a robust standard error of 0.02. 'One in NYC' has a coefficient of -0.99 with a robust standard error of 0.01.