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One reason countries pay their debts: renegotiation and international trade

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Abstract

I estimate the effect of sovereign debt renegotiation on international trade. Sovereigns may fear the trade consequences of default; because creditors deter default, or because trade finance dries up. I use an empirical gravity model of trade and a panel data set covering 50 years, over 150 countries, and other factors that influence bilateral trade. Debt renegotiation is associated with an economically and statistically significant decline in bilateral trade between a debtor and its creditors. The decline in bilateral trade is approximately 8% a year and persists for around 15 years.

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1. Introduction

Why do countries pay their international debts? Three reasons are typically proposed. First, countries that renege on their debts may have their overseas assets seized by foreign creditors. Second, countries with poor repayment reputations may be cut off from capital flows in the future. Third, delinquent countries may suffer reduced

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benefits of international trade. While all three penalties are of interest, this paper is concerned with the last explanation. The first sanction is of limited potency for net debtors with little foreign collateral. A number of economists (most visibly Bulow and Rogoff) have disputed the importance of future exclusion from capital markets. The third explanation is widely accepted, but has never been quantified. The objective of this paper is to estimate empirically the effect of sovereign debt renegotiations on international trade.

There are a number of reasons why international default may reduce trade in principle. First and most plausibly, trade credit may naturally shrink after default. Alternatively, creditors may wish to punish default with reduced trade benefits, in order to discourage future default, or default by third parties. While these theories are of interest, the focus in this paper is wholly empirical in nature. It turns out that, for whatever reason, it is logical for sovereigns to fear default since in practice, default is strongly associated with reduced trade. I demonstrate this by using a large panel data set covering over 200 trading partners over 50 years of data to estimate a "gravity" model of trade. I show that debt renegotiation is associated with a decline in bilateral trade that is both economically and statistically significant, adding up to a year's worth of trade, although the effect is spread over 15 years.

The next section presents an intuitive and institutional framework to understand the relationship between sovereign default and international trade, focusing on debt renegotiations at the Paris Club. Next, the empirical methodology and data set are discussed. The actual empirical results are presented in the fifth section, which includes sensitivity analysis. The paper finishes with some suggestions for future work and a brief conclusion.

2. Why might sovereign default affect trade?

There is a large literature on the issue of sovereign default; Eaton and Fernandez (1995) and Obstfeld and Rogoff (1996, chapter 6) provide recent surveys.[2] However, little of it has been explicitly concerned with the interaction between default and trade. In this section, I provide a mostly institutional description of debt rescheduling through the Paris Club. A theoretical framework for the empirical analysis that follows is relegated to an appendix.

From a theoretical perspective, there are many reasons why sovereign default could affect trade. A theoretically interesting reason is that a creditor may want to discourage further default (either by the debtor in the future or by other debtors), with a punishing decline in trade. A more banal (but probably more realistic) reason is that default may naturally result in a drying up of short-term trade credit, the vehicle used to finance most international trade. In practice, it is difficult and, for my purposes, unimportant, to differentiate between these explanations (and others), so long as sovereigns fear the

[2] See also Tomsz (2003) and Wright (2001) for recent work on the reputation argument.

trade effects of debt renegotiation. These ideas are explored in more detail in a longer working paper version, which is freely available on the internet.

2.1. Sovereign debt renegotiation in practice

In practice, it is rare for a country simply to default on (let alone repudiate) its international financial obligations. Instead, it typically renegotiates its debts, usually through the "Paris Club." In this section, I provide a brief overview of the debt renegotiation process. More information on the Paris Club is provided by Sevigny (1990), Eichengreen and Portes (1995), the website of the Paris Club, and the longer version of this paper.

The Paris Club is an informal group of official creditors that meets to discuss issues associated with external debts of developing countries, and renegotiate these debts.[3] The Paris Club is informal and has no legal basis or status; instead it adheres to a set of principles. Three of the key principles are particularly germane. First, all decisions by creditors are taken by consensus, ensuring "creditor solidarity." Second, the Paris Club preserves "comparability of treatment" between all creditors, with the exceptions of the international financial institutions that are treated as preferred. Third, the Paris Club prefers that deals be negotiated only for countries that are engaged in an IMF-approved program.

Paris Club agreements apply to public sector debt as well as private debt guaranteed by the public sector.[4] It is important to note that only medium and long-term debts (which constitute over 80% of all developing country debt) are rescheduled.[5] To quote the Paris Club: "Short term debt (debt with a maturity of one year or less) is excluded from the treatments, as their restructuring can create a significant disruption of the capacity of the debtor country to participate in international trade."[6]

The Paris Club provides four different types of renegotiation. "Classic terms" include: 5 years of grace; semi-annual principal repayment terms in years 6 to 10; and a moratorium interest rate which is designed to keep the net present value of the debt intact. Additional terms have been made available more recently which involve a grant element, and are not considered in this paper.

Paris Club agreements seem to be the most appropriate dates for measuring sovereign default. Potential alternative dating schemes use either the onset of arrears of international payments (of interest, principal, or both), or the onset of sovereign defaults to private creditors measured by Standard and Poor's. Both seem to be inferior measures. For instance, there were 283 Paris Club deals through 1997 (some of which

[3] Technically speaking, "rescheduling" amends the terms of a loan so as to stretch out payments due over time, while "refinancing" achieves the same effect by providing a new loan equal to the debt service due; Sevigny (1990). For simplicity, I use the term "renegotiation."

[4] The "London Club" handles the renegotiation of international banks' exposure to sovereign borrowers.

[5] The Paris Club website indicates that on December 31, 1999, developing countries owed $2550 billion of which $2071 was long-term debt; $1580 (76%) of the latter was public- and publicly guaranteed. See http://www.clubdeparis.org/en/presentation/presentation.php?BATCH=B03WP01.

[6] www.clubdeparis.org/en/presentation/presentation.php?BATCH=B01WP04#B1.

were not "classic"), 163 spells of arrears, and only 82 spells of S&P defaults. The overlap is often low. While some of the arrears spells were clearly defaults, some defaults or arrears were officially or quietly encouraged (Dooley, 2000). Further, arrears were rarely absolute; partial debt service was routinely continued during periods of arrears and was usually comparable to (or higher than) the size of arrears. This makes it difficult to measure the nature and scope of default simply though using the presence of arrears. Further, arrears and defaults are multilateral concepts, whereas Paris Club information is available on a bilateral basis. For all these reasons, I use the dates of Paris Club deals to date sovereign debt renegotiation, though I do use arrears and sovereign defaults as sensitivity checks.

3. Empirical methodology and data

3.1. Estimation strategy

I use a conventional gravity model to model bilateral trade flows, augmented with a number of extra controls:

$$\begin{aligned}\ln(X_{ijt}) = {} & \beta_0 + \beta_1 \ln(Y_i Y_j)_t + \beta_2 \ln\left(Y_i Y_j / \mathrm{Pop}_i \mathrm{Pop}_j\right)_t + \beta_3 \ln D_{ij} + \beta_4 \mathrm{Lang}_{ij} \\ & + \beta_5 \mathrm{Cont}_{ij} + \beta_6 \mathrm{FTA}_{ijt} + \beta_7 \mathrm{Landl}_{ij} + \beta_8 \mathrm{Island}_{ij} + \beta_9 \ln(\mathrm{Area}_i \mathrm{Area}_j) \\ & + \beta_{10} \mathrm{ComCol}_{ij} + \beta_{11} \mathrm{CurCol}_{ijt} + \beta_{12} \mathrm{Colony}_{ij} + \beta_{13} \mathrm{ComNat}_{ij} + \beta_{14} \mathrm{CU}_{ijt} \\ & + \beta_{15,0} \mathrm{IMF}_{ijt} + \Sigma_k \beta_{15,k} \mathrm{IMF}_{ijt-k} + \phi \mathrm{RENEG}_{ijt} + \Sigma_m \phi_m \mathrm{RENEG}_{ijt-m} + \varepsilon_{ijt}\end{aligned}$$

where i and j denotes countries, t denotes time, and the variables are defined as:

- X_{ijt} denotes the average value of real bilateral trade between i and j at time t,
- Y is real GDP,
- Pop is population,
- D_{ij} is the distance between i and j,
- Lang is a binary variable which is unity if i and j have a common language,
- Cont is a binary variable which is unity if i and j share a land border,
- FTA is a binary variable which is unity if i and j belong to the same regional trade agreement,
- Landl is the number of landlocked countries in the country-pair dyad (0, 1, or 2).
- Island is the number of island nations in the pair (0, 1, or 2),
- Area is the land mass of the country,
- ComCol is a binary variable which is unity if i and j were ever colonies after 1945 with the same colonizer,
- CurCol is a binary variable which is unity if i and j are colonies at time t,
- Colony is a binary variable which is unity if i ever colonized j or vice versa,
- ComNat is a binary variable which is unity if i and j remained part of the same nation during the sample (e.g., France and Guadeloupe, or the UK and Bermuda),

- CU is a binary variable which is unity if i and j use the same currency at time t,
- IMF is one/two if one/both of i or/and j began an IMF program at t and zero otherwise,
- RENEG is a binary variable which is unity if i and j renegotiated international debt at time t and zero otherwise,
- K and M are unknown lag lengths,
- β are a set of nuisance coefficients, and
- ε represents the myriad other influences on bilateral trade, assumed to be well behaved.

The coefficients of interest to me are $\{\phi\}$, the effect of current and lagged debt renegotiations on trade.

I estimate the model with both fixed and random effects panel data estimators. The fixed-effects ("within") estimator is equivalent to adding a comprehensive set of (11,178) country pair-specific intercepts to the estimating equation. Thus any time-invariant factor that is common to the pair of countries is automatically taken into account (e.g., distance). This ensures consistent estimation of ϕ under a wide range of circumstances, but may not be efficient. GLS/random-effects ("variance components") can be more efficient, but is well known to be consistent only in a more restricted set of circumstances.

3.2. The data set

The trade data used in this paper are taken from the "Direction of Trade" data set developed in CD-ROM form by the International Monetary Fund (IMF); the same data set is used by Glick and Rose (2002). The data set covers bilateral trade between all 217 entities measured by the IMF between 1948 and 1997 (thought many observations are missing). Not all of the trading partners are "countries" in the conventional sense of the word; colonies (e.g., Bermuda), territories (e.g., Guam), overseas departments (e.g., Guadeloupe), countries that gained their independence (e.g., Guinea-Bissau), and so forth are all included. I use the term "country" simply for convenience. (A comprehensive list of the countries is available in the longer version of this paper.) Bilateral trade on FOB exports and CIF imports is recorded in American dollars; I deflate trade by the American CPI. I create an average value of bilateral trade between a pair of countries by averaging all four trade flows available.

To this data set, I add a number of other variables that are necessary to estimate the gravity model. Population and real GDP data (in constant dollars) are taken from three sources. Wherever possible, I use "World Development Indicators" (taken from the World Bank's WDI 2000 CD-ROM) data. When the data are unavailable from the World Bank, I fill in missing observations with comparables from the Penn World Table Mark 5.6, and (when all else fails), from the IMF's "International Financial Statistics". The series have been checked and corrected for errors.

I exploit the CIA's "World Factbook" for a number of country-specific variables. These include: latitude and longitude, land area (in square kilometers), landlocked and island status, physically contiguous neighbors, language, colonizers, and dates of independence. I use these to create great-circle distance (in miles) and other controls. I obtain data from the

World Trade Organization to create an indicator of regional trade agreements, and include: EEC/EC/EU; US-Israel FTA; NAFTA; CARICOM; PATCRA; ANZCERTA; and Mercosur.[7] Currency union pairs are taken from Glick and Rose (2002).

The Paris Club's website provides data on all agreements including: the date; the cutoff date; the type of treatment (Classic/Naples, etc.); the list of participating creditor and observer countries; the amount of debt treated; the current status of the agreement; and so forth. I use these data in order to construct my dummy variable for debt renegotiations, RENEG, which is unity in the year when a pair of countries was involved with each other in a Paris Club deal, and zero otherwise. (Details on these renegotiations are available at my website.)[8]

"Classic" Paris Club agreements are almost always conditioned on IMF programs; in my sample, over 80% of Paris Club agreements coincide with an IMF program signed in the same year. However, not all IMF programs are associated with Paris Club agreements. Indeed, while there were 283 Paris Club deals though 1997 (of which 163 were "classic"), there were 898 IMF programs initiated during the same time. (Of these, over 80% (739) were "Stand-bys Arrangements," designed to address short-term payments imbalances.) Since the implementation of an IMF program is often associated with economic trauma and/or reform, it is important to condition on the existence of an IMF program in determining the additional marginal effect of any debt renegotiations. My variable, IMF_{ijt} is a dummy variable that is unity if either country i or j *initiated* an IMF program (of any type) during year t. It takes on a value of two if both i and j begin an IMF program in the year, and zero otherwise.[9]

4. Empirical results

4.1. Benchmark results

Benchmark results are reported in Table 1. In the middle of the table, I tabulate fixed- and random-effects estimates for an empirical model with contemporaneous and fifteen lags of the dummy variable for debt renegotiation, five lags of IMF program inception (i.e., K=5, M=15). The gaps in the fixed-effect columns reflect the fact that

[7] All FTAs are treated as being equal for simplicity.

[8] A few multilateral official debt renegotiations have been conducted outside the Paris Club forum, e.g., by the OECD, creditor groups, or special task forces. Information on these has been included from records of the Paris Club and *Global Development Finance*.

[9] Descriptive statistics for the variables are provided in the working paper version, along with OLS coefficients from a simple regression of the log of trade on the contemporaneous regressors (and an unrecorded intercept), and simple bivariate correlations. It is interesting to note that the OLS coefficient for renegotiation is positive. Negative estimates (which are presented below) manifestly depend on a more sophisticated estimator that takes into account the panel nature of the data set. It is also worth noting that the simple correlation between Paris Club negotiations and trade is positive; any negative effect relies on conditioning and/or a more sophisticated estimator. Further, the incidence of bilateral Paris Club negotiations has only low correlations with the other (nuisance) variables. While the correlations are statistically significant given the sample size, none exceeds 0.1 in magnitude.

Table 1
Debt renegotiation and trade

	Fixed	Random	Fixed	Random	Fixed	Random
RENEG	−0.06 (0.04)	−0.08 (0.04)	−0.05 (0.04)	−0.06 (0.04)	−0.07 (0.03)	−0.09 (0.03)
RENEG: lag 1	−0.07 (0.04)	−0.09 (0.04)	−0.07 (0.04)	−0.09 (0.04)		
RENEG: lag 2	−0.06 (0.04)	−0.08 (0.04)	−0.07 (0.04)	−0.08 (0.04)		
RENEG: lag 3	−0.06 (0.04)	−0.07 (0.04)	−0.06 (0.04)	−0.08 (0.04)		
RENEG: lag 4	−0.03 (0.04)	−0.04 (0.04)	−0.03 (0.04)	−0.04 (0.04)		
RENEG: lag 5	−0.04 (0.04)	−0.05 (0.04)	−0.02 (0.04)	−0.04 (0.04)		
RENEG: lag 6	0.00 (0.04)	−0.02 (0.04)	0.00 (0.04)	−0.02 (0.04)		
RENEG: lag 7	−0.04 (0.04)	−0.07 (0.04)	−0.04 (0.04)	−0.07 (0.04)		
RENEG: lag 8	−0.06 (0.04)	−0.09 (0.04)	−0.07 (0.04)	−0.11 (0.04)		
RENEG: lag 9	−0.06 (0.04)	−0.09 (0.04)	−0.09 (0.04)	−0.13 (0.04)		
RENEG: lag 10	−0.07 (0.04)	−0.10 (0.05)	−0.11 (0.04)	−0.16 (0.04)		
RENEG: lag 11	−0.12 (0.05)	−0.16 (0.05)				
RENEG: lag 12	−0.06 (0.05)	−0.09 (0.05)				
RENEG: lag 13	−0.10 (0.06)	−0.13 (0.06)				
RENEG: lag 14	−0.09 (0.06)	−0.13 (0.06)				
RENEG: lag 15	−0.09 (0.07)	−0.12 (0.07)				
IMF	−0.09 (0.01)	−0.10 (0.01)	−0.10 (0.01)	−0.11 (0.01)	−0.10 (0.01)	−0.11 (0.01)
IMF: lag 1	−0.02 (0.01)	−0.03 (0.01)				
IMF: lag 2	−0.01 (0.01)	−0.02 (0.01)				
IMF: lag 3	−0.01 (0.01)	−0.02 (0.01)				
IMF: lag 4	−0.00 (0.01)	−0.01 (0.01)				
IMF: lag 5	0.03 (0.01)	0.01 (0.01)				
Log Distance		−1.4 (0.03)		−1.4 (0.03)		−1.3 (0.03)
Log Real GDP	0.07 (0.01)	0.30 (0.01)	0.06 (0.01)	0.28 (0.01)	0.05 (0.01)	0.28 (0.01)
Log GDP p/c	0.77 (0.01)	0.49 (0.01)	0.78 (0.01)	0.51 (0.16)	0.78 (0.01)	0.51 (0.01)
Language		0.19 (0.06)		0.18 (0.06)		0.18 (0.06)
Border		0.52 (0.16)		0.53 (0.16)		0.53 (0.16)
Regional FTA	0.68 (0.04)	0.65 (0.04)	0.68 (0.04)	0.65 (0.04)	0.69 (0.04)	0.66 (0.04)
Landlocked		−0.86 (0.04)		−0.86 (0.04)		−0.86 (0.04)
Island		−0.05 (0.05)		−0.06 (0.05)		−0.06 (0.05)
Log Area		0.24 (0.01)		0.25 (0.01)		0.25 (0.01)
Com. colonizer		−0.26 (0.08)		−0.27 (0.08)		−0.27 (0.08)
Cur. colony	0.37 (0.09)	0.44 (0.09)	0.37 (0.09)	0.43 (0.09)	0.37 (0.09)	0.44 (0.09)
Ex-colonizer–colony		3.2 (0.20)		3.2 (0.20)		3.2 (0.20)
Same country		1.2 (0.20)		1.3 (1.58)		1.3 (1.58)
Currency union	0.64 (0.05)	0.68 (0.05)	0.64 (0.01)	0.69 (0.05)	0.64 (0.05)	0.69 (0.05)
P(All RENEG=0)	0.0000	0.0000	0.0000	0.0000	0.04	0.01
$\sum$RENEG	−0.99 (0.13)	−1.4 (0.13)	−0.60 (0.09)	−0.88 (0.09)		
R^2 within	0.12	0.12	0.12	0.12	0.12	0.12
R^2 between	0.25	0.53	0.24	0.53	0.24	0.52
R^2 overall	0.25	0.47	0.24	0.47	0.23	0.47

Intercepts not recorded. Standard errors in parentheses. 219,573 observations in 11,178 dyads.

any time-invariant characteristic of the country-pair is wiped out by the pair-specific intercepts.

The model works well in a number of senses. The standard "gravity" effects are present; countries that are further apart geographically trade less, while larger and richer pairs of countries trade more. Countries that share a common currency, a

common language, a common border, or membership in a regional free trade agreement trade more. Landlocked countries and islands trade less, and most of the colonial effects are large and positive. Almost all these effects are economically and statistically significant. The model also explains a reasonable percentage of the data variation. The inception of IMF programs is associated with a drop in bilateral trade of about 10%, holding other things equal. This effect is economically and statistically large, but transient. After around 3 years, this effect dies away, and turns slightly positive after 5 years.

Above and beyond all these (mostly) conventional effects on bilateral trade, debt renegotiations seem to have a substantial negative effect on international trade. The effect is somewhat sensitive to the exact method of estimation; the fixed effects estimator indicates a decline of trade of about 7% annually, while the GLS estimator shows a larger effect of 9%. Both effects are highly persistent, lasting around 15 years at more or less constant levels. While the individual ϕ coefficients are often statistically insignificant because of multicollinearity, the hypothesis that debt renegotiations have no effect on trade can be rejected at any reasonable significance level. Further, the cumulative effect of renegotiations on trade is also large negative and significant. The effect averages about 8% annually and persists for about 15 years. The two middle columns of Table 1 show that these effects are not especially sensitive to the exact specification of the lag length; eliminating the lags of IMF program inception and dropping the last five renegotiation lags does not destroy the negative effect of debt renegotiation on trade. The two columns at the right demonstrate that the effect is smaller but still present without any lags at all.[10]

4.2. Lag length

The appropriate number of lags of debt renegotiation (M) is unknown. Does uncertainty about M affect any economic conclusions? No. Table 2 explores the effects of different lag lengths for the debt renegotiation variable. To simplify the analysis, I impose equality on the coefficients of lagged debt renegotiations. Thus in the left-hand columns of the top panel of Table 2, I tabulate the fixed- and random-effects estimates of ϕ_k for k=1,...,5 where a single coefficient is estimated for lags of RENEG between one and five. (Coefficients for the contemporaneous and 5 values of IMF program inception and the other nuisance coefficients are not reported.) In the next columns to the right, I add a tenth-order term to the fifth-order term.[11] At the extreme right of the table, I have four separate coefficients, representing lags up to twenty, up to fifteen, up to ten, and up to 5 years after debt renegotiations. The top panel includes five lags of IMF program inception as well as the contemporaneous impact, while the middle panel reports the analogous statistics when IMF program inception is modeled as only having a contemporaneous effect.

[10] An "event-study" approach corroborates the main finding of an economically and statistically significant drop in trade for debt renegotiators, and is available in the longer version of the paper.

[11] Thus, the fixed-effect estimation of renegotiations between 1 and 5 years ago is derived by adding 0.09 and −0.23, while the effect of renegotiation between 6 and 10 years ago is simply −0.23.

Table 2
Varying the lag structure of renegotiation

	Fixed	GLS	Fixed	GLS	Fixed	GLS	Fixed	GLS
Including 5 lags of IMF program								
MA(5) of RENEG	−0.11	−0.15	0.09	0.13	0.09	0.14	0.09	0.14
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
MA(10) of RENEG			−0.23	−0.32	−0.01	−0.00	−0.01	−0.00
			(0.03)	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)
MA(15) of RENEG					−0.24	−0.35	−0.22	−0.25
					(0.05)	(0.05)	(0.11)	(0.10)
MA(20) of RENEG							−0.02	−0.10
							(0.09)	(0.09)
Without IMF lags								
MA(5) of RENEG	−0.12	−0.17	0.08	0.11	0.08	0.12	0.08	0.12
	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
MA(10) of RENEG			−0.23	−0.32	−0.01	−0.00	−0.22	−0.00
			(0.04)	(0.03)	(0.05)	(0.06)	(0.10)	(0.06)
MA(15) of RENEG					−0.24	−0.34	−0.02	−0.25
					(0.05)	(0.05)	(0.09)	(0.10)
MA(20) of RENEG							−0.02	−0.09
							(0.09)	(0.09)

	With 5 IMF lags	With 5 IMF lags	Without IMF lags	Without IMF lags
	Fixed	GLS	Fixed	GLS
Are 5 extra lags required?				
Conditional on 5 lags	0.0001	0.0000	0.0002	0.0000
Conditional on 10 lags	0.0002	0.0000	0.0002	0.0000
Conditional on 15 lags	0.1542	0.0177	0.1783	0.0329

Probability value for hypothesis $\Pi\phi_k=0$.
Regressors not recorded include: Contemporaneous values of RENEG and IMF; currency union; log distance; real GDP; real GDP per capita; common language; border; regional FTA; landlocked; island; log area; common colonizer; current colony; ex-colony; common country; and intercept.
Number of observations=219,573 in 11,178 dyads.

The results indicate that debt renegotiations have a persistent effect, one that seems to last about 15 years. This result does not depend very strongly on which estimation method is used, or whether lags of the IMF variable are included. The bottom panel of Table 2 confirms this. It reports probability values for the hypothesis $\Pi_m\phi_m=0$ for values of $m>M$, where M=5,10,15. As M rises to 15, the hypothesis that an additional five lags are not required becomes reasonable with the within estimator (though it is more marginal with GLS). Including fifteen lags of debt renegotiation and five of the IMF variable seems both intuitively and statistically reasonable. Still, it is inappropriate to place much confidence in the exact lag length, given that: (a) many debt renegotiations have only taken place in the last 15 years, and (b) the lags seem quite long.

In passing, I note that adding one or two *leads* of Paris Club renegotiation has no effect on the economic or statistical significance of debt renegotiation; the leads themselves are

insignificant. This provides further evidence that the Paris Club dates are appropriate dates for debt renegotiation.

4.3. Censoring, simultaneity and sensitivity analysis

Trade is bounded below by zero, so a technique that takes this constraint into account may be preferable to my default estimators, which are both linear. Thus Table 3 presents a random-effects panel Tobit estimator.[12] Reassuringly, the results are quite similar to those of Table 1 (though they are considerably more computationally demanding).

Debt renegotiation may be caused by shocks that also cause trade flows to shrink; that is, the estimation strategy may be biased because trade and debt renegotiation are simultaneously determined by some other factor that has been omitted from the statistical analysis. While theoretically plausible, there is no direct evidence indicating that this issue is important in practice. A long unsuccessful research program attempted to find variables systematically associated with sovereign default in order to create leading and contemporaneous indicators of default. Babbel (1996) provides an annotated bibliography of the literature, while Eaton et al. (1986) provide an earlier survey. Finally, it is worth repeating that my findings indicate that renegotiation causes trade to shrink relative to income; shocks that cause both income and trade to fall proportionately would not explain my results.

Still, there is no reason in principle not to analyze regressors that are potentially associated with sovereign default. I proceed by using potential causes of default as instrumental variables in the trade equation. Table 3 uses three instrumental variables: (1) the government budget surplus/deficit (expressed as a percentage of GDP); (2) the CPI inflation rate; and (3) the current account surplus/deficit (percentage of GDP). In each case, I use values (for both i and j) of these instrumental variables for contemporaneous debt renegotiation and the onset of IMF programs. All the regressors were taken from the World Bank's *WDI* 2000 CD-ROM.

Reassuringly, both fixed- and random-effects indicate that simultaneity bias is not responsible for the negative effect of renegotiation on trade; both the joint and the cumulatively negative effects remain significant. Nevertheless, the IV estimates are obtained only with a dramatic reduction in observations since the macroeconomic instrumental variables are missing for many of the original observations.[13] Further, the instrumental variables are poor in the sense that they deliver imprecise estimates; while $\Pi_m \phi_m$ and $\Sigma_m \phi_m$ remain negative and significant, the standard errors are much larger.

More sensitivity analysis is presented in Tables 4a–4c. The top panel (Table 4a) performs a variety of sensitivity experiments with respect to the sample. It reports probability values for a key hypothesis, namely $\Pi_m \phi_m = 0 \forall m$, as well as the point estimate of $\Sigma_m \phi_m$, along with an appropriate standard error. The statistics are reported for both fixed- and random-effects estimators for four different samples: (1) the default

[12] For the Tobit estimation, small values of trade (less than $1000) are set to zero.

[13] I have also used different sets of IVs with similar, though usually weaker results.

Table 3
Estimator sensitivity: panel Tobit and instrumental variables estimates

	Random effect Tobit	Fixed effects, IV	Random effects, IV
RENEG	−0.08 (0.04)	−2.26 (1.52)	−9.35 (2.29)
RENEG: lag 1	−0.09 (0.04)	−0.24 (0.06)	−0.29 (0.07)
RENEG: lag 2	−0.08 (0.04)	0.14 (0.21)	1.33 (0.34)
RENEG: lag 3	−0.08 (0.04)	−0.16 (0.06)	−0.28 (0.07)
RENEG: lag 4	−0.05 (0.04)	−0.07 (0.05)	0.15 (0.08)
RENEG: lag 5	−0.05 (0.04)	−0.11 (0.06)	−0.15 (0.07)
RENEG: lag 6	−0.03 (0.04)	−0.11 (0.06)	−0.27 (0.08)
RENEG: lag 7	−0.07 (0.04)	−0.27 (0.16)	−1.02 (0.23)
RENEG: lag 8	−0.10 (0.04)	−0.33 (0.19)	−1.24 (0.27)
RENEG: lag 9	−0.09 (0.04)	−0.15 (0.07)	−0.25 (0.09)
RENEG: lag 10	−0.11 (0.05)	−0.19 (0.12)	−0.60 (0.16)
RENEG: lag 11	−0.18 (0.05)	−0.29 (0.19)	−1.24 (0.27)
RENEG: lag 12	−0.09 (0.05)	−0.25 (0.17)	−0.97 (0.23)
RENEG: lag 13	−0.13 (0.06)	−0.12 (0.11)	−0.50 (0.14)
RENEG: lag 14	−0.15 (0.06)	−0.14 (0.11)	−0.51 (0.15)
RENEG: lag 15	−0.14 (0.07)	−0.07 (0.09)	−0.13 (0.12)
IMF	−0.11 (0.01)	0.14 (0.17)	−0.39 (0.25)
IMF: lag 1	−0.03 (0.01)	0.03 (0.03)	−0.01 (0.03)
IMF: lag 2	−0.02 (0.01)	0.01 (0.01)	0.04 (0.02)
IMF: lag 3	−0.02 (0.01)	0.00 (0.01)	0.05 (0.02)
IMF: lag 4	−0.01 (0.01)	−0.01 (0.01)	−0.01 (0.016)
IMF: lag 5	0.00 (0.01)	−0.01 (0.01)	0.01 (0.02)
Log Distance	−1.47 (0.02)		−1.46 (0.04)
Log Real GDP	0.39 (0.005)	0.27 (0.04)	0.80 (0.02)
Log GDP p/c	0.43 (0.01)	0.75 (0.06)	0.42 (0.04)
Language	0.10 (0.03)		0.42 (0.08)
Border	−1.57 (0.05)		0.09 (0.24)
Regional FTA	0.48 (0.04)	0.21 (0.07)	0.07 (0.09)
Landlocked	−0.76 (0.02)		−0.50 (0.07)
Island	0.24 (0.02)		0.06 (0.07)
Log Area	0.24 (0.01)		0.04 (0.02)
Com. colonizer	−0.18 (0.07)		0.01 (0.12)
Cur. colony	0.53 (0.08)	−1.39 (0.47)	−0.86 (0.63)
Ex-colonizer–colony	2.33 (0.04)		2.40 (0.25)
Same country	2.72 (0.19)		
Currency union	0.68 (0.06)	0.00 (0.30)	0.83 (0.28)
P(All RENEG=0)	0.0000	0.0000	0.0000
$\sum$RENEG	−1.54 (0.12)	−4.61 (2.49)	−15.3 (3.5)
R^2 within		0.02	0.01
R^2 between		0.52	0.64
R^2 overall		0.52	0.56
Observations	219,573	59,481	59,481

Standard errors in parentheses. Instrumental variables: domestic and foreign CPI inflation rates, current accounts and budget surplus/deficit (latter expressed as percentage of GDP).

entire sample; (2) the sample without the 1990s; (3) the sample without African observations; and (4) the sample without Latin-American observations. All the evidence indicates that debt renegotiation has a statistically significant effect on trade,

Table 4a
Sample sensitivity analysis

	Fixed effects	Fixed effects	Random effects/GLS	Random effects/GLS
	All RENEG=0	∑RENEG	All RENEG=0	∑RENEG
Default	0.00	−0.99 (0.13)	0.00	−1.43 (0.13)
Without 1990s	0.01	−0.23 (0.23)	0.00	−0.57 (0.23)
Without Africa	0.00	−0.59 (0.16)	0.00	−0.80 (0.16)
Without Latins	0.00	−1.00 (0.14)	0.00	−1.54 (0.14)

Probability values for "All RENEG=0;" coefficient values and standard error for ∑RENEG.
Benchmark regression: Contemporaneous and 15 lags of RENEG; contemporaneous and 5 lags of IMF; currency union; log distance; real GDP; real GDP per capita; common language; border; regional FTA; landlocked; island; log area; common colonizer; current colony; ex-colony; common country; and intercept.
Number of observations=219,573 in 11,178 dyads.

and that the cumulative effect is negative. For one of the perturbations (when the fixed effects estimator is used without the 1990s), the cumulative effect is negative but with a *t*-statistic of unity.[14]

The second panel of Tables 4a–4c replaces the dates of Paris Club deals (and their lags) with two other measures of default. The first measure is simply the presence of arrears. The question is whether (either interest or principal) arrears in either country has a negative effect of trade, holding other factors constant. I use arrears data from the Global Development Finance, 2001 CD-ROM, which provide series for 137 developing countries from 1970 through 1997. The sample is thus considerably smaller than that of my benchmark results, since there are no early observations, nor are there observations for rich or small countries. Still, the results at the left of Table 4b show that the presence of arrears seems to dampen trade considerably. The fixed effects estimate is 8%, while the effect is over 20% with GLS.

The second measure I use in Table 4b is the log of the product of *Institutional Investor* country ratings. These are available for 118 countries from 1979 through 1997 (though some observations are missing), so that the sample is again much smaller. The country ratings are derived from surveys of leading banks who are asked to rank each country's creditworthiness on a scale from 0 to 100 (best); Haque et al. (1997) provide more detail. Since it is unclear exactly what is being measured, or how the banks are measuring it, it is important not to over-interpret these data. Still, it seems interesting to ask if higher creditworthiness is associated increased trade. The results at the right of Table 4b show that higher country ratings are indeed associated with much higher trade, and thus reduced creditworthiness is associated with less trade.

Finally, in Table 4c I use sovereign defaults as measured by Standard and Poor's. In particular, I examine the onset of defaults of foreign currency debt in the form of bonds and bank loans.[15] I examine both rated sovereigns and unrated issuers, and test the

[14] Adding quadratic GDP terms only increases the size of the debt effects on trade.
[15] More analysis and discussion of sovereign default as measured by S&P, as well as the raw data set is available from Tables 4a–4c and 5 of *Sovereign Defaults: Heading Lower into 2004* available at: http://www2.standardandpoors.com/NASApp/cs/ContentServer?pagename=sp/Page/PressSpecialCoveragePg&c=sp_speccoverage&cid=1025056354607&r=1&l=EN&b=5.

Table 4b
Arrears and institutional investor ratings

	Fixed effects	Random effects	Fixed effects	Random effects
Arrears	−0.08 (0.02)	−0.25 (0.02)		
Log Product II Ratings			0.86 (0.03)	1.25 (0.03)
IMF	−0.09 (0.01)	−0.08 (0.01)	−0.01 (0.01)	0.01 (0.01)
Log Distance		−1.52 (0.05)		−1.22 (0.04)
Log Real GDP	0.23 (0.02)	0.55 (0.02)	0.41 (0.02)	0.77 (0.01)
Log GDP p/c	0.71 (0.03)	0.41 (0.03)	0.48 (0.03)	0.25 (0.02)
Language		0.23 (0.10)		0.53 (0.07)
Border		0.92 (0.21)		0.66 (0.18)
Regional FTA	0.43 (0.19)	0.75 (0.17)	0.27 (0.07)	0.23 (0.07)
Landlocked		−0.50 (0.06)		−0.56 (0.05)
Island		0.09 (0.09)		−0.10 (0.06)
Log Area		0.18 (0.02)		−0.01 (0.01)
Com. colonizer		0.39 (0.11)		0.05 (0.10)
Ex-colonizer–colony		0.12 (0.85)		1.45 (0.21)
Cur. colony			−1.77 (0.79)	−1.23 (0.73)
Currency union	0.31 (0.13)	0.33 (0.12)	−0.14 (0.37)	0.39 (0.26)
R^2 within	0.04	0.04	0.07	0.06
R^2 between	0.22	0.49	0.63	0.74
R^2 overall	0.19	0.44	0.59	0.70
Observations	71,925	71,925	72,654	72,654

Intercepts not recorded. Standard errors in parentheses.

hypothesis that any default affects trade between the defaulter and all permanent members of the Paris Club. Table 4c uses the same specification and techniques of Table 1, but substituting the onset of S&P default dates for Paris Club dates. The results are quite similar in economic and statistical significance to those of Table 1; after default, trade falls by a large magnitude for an extended period of time.

To summarize: the finding that debt renegotiation seems to affect trade adversely seems robust to uncertainty with respect to lag lengths, censoring, simultaneity, the exact sample, and using arrears, *Institutional Investor* country ratings, or S&P defaults instead of Paris Club dates.

4.4. Trade diversion

There seems to be evidence that countries that default engage in less bilateral trade with their creditors for a number of years after renegotiation. The costs of this reduced trade to the debtor may be alleviated if trade is merely diverted from creditor countries to others. Thus it is important to test for trade diversion after debt renegotiation.

I test for trade diversion by adding to the default equation, contemporaneous and lagged values of a dummy variable that is unity if (at least) one of the countries rescheduled its debt but the pair of countries was not directly involved in a renegotiation. For instance, Albania rescheduled debt with Austria in 1993, but not with Australia (since Australia is a permanent member of the Paris Club, this implies that its Albanian assets did not exceed the de minimis level). My variable "RENEG" is one for Albania–Austria in 1993, but zero for Albania–Australia; my variable "DIVERT" is exactly the opposite. A positive

Table 4c
Standard and Poor's dates for onset of foreign bank/bond defaults

	Fixed	Random	Fixed	Random
RENEG	−0.07 (0.03)	−0.11 (0.03)	−0.05 (0.03)	−0.08 (0.03)
RENEG: lag 1	−0.13 (0.03)	−0.16 (0.03)	−0.11 (0.03)	−0.15 (0.03)
RENEG: lag 2	−0.16 (0.03)	−0.21 (0.04)	−0.16 (0.03)	−0.20 (0.03)
RENEG: lag 3	−0.14 (0.04)	−0.18 (0.04)	−0.14 (0.03)	−0.18 (0.04)
RENEG: lag 4	−0.16 (0.04)	−0.20 (0.04)	−0.17 (0.04)	−0.21 (0.04)
RENEG: lag 5	−0.14 (0.04)	−0.19 (0.04)	−0.14 (0.04)	−0.19 (0.04)
RENEG: lag 6	−0.13 (0.04)	−0.18 (0.04)	−0.13 (0.04)	−0.18 (0.04)
RENEG: lag 7	−0.12 (0.04)	−0.17 (0.04)	−0.13 (0.04)	−0.19 (0.04)
RENEG: lag 8	−0.15 (0.04)	−0.21 (0.04)	−0.18 (0.04)	−0.25 (0.04)
RENEG: lag 9	−0.13 (0.04)	−0.19 (0.04)	−0.15 (0.04)	−0.22 (0.04)
RENEG: lag 10	−0.14 (0.04)	−0.21 (0.04)	−0.15 (0.04)	−0.22 (0.04)
RENEG: lag 11	−0.17 (0.04)	−0.24 (0.04)		
RENEG: lag 12	−0.14 (0.05)	−0.21 (0.05)		
RENEG: lag 13	−0.14 (0.05)	−0.23 (0.05)		
RENEG: lag 14	−0.12 (0.05)	−0.21 (0.05)		
RENEG: lag 15	−0.07 (0.06)	−0.16 (0.06)		
IMF	−0.09 (0.01)	−0.10 (0.01)	−0.10 (0.01)	−0.11 (0.01)
IMF: lag 1	−0.03 (0.01)	−0.03 (0.01)		
IMF: lag 2	−0.01 (0.01)	−0.02 (0.01)		
IMF: lag 3	−0.01 (0.01)	−0.02 (0.01)		
IMF: lag 4	−0.00 (0.01)	−0.01 (0.01)		
IMF: lag 5	0.02 (0.01)	0.01 (0.01)		
Log Distance		−1.35 (0.03)		−1.35 (0.03)
Log Real GDP	0.08 (0.01)	0.32 (0.01)	0.07 (0.01)	0.30 (0.01)
Log GDP p/c	0.76 (0.01)	0.49 (0.01)	0.77 (0.01)	0.51 (0.01)
Language		0.19 (0.06)		0.18 (0.06)
Border		0.52 (0.16)		0.52 (0.16)
Regional FTA	0.67 (0.04)	0.63 (0.04)	0.67 (0.04)	0.64 (0.04)
Landlocked		−0.85 (0.04)		−0.85 (0.04)
Island		−0.05 (0.05)		−0.05 (0.05)
Log Area		0.24 (0.01)		0.24 (0.01)
Com. colonizer		−0.26 (0.08)		−0.27 (0.08)
Cur. colony	0.36 (0.09)	0.43 (0.09)	0.36 (0.09)	0.43 (0.09)
Ex-colonizer–colony		3.16 (0.20)		3.18 (0.20)
Same country		1.21 (1.54)		1.23 (1.58)
Currency union	0.64 (0.05)	0.68 (0.05)	0.64 (0.05)	0.68 (0.05)
P(All RENEG=0)	0.0000	0.0000	0.0000	0.0000
$\sum$RENEG	−2.13 (0.16)	−3.07 (0.16)	−1.51 (0.12)	−2.06 (0.12)
R^2 within	0.12	0.12	0.12	0.12
R^2 between	0.26	0.53	0.25	0.53
R^2 overall	0.25	0.48	0.25	0.47

Intercepts not recorded. Standard errors in parentheses. 219,573 observations in 11,178 dyads.

coefficient for DIVERT indicates that (e.g., Albanian) trade is diverted away from creditors (e.g., Austria) towards non-creditors (e.g., Australia).

Table 5 adds contemporaneous and lagged values of DIVERT. Independent of how many lags of DIVERT are included, its contemporaneous value has a significantly negative coefficient. Thus the trade of a debtor not only falls with its creditors at the time of

Table 5
Estimating trade diversion

	Fixed	GLS	Fixed	GLS	Fixed	GLS
DIVERT	−0.16 (0.01)	−0.25 (0.01)	−0.16 (0.01)	−0.24 (0.01)	−0.16 (0.01)	−0.24 (0.01)
DIVERT: lag 1			−0.06 (0.01)	−0.13 (0.01)	−0.06 (0.01)	−0.14 (0.01)
DIVERT: lag 2			0.01 (0.01)	−0.05 (0.01)	0.00 (0.01)	−0.06 (0.01)
DIVERT: lag 3			0.03 (0.01)	−0.03 (0.01)	0.01 (0.01)	−0.03 (0.01)
DIVERT: lag 4			0.05 (0.02)	−0.01 (0.02)	0.02 (0.02)	−0.02 (0.02)
DIVERT: lag 5			0.08 (0.02)	0.02 (0.02)	0.05 (0.02)	0.01 (0.02)
DIVERT: lag 6					0.09 (0.02)	0.05 (0.02)
DIVERT: lag 7					0.08 (0.02)	0.03 (0.02)
DIVERT: lag 8					0.04 (0.02)	−0.01 (0.02)
DIVERT: lag 9					0.06 (0.02)	0.02 (0.02)
DIVERT: lag 10					0.07 (0.02)	0.01 (0.02)
DIVERT Lags=0			0.00	0.00	0.00	0.00
∑DIVERT			−0.05 (0.03)	−0.43 (0.03)	0.21 (0.04)	−0.37 (0.04)

"DIVERT" is trade between non-rescheduler and rescheduler.
Standard errors in parentheses.
Regressors not reported: contemporaneous and 15 lags of RENEG; contemporaneous and 5 lags of IMF; currency union; log distance; real GDP; real GDP per capita; common language; border; regional FTA; landlocked; island; log area; common colonizer; current colony; ex-colony; common country; and intercept.
Number of observations=219,573 in 11,178 dyads.

renegotiation, it falls with other countries (and thus the world) as well. But it is interesting to note that this negative effect is much less persistent than that of RENEG. It turns positive within a couple of years using the fixed-effects estimator, and within 5 years using the random-effects estimator. The exact results are sensitive to both the estimator and the number of lags used, so that it is not possible to conclude with any confidence whether or not there has been any trade diversion. But it is clear that trade between debtors and non-creditors is not as dramatically affected by renegotiation as trade between debtors and creditors. This pushes one towards the hypothesis that creditor countries are seeking to punish default, since trade credit might be expected to dry up uniformly.

4.5. Differential effects on exports and imports

Thus far the analysis has focused on total bilateral trade between a pair of countries, rather than on exports and imports separately. But there is no reason why default need have the same effect on a defaulting country's exports and imports. I explore this possibility further in Table 6.

Table 6 is based on estimation of bilateral export flows, rather than total bilateral trade flows. Instead of using a single dummy variable to indicate a Paris Club deal that involved the pair of countries (and fifteen of its lags), I include two variables (and their lags); one for default by the exporting country, and another for default by the importer. The other nuisance variables are included, and results are, as usual, reported for both fixed- and random-effects estimators.

The results indicate that Paris Club renegotiation has similar effects on both exporting and importing countries. As is clear from the first two rows, the joint effect of the

Table 6
Exports and imports

Hypothesis tested	Fixed effects	Random effects/GLS
P(Exporters RENEG=0)	0.0000	0.0000
$\sum$(Exporters RENEG), se	−1.29 (0.14)	−1.76 (0.14)
P(Importers RENEG=0)	0.0000	0.0000
$\sum$(Importers RENEG), se	−0.83 (0.13)	−1.30 (0.13)
P(Exporters RENEG=Importers RENEG)	0.63	0.65
$\sum$(Exporters RENEG)−$\sum$(Importers RENEG), se	−0.46 (0.19)	−0.46 (0.19)

Bilateral real exports. Regressors not reported: contemporaneous and 15 lags of RENEG for both exporting and importing countries; contemporaneous and 5 lags of IMF; currency union; log distance; real GDP; real GDP per capita; common language; border; regional FTA; landlocked; island; log area; common colonizer; current colony; ex-colony; common country; and intercept.
Number of observations=375,364 in 20,643 dyads.

contemporaneous and (fifteen) lagged coefficients of renegotiation on exports is highly statistically significant for both estimators, while the cumulative effect is economically and statistically large. The middle rows indicate that much the same effects characterize imports, though the cumulative effects are smaller. At the bottom, I test two hypotheses. The second line from the bottom is a test of the hypothesis that the joint effect on exports is equal to the joint effect of imports; that hypothesis cannot be rejected at standard significance levels. Still, the cumulative effect on exports is somewhat larger than the effect on imports, as is clear from the last line.

To summarize, the effects of default on exports seem somewhat higher than those on imports. Still, the most striking result is really that default has a substantive effect on trade.

4.6. Other effects

I have searched for other signs that debt renegotiation dampens international trade by examining other aspects of Paris Club deals. However, there seems to be only weak evidence that the dollar size of the Paris Club deal, the length of time since the last renegotiation, or the number of renegotiations has an impact on trade, once other factors have been taken into account. I have also deflated trade by three different price deflators instead of the CPI, and added the dates of debt *reduction* packages to the "classic" Paris Club debt *renegotiation* dates, all without substantively altering my key results.

5. Conclusion

On June 15, 1979, Togo renegotiated $280 million in sovereign debt through the Paris Club, a package that was subsequently repaid in full. Yet Togo's trade with the UK (one of Togo's creditor's in the Paris Club) fell from $30.7 million in 1979 to $5.5 million in 1994 (after adjusting for inflation). Togo's trade with other key creditors such as France, Germany, the Netherlands, and Switzerland also fell at rates exceeding

10% during the same period. Similarly, other countries renegotiating their debts through the Paris Club (such as Peru in 1968 and Senegal in 1982) suffered large and persistent trade declines (with, e.g., Finland and Denmark respectively). The question asked in this paper is: Has trade *typically* fallen after sovereign debt renegotiation, after other factors (such as income) are taken into account?

The answer is that trade typically *has* fallen after sovereign debt renegotiations. Paris Club deals are, on average associated with a decline in bilateral trade between debtors and creditors, holding other factors (such as income) constant. The reduction in trade is economically and statistically significant. While the results are somewhat sensitive to the exact specification, trade falls by about 8% a year for around 15 years, after taking other factors into account. That is, international default has significant negative consequences for trade. This result is robust to a number of econometric perturbations concerning lag length, treatment of simultaneity, censoring, sample size, and the exact measures of default and trade. There is weak evidence of trade diversion, and the exports of defaulters are hit somewhat harder than imports. Without denying the potential relevance of other reasons, it seems clear that one reason why sovereigns are reluctant to default on their external debts is that they fear the negative effects that debt renegotiation has for trade.

It would be interesting to extend this analysis to cover "London Club" negotiations between debtors and private sector banks. The primary obstacle to this lies in determining the default dates. London Club activity proceeds with a much longer lag than does the Paris Club, since the bank advisory committees require near or total unanimity from a more heterogeneous group than the Paris Club; Eichengreen and Portes (1995) provide more discussion.

I have not identified whether the effect of default on international trade appears because of a natural shrinking of trade finance, because creditors seek to punish and deter default, or some other reason. Providing direct evidence on the mechanism that links default and trade is a natural project for future research.

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