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RESEARCH ARTICLE

Do Sovereign Defaults Hurt Exporters?

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Abstract This paper uses a difference-in-difference methodology similar to the one originally proposed by Rajan and Zingales to test whether sovereign defaults hurt the more export-oriented industries disproportionately, and it finds strong support for this hypothesis. However, contrary to the findings of previous studies, our estimates suggest that the effect of defaults is short-lived.

Keywords Sovereign debt · Defaults · Exports

JEL Classification F34 · F10

1 Introduction

A clear understanding of the principles on which the sovereign debt market operates remains elusive. The legal framework is not as complete and transparent as in the case of private debts, and the enforceability of creditor rights remains untested or unreliable. Absent dependable legal rights, investment in sovereign debt instruments

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is based on the expectation that governments will do a faithful effort to service their debts even in trying circumstances. In an often quoted judgment of 1875, a British judge stated that “these so-called bonds amount to nothing more than engagements of honour.” Economists tend to see the decision to service government bonds in a somewhat different perspective: as the result of the comparison of the cost of servicing interest and principal of the debt with the adverse consequences that would follow from defaulting on those payments, that is, the costs of default. This is why understanding the costs of default is a critical part of understanding the working of the sovereign debt markets.

The literature on the costs of sovereign default has traditionally focused on two channels: reputation and international trade. There is considerable debate, conceptually and empirically, on the validity and quantitative importance of each of them.[1] In this paper, we focus on the trade channel, and apply an empirical technique novel to the sovereign debt literature but that has been used elsewhere, to test for the significance and magnitude of the alleged harmful effects of sovereign defaults on international trade. The empirical technique is the difference-in-difference approach of Rajan and Zingales (1998), which we apply more broadly to include also cross-country effects. We find evidence to support a statistically significant and economically sizeable effect of defaults on trade.

There are two main mechanisms through which trade may be affected: direct import sanctions or restrictions, and damage to the creditworthiness of exporters. The evidence for direct trade sanctions is not abundant. There are not many recorded cases when countries have applied quotas or tariffs or trade embargoes in retaliation for non-compliance in debt service. Yet, it is possible that in those cases where trade sanctions were likely to be applied, debtor countries made an extra effort to avoid default. From a theoretical perspective, trade sanctions are probably not “renegotiation proof” in the sense that creditor countries would be reluctant to apply them ex post because they would also suffer from the trade loss.[2] Moreover, an action of this type could be controversial because it would benefit one group (the investors) to the detriment of others (firms engaged in international trade and consumers in general) in addition, perhaps, to being inconsistent with the general strategic interest of the creditor country that may consider them.

We find a more solid basis to the case for trade finance as a mechanism that hurts a defaulting country in almost every case. When a country is in a situation of external financing distress it often resorts to exchange controls or capital outflow restrictions of various sorts. This affects the repayment capacity of all private debtors, even if they are not facing any solvency problems. Although strict capital controls are usually temporary, export-linked credits are of short maturity, and repayment obligations are likely to fall in the period of capital controls. In fact, international credit rating agencies like Moody’s and Standard & Poor’s recognize

[1] We will not review this literature here. For recent reviews, see Sturzenegger and Zettelmeyer (2007), Tomz (2007), and Obstfeld and Rogoff (1996).

[2] A commonly cited case is the nondefault by Argentina in the 1930s, when the country had most of its debts and a large fraction of its exports with England. Tomz (2007), however, questions the validity of this interpretation. See Panizza et al. (2008) for a discussion.

that the credit rating of any private debtor is affected by the probability of a sovereign default because a sovereign default raises the possibility of imposition of exchange controls that would impair the debt service ability of private debtors. This effect has been shown to be empirically strong (see Borensztein et al. 2007)

Systematic empirical research on the trade consequences of debt defaults has only started recently. Rose (2005) examines how debt restructurings granted by the Paris Club of official lenders have affected bilateral trade flows. He finds that defaults have strong and persistent effects on bilateral trade, with the implication that the nonpayment of the financial obligations with a given official creditor affects trade mostly with that country.

In this paper, we look for a more general effect and focus on the performance of export-intensive industries following episodes of sovereign default. This would be consistent with a number of channels including, for example, with a credit quality deterioration of exporters even when no trade action is taken by any country. We find that the effect is significant. Our estimates suggest that a more export-oriented industry (in the 75th percentile of the distribution) would see its growth drop by two percentage points relative to a less export-oriented industry (in the 25th percentile of the distribution) in each year in which the sovereign is in default. In contrast to Rose (2005), however, we find that the effect is short-lived, as we find little evidence of any residual effect after the sovereign emerges from default.

The plan of the paper is as follows. Section 2 explains the estimation methodology in detail. Section 3 provides details of the data that we utilized. Sections 4 and 5 present the econometric results for aggregate debt, and a disaggregation of bank and bond debts, respectively. Section 6 offers some conclusions.

2 Empirical methodology

To test whether sovereign default is particularly costly for exporters, we use a difference-in-difference approach similar to the one originally used by Rajan and Zingales (1998) and test if export-oriented industries experience more severe output loss at time of default. One key difference with the work of Rajan and Zingales (1998) is that, instead of focusing on a cross-section of industries and countries, we follow the same strategy adopted by Dell'Ariccia et al. (2005) and use panel data at the industry-country-year level.[3] Specifically, we use industry-level data for the manufacturing sector and ordinary least squares with robust standard errors to estimate the following regression:

$$\begin{aligned} VAGR_{i,j,t} = a_{i,j} + b_{i,t} + c_{j,t} + \alpha SHVA_{i,j,t-1} + \\ + (\beta DEF_{i,t} + \delta dRER_{i,t} + \gamma GDPGR_{i,t}) * EXPOU_{i,j} + \varepsilon_{i,j,t} \end{aligned}$$

where $VAGR_{i,j,t}$ measures real value added growth for industry j in country i at time t; $a_{i,j}$ denotes a set of country-industry fixed effects, $b_{i,t}$ a set of country-year fixed

[3] Dell'Ariccia et al. (2005) use data on external financial dependence of industries to study how banking crises affect the economic performance of firms.

effects, and $c_{j,t}$ a set of industry-year fixed effects. This set of fixed effects controls for all the country specific, industry specific, and time-invariant country-industry specific shocks. This specification is in line with Rajan and Zingales (1998) and captures most of the factors—other than the variable of interest—that are likely to affect the performance of a given industry and greatly attenuate problems of omitted variable bias. $SHVA_{i,j,t-1}$ is the share of value added in industry j country i measured at time $t-1$. This variable is mainly introduced to control for convergence and mean reversion effects. Thus, we expect α to be negative. *EXPOU* is an index of export orientation measured at the country-industry level (i.e. it does not vary over time but, unlike the Rajan and Zingales 1998, index of financial dependence, it does vary across countries) and *DEF* is a dummy variable that takes value 1 during default episodes and 0 otherwise. The interaction between default and export orientation (DEF*EXPOU) is our main variable of interest and tests whether sovereign defaults are particularly costly for export-oriented firms. In particular, a negative value of β implies that export-oriented firms are harmed by sovereign defaults more than other firms, and would support the idea that sovereign defaults generate costs that operate through international trade channels, either because of direct trade retaliation or through weaker credit access.

As debt defaults cause economic recessions but an economic slowdown can itself contribute to increase the likelihood of a default, endogeneity is often a problem for estimating the trade or growth effects of default. However, we think that this is not an issue under the Rajan–Zingales inspired approach that we follow. In our model, the identification of the main variable of interest comes from differences across sectors *within* a country. All the country-level effects are effectively controlled for by the country-year and country-industry fixed effects. Since our approach focuses on differences in growth across sectors within a country our estimation would suffer from reverse causality only if the *relative* performance of a given industry compared to other industries within the manufacturing sector of the same country, (for a given level of GDP growth) had a causal effect on the probability of default. This seems much less likely to be the case than if we were comparing aggregate growth across countries and/or time. Although a possible shortcoming of our estimations strategy is that serial correlation can bias the statistical significance of the estimators, we follow Bertrand et al. (2004) in performing simulations based on "placebo" defaults, and find no evidence that our results are affected by serial correlation bias.

The regression includes two additional controls, the interaction between real exchange rate depreciation and export orientation (dRER*EXPOU) and the interaction between GDP growth and export orientation (GDPGR*EXPOU). These additional variables are necessary to control for the sharp changes in the domestic economy that normally take place at the time of defaults. In the first case, defaults usually result in large real exchange rate depreciations, which in fact benefit export-oriented industries strongly. That benefit would be reflected in a positive δ. In the second place, debt crises are also usually underscored by sharp economic recessions and, as export-oriented firms rely essentially on external demand, we also expect export-oriented industries to be less affected by cyclical developments than industries that sell products in domestic markets. Hence, we expect the coefficient β to be negative. Controlling for these two interactions is particularly important because the real exchange rate and GDP growth are strongly correlated with default episodes.[4]

3 Data

Our main sources of industry-level data are the industrial statistics from UNIDO (2003 CD ROM) and trade data from Nicita and Olarreaga (2007). Our main sources of country-level variables are the IMF's International Financial Statistics for the consumer price index and the exchange rate, the World Bank's World Development Indicators for real GDP growth, and Standard and Poor's for the history of default episodes.

Following the work of Dell'Ariccia et al. (2005) we use three-digit ISIC level data from UNIDO and the CPI deflator from the IMF to compute industry-level real value added growth for 28 manufacturing sectors for 24 countries over the 1980–2000 period. We impose three restrictions on our sample. First, we exclude all country-years for which we have less than ten industries. The rationale for this exclusion is to guarantee sufficient within-country-year variation in the interaction between export orientation and default (the results are robust to eliminating this restriction or to using different thresholds). Second, after calculating industry-level value added growth, we exclude outliers by dropping the top and bottom 2% of the distribution. Dropping the top and bottom 2% is standard with industry-level data which tend to be rather noisy (we later show that our results are robust to dropping the top and bottom 1%). Finally, we exclude all countries that did not have a default episode over the 1980–2000 period. This third exclusion is innocuous from the point of view of the estimation of our main parameter of interest (because in countries that never defaulted over the period, the variable DEF*EXPOU is always equal to zero) and allows us to greatly reduce the number of parameters to be estimated (our reduced sample still requires the estimation of more than 1,600 parameters).[5] These restrictions yield an unbalanced panel of 24 countries with a total of over 9,700 observations, with value added annual growth data ranging from −55% to 140% and averaging 4.6% (the median is 1.8%, see Table 1).

We compute export orientation as the average ratio of exports over total sales (output in the UNIDO terminology) by industry and country. That is, for industry j in country i the index of export orientation is defined as:

$$EXPOU_{i,j} = \frac{1}{N} \sum_{t=1999-N+1}^{1999} \left(\frac{EXPORTS_{i,j,t}}{SALES_{i,jt}} \right)$$

While data on exports are available for the 1976–1999 period, we found large breaks in the exports over sales series for the period before 1982 and hence we focus on the

[4] For evidence of the correlation between default and the real exchange rate see Calvo et al. (2003) and for evidence of the correlation between default and growth see Sturzenegger (2004), Borensztein and Panizza (2008), Borensztein et al. (2006) and Levy Yeyati and Panizza (2005).

[5] We need to estimate 588 country-industry fixed effects, 401 country-year fixed effects, and 639 industry-year fixed effects. In the robustness analysis, we show that the results do not change if we also include countries with no default episodes.

Table 1 Summary statistics

	Mean	Median	St. dev	25 pctile	75 pctile	Max	Min	N. obs
Variables measured at the country-sector-year level								
VAGR	0.046	0.018	0.29	−0.10	0.21	1.40	−0.55	9,735
SHAREVA	0.039	0.023	0.05	0.009	0.05	0.52	5e−06	9,735
Variables measured at the country-sector level								
EXPOU	0.07	0.03	0.11	0.01	0.09	0.96	0	639
Variables measured at the country-year level								
RER	106.7	100	80.7	79.3	113.5	1,435	8.40	386
DRER	0.11	0.01	1.14	−0.06	0.08	21.31	−0.86	386
GDPGR	0.03	0.04	0.05	0.005	0.06	0.19	−0.13	386
DEFB2	0.13	0	0.34	0	0	1	0	386
DEF_ALL	0.48	0	0.50	0	1	1	0	386

1982–1999 sample. Therefore, the maximum value of N is 18. The average value of EXPOU is 7% (median 3%) with a range between 0 and 96% (Table 1). Table 10 in the Appendix reports average values of EXPOU for our sample of 24 countries and Table 11 reports average value for the 28 three-digit ISIC industrial sectors.

Our main default variable, DEFB2, takes a value of one in the first 2 years of the default episode (i.e., in the year in which the country defaults and in the year immediately after that). We use a 2-year window because it is hard to determine precisely in which year the default may have its strongest effect. If the default episode happens at the end of the year, the effects are likely to be felt in the following year except, of course, to the extent that it was widely anticipated by the markets. In the estimation, we experiment with different lag structures. We use the default dates recorded by Standard and Poor's ("Sovereign Ratings History since 1975," Standard & Poor's, October 5, 2005), and include both defaults on international bank loans and defaults on sovereign bonds (in the robustness analysis we check whether there is a difference between these two types of default). The 24 countries included in our sample experienced 34 default episodes over the 1980–2000 period (one country, Iran, defaulted in 1978 and entered the sample period in default). Among these 34 episodes, 30 were defaults on international bank loans, three were defaults on sovereign bonds, and one was a joint bank-bond default (Table 12).[6]

4 Regression results

Our basic results are reported in Table 2. Column (1) reports our baseline specification. As expected, we find evidence of convergence as indicated by a negative and statistically significant coefficient for the lagged share of value added. We also find that export oriented industries tend to benefit from real depreciations (as indicated by the positive coefficient for dRER*EXPOU) and tend to be less procyclical than industries oriented to the domestic markets (as indicated by the negative, albeit not statistically significant, coefficient for GDPGR*EXPOU).

[6] Unfortunately, limitations on the availability of data on value added growth and export orientation did not allow us to include most of the bond defaults of the 1990s (such as Pakistan, Russia and Ukraine).

Table 2 Benchmark regression

	(1) Real VA growth	(2) Real VA growth	(3) Real VA growth	(4) Real VA growth	(5) Real VA growth 1% outliers	(6)
Lagged share in value added	−2.892	−2.896	−2.896	−2.898	−3.200	−3.204
	(12.22)***	(12.22)***	(12.22)***	(12.20)***	(11.69)***	(11.70)***
dRER*EXPOU	0.201	0.199	0.199	0.201	0.137	0.134
	(2.53)**	(2.46)**	(2.46)**	(2.53)**	(1.81)*	(1.75)*
GDPGR*EXPOU	−1.137	−0.992	−0.992	−0.878	−1.607	−1.464
	(1.35)	(1.14)	(1.14)	(1.04)	(1.71)*	(1.53)
DEFB2*EXPOU	−0.218				−0.202	
	(2.54)**				(2.10)**	
DEF_ALL1*EXPOU		−0.141	−0.137			−0.088
		(1.07)	(0.96)			(0.59)
DEF_ALL 2*EXPOU		−0.292	−0.289			−0.291
		(2.82)***	(2.75)***			(2.57)**
DEF_ALL 3*EXPOU		−0.014	−0.012			0.011
		(0.14)	(0.11)			(0.08)
DEF_ALL 4*EXPOU		−0.169	−0.167			−0.130
		(1.70)*	(1.63)			(1.12)
DEF_ALL 5*EXPOU		−0.181	−0.179			−0.189
		(2.00)**	(1.88)*			(1.69)*
DEF_ALL_ALL*EXPOU			−0.005	−0.116		
			(0.05)	(1.44)		
Constant	−0.123	−0.131	0.102	0.104	0.390	0.210
	(0.97)	(1.16)	(1.04)	(1.05)	(2.37)**	(1.12)
Observations	9,360	9,360	9,360	9,360	9,525	9,525
R-squared	0.43	0.43	0.43	0.43	0.37	0.38
F test def*EXPOU jointly significant		2.72	2.36			1.93
Prob>*F*		0.019	0.028			0.086

Robust *t* statistics in parentheses
*Significant at 10%; **significant at 5%; ***significant at 1%

More interestingly for our purposes, we find that that export-oriented industries are particularly affected by default episodes as indicated by the negative and statistically significant coefficient of DEFB2*EXPOU. Besides its statistical significance, the impact of default is also quantitatively important, as it implies that moving from the 25th to the 75th percentile of the export orientation distribution increases the impact of default on value added growth by 1.7 percentage points ($-0.017=0.218*0.01-0.218*0.09$).

In column (2) we experiment with a different lag structure. In particular, we create a default indicator variable that takes a value of 1 in the first period of the default episode (DEF_ALL1) and four lags of this variable (DEF_ALL2 to DEF_ALL5). We find that the interaction of export orientation with all these variables has a negative coefficient (reaching a maximum in the year after the default), and three of them are individually statistically significant. More importantly, we find that the five variables are jointly significant as indicated by the *F* test reported in the bottom row of Table 2.

Next, we explore a default indicator that takes a value of 1 for every year in which a sovereign was rated as "selective default" by Standard & Poor's. This means that if a sovereign went into default and took 20 years to restructure its debts and emerge from insolvency, the default indicator variable will take a value of 1 for all of these 20 years. We term this variable DEF_ALL_ALL, and report the coefficient on its interaction with export orientation in columns (3) and (4). One would expect this variable to be less significant than the variables that measure the immediate impact of default because export-oriented firms probably find ways to adjust to the situation, and learn how to operate under this environment, even if the trade sanctions or credit access problems remain in full force throughout. When we include this variable together with DEF_ALL1-DEF_ALL5 (column 3), we find that it has the right negative sign but a very small coefficient (−0.005) and t statistics; however, all default variables are still jointly significant in this case. In column (4), where we drop DEF_ALL1-DEF_ALL5, we find that the coefficient of DEF_ALL_ALL increases (in absolute value) to 0.116 (about half the value of the coefficient for DEFB2) but remains statistically insignificant (although with a p value of 0.15). These results suggest that defaults have a large negative effects on export-oriented firms but that this effect tends to die out for long lasting default episodes. The last two columns of Table 2 repeat the experiment of the first two columns by dropping the top and bottom 1% of outliers, instead of the top and bottom 2% and shows that the results are broadly unchanged.

Having established our basic finding that default episodes lead to lower growth in export oriented industrial sectors, we now check whether our result is robust to changes in the sample or in the econometric specification.

We first check whether dropping from the sample countries that did not experience a default is a rather innocuous simplification, as we stated above. In Table 3, we repeat our baseline estimations but including all countries for which we have data (the sample increases from 9,360 to 25,397 observations) and find that our results are broadly unchanged. The first two columns of the tables drop top and bottom 2% outliers and the last two columns of the table drop top and bottom 1% outliers.

One possible problem with our data has to do with the fact that in some country-industries we have very short series of data and, while we dropped all countries with less than ten industrial sectors in selecting our sample, we did not impose any restriction in the number of yearly observations available for each given sector in a specific country. As a consequence, our sample includes 792 observations for country-sectors in which we have less than 10 years of data and 1,816 observations for country-sectors in which we have less than 15 years of data. As a first robustness check, we re-estimated our model by dropping all country-sectors for which we have less than 15 years of data (we obtain identical results if we drop all country-sectors for which we have less than 10 years of data). While this restriction leads to a much smaller sample (the sample size drops to 7,544), our basic result remains unchanged (Table 4). In particular, the coefficient of DEFB2*EXPOU remains negative (with a coefficient which is basically identical to that of column (1) of Table 2) and statistically significant, and the coefficients of DEF_ALL1*EXPOU-DEF_ALL5*EXPOU remain negative (with the exception of DEF_ALL3*EXPOU, which is positive but close to zero) and jointly significant.

Our next robustness test focuses on the definition of the DEF_ALL2-DEF_ALL5 variables. As these variables take the lagged value of DEF_ALL1 (which takes value

Table 3 Robustness analysis, including countries with no defaults

	(1) Real VA growth	(2) Real VA growth	(3) Real VA growth	(4) Real VA growth
	Dropping 2% outliers		Dropping 1% outliers	
Lagged share in value added	−2.333	−2.334	−2.740	−2.741
	(17.35)***	(17.37)***	(17.68)***	(17.69)***
dRER*EXPOU	0.210	0.207	0.128	0.124
	(2.90)***	(2.79)***	(1.75)*	(1.66)*
GDPGR*EXPOU	−0.868	−0.840	−0.970	−0.932
	(1.87)*	(1.79)*	(1.67)*	(1.59)
DEFB2*EXPOU	−0.142		−0.185	
	(1.98)**		(2.04)**	
DEF_ALL1*EXPOU		−0.041		−0.081
		(0.33)		(0.57)
DEF_ALL 2*EXPOU		−0.235		−0.275
		(2.29)**		(2.60)***
DEF_ALL 3*EXPOU		−0.047		−0.020
		(0.48)		(0.15)
DEF_ALL 4*EXPOU		−0.139		−0.102
		(1.40)		(0.94)
DEF_ALL 5*EXPOU		−0.157		−0.165
		(1.73)*		(1.57)
Constant	−0.079	−0.070	0.293	−0.149
	(0.46)	(0.46)	(2.30)**	(1.19)
Observations	25,397	25,397	25,869	25,869
R-squared	0.38	0.38	0.36	0.36
F test def*EXPOU jointly significant		2.01		2.15
Prob>*F*		0.048		0.0.32

Robust *t* statistics in parentheses
*Significant at 10%; **significant at 5%; ***significant at 1%

1 in the first year of default episode) and it would be possible for these variable to take value one for countries that are no longer in default (this would happen if the resolution of the default episode takes less than 4 years). This approach may be justified because the default may still harm exporters even after the country has exited default, for example because reputation issues would remain in financial and international trade markets. It seems nevertheless reasonable to check whether our results are robust to setting the DEF_ALL2-DEF_ALL5 variables equal to zero for all the years after the sovereign has exited from default. Table 5 reproduces columns (2) and (3) of Table 2 with this modified definition of DEF_ALL2-DEF_ALL5, and shows that this alternative definition does not affect the basic results of Table 2.

After having explored whether our results are robust to different samples and different definitions of the default variables, we now check whether small changes in the set of controls affect our main result (for the sake of conciseness, we will focus this robustness analysis on the basic specification in column 1).[7] We start, by

[7] The results of columns 2–4 of Table 2 are also robust to these alternative specifications.

Table 4 Robustness analysis, using country-sectors with at least 15 years of data

	(1) Real VA growth	(2) Real VA growth	(3) Real VA growth	(4) Real VA growth
Lagged share in value added	−3.636	−3.637	−3.638	−3.647
	(10.32)***	(10.31)***	(10.31)***	(10.32)***
vardRER	0.015	0.005	0.001	0.016
	(0.14)	(0.05)	(0.01)	(0.14)
GDPGR*EXPOU	−1.395	−1.282	−1.293	−1.181
	(1.55)	(1.36)	(1.37)	(1.30)
DEFB2*EXPOU	−0.208			
	(2.33)**			
DEF_ALL1*EXPOU		−0.109	−0.062	
		(0.81)	(0.43)	
DEF_ALL 2*EXPOU		−0.287	−0.256	
		(2.57)**	(2.27)**	
DEF_ALL 3*EXPOU		0.005	0.034	
		(0.05)	(0.32)	
DEF_ALL 4*EXPOU		−0.133	−0.110	
		(1.35)	(1.07)	
DEF_ALL 5*EXPOU		−0.185	−0.161	
		(2.27)**	(1.79)*	
DEF_ALL_ALL*EXPOU			−0.059	−0.136
			(0.59)	(1.67)*
Constant	−0.175	−0.089	0.029	−0.081
	(1.19)	(0.93)	(0.20)	(0.80)
Observations	7,544	7,544	7,544	7,544
R-squared	0.46	0.46	0.46	0.46
F test def*EXPOU jointly sign.		2.60	2.17	
Prob>F		0.024	0.043	

Robust t statistics in parentheses
*Significant at 10%; **significant at 5%; ***significant at 1%

dropping the interaction between export orientation and GDP growth (column (1) of Table 6). Given that growth tends to be low during default periods, and given our previous finding that export oriented sectors are less procyclical than sectors that target the domestic market, we expect that estimating the model without GDPGR* EXPOU should bias downward the estimate of DEFB2*EXPOU (because this variable would capture the effect of GDPGR*EXPOU and the effect of this variable goes in opposite direction relative the effect of default). In fact, the coefficient of DEFB2*EXPOU drops by approximately 20% (from −0.22 to −0.17) but we also find that that this variable remains statistically significant with a t statistic of 2.22.

In column (2), we drop the interaction between export orientation and changes in the real exchange rate. Also in this case, omitting the effect of the real exchange rate should lead to lower point estimates of the coefficient of DEFB2*EXPOU. In fact, we find that neither the point estimate nor the significance of DEFB2*EXPOU changes with respect to the baseline regression of Table 2. However, we find that in this specification GDPGR*EXPOU becomes statistically significant (with the expected negative sign). In column (3), we drop both GDPGR*EXPOU and DRER*EXPOU. We find that the coefficient of DEFB2*EXPOU drops to −0.168

Table 5 Robustness analysis, setting DEF_ALL2-DEF_ALL5=0 if DEF_ALL_ALL=0

	(1) Real VA growth	(2) Real VA growth
Lagged share in value added	−2.900	−2.900
	(12.21)***	(12.21)***
vardRER	0.203	0.203
	(2.57)**	(2.56)**
GDPGR*EXPOU	−0.933	−0.939
	(1.07)	(1.07)
DEF_ALL1*EXPOU	−0.117	−0.104
	(0.88)	(0.73)
DEF_ALL 2*EXPOU	−0.293	−0.280
	(2.40)**	(2.12)**
DEF_ALL 3*EXPOU	0.057	0.070
	(0.50)	(0.55)
DEF_ALL 4*EXPOU	−0.151	−0.139
	(1.14)	(0.96)
DEF_ALL 5*EXPOU	−0.128	−0.119
	(1.28)	(1.10)
DEF_ALL_ALL*EXPOU		−0.020
		(0.18)
Constant	−0.135	0.097
	(1.19)	(0.98)
Observations	9,360	9,360
R-squared	0.43	0.43
F test def*EXPOU jointly significant	2.13	1.81
Prob>*F*	0.059	0.092

Robust *t* statistics in parentheses

*Significant at 10%; **significant at 5%; ***significant at 1%

but remains statistically significant at the 5% confidence level. In column (4), we substitute the change in the real exchange rate with the *level* of the real exchange rate. While the coefficient on RER*EXPOU has the expected positive sign but it is not statistically significant (the p value is 0.15), we find that DEFB2*EXPOU remains negative and highly significant (if anything both the point estimate and the t statistics are higher than in the baseline case). In column (5) we augment the baseline model with the lag of the change of the real exchange rate (lagdRER*EXPOU). Again, this does not affect our basic result.

Finally, in column (6), we augment our regression with the interaction between export orientation and an external shock defined as average (weighted by trade shares) GDP growth of country's i trading partners to account for the evolution of demand for exports. Formally, we define the external shock as follows:

$$EXTSH_{i,t} = \frac{EXP_i}{GDP_i} \sum_j \phi_{ij,t-1} GDPGR_{j,t}$$

where $GDPGR_{j,t}$ measures real GDP growth in country j in period t, $\phi_{ij,t}$ is the fraction of export from country i going to country j, and EXP_{ii} measures country i average exports expressed as a share of GDP (see Jaimovich and Panizza 2007, for a detailed discussion of the properties of $EXTSH$). The rationale for including this variable is that default episodes may happen at a time of a global economic

Table 6 Robustness analysis, different controls

	(1) Real VA growth	(2) Real VA growth	(3) Real VA growth	(4) Real VA growth	(5) Real VA growth	(6) Real VA growth
Lagged share in v.a.	−2.890	−2.897	−2.816	−2.906	−3.004	−2.891
	(12.18)***	(12.23)***	(12.37)***	(12.35)***	(11.28)***	(12.01)***
dRER*EXPOU	0.221				0.203	0.212
	(3.15)***				(2.62)***	(2.82)***
GDPGR*EXPOU		−1.545		−1.467	−0.925	0.222
		(1.90)*		(1.77)*	(1.05)	(0.27)
RER*EXPOU				0.001		
				(1.44)		
lagdRER*EXPOU					0.125	
					(1.32)	
EXT_SH*EXPOU						1.404
						(0.17)
DEFB2*EXPOU	−0.179	−0.228	−0.168	−0.239	−0.232	−0.177
	(2.20)**	(2.66)***	(2.08)**	(2.80)***	(2.52)**	(2.19)**
Constant	−0.120	−0.007	−0.116	−0.303	−0.780	−0.363
	(0.95)	(0.06)	(1.18)	(2.95)***	(5.01)***	(3.64)***
Observations	9,360	9,360	9,651	9,360	9,020	8,908
R-squared	0.43	0.43	0.42	0.43	0.43	0.44

Robust *t* statistics in parentheses
*Significant at 10%; **significant at 5%; ***significant at 1%

slowdown, and hence the poor performance of exporters in country *i* might owe to the fact that its main trading partners are also in a period of low growth, rather than the default episode itself.[8] We find that EXTSH*EXPOU has the expected (positive) sign but it is not statistically significant. While controlling for demand in partner countries reduces the coefficient of DEFB2*EXPOU (from 0.218 to 0.177), we still find that this variable remains a significant determinant of value added growth.

A potential problem with difference-in-difference estimates is that the *t*-statistics might be inflated by the presence of serial correlation. Bertrand et al. (2004) discuss four types of corrections that can address this problem, but none of these corrections is applicable to our setup.[9] Hence, we follow a suggestion of Bertrand et al. (2004) and simulate our model using a set of "placebo" defaults. In particular, we run regressions such as that reported in column 1 of Table 2 100 times, each time replacing our 34 actual default episodes with 34 randomly generated default episodes.[10] If our standard errors were correctly estimated, we should expect to

[8] We would like to thank Kevin Cowan for suggesting this interpretation.

[9] The four corrections are: bootstrapping; two asymptotic approximations of the variance-covariance matrix; and collapsing the time series inflation into "pre" and "post" period states. The first three corrections are computationally intensive and cannot be applied with our framework that requires estimating more than 1,600 parameters. The last correction cannot be applied to our framework because would work only if all defaults happened the same year (Bertrand et al. 2004).

[10] We would have liked to run 1,000 simulations but since each estimation takes about 1 h, running 1,000 simulation would have required more than 40 days of computer time.

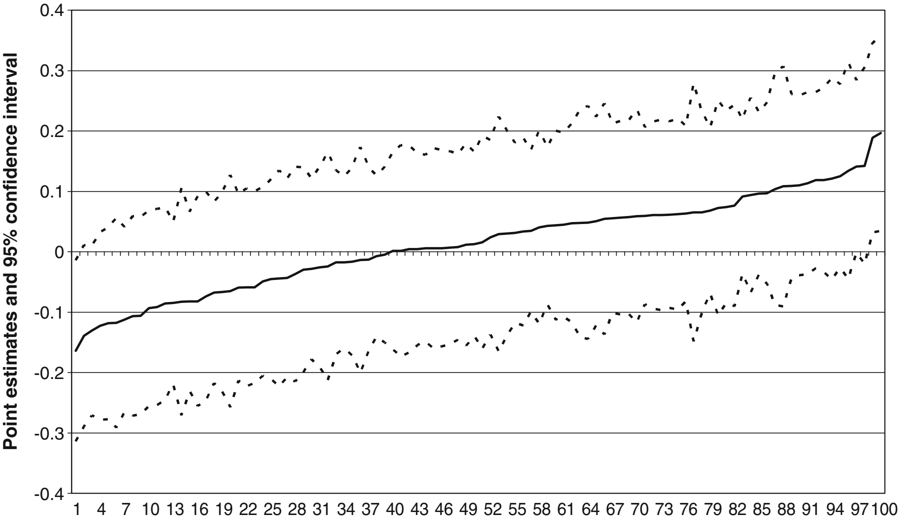
reject the null of no significant effect of default in approximately 5% of the estimations. If, instead, serial correlation is an issue we should expect a much higher rejection rate (Bertrand et al. 2004, reject the null in more than 40% of their cases). In our simulation, we find that the average point estimate is 0.13 and ranges between −0.16 and 0.20 (Fig. 1 plots the point estimates with a 95% confidence interval). More interestingly, the coefficient is statistically significant only in four cases (two times with a positive sign and two times with a negative sign). As we indeed reject the null in approximately 5% of our simulations, this suggests that our standard errors are correct and not biased by serial correlation.

So far we focused on defaults on international bank loans and sovereign bonds (as classified by Standard and Poor's) and did not consider Paris Club defaults which are instead the focus of Rose's (2005) analysis. As a final robustness check we look at what happens when we focus of Paris club default (the last column of Table 12 shows that our sample includes 26 Paris Club defaults).[11] In columns 1 and 2 of Table 7 we reproduce our baseline specification replacing S&P defaults with Paris Club Default (as we limit our sample to countries with at least one default episode, the regressions now include 15 countries and 5,223 observations). We find that the coefficient of DEFB2 is similar to that of our benchmark specification and close to being statistically significant at the ten percent confidence level (the p value is 0.105). The second column of the table shows a large and negative coefficient in the year after default and no significant coefficients in the following and previous years. We also find that the coefficients are not jointly significant. In columns 3 and 4, we repeat the experiment by including all types of default (both Paris Club and S&P), our sample now includes 27 countries (with 56 default episodes) and 10,482 observations. We find that the results are similar to those of our benchmark regressions in Table 2. In particular, we find that DEFB2 has a large and statistically significant coefficient and that the five lags of the default dummy are jointly significant with the largest effect in the year that follows the default episode.

5 Is there a difference between bond defaults and bank defaults?

In the specifications of the previous section we did not differentiate between defaults on international bank loans and defaults on sovereign bonds. Yet, these two types of defaults may have different effects. For example, if the main channel through which exporters are affected is a retrenchment of trade finance, sovereign defaults on bank loans may have a stronger impact. Moreover, bond defaults tend to affect a much larger number of creditors (which often include domestic banks) and the harmful effects may affect export and non-export-oriented firms equally. In Table 8, we estimate the effect of these two types of default separately. In column (1), our main explanatory variable is BANKDEFB2*EXPOU. This variable is defined as DEFB2 *EXPOU but only considers episodes of defaults on international bank loans (there

[11] In most cases the Paris Club defaults are the same as the S&P default with a slightly different timing.



The graph reports the point estimate (solid line) and the 95% confidence interval (dotted line) of 100 simulation of the model of Table 2, column 1, using "placebo" defaults.

Fig. 1 Simulation with "placebo" defaults. The graph reports the point estimate (*solid line*) and the 95% confidence interval (*dotted line*) of 100 simulation of the model of Table 2, column 1, using "placebo" defaults

are 31 of these episodes in our sample). The results are similar to those in our benchmark model suggesting that bank defaults have a negative, large (at −0.197, the point estimate is just below that of the baseline regression) and statistically significant impact on the performance of export oriented sectors.

In column (2), we estimate the effect of bond defaults (BONDDEFB2 is defined like BANKDEFB2 but only takes a value of 1 during defaults on sovereign bonds). We find that, although the effect of bond defaults is stronger than that of bank defaults (the point estimate is equal to −0.232), the coefficient is not even close to being statistically significant. The fact that our sample contains only a small number of bonded debt default episodes, however, does not give us a good basis to draw any strong conclusions on this issue.

6 Conclusions

This paper uses a difference-in-difference methodology similar to the one originally proposed by Rajan and Zingales (1998) and Dell'Ariccia et al. (2005) to test whether sovereign defaults hurts the more export-oriented industries disproportionately, and we find strong support for this hypothesis. However, contrary to the findings of previous studies, our estimates suggest that the effect of defaults is short-lived.

It should be clear that our paper does not say anything on whether default affects total exports or total growth. For instance, it would be possible (albeit unlikely) that

Table 7 Paris club defaults

	(1) Real VA growth	(2) Real VA growth	(3) Real VA growth	(4) Real VA growth
	Only Paris Club defaults		Paris Club and S&P defaults	
Lagged share in value added	−2.177	−2.178	−2.698	−2.699
	(8.62)***	(8.59)***	(12.65)***	(12.65)***
dRER*EXPOU	−0.091	−0.086	0.167	0.161
	(0.50)	(0.48)	(1.95)*	(1.83)*
GDPGR*EXPOU	−2.061	−2.054	−1.475	−1.459
	(1.74)*	(1.72)*	(1.67)*	(1.62)
DEFB2*EXPOU	−0.202		−0.230	
	(1.62)		(2.92)***	
DEF_ALL1*EXPOU		−0.006		−0.079
		(0.03)		(0.72)
DEF_ALL 2*EXPOU		−0.367		−0.338
		(2.28)**		(3.55)***
DEF_ALL 3*EXPOU		0.117		0.020
		(0.59)		(0.17)
DEF_ALL 4*EXPOU		0.123		−0.063
		(0.84)		(0.70)
DEF_ALL 5*EXPOU		−0.036		−0.053
		(0.24)		(0.65)
Constant	0.021	−0.233	−0.232	0.078
	(0.08)	(0.88)	(1.86)*	(0.56)
Observations	5,223	5,223	10,482	10,482
R-squared	0.36	0.36	0.40	0.40
F test def*EXPOU jointly significant		1.68		2.72
Prob>*F*		0.137		0.019

*Significant at 10%; **significant at 5%; ***significant at 1%

Table 8 Bond and bank defaults

	(1) Real VA growth	(2) Real VA growth
Lagged share in value added	−2.891	−2.902
	(12.20)***	(12.21)***
dRER*EXPOU	0.200	0.205
	(2.52)**	(2.67)***
realgdpgr*EXPOU	−1.111	−0.750
	(1.31)	(0.90)
BANKDEFB2*EXPOU	−0.196	
	(2.24)**	
BONDDEFB2*EXPOU		−0.232
		(0.73)
Constant	−0.123	−0.143
	(0.97)	(1.26)
Observations	9,360	9,360
R-squared	0.43	0.43

Robust *t* statistics in parentheses
*Significant at 10%; **significant at 5%; ***significant at 1%

at time of default non-export oriented industries pick up growth (or exports) and that this more than compensate the relative decline of export oriented industries. In fact, the Rajan and Zingales (1998) difference in difference methodology can only tell us how one sector moves relative to other but does not give us any information on the global behavior of a country's output.[12] Furthermore, our data only cover the industrial sector and hence cannot shed any light on what happens to the service or agriculture sectors. With this caveat in mind, if one believes that the export sector is the economy's most dynamic sector and a source of important positive externalities (Hausmann and Rodrik 2003), then any negative shock to this sector is likely to have important negative repercussion on overall welfare.

Appendix

Table 9 Description of the variables

VAGR	3 digit ISIC manufacturing value added growth in constant dollars. Calculated and deflated using CPI from the IMF International Financial Statistics
SHVA	Share of value added in sector j over total manufacturing value added. Calculated (using data from UNIDO Industrial statistics 2003) as: $SHVA_{i,j,t} = \frac{VA_{i,j,t}}{\sum_j VA_{i,j,t}}$
EXPOU	Average share of exports over sales for country i, sector j. Data for exports are from Nicita and Olarreaga (2007) and data for sales (output) are from UNIDO Industrial statistics 2003. The index is calculated as follow: $EXPOU_{i,j} = \frac{1}{18}\sum_{t=1982}^{1999}\left(\frac{EXPORTS_{i,j,t}}{SALES_{i,jt}}\right)$, We use data over the 1982–1999 period because the Nicita and Olarreaga (2007) data-set ends in 1999 and data prior to 1982 are poor quality. As the data tend to be noisy, we drop the top and bottom 2% of observations for $\frac{EXPORTS_{i,j,t}}{SALES_{i,jt}}$
RER	Real bilateral (with respect to the US) exchange rate index. 1999=100. A higher value indicates a depreciated exchange rate
DRER	Percentage change in RER
GDPGR	GDP growth calculated using real local currency data from the World Bank's World Development Indicators
DEFB2	Dummy variable that takes value one in the first and second year of a default episode. Source Standard and Poor's
DEF_ALL	Dummy variable that takes value one whenever a country is in default. Source Standard and Poor's
EXTSH	External demand shock that measures GDP growth of trading partners weighted by export share. The external shocks is calculated using the following formula: $EXTSH_{i,t} = \frac{EXP_i}{GDP_i}\sum_j \phi_{ij,t-1} GDPGR_{j,t}$

[12] For instance, Cetorelli and Gambera (2001) use a methodology similar to the one used in this paper to estimate whether bank concentration affects value added growth in industries that require more external financing (this was Rajan and Zingales 1998, method to differentiate industries) and find conflicting results. One the one hand, they find that concentration promotes growth of industries that require more external finance. On the other hand, they find that bank concentration leads to lower overall GDP growth.

Table 10 Summary export orientation by country

Country	Mean	Std. dev.	Freq.
ARG	0.024	0.025	28
BGR	0.095	0.086	27
BOL	0.064	0.091	27
CHL	0.043	0.046	28
CMR	0.049	0.061	26
CRI	0.097	0.077	28
ECU	0.029	0.032	28
ETH	0.025	0.067	22
HND	0.177	0.198	20
IDN	0.089	0.084	27
IRN	0.011	0.036	28
JOR	0.139	0.175	27
KEN	0.055	0.066	26
MAR	0.059	0.080	26
MEX	0.152	0.139	28
MWI	0.090	0.214	20
PAN	0.033	0.042	27
PER	0.024	0.040	28
PHL	0.100	0.100	28
POL	0.063	0.041	28
TUR	0.042	0.039	28
URY	0.055	0.045	28
VEN	0.031	0.052	28
ZAF	0.233	0.174	28
All countries	0.073	0.108	639

Table 11 Summary export orientation by industrial sector

ISIC3 Product classification	Mean	Std. dev.	Freq.
313 Beverages	0.015	0.022	24
342 Printing and publishing	0.019	0.027	24
369 Other non-metallic mineral products	0.025	0.045	24
354 Misc. petroleum and coal products	0.033	0.035	15
356 Plastic products	0.034	0.039	24
341 Paper and products	0.035	0.036	24
371 Iron and steel	0.039	0.05	22
381 Fabricated metal products	0.039	0.04	24
314 Tobacco	0.053	0.194	24
352 Other chemicals	0.056	0.124	24
362 Glass and products	0.058	0.064	22
353 Petroleum refineries	0.061	0.08	20
355 Rubber products	0.066	0.101	24
361 Pottery, china, earthenware	0.07	0.135	22
321 Textiles	0.072	0.049	24
332 Furniture, except metal	0.081	0.134	24
383 Machinery, electric	0.081	0.106	23
324 Footwear, except rubber or plastic	0.083	0.13	24
311 Food products	0.087	0.088	24
331 Wood products, except furniture	0.088	0.086	24
351 Industrial chemicals	0.09	0.076	23
382 Machinery, except electrical	0.099	0.117	23
384 Transport equipment	0.108	0.193	23

Table 11 (continued)

ISIC3 Product classification	Mean	Std. dev.	Freq.
322 Wearing apparel, except footwear	0.109	0.091	24
390 Other manufactured products	0.136	0.127	22
385 Professional and scientific equipment	0.137	0.165	20
323 Leather products	0.14	0.133	24
372 Non-ferrous metals	0.145	0.101	20
All sectors	0.073	0.108	639

Table 12 Default episodes included in the sample

Standard and Poor's					Paris Club
Country	Year	Bank defaults	Bonds default	All	
ARG	1982	1	0	1	0
ARG	1985	0	0	0	1
BGR	1990	1	0	1	0
BGR	1991	0	0	0	1
BOL	1986	1	0	1	1
BOL	1989	0	1	1	0
CHL	1983	1	0	1	0
CHL	1985	0	0	0	1
CMR	1985	1	0	1	0
CMR	1989	0	0	0	1
CRI	1981	1	0	1	0
CRI	1983	1	1	1	1
CRI	1989	0	0	0	1
ECU	1982	1	0	1	0
ECU	1983	0	0	0	1
ECU	1988	0	0	0	1
ECU	1999	0	1	1	0
EGY	1987	0	0	0	1
EGY	1991	0	0	0	1
ETH	1991	1	0	1	0
HND	1981	1	0	1	0
IDN	1998	1	0	1	0
IRN	1978	1	0	1	0
JOR	1989	1	0	1	1
JOR	1992	0	0	0	1
KEN	1994	1	0	1	0
MAR	1983	1	0	1	0
MAR	1986	1	0	1	0
MEX	1982	1	0	1	0
MEX	1983	0	0	0	1
MEX	1986	0	0	0	1
MEX	1989	0	0	0	1
MWI	1982	1	0	1	1
PAK	1981	0	0	0	1
PAN	1983	1	0	1	0
PAN	1985	0	0	0	1
PAN	1987	0	1	1	0
PAN	1990	0	0	0	1
PER	1984	1	0	1	0

Table 12 (continued)

Standard and Poor's Country	Year	Bank defaults	Bonds default	All	Paris Club
PHL	1983	1	0	1	0
PHL	1984	0	0	0	1
PHL	1987	0	0	0	1
POL	1981	1	0	1	1
POL	1985	0	0	0	1
POL	1990	0	0	0	1
TTO	1984	0	0	0	1
TUR	1982	1	0	1	0
URY	1983	1	0	1	0
URY	1987	1	0	1	0
URY	1990	1	0	1	0
VEN	1983	1	0	1	0
VEN	1990	1	0	1	0
VEN	1995	0	1	1	0
ZAF	1985	1	0	1	0
ZAF	1989	1	0	1	0
ZAF	1993	1	0	1	0
Total		31	5	35	26

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