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The elusive costs of sovereign defaults[☆]

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ABSTRACT

The evidence supporting the presence of output losses associated with sovereign defaults is based on annual observations and suffers from measurement and identification problems. This paper examines the impact of default on growth using quarterly data and finds that output contractions precede defaults and that output starts growing after the quarter in which the default took place. This indicates that default episodes mark the beginning of the economic recovery and that the negative effects of a default on output are likely to be driven by the anticipation of default, independently of whether or not the country ultimately decides to validate it.

> Hell, the last thing I should be doing is tell a country we should give up our claims. But there comes a time when you have to face reality.
>
> The problem historically has not been that countries have been too eager to renege on their financial obligations, but often too reluctant.[1]

1. Introduction

As conventional wisdom has it, a sovereign's unilateral decision to stop servicing its debt carries important and persistent economic costs. This is what is assumed (either explicitly or implicitly) by the sovereign debt literature as a government's main incentive to honor its obligations. There is in fact evidence that default episodes are negatively correlated with GDP growth measured at annual frequency. In this paper, we argue that using higher frequency data yields a starkly different message. In particular, we show that defaults have no significant negative impact on successive output growth and, if anything, mark the final stage of the crisis and the beginning of economic recovery.

The empirical literature has looked at the costs of default mainly in three ways: (i) by testing the effect of default on access to the international credit market; (ii) by testing the effects of default on international trade; and (iii) by testing the effects of default on GDP growth. In all cases, the costs specifically associated with default (that is, in excess of those related to the ongoing crisis, or to the memory of a recent crisis) are difficult to identify.

While Ozler (1993) found that defaults in the 1930s were associated with an increase in spreads of approximately 20 basis points 40 years later, more recent work found that the effect of default on spreads and market access is short-lived (two recent surveys are Borensztein and Panizza, 2009, and Panizza et al., 2009).

Focusing on the trade cost of defaults, Rose (2005) found that countries that defaulted on official (Paris Club) debt trade less with creditor countries and Borensztein and Panizza (2010) find that export-oriented industries tend to suffer more in the aftermath of a default. However, while Rose (2005) finds a long-lasting effect, Borensztein and Panizza (2010) find that the effect is transitory.

[☆] The first draft of this paper was written while both authors were with the Research Department of the Inter-American Development Bank. The views expressed in this paper are the authors' and do not necessarily reflect those of the Inter-American Development Bank or UNCTAD.

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[1] The first quote is from an unnamed financial industry official; the second comes from a memo prepared by the Central Banks of England and Canada. Both are taken from Blustein (2005), pp. 163 and 102.

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Moreover, Borensztein and Panizza (2009) have not been able to identify the channel through which defaults affect trade.

With respect to the direct effect of default on output, Sturzenegger (2004) finds a strong (albeit short-lived) negative contemporaneous effect on growth, a result confirmed by Borensztein and Panizza (2009) and De Paoli et al. (2006).[2] However, these studies are based on annual observations and their results may be misleading. Since GDP is an average, high-frequency shocks tend to spill over to the subsequent period when output is reported at a lower frequency. Thus, the sharp GDP contraction in the second half of a given year can be registered as an output decline in the following year, despite the fact that the economy started to grow early that year. This also poses a serious identification problem, namely, disentangling the effect of the default decision on economic performance from that of the crisis per se.

The objective of this paper is to contribute to this strand of the literature by examining the output cost of default using higher frequency data. The use of higher frequency data does not fully solve identification and measurement problems but helps in providing a more accurate picture. In particular, it shows more clearly the timing of the default in the context of the evolving crisis, and sheds light on the timing of the events. With this objective in mind, we conduct a simple exercise and replicate the standard tests of the effect of default on growth and output using quarterly data for emerging economies.

We find that growth rates in the post-default period are never significantly lower than in normal times. Moreover, the evidence indicates that, contrary to what is typically assumed, the output contractions often attributed to defaults actually precede the default episode. Indeed, defaults mark the inflection point at which output reaches its minimum and starts to recover. This should not be interpreted as proof that defaults in general do not matter. On the contrary, much in the way of a standard liquidity run, most of the financial distress that precedes the default decision may be due to its anticipation. However, our findings have distinct implications from a policy perspective. If defaults were costly a posteriori, the decision to default should weigh these costs against the fiscal effort needed to service the debt. However, if once the default is anticipated (and its concomitant cost brought forward) by the market the formal decision to stop servicing the debt does not entail any additional cost, then there is no tradeoff and default is therefore optimal (or even overdue).

Our paper also relates to the literature on the timing of default. According to the canonical model of sovereign debt developed by Eaton and Gersovitz (1981), countries issue sovereign debt to smooth consumption. Hence, sovereign borrowing should be countercyclical with countries borrowing during bad times and repaying during good times. Given that in this class of models there are no repayments during bad times, there should be no defaults in bad times either. As a consequence, the classic literature on sovereign debt focuses on the "willingness to pay problem" and defaults are modeled as "strategic" i.e. taking place in good times when the country could easily pay but finds that the benefit of not paying are greater than the costs associated with a sovereign default. This is a counterintuitive result that does not seem to match the empirical evidence showing that defaults tend to happen during recessions (for a detailed discussion, see Panizza et al., 2009).

More recent works by Aguiar and Gopinath (2006) and Rochet (2006) show that by adding persistent shocks to a simple Eaton and Gersovitz's (1981) model it is possible to predict procyclical borrowing and default episodes. Along similar lines, Lambertini (2004) shows that, in a model characterized by incomplete markets and atomistic lenders, negative *i.i.d* productivity shocks lead to persistent output effects (the channel is lower investment) and self-fulfilling debt crises. Arellano (2008) develops a model in which, even in the presence of *i.i.d* endowment shocks, it is possible to generate a region of risky borrowing in which net lending becomes procyclical and defaults take place after a negative shock.[3] An alternative modeling strategy, exploited by Mendoza and Yue (2008), is to assume that defaults limit the ability of private agents to obtain the working capital necessary to buy imported inputs and thus have a negative effect on total factor productivity because they lead to an inefficient reallocation of labor. Mendoza and Yue (2008) show that a model with these characteristics is consistent with the rapid output collapses and rapid recoveries often observed around default episodes.

Thus, while the original theoretical literature on sovereign debt found that defaults either never happen or happen during good times,[4] more recent theoretical work is consistent with the empirical evidence showing that defaults tend to happen during recessions. In fact, Tomz and Wright (2007) calibrate Aguiar and Gopinath's (2006) model and argue that the puzzle is not related to the fact that countries default in bad times, but that they do not default enough in bad times.

The paper proceeds as follows. Section 2 describes the data. Section 3 presents the main empirical results. Section 4 discusses the implications of our findings for the optimal timing of default, and concludes.

Table 1
Summary statistic.

Variable	Obs	Mean	Std. dev.	Min	Max
A. Yearly variables					
GDP growth	482	1.68	4.88	−14.57	11.19
INV/GDP	460	1.77	0.73	0.33	4.22
Population growth	523	1.54	1.02	−3.25	3.39
Secondary education	415	21.89	10.84	3.80	52.40
Population	523	45.09	43.46	3.06	212.00
Government consumption	460	0.04	0.10	−0.26	0.70
Civil rights	495	3.74	1.23	1.00	7.00
Δ Terms of trade	474	−0.01	0.16	−2.08	1.07
Trade openness	484	0.30	0.17	0.05	1.15
Banking crisis dummy	533	0.17	0.37	0	1
Default dummy	533	0.04	0.20	0	1
B. Quarterly variables					
GDP growth	2346	0.69	5.59	−29.04	28.28
GDP growth per capita	2346	0.302	5.61	−29.54	27.74
Market pressure	1808	0.016	0.49	−5.32	13.67

2. The data

Table 1 reports the summary statistics for both yearly and quarterly variables and shows that the annual growth rate of GDP per capita in the quarterly sample is almost identical to that of the annual sample (1.55 versus 1.68%). Table A1 in the appendix provides a definition of our variables and the sources of the data and Table A2 lists all countries, periods, and default episodes covered in our analysis.

Moving from yearly to quarterly observations entails properly dating the default, which poses non-trivial problems. Take for instance the Argentinean default of 2001/2002. While Standard and Poor's gave a selective default rating in the last quarter of 2001 after a quasi-voluntary debt exchange, most observers argue that a more accurate date of the default on international bonds is January 2002, when the default was actually announced.[5] We include several leads and lags to ensure that the results are not driven by dating errors.

[2] In fact, the 2% negative growth effect of default identified by Sturzenegger (2004) has become the standard value for the output cost of default in calibrated models (see Tomz and Wright, 2007).

[3] There are, however, several parameterizations for which the risky borrowing region disappears. In fact, Arellano's calibration uses a persistent income process.

[4] Eaton and Gersovitz's (1981) seminal paper contains an extension (section 2.3) in which default takes place after a series of negative shocks.

[5] Importantly, Argentina had not missed a payment before that date. This example shows that dating errors are magnified when we look at annual data, yet another reason to go quarterly. Note that we stack the deck against our hypothesis by dating the Argentinean default with the official (earlier) date.

We restrict our attention to a particular type of default, namely, default on debt with private international investors. For this reason, we focus on emerging economies, which by definition are comprised of globally integrated economies with a substantial volume of cross-border debt. Emerging market countries provide a reasonably homogenous group exhibiting comparable external vulnerability to capital account reversals and sudden stops and, possibly, the highest propensity to suffer from default episodes.[6]

Our tests do not include all the 24 default episodes listed in Table A2. This is because some of them occurred within a relatively short window and should be considered as spin-offs of the previous episode. Therefore, their inclusion may bias the results against finding a significant default cost. For this reason, we exclude default episodes that happen within three years of the previous default (which leaves out the Indonesian defaults of 2000 and 2002). Furthermore, we only include default on private lenders and hence exclude the Pakistani default on Paris Club debt of 1997. As a consequence, our working sample includes 20 default episodes. Ten of these episodes took place in the 1980s and mostly concerned international bank loans. The remaining ten took place between 1990 and 2006 and affected mostly sovereign bonds.

2.1. First impressions

A graphical illustration can be useful in showing how the evolution of GDP around default episodes varies when we move from annual to quarterly observations. Fig. 1A uses annual data to show GDP levels over a 6-year window centered on the default period, for a full sample of emerging and non-emerging economies. The *X* axis is defined in event time, where 0 indicates the year of the default episode, −3 indicates three years before the episode, and 3 indicates three years after the event (the solid line measures average GDP level, the dashed lines measure trend GDP before and after the default, and the dotted lines are 95% confidence intervals). It shows that GDP starts decreasing two years before the event and keeps decreasing (albeit, at a slower rate) in the following three years; a picture broadly consistent with the output cost of defaults identified in panel growth regressions that use annual data.

In the second panel (Fig. 1B), we repeat the exercise for our emerging market sample. As before, we see a clear drop in GDP in the three years before the default episode, whereas now the output remains stable and close to its minimum in the following three years. Again, the declining trend precedes the default event, but growth remains either negative or close to zero thereafter.

The third panel (Fig. 1C) replicates the exercise once more, this time for the emerging market sample and using quarterly data (in this figure the *X* axis is measured in quarters).[7] Now, we find a negative trend in the 3 years preceding the default episode combined with a steep drop in the last three quarters of the pre-episode window. While GDP still falls in the quarter after the event, it reverts the trend immediately thereafter to a quick and steady recovery to above pre-crisis levels. Thus, at least at this preliminary graphic level, going from annual to quarterly data appears to change the message in an important way.

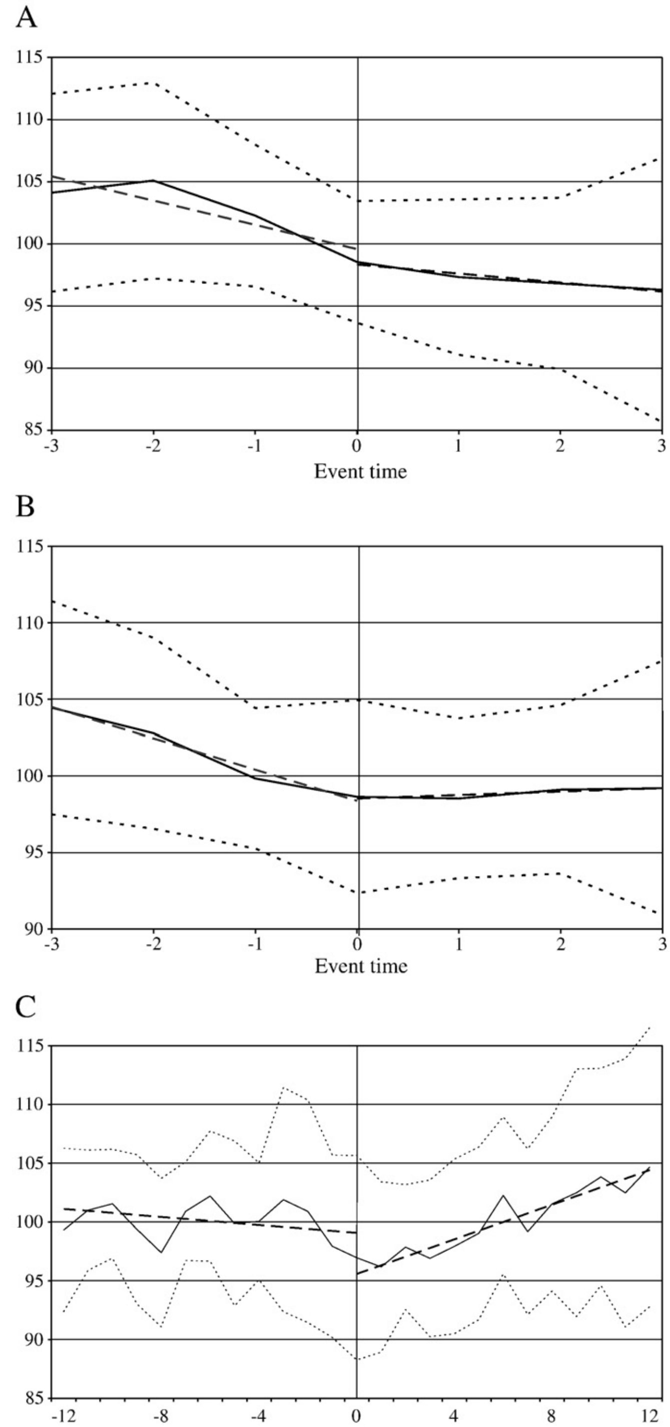

Fig. 1. A: Evolution of GDP around default episodes. Annual data using both developing and emerging market countries. The solid line plots the evolution of the level of GDP measured at yearly frequency, the dashed lines measure trend GDP before and after default, and the dotted lines are 95%confidence intervals. B: Evolution of GDP around default episodes. Annual data using only emerging market countries with quarterly data. The solid line plots the evolution of the level of GDP measured at yearly frequency, the dashed lines plot the linear trend of GDP before and after default, and the dotted lines are 95% confidence intervals. C: Evolution of GDP around default episodes. Quarterly data for emerging market countries. The solid line plots the evolution of the level of GDP measured at quarterly frequency, the dashed lines plot the linear trend of GDP before and after default, and the dotted lines are 95% confidence intervals.

3. Regression results

Fig. 1 provides suggestive evidence that the output costs of default are hard to find when measured using quarterly data, but does not amount to a formal test. Developing such a test is the focus of this section. We move gradually from the existing literature to our preferred specification.

[6] Extending this exercise to other countries is not straightforward. There are no recent defaults by industrial countries, and their inclusion as a control group is questionable. On the other hand, while there are defaults on debt with foreign banks in non-emerging, low income economies, availability of quarterly output data is in these cases virtually null.

[7] We use seasonally adjusted quarterly GDP (the seasonal adjustment procedure excludes the default period).

Table 2
Growth regressions (yearly data).

	Dependent variable: yearly GDP growth										
	Full sample							Emerging economies[a]			
Independent variables	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6	Reg 7[b]	Reg 8	Reg 9	Reg 10	Reg 11
INV/GDP	1.834	1.784	1.780								
	(7.18)***	(6.99)***	(6.95)***								
Population growth	−0.452	−0.444	−0.452								
	(3.10)***	(3.00)***	(3.07)***								
Secondary education	−0.015	−0.018	−0.022								
	(0.99)	(1.22)	(1.47)								
Ln(Population)	0.003	0.003	0.003								
	(0.72)	(0.78)	(0.89)								
Government consumption	3.161	3.201	3.086								
	(3.16)***	(3.20)***	(3.08)***								
Civil rights	0.027	0.028	0.029								
	(0.24)	(0.25)	(0.27)								
Δ Terms of trade	−0.331	−0.214	0.911								
	(0.28)	(0.18)	(1.93)*								
Trade openness	5.809	5.993	6.797								
	(3.75)***	(3.87)***	(4.45)***								
Banking crisis dummy	−0.979	−0.920	−0.948								
	(4.04)***	(3.81)***	(3.95)***								
def($T-1$)		−2.638	−2.618		−3.013	−3.001		−3.297	−3.410		
		(3.76)***	(3.75)***		(5.34)***	(5.31)***		(2.43)**	(2.54)**		
def	−3.010	−3.207		−3.549	−3.824		−3.343	−3.715		−3.051	−3.505
	(3.45)***	(3.67)***		(6.56)***	(7.08)***		(3.98)***	(3.56)***		(3.15)***	(2.11)**
def($T+1$)		−0.552	−0.458		−2.212	−2.188		−2.352	−2.348		
		(0.89)	(0.73)		(3.74)***	(3.64)***		(2.05)**	(2.01)**		
Constant	−2.179	−2.061	−2.236	1.604	1.689	1.697	1.724	2.080	2.118	1.814	1.828
	(2.69)***	(2.53)**	(2.74)***	(26.04)***	(27.11)***	(27.34)***	(21.14)***	(8.90)***	(9.20)***	(8.41)***	(7.37)***
Observations	2153	2153	2114	4841	4839	4763	2153	454	433	482	287
Countries	89	89	89	181	181	181	89	28	28	28	14
R-squared	0.10	0.11	0.10	0.01	0.02	0.01	0.10	0.05	0.03	0.02	0.03

All regressions include country fixed effects. Robust *t*-statistics in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.
[a] Sample used in the quarterly regressions.
[b] Same sample as in column (1).

3.1. Default and growth

Table 2 estimates the cost of default using a standard growth regression based on yearly data. In columns 1 to 3, we take the specification adopted by Sturzenegger (2004) and Borensztein and Panizza (2009) and add country fixed effects. In column 1, we capture the cost of default with a dummy variable that takes value 1 in the year in which a country defaults and zero otherwise (*def*). The regression shows the standard results: defaults are associated with a drop in growth of approximately 3 percentage points and the coefficient is highly significant. Column 2 also includes a dummy taking value 1 in the year that precedes the episode (*def* $T-1$) and a dummy taking value 1 in the year that follows the episode (*def* $T+1$). We find negative coefficients for both dummies, although only (*def* $T-1$) is statistically significant. Column 3 replicates column 2, this time dropping the year of default episode and finds identical results. In columns 4 to 6, we run the same regressions without the control variables, as an intermediate step towards our quarterly specification, which excludes controls due to data availability. Now *def* $T+1$ becomes statistically significant and large, indicating that growth in the year after default is 2 percentage points lower than in tranquil times. So, annual data regressions estimated without the set of standard controls used in columns 1–3 suggest that there is an output cost in both the year before and the year after the default. The same applies to column 7 (where we replicate the specification of column 4 for the sample of column 1), and to columns 8 to 10 (where we run similar regressions for our emerging market sample). Results are virtually unaltered in all cases.

The last four columns show that annual data regressions with no controls applied to the sample of emerging market countries for which we have quarterly data suggest that there is an output cost in both the year before and the year after the default. In sum, based on annual data, during the three-year window around default, growth rates appear to be significantly (and substantially) lower than average — although, judging from the value of the coefficients, there is no indication that the decline accelerates after the actual default event.

Reassured by the robustness of the previous results to sample and specification changes, in Table 3 we repeat the exercise using quarterly GDP data. Column 1 includes only two regressors: a dummy variable taking value 1 in the default quarter and a market pressure index along the lines of Kaminsky and Reinhart (1999). The coefficient of the default dummy is negative and large, suggesting that at the time the default materializes (quarterly) growth is a hefty 1.7% lower than in normal times (this corresponds to an annualized rate of 7%). The coefficient, however, is not statistically significant. As expected, the market pressure variable is also negative and statistically significant.[8] Column 2 adds dummies for the quarters that precede and follow the default event. Unlike in Table 2, growth is now significantly lower in the quarter before (at 3.4%, the coefficient is extremely large) but not in the quarter after default. In fact, we now find that the post-default coefficient is positive (albeit not statistically significant). The same message is delivered when we include separate dummies for two and three periods before and after default (columns 3 and 4); when we use one dummy that takes value 1 in the two periods before the default and one dummy that takes value 1 in the two periods after the default (these are the *def*($T-1\ldots T-2$) and *def*($T+2\ldots T+1$) variables in column 5); and when we use one dummy that takes value 1 in the three periods before the default and one dummy that takes value 1 in the three periods after the default (these are the *def*($T-1\ldots T-3$) and *def*($T+3\ldots T+1$)

[8] Dropping this variable or including a variable measuring changes in the real exchange rate do not affect our results.

Table 3
Quarterly data, baseline model.

Independent variables	Dependent variable: seasonally adjusted quarterly GDP growth					
	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6
def($T-3$)				−0.002		
				(0.00)		
def($T-2$)			−1.112	−1.106		
			(1.47)	(1.43)		
def($T-1$)		−3.403	−3.362	−3.359		
		(3.29)***	(3.30)***	(3.31)***		
def	−1.738	−1.802	−1.758	−1.752	−1.802	−1.826
	(1.11)	(1.14)	(1.12)	(1.12)	(1.14)	(1.15)
def($T+1$)		0.928	0.970	0.975		
		(0.63)	(0.64)	(0.64)		
def($T+2$)			0.714	0.721		
			(0.73)	(0.73)		
def($T+3$)				−0.902		
				(0.62)		
def($T-1\ldots T-2$)					−2.279	
					(3.31)***	
def($T+2\ldots T+1$)					0.799	
					(0.90)	
def($T-1\ldots T-3$)						−1.560
						(2.52)**
def($T+3\ldots T+1$)						0.190
						(0.24)
mkt pressure	−0.959	−0.935	−0.906	−0.886	−0.933	−0.932
	(3.93)***	(3.92)***	(3.91)***	(3.89)***	(3.93)***	(3.93)***
Constant	0.858	0.879	0.879	0.869	0.881	0.891
	(6.58)***	(6.63)***	(6.44)***	(6.17)***	(6.61)***	(6.61)***
Observations	1755	1739	1699	1656	1755	1755
R-squared	0.02	0.02	0.02	0.02	0.02	0.02
$\sum_i def(T-i)$			−4.474	−4.467		
			0.001	0.011		
$\sum_i def(T+i)$			1.685	0.795		
			0.356	0.740		
$\sum_i def(T+i)$ $=\sum_i def(T-i)$		−4.331	−6.159	−5.262	−3.077	−1.750
		0.015	0.005	0.061	0.005	0.068

All regressions include country fixed effects. Robust *t*-statistics in parentheses, ** significant at 5%; *** significant at 1%. *P*-value of *F*-tests in brackets.

Table 4
Robustness I. Restricting the estimation to a 3 + 3 years window.

Independent variables	Dependent variable: seasonally adjusted quarterly GDP growth					
	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6
def($T-3$)				0.189		
				(0.17)		
def($T-2$)			−0.921	−0.921		
			(1.21)	(1.17)		
def($T-1$)		−3.216	−3.183	−3.183		
		(3.06)***	(3.10)***	(3.11)***		
def	−1.541	−1.601	−1.562	−1.562	−1.614	−1.637
	(0.94)	(0.97)	(0.96)	(0.96)	(0.98)	(0.99)
def($T+1$)		1.122	1.158	1.157		
		(0.75)	(0.74)	(0.73)		
def($T+2$)			0.918	0.918		
			(0.95)	(0.93)		
def($T+3$)				−0.699		
				(0.48)		
def($T-1\ldots T-2$)					−2.099	
					(2.97)***	
def($T+2\ldots T+1$)					0.987	
					(1.08)	
def($T-1\ldots T-3$)						−1.377
						(2.11)**
def($T+3\ldots T+1$)						0.382
						(0.47)
mkt pressure	−0.851	−0.822	−0.777	−0.774	−0.823	−0.823
	(4.21)***	(4.29)***	(4.48)***	(4.49)***	(4.30)***	(4.32)***
Constant	0.501	0.550	0.525	0.533	0.561	0.582
	(2.42)**	(2.55)**	(2.25)**	(2.10)**	(2.44)**	(2.40)**
Observations	522	521	509	497	522	522
R-squared	0.04	0.06	0.05	0.05	0.05	0.05
$\sum_i def(T-i)$			−4.104	−3.916		
			0.003	0.037		
$\sum_i def(T+i)$			2.076	1.376		
			0.269	0.580		
$\sum_i def(T+i)$ $=\sum_i def(T-i)$		−4.338	−6.179	−5.291	−3.086	−1.760
		0.016	0.004	0.060	0.004	0.067

All regressions include country fixed effects. Robust *t*-statistics in parentheses, ** significant at 5%; *** significant at 1%. *P*-value of *F*-tests in brackets.

variables in column 6). We find that growth is always significantly lower in the quarters leading to default but not in the quarters following default. The joint tests reported in the bottom panel of the table shows that growth after default is always significantly higher than growth before default.

Thus, a simple comparison of Tables 2 and 3 suggests that default materializes when the crisis is already underway: the negative link revealed by annual data simply captures the fact that defaults tend to occur in the context (and often as a result) of a crisis.

As quarterly data may be more sensitive to (autocorrelated) measurement error that tends to be corrected over time, we re-estimated our model by including the lagged dependent variable and found that the results are unchanged.[9]

Table 4 explores the pre- and post-default periods in more detail by narrowing the estimation window to a six-year period centered on the default event. The results are again unchanged.

Since the regressions with annual data are based on income per capita and those with quarterly data are based on total income, we also re-estimated our model using data on per capita income and found no difference with respect to the baseline model of Table 3.[10] We also used quarterly data to compute annual growth (i.e., we compute growth from quarter $T-4$ to quarter T, since this generates overlapping growth period we estimate the model by using Newey's correction for serial correlation in the error term). In this case the *def* ($T+1$) dummy captures the effect of default on growth from quarter 0 (i.e., the quarter of default) to quarter 4; *def*($T-1$) captures the effect of default on growth from quarter −4 to quarter 0; and *def* measures the effect of the default in the three growth periods that include the quarter of default (i.e., quarter −3 to quarter +1, quarter −2 to quarter +2 and quarter −1 to quarter 3). As before, we did not find any significant negative growth effect in the post-default period.[11]

Table 5 explores the role of outliers (of particular relevance given the relatively small number of events in our sample) by running the regression of Table 3, column 2 dropping one country at a time. The table, which highlights the extreme values for the coefficients and *t*-statistics, shows that the contemporaneous effect ranges between −0.86. and −2.81 and that it is rarely statistically significant. The effect at $T-1$ ranges between −2.90 and −4.25 and is always statistically significant. By contrast, the effect at $T+1$ ranges between −0.31

[9] Results are available upon request. The joint presence of fixed effects and the lagged dependent variable can be a source of bias (Arellano and Bond 1991). However, this is not a problem if T is bigger than 25 and in our default sample T is always greater than 30. In any case, our results are robust to using the Arellano and Bond GMM difference estimators. Our results are also robust to controlling for country-year fixed effects to capture the evolution of country-specific fundamentals at an annual frequency. In a regression with country-year fixed effects, the coefficients should be interpreted as the deviation from the average growth rate in the year of default, and not as the deviation from the average growth rate in normal times. As such, this test encompasses all annual variables typically used in standard specifications, including a crisis year dummy.

[10] We would like to thank an anonymous referee for suggesting this robustness check. Since quarterly data on population are generally not available, we build data on quarterly population growth by interpolating annual data. Results are available upon request.

[11] We would like to thank an anonymous referee for suggesting this robustness check. Results are available upon request.

Table 5
Robustness II. Dropping one country at a time.

Dropping	Contemporaneous effect		$T+1$		$T-1$	
	Coefficient	|t-statistics|	Coefficient	|t-statistics|	Coefficient	|t-statistics|
Keeping all countries	−1.41	0.87	1.42	0.92	−2.81	2.65
Argentina	−2.15	1.29	0.74	0.47	−3.19	2.93
Chile	−2.81	2.08	1.23	0.80	−3.59	3.26
Dominican Republic	−1.82	1.07	1.01	0.63	−3.32	2.99
Ecuador	−1.07	0.70	1.23	0.79	−3.38	3.05
Indonesia	−1.83	1.07	1.18	0.75	−3.36	3.02
Mexico	−1.80	1.14	0.93	0.63	−3.40	3.29
Nigeria	−0.86	0.62	−0.31	0.34	−2.90	2.98
Pakistan	−1.81	1.14	0.92	0.63	−3.41	3.29
Peru	−1.59	0.95	0.68	0.43	−3.01	2.90
Philippines	−1.97	1.17	0.47	0.31	−3.33	3.01
Russia	−1.83	0.99	0.97	0.56	−3.70	3.12
South Africa	−2.02	1.19	1.14	0.73	−3.91	3.93
Ukraine	−2.29	1.28	1.48	0.89	−4.25	4.21
Uruguay	−1.41	0.87	1.42	0.92	−2.81	2.65

Specification (2) of Table 3.

(this is the only negative coefficient; we obtain this coefficient when we exclude Pakistan from the sample) and 1.48 and it is never statistically significant. Again, leads (but not lags) help explain the evolution of output.

Another possible problem relates to the presence of measurement error in the dating of the default dummy. The presence of such measurement error should not bias the results in our favor because, under standard assumptions, measurement error produces a downward bias for all coefficients. In any case, we checked the possible effects of measurement error by conducting a *non-robustness* analysis which studied the consequences of amplifying the measurement error. In particular, we ran a Monte Carlo simulation with 1000 replications in which we estimated the specification of the first column of Table 3 by adding extra noise to our default variable (in each replication we randomly split the sample into three groups: we left the default variable unchanged in 50% of the observations, we moved the default variable one period ahead in 25% of the observations, and we moved the default variable backward in the remaining 25% of observations). As expected, we found that adding noise makes all point estimates biased towards zero, but that there is no pattern suggesting that the presence of additional noise in the default variable tends to push our results toward finding a positive coefficient in $T+1$ and a negative coefficient in $T-1$.[12]

Next, we ran an event-study like test that compares cumulative growth rate before and after the default event for different windows centered on the default quarter (which is dropped for the purposes of this computation).[13] In this way, we want to confirm not only that output trend declines before the actual default takes place, but also that the declining trend reverts after default. The results support this hypothesis: cumulative growth goes from negative to positive, and the difference between growth rates before and after the default increases (Fig. 2) as the window widens. Default, rather than a trigger, represents the turning point of the crisis, possibly due to non-trivial costs of avoiding default and to the fact that most of the consequences of default are typically reflected in the markets before the decision is made official.

3.2. Growth-inducing defaults?

The finding that defaults are followed by an economic recovery should not be mistaken as evidence of causality — much in the same way as the correlation between default and annual growth should not be mistaken as saying that defaults are costly.

The pre-default contraction may be caused by an external shock, by the anticipation of a probable credit event, or by both. In fact, the pre-default contraction can be due to a common shock. For instance, growth and default expectations have a common driver in capital account contractions that jointly depress investment and inhibit debt roll-over. Alternatively, it can be driven by the output decline, as is the case when a real shock deteriorates fiscal revenues and negatively affects the ability to repay the debt. Identifying the causal effect going from default anticipation to GDP growth and separating this effect from that going from growth to default expectations would require an instrument for default expectations (i.e., a variable that affects the probability of default and is not influenced by the growth outlook or other growth-related variables). While we cannot think of any suitable instruments for identifying such a causal relationship, our results indicate that default is systematically associated with slower growth prior to the default decision and with higher growth after the default is announced. For this to be solely the reflection of a causal link going from growth to default, one would need to argue that growth dynamics on average revert right after the default event. While this is possible, we believe that it is more likely that causality goes in both directions, namely that slow growth is the cause and the consequence

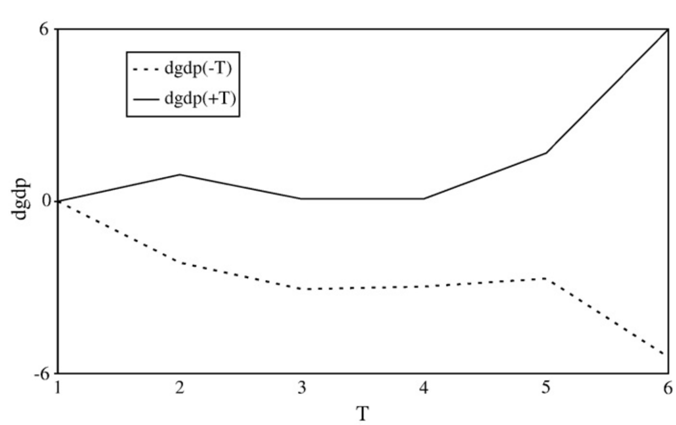

Fig. 2. Cumulative output growth before and after default (seasonally adjusted GDP growth).

[12] Results are available upon request. Of course a bias would still be possible if we were making a systematic error in dating the default variable (i.e., if we were systematically dating the default after the default really happened). However, we do not think that we are making this error. In fact, in the case of Argentina, we are making exactly the opposite error in order to stack the deck against our hypothesis (see footnote 5).

[13] Specifically, setting $t=0$ for the default quarter, DGDP$(-s)=$[GDP(-1)/GDP$(-s)]-1$, and DGDP$(+s)=$[GDP(s)/GDP$(+1)$]-1. Full results are available upon request.

Table 6
Maximum depth of the recession and default.

Sample	max CDR	Obs	Mean	*P*-value		
				CDRnodef–CDRdef<0	CDRnodef–CDRdef different from 0	CDRnodef–CDRdef>0
Full sample	No default	625	0.0578	0.0015	0.0030	0.9985
	Default	20	0.1079			
Countries having at least one default	No default	309	0.0511	0.0007	0.0014	0.9993
	Default	20	0.1079			
Argentina	No default	31	0.0452	0.0017	0.0034	0.9983
	Default	2	0.1330			
Nigeria	No default	31	0.0614	0.0697	0.1394	0.9303
	Default	2	0.1340			
Peru	No default	23	0.0567	0.6649	0.6702	0.3351
	Default	2	0.0316			
Russia	No default	11	0.0641	0.0260	0.0521	0.9740
	Default	2	0.1828			
Uruguay	No default	19	0.0322	0.6885	0.6230	0.3115
	Default	2	0.0200			
South Africa	No default	27	0.0140	0.2348	0.4696	0.7652
	Default	2	0.0208			

CDR is defined as cumulated drop since the last peak. It is measured as $CDR_t = y_s - y_t$. Where y_s measures log GDP at the last peak and y_t measures log of GDP at time t.

of an imminent default (Cerra and Saxena, 2008, find no evidence of large positive shocks in the aftermath of recessions). If this is the case, the anticipation of a default causes low growth and the validation of these default expectations does not entail additional costs.[14]

While we cannot formally test whether our results are driven by large differences in the shock or differences in the cost of anticipated and realized default, we can check whether the benign post-default outcome is simply reflecting the association of defaults with particularly deep economic downturns. Beaudry and Koop (1993) have shown that output expansions depend positively on the "current depth of recession" (CDR), defined as the gap between the current level of output and the level of output reached in the last peak. More generally, a growing body of literature has highlighted the nonlinear nature of the business cycle and, in particular, the fact that growth rates depend positively on the depth of the current output gap.[15]

We examine whether this argument can explain the finding of "growth-inducing defaults" in two steps. First, we document that defaults are indeed associated with larger than average recessions (note that so far we have only documented an association between recessions and defaults–more precisely, that the latter are typically preceded by the former–but not that the fact that recessions are more pronounced prior to default events). Second, we rerun the baseline regressions of Table 3 controlling for the current depth of the recession, captured by Beaudry and Koop's (1993) CDR variable, to see whether the link between defaults and growth is due to the omission of the recession depth variable.

Table 6 reports the results from the first step, showing that the depth of the recession is significantly larger for recessions leading to debt defaults. In the table, we first compare the maximum recession depth in the absence of default for the whole sample, with the maximum CDR reached in recessions that coincided with default events. As the means test indicates, recessions are 5 percentage points deeper during default episodes. The difference is even larger when we look at countries that defaulted at least once in the period under study. For defaulters, recessions have been nearly twice as deep when they ended in default than otherwise. The same conclusion can be reached by looking at defaulters individually (see bottom panel of Table 6): pre-default recessions are always deeper (and often significantly so).

Table 7 replicates the baseline regressions of Table 3, including the CDR variable to test whether the expansionary effect of defaults can be attributed to the larger depth of the preceding recessions. As expected, we find that CDR has a positive and statistically significant effect on growth, which is robust to the inclusion of the lagged dependent variable (which replicates Beaudry and Koop's, 1993, specification for the U.S.). More to the point of our test, including the CDR variable does not alter our baseline result. First of all, we find that the leading effect of the crisis remains virtually unaltered (negative and statistically significant). Second, we find that including CDR and explicitly controlling for the effect of excess capacity somewhat reduces the post-default recovery documented in Table 3 (for instance, the coefficient of $def(T+1)$ in column 2 goes from 0.928 to 0.213), but the coefficients for the post-default dummies remain positive and insignificant. This confirms that our previous results were not driven by the fact that we were not controlling for differences in the depth of the recession. In the last two columns of Table 7, we interact the default dummies with the CDR variables. We find that the interacted variables are not statistically significant and that including the interacted variables does not affect the baseline results.

3.3. Default and growth in the long run

Defaults may not have immediate effects on output, but may exert their influence over the long run, either through lower investment or reduced access to capital markets. Because of that, the analysis in this paper would not be complete without a look at the connection between default episodes and the evolution of long-run growth.

We look at this issue in two ways. First, we compare growth rates before and after the event with log linear trend growth. More precisely, we divide the defaults in our sample into three groups according to whether post-default growth was below pre-default growth; above pre-default growth but below long-term growth; and above long-term growth.[16] We find that whereas growth was stronger after default in 70% of the cases (in line with our previous findings), it exceeded long-term growth in 50% of the cases, suggesting that default, on average, does not deteriorate growth prospects (the crisis that precedes the default can, however, have a long-lasting effect on the *level* of output, Cerra and Saxena, 2008).

Next, we look at the same issue from a different angle. Exploiting the variability of HP-filtered long-run growth, we rerun the baseline regressions in Table 3 replacing the growth rate by the long-term growth rate (computed country by country over the full sample period).[17] We find that the decline in trend output that characterizes the period surrounding default precedes the default event, and does not appear to elicit an additional negative impact ex-post. Thus, there seems to be no negative effect on the long-run output immediately after the default event. Indeed, long-run growth appears to increase in the post-default period, as illustrated by Fig. 3, where we compare average HP-filtered output before and after the event. While the

[14] A simple analytical model that illustrates this point is available upon request.

[15] The underlying assumption is that, because of the excess capacity during the contractionary phase of the cycle, positive real shocks will have more persistent effects than negative shocks. See also Hamilton (1989), Jansen and Oh (1999), and Neftci (2001).

[16] Results are available upon request.

[17] We set the smoothing parameter in the HP filter to be equal to 1600. This is the standard choice for quarterly data.

Table 7
Current depth of the recession and growth after default.

	Dependent variable: seasonally adjusted quarterly GDP growth							
Independent variables	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6	Reg 7	Reg 8
def(*T*−3)				−0.954				
				(0.73)				
def(*T*−2)			−1.182	−1.184				
			(1.29)	(1.25)				
def(*T*−1)		−3.742	−3.699	−3.721				−3.303
		(3.61)***	(3.61)***	(3.64)***				(2.89)***
def	−2.356	−2.444	−2.399	−2.438	−2.459	−2.492	−1.577	−1.655
	(1.47)	(1.52)	(1.50)	(1.53)	(1.52)	(1.54)	(0.81)	(0.85)
def(*T*+1)		0.213	0.256	0.210				−0.373
		(0.15)	(0.18)	(0.14)				(0.24)
def(*T*+2)			0.276	0.252				
			(0.26)	(0.24)				
def(*T*+3)				−0.207				
				(0.18)				
def(*T*−1... *T*−2)					−2.500			
					(3.36)***			
def(*T*+2... *T*+1)					0.206			
					(0.23)			
def(*T*−1... *T*−3)						−1.784		
						(2.70)***		
def(*T*+3... *T*+1)						−0.235		
						(0.31)		
mkt pressure	−1.003	−0.972	−0.944	−0.923	−0.970	−0.969	−0.997	−0.968
	(3.73)***	(3.74)***	(3.71)***	(3.68)***	(3.74)***	(3.74)***	(3.74)***	(3.73)***
CDR (−1)	0.204	0.205	0.206	0.220	0.206	0.207	0.208	0.210
	(5.13)***	(5.18)***	(5.17)***	(5.42)***	(5.20)***	(5.22)***	(5.11)***	(5.08)***
*Def** CDR (−1)							−0.154	−0.156
							(1.16)	(1.17)
Def(*T*−1)* CDR (−1)								−0.120
								(0.58)
Def(*T*+1)* CDR (−1)								0.104
								(0.28)
Constant	0.385	0.411	0.403	0.367	0.419	0.429	0.376	0.401
	(2.38)**	(2.52)**	(2.40)**	(2.14)**	(2.57)**	(2.61)***	(2.31)**	(2.42)**
Observations	1746	1730	1690	1647	1746	1746	1746	1730
R-squared	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
$\sum_i def(T-i)$			−4.881	−5.113				
			0.001	0.007				
$\sum_i def(T+i)$			0.532	−0.492				
			0.769	0.831				
$\sum_i def(T+i) = \sum_i def(T-i)$		−3.955	−5.413	−4.621	−2.706	−1.549		
		0.023	0.015	0.101	0.016	0.105		

All regressions include country fixed effects. Robust *t*-statistics in parentheses, ** significant at 5%; *** significant at 1%. *P*-value of *F*-tests in brackets.

difference in trends is not significant (as the two-standard deviation intervals indicate), the figure further confirms that defaults do not seem to exert a negative effect on output over the long run.

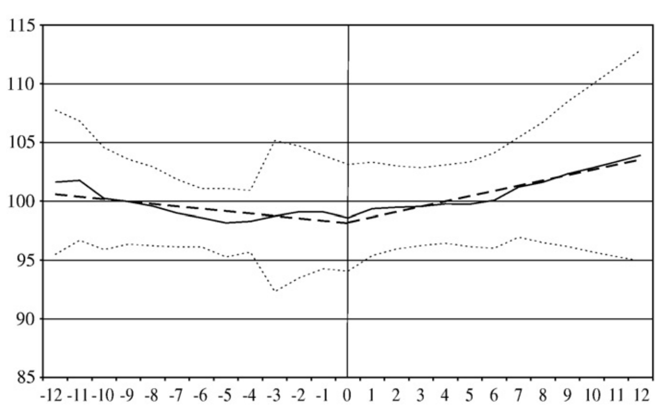

Fig. 3. HP trend GDP around default episodes. The solid line plots the evolution of the level of HP trend GDP measured at quarterly frequency, the dashed lines plot the linear trend of GDP before and after default, and the dotted lines are 95% confidence intervals.

3.4. Default and unemployment

Many observers would agree that, once income distribution is taken into account, unemployment may be a more important–and persistent–determinant of social welfare than output growth (Pernice and Sturzenegger, 2004). One would expect unemployment and real growth to be closely correlated, so that the conclusions from the previous tests should extend to this new variable. We show here that this is indeed the case.

Table 8 provides a preliminary look at the impact of sovereign defaults on unemployment. The table replicates the specifications of Table 3 using unemployment instead of real output growth as the dependent variable (all regressions include the lagged dependent variable to control for unemployment persistence). As before, we find that whatever negative influence default may have on unemployment, it materializes before the actual default takes place. In fact, we find that unemployment either increases or remains stable in the quarters that precede the default episode (the coefficients of the lagged default variables tend to be positive but not statistically significant) but that this trend reverts when the default takes place. In particular, the regression results indicate that unemployment starts decreasing (the coefficient is negative and statistically significant) in the quarter in which the default takes place and continues to decrease in the quarter after the default.

Table 8
Default and unemployment.

Independent variables	Dependent variable: seasonally adjusted quarterly unemployment rate					
	Reg 1	Reg 2	Reg 3	Reg 4	Reg 5	Reg 6
def(*T*−3)				0.058		
				(1.00)		
def(*T*−2)			−0.068	−0.073		
			(1.32)	(1.37)		
def(*T*−1)		0.070	0.064	0.060		
		(0.69)	(0.63)	(0.60)		
def	−0.112	−0.113	−0.119	−0.122	−0.120	−0.127
	(2.86)***	(2.72)***	(2.70)***	(2.51)**	(2.81)***	(2.71)***
def(*T*+1)		−0.111	−0.118	−0.122		
		(5.20)***	(4.89)***	(4.41)***		
def(*T*+2)			−0.041	−0.046		
			(0.42)	(0.46)		
def(*T*+3)				−0.107		
				(1.68)*		
def(*T*−1... *T*−2)					−0.003	
					(0.04)	
def(*T*+2... *T*+1)					−0.079	
					(1.43)	
def(*T*−1... *T*−3)						0.003
						(0.04)
def(*T*+3... *T*+1)						−0.095
						(2.19)**
mkt pressure	0.102	0.098	0.098	0.095	0.102	0.102
	(2.50)**	(2.38)**	(2.39)**	(2.17)**	(2.51)**	(2.43)**
Constant	0.012	0.012	0.012	0.013	0.012	0.013
	(2.49)**	(2.48)**	(2.56)**	(2.62)***	(2.58)**	(2.68)***
Observations	457	457	457	457	457	457
R-squared	0.05	0.06	0.06	0.07	0.05	0.06
$\sum_1 def(T-i)$			−0.004	0.045		
			[0.968]	[0.765]		
$\sum_1 def(T+i)$			−0.159	−0.275		
			[0.139]	[0.037]		
$\sum_1 def(T+i)=\sum_1 def(T-i)$		0.181	0.155	0.320	0.076	0.098
		[0.069]	[0.289]	[0.059]	[0.355]	[0.152]

All regressions include country fixed effects. Robust *t*-statistics in parentheses * significant at 10%; ** significant at 5%; *** significant at 1%. *P*-value of *F*-tests in brackets.

4. Conclusions

This paper delivers a simple and sobering message: contrary to what it is typically presumed, defaults have not been followed by output contractions. In fact, we find that the opposite seems to be the case: the default quarter coincides with the trough of the output contraction, and marks the start of the economic recovery. This, however, does not appear to reflect a “benign” effect of defaults but rather the fact that the latter are typically associated with particularly deep recessions which may be caused by the anticipation of a default episode and, as a result, particularly steep recoveries.

These findings have important policy implications for debt management policies — and, indirectly, for debt sustainability and the optimal timing of default. If the default decision entails a tradeoff between the burden of servicing the debt (which grows as the crisis deepens and roll-over costs mount), and the additional cost of default (which declines as the crisis takes its toll), is the absence of observed costs documented here an indication that defaults are often deferred for too long?

The first thing to note is that our findings do not imply that policies that lead to default have no cost; on the contrary, the large GDP decline that typically precedes a default may reflect in part the anticipation of the default decision, which suggest that an early, strategic default may trigger large economic costs. Indeed, it could be argued that, if policymakers subscribe the view that strategic defaults are costly while “excusable” defaults carry a much lower penalty (Grossman and Van Huyck, 1988), they would only choose to default when costs have already materialized and the country's inability to pay has become apparent (hence, the absence of observed costs documented in this paper).[18] By delaying the default decision, policymakers may be signaling to the market that the default is indeed unavoidable. In other words, policymakers are willing to incur the toll associated with a delayed default in order to avoid a larger and more persistent cost to be paid if markets perceived the default as strategic.[19] If this interpretation is correct, a third-party institution that can sanction when countries cannot avoid a debt restructuring could play an important role in reducing the deadweight loss of default.

However, the fact that sometimes policy makers refuse to throw in the towel even after the IMF and the market accepts that a default is unavoidable (the Argentine default being a case in point) suggests an alternative, less altruistic, account: policymakers understand that, almost as a rule, they would not survive the political cost of a default (Borensztein and Panizza, 2009, discuss this hypothesis). In this case, the delay could be interpreted as the policymaker's way of gambling for resurrection — at the expense of his constituency.

Ecuador's 2008 partial default adds a new layer to the discussion.[20] The fact that the first strategic sovereign default in recent history happened in a country where opinion polls, unlike elsewhere, seemed

[18] The finding that virtually all defaults appear to be driven by an adverse external context rather than by opportunistic behavior in times of bonanza is consistent with this view.

[19] Alfaro and Kanczuk (2005) make a similar point.

[20] On December 12, 2008, Ecuador defaulted on two of its three outstanding global bonds (Ecuador 12 and Ecuador 30) after a government-sponsored commission concluded that these bonds (issued in the 2000 debt exchange that followed a previous default in 1999) have been exchanged for debt that, according to the commission, was illegitimate. The country has so far kept current on a third global bond (Ecuador 15), issued through a primary placement after the 2000 debt exchange.

to favor the debt renegotiation initiative would indicate that political costs may play a decisive role in inhibiting (or fostering) a unilateral debt restructuring.

To what extent a delayed default reflects the signaling of inability to pay or the policy makers' self-interested behavior remains a fruitful question for future research.

Acknowledgements

We would like to thank the editor, two anonymous referees, Eduardo Cavallo, and other participants in the IPES pre-conference and LACEA 2007 conference for comments and suggestions and Mariano Alvarez and Carol Saade for excellent research assistance.

Appendix A

Table A1
Variable definition and sources.

Variable	Definition	Source
Yearly variables		
GDP growth	Real GDP per capita growth (annual %)	World Development Indicators
INV/GDP	Investment share of CGDP (current prices)	World Development Indicators
Population growth	Population growth rate	World Development Indicators
Secondary education	Percentage of secondary school attained in the total pop	Barro and Lee (1993)
Population	Total population	World Development Indicators
Government consumption	One period lag of general government final consumption expenditure (annual % growth)	World Development Indicators
Civil rights	Index of civil rights	Freedom in The World
Δ Terms of trade	Terms of trade (tt) variation, computed as tt − tt(− 1)	World Development Indicators
Trade openness	Average exports plus imports to GDP (current US$)	World Development Indicators
Banking crisis dummy	Bank Crisis Measure (binary, 1 = crisis)	Caprio and Kinglebiel (2003)
Def	Beginning of Foreign Currency Bank and Bond Debt Default	The main source of information is Standard & Poor's, but information was also obtained from The World Bank's Global Development Finance database (analysis and statistical appendix) and press reports.
def(+ 1)	Forward of *def*	
def(− 1)	Lag of *def*	
Quarterly variables		
Quarterly growth	Real quarterly (i.e., growth from quarter t to quarter $t-1$) seasonally adjusted GDP growth (% change). Observations where the absolute value of quarterly GDP growth was greater than 30% were dropped from the sample.	International Financial Statistics and national sources
Def	Beginning of Foreign Currency Bank and Bond Debt Default	The main source of information is Standard and Poor's, but information was also obtained from. The World Bank's Global Development Finance database (analysis and statistical appendix) and press reports.
Quarterly variables		
x (− i)	ith lag of variable x	
x (+ i)	ith lead of variable x	
CDR	Percentage difference between the current level of output and its and the level of output reached in the last peak. Thus, CDR is measures the cumulated output drop since the last peak and is calculated as: $CDR_t = y_s - y_t$. Where y_s measures log GDP at the last peak and y_t measures log of GDP at time t.	
mkt pressure	High-frequency market pressure index (reserves + depreciation weighted by the inverse of their standard deviation)	International Financial Statistics

Table A2
List of countries and years included in the quarterly analysis.

Country	Years and quarters	Default episodes
Algeria	From 1995:2 to 2005:1	
Argentina	From 1970:2 to 2005:4	1982: Q1; 2001: Q4
Bangladesh	From 2000:2 to 2004:4	
Barbados	From 2000:2 to 2005:3	
Brazil	From 1991:2 to 2002:4	
Bulgaria	From 2000:2 to 2005:3	
Chile	From 1980:2 to 2002:4	1983: Q1
Colombia	From 1994:2 to 2002:4	
Cote D'Ivoire	From 2000:2 to 2003:4	
Croatia	From 1991:2 to 2005:3	
Cyprus	From 2000:2 to 2005:3	
Dominican Republic	From 1980:2 to 2002:4	1982: Q4; 1999: Q2
Ecuador	From 1991:2 to 2002:4	1999: Q3
Fiji	From 2000:2 to 2002:4	
Hungary	From 1979:2 to 2005:3	
India	From 2000:2 to 2005:3	
Indonesia	From 1993:2 to 2002:4	1998: Q2; 2000: Q1; 2002:Q1
Korea	From 1970:2 to 2005:3	
Luxembourg	From 2000:2 to 2005:3	
Macedonia, FYR	From 2000:2 to 2004:4	
Malawi	From 2000:2 to 2004:2	
Malaysia	From 1970:2 to 2005:3	
Mexico	From 1970:2 to 2005:3	1982: Q3
Morocco	From 1993:2 to 2002:4	
Nigeria	From 1970:2 to 2005:3	1983: Q3; 1986: Q3
Pakistan	From 1995:3 to 2002:2	1997: Q3; 1998: Q2
Peru	From 1979:2 to 2002:4	1980: Q1; 1983: Q1
Philippines	From 1981:2 to 2005:2	1983: Q4
Poland	From 1982:2 to 2005:3	
Russia	From 1991:2 to 2002:4	1991:Q4; 1998:Q3
Senegal	From 2000:4 to 2003:4	
South Africa	From 1970:2 to 2002:4	1985: Q3; 1989: Q4
Thailand	From 1993:2 to 2002:4	
Trinidad and Tobago	From 2000:2 to 2004:4	
Tunisia	From 1970:2 to 2005:3	
Turkey	From 1980:2 to 2005:3	
Ukraine	From 1993:2 to 2002:4	1998: Q3
Uruguay	From 1988:2 to 2004:4	1990: Q1; 2003: Q2
Venezuela	From 1993:2 to 2002:4	

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