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Sovereigns going bust: estimating the cost of default

by

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Sovereigns going bust: estimating the cost of default

Dmitry Kuvshinov and Kaspar Zimmermann *

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Abstract

This paper estimates the cost of sovereign default by using novel econometric methods – dynamic local projections applied to a sample that is re-randomised using inverse propensity score weights. We find that the impact of default on output is negative, significant and persistent – around 2.8% of GDP on impact and 4.8% at peak. The downturn is driven by sharp falls in investment, accompanied by a collapse in gross trade. The cost rises dramatically if the default is followed by a systemic banking crisis, peaking at 9.5% GDP. Our findings suggest that while autarky costs play an important role, sovereign-banking spillovers are central to the cost of default.

Keywords: Sovereign default; sovereign debt; banking crises; treatment effects; local projections; inverse propensity score weighting

JEL-Codes: H63, F34

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1 Introduction

In the summer of 2015, the Greek prime minister Alexis Tsipras had to decide whether to default on the country's sovereign debt or accept the conditions set by the country's creditors. This decision was greatly complicated by the lack of agreement about what the economic consequences of a default would be. This lack of information points to a fundamental issue for models of sovereign debt in international macroeconomics. Sovereign debt contracts are not directly enforceable and therefore, the existence of sovereign debt markets hinges on an indirect punishment mechanism in the form of default costs. And yet our empirical knowledge of these costs remains rather limited.

This paper seeks to improve the understanding of how costly sovereign default is, and which mechanisms make default costly. Estimating the cost of default is, however, fraught with a number of difficulties. These can be illustrated by looking at the GDP performance of defaulters, depicted in Figure 1; with GDP scaled to 1 in the year before default (time 0).

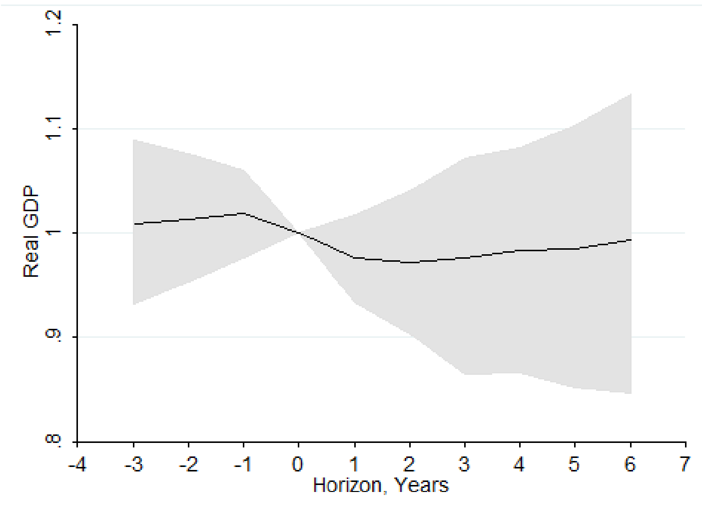


Figure 1: Real GDP path around sovereign default

Notes: Real GDP per capita, given default between years 0 and 1. GDP scaled to equal 1 in year 0. Black line is the mean path of GDP, grey bands – the 20/80 percentile range. Based on 116 defaults during 1970 – 2010. Defaults on external debt only, as defined by Standard & Poors (S & P). Default is defined as failure to meet a principal or interest payment on the due date contained in the original terms of the debt issue.

First, the decision to default may be endogenous to the country's economic conditions – for example, most countries' output is already declining in the year before default. This makes it difficult to separate the impact of default from that of other factors influencing the economy. Second, the wide range of economic outcomes both before and after default (shaded areas in Figure 1) suggests that the cost could be state-contingent, with something systematic separating the costly from the costless defaults. Third, the post-default GDP slump persists over a number of years, making any estimates focussing on one particular year potentially misleading. These problems have made it difficult to establish a conclusive empirical cost estimate. Existing empirical studies place the default cost anywhere between zero (see Yeyati and Panizza, 2011) and a fifth of a country's output (see De Paoli, Hoggarth, and Saporta, 2009; Furceri and Zdzienicka, 2012).[1] In other words, judging by the literature, default could either be costless, or its cost could eclipse that of most other historical crises.

In this paper, a novel econometric method is employed to provide a conclusive estimate of the sovereign default cost. We base our analysis on a comprehensive panel that spans the period of 1970 to 2010 on an annual basis. The dataset covers 114 countries and 88 external sovereign defaults. Our sample of defaulters was not selected at random, and therefore, like all preceding studies, suffers from potential concerns about endogeneity. To get around these concerns, we rebalance the sample to mimic a situation where the default decision had been random, by using the inverse propensity score weighting methodology of Angrist, Jordà, and Kuersteiner (2013) and Jordà and Taylor (2015). We then turn to local projections to estimate the cost of sovereign default. Local projections place relatively few restrictions on the data, allowing us to compute a non-linear, time-varying and state-contingent default cost estimate.[2] Combined with inverse propensity score weighting, this gives us an estimate that is "doubly-robust" to misspecification.

Our core finding is that sovereign default is costly. Default reduces GDP by 2.8% on impact, and continues to drag down output for the subsequent 10 years. During the first five years following default the cost gradually increases, peaking at 4.8% of GDP, and slowly dissipates thereafter. This result remains robust under a wide range of alternative default definitions and types of default, as well as after controlling for expectations encompassed in forward-looking variables. Another key finding is that the cost doubles if the sovereign default is followed by a systemic banking crisis. In this case GDP drops by 9.5% after the first three years alone. We show that the cost is driven by a sharp decline in investment, which is particularly stark in the case of banking-crisis defaults. Defaulting countries tend to undergo a speedy and sizeable external adjustment: they move closer to a trade surplus by increasing net exports at the same time as gross trade collapses.

How do our results map into the theory literature on sovereign risk? The default cost estimate is higher than the temporary 2% endowment penalty typically assumed in the literature (see,

[1] See Section 2 for a more detailed review of the existing literature. The Furceri and Zdzienicka (2012) estimate refers to the medium-term cost of a sovereign crisis occurring in isolation, which is larger than their baseline estimate (10% of output).

[2] These features have also made local projections attractive to the literature on fiscal multipliers (see Auerbach and Gorodnichenko, 2012; Owyang, Ramey, and Zubairy, 2013; Ramey and Zubairy, 2014).

for example Aguiar and Gopinath, 2006; Yue, 2010), but lower than the output cost attributed to the endogenous reinforcement mechanism in Mendoza and Yue (2012). The increase in net exports and the collapse in gross trade indicate that autarky costs – the key mechanism in most sovereign default models – do play an important role in explaining the cost of default. However, we also demonstrate that banking distress acts as a key amplifier of default costs. A better understanding of this second mechanism and its interaction with autarky costs would enhance both the intuitive appeal and the applicability of sovereign default models.

Our findings have important implications for the understanding of sovereign default and its aftermath. First, we show that even after endogenising the decision to default there is still a significant and persistent sovereign default cost. Second, the cost of default is substantial, but not extreme – it still lies below that of the most costly crisis events throughout history. Third, the cost of defaults that are followed by systemic banking crises is rather extreme in the historical context: for example, it is higher than that of financial recessions in Jordà, Schularick, and Taylor (2013), and systemic banking crises in Cerra and Saxena (2008). This suggests that accounting for spillovers to the banking sector is important for practical policy considerations around the time of default.

This paper is structured as follows: the next section reviews the theoretical and empirical literature on default costs. We then describe the methodology and data used in our estimation, and present our results. A final section concludes.

2 What we know about sovereign default costs

Theoretical economic models assume that sovereign default is costly. Because sovereign debt contracts are not enforceable, defaulters have to face a credible punishment in order to ensure debt repayment and facilitate sovereign borrowing in the first place. The classic analysis in Eaton and Gersovitz (1981) assumes that this punishment takes the form of a temporary exclusion from international borrowing markets, or autarky. This enforcement mechanism is sufficient to sustain sovereign borrowing in theory, but nevertheless has some troublesome implications. First, the basic model predicts that countries should default more during times of good economic performance or high productivity, because the cost of being subjected to autarky should be smaller under such conditions. As Tomz and Wright (2007) have noted however, countries tend to default in bad times. Second, the standard setting typically takes output as exogenously given. As a consequence, there is no output cost of sovereign default.

To get around these problems, theoretical models have introduced a number of modifications. These modifications generally make default more costly relative to the simple framework of autarky, where countries only lose access to borrowing and lending. Some papers – for example Aguiar and Gopinath (2006) and Yue (2010) – add a direct output cost, typically parametrised at 2% of the country's economic potential. Such a cost helps the models attain higher sustainable debt levels. To prevent countries from defaulting too frequently during good times, Arellano (2008) imposes a direct endowment penalty for defaulting when output is high. Additional amplification mechanisms can also make defaulting less attractive: for example, Mendoza and Yue (2012) show that post-default autarky can harm firms' investment and production capabilities because they would not be able to import the necessary intermediate inputs. This amplification mechanism is responsible for roughly half of the decline in output around default, or 6% GDP in their baseline calibration. Such an approach is particulary attractive because it introduces the idea of an endogenous output cost into the theoretical literature, where output is typically determined exogenously.

The empirical literature on sovereign default costs can be broadly divided into two strands: those measuring the overall cost of default and those focussing on individual transmission channels through which default affects economic activity. Studies focussing on individual channels tend to find some evidence in support of autarky. For example, Cruces and Trebesch (2013) show that markets tend to penalise defaulters via higher credit spreads or outright exclusion, and studies by Rose (2005) and Borensztein and Panizza (2010) document a negative impact of default on trade and export-oriented firms. Additionally, recent studies by Gennaioli, Martin, and Rossi (2014a) and Gennaioli, Martin, and Rossi (2014b) find that default has a negative impact on the financial system, for example by forcing banks holding government debt to cut back on lending. Still, studying one transmission channel at a time makes it difficult to say which channel is more important, and what the overall contribution of all transmission channels is.

Studies of overall default costs have tackled this problem in a number of ways, with the results summarised in Table 1. Whilst historical studies (see Reinhart and Rogoff, 2011a; Tomz and Wright, 2007) have documented a general negative correlation between default and output growth, other studies based on more recent data have attempted to disentangle the effect of sovereign default from that of other observed confounders. The range of these conditional sovereign default cost estimates, however, is extremely broad. At one end, there are the estimates of De Paoli, Hoggarth, and Saporta (2009) and Furceri and Zdzienicka (2012) who find sovereign default costs of 6% or more of a country's GDP on impact, and a permanent cost upwards of 10% GDP in the longer term. At the other end, Yeyati and Panizza (2011) who base their findings on quarterly data, find no default cost at all. Lying between these two extremes is Borensztein and Panizza (2008)'s estimate of a 2.6% GDP cost on impact.

Table 1: Existing estimates of the cost of sovereign default

Paper	Default cost, % GDP		Method
	First year	Medium term	
Historical unconditional estimates:			
Reinhart and Rogoff (2011a)	3–4%†	5%†	Average path of GDP
Tomz and Wright (2007)	1.6%	1.4%	Deviation from HP trend
Conditional estimates using more recent data:			
De Paoli, Hoggarth, and Saporta (2009)	5.5 – 10.5% †† per year		Fixed effects panel + counterfactual comparison. Defaults with high arrears only.
Furceri and Zdzienicka (2012)	5.6%	10%	Two-stage GMM panel. Also local projections. Sovereign crises only.
Borensztein and Panizza (2008)	2.6%	not sig.	Fixed effects panel + controls
Yeyati and Panizza (2011)	not sig.	not sig.	Fixed effects panel, quarterly data

† We use the estimates determined by Reinhart and Rogoff (2011a) for GDP growth after a default on external debt, and subtract a 2% annual GDP growth trend to arrive at the estimate in the table.

† † De Paoli, Hoggarth, and Saporta (2009) median cost estimates, baseline results. The average cost is higher (12 – 13% GDP).

Notes: not sig. means "not significant". All estimates are based on annual data unless otherwise specified.

What, then, are we to make of this wide variety of overall cost estimates? It is difficult to reconcile the results of different studies with each other because they rely on a number of different default definitions, estimation methods and data samples. Instead, we apply an up-to-date econometric method to a comprehensive dataset, in order to provide a more conclusive estimate of the overall default cost. By additionally exploring the impact of different default types on different GDP components, we also hope to build a bridge between the literature on overall default costs and that on the individual transmission channels.

3 Estimating sovereign default costs

3.1 Endogenising defaults

To calculate the cost of sovereign default, we need to compare two counterfactual scenarios: one where the representative country in our sample defaulted and the other where it did not. If the default decision was random – or exogenous – it would be sufficient to compare the average performance of defaulters to that of non-defaulters. But countries do not default at random. Table 1 shows that the decision to default is endogenous to a number of observable variables: for example, defaulters tend to have higher debt and lower growth, with many still recovering from another crisis – all factors that could suppress future economic performance. A simple means comparison would conflate the impact of these confounding factors with that of the default itself.

Table 2: Characteristics of treatment and control groups

	Treatment (defaulters)	Control (non-defaulters)
GDP growth	-1.67	2.33
External public debt/GDP	40.74	27.06
Inflation	14.57	7.01
Openness	52.22	71.22
Governance quality score (Polity)	-5.50	-1.00
Financial crisis probability	0.10	0.03
Currency crisis probability	0.11	0.04
War intensity (scale 0 – 10)	0.99	0.86
Coup probability	0.09	0.06

Notes: All values refer to the year preceding default, and in the case of financial and currency crisis probabilities, to two years before default. Openness is the ratio of gross imports and exports to GDP. Governance quality is scored on a scale from -10 to 10, with a higher score meaning better governance. All ratios are presented as percentage points, all growth rates in percent.
Median: GDP growth, Ext. public debt/GDP, Inflation, Openness, Polity score.
Mean: Crisis probabilities, war intensity, and coup probability.

To negotiate this problem, we need to capture the exogenous variation of default decisions. We cannot do this by means of an experiment; moreover there are no apparent historical natural experiments or plausible exogenous instruments when it comes to analysing sovereign defaults. Therefore this analysis proceeds in a different direction: we accept that the default decisions in our dataset are endogenous, but we seek to explicitly model this endogenous decision process and account for it in our estimation. Modelling the default decision allows us to effectively reverse-engineer it and rebalance the sample "as if" it were taken at random. To do this, we use the inverse propensity score weighting methodology developed by Angrist, Jordà, and Kuersteiner (2013) and Jordà and Taylor (2015), described in the following section.

3.2 Estimation procedure

The inverse propensity score weighting (IPSW) estimation proceeds in two stages. The first stage models the default decision by estimating a policy propensity score for each observation in our sample. This score is simply the likelihood of default predicted by a logit model, as follows:

$$\widehat{PD}_{i,t} = \Lambda(Z_{t-1}, \tilde{Z}_{t-1}, \tilde{Z}_{t-2}, \hat{\beta}) \tag{1}$$

Here $\widehat{PD}_{i,t}$ is the predicted default probability for country i at time t, conditional on a set of observed confounders $Z, \tilde{Z}$; some (Z) included with one lag and others $(\tilde{Z})$ with two. Λ is the logistic distribution function. Under our definition of a default, countries that are still negotiating a past default cannot default again. We therefore exclude these from the logit estimation and manually assign them a zero default probability.

The second stage rebalances the sample using inverse propensity scores, to mimic a setting where the default decision was random. When compared to a randomised sample, our group of defaulters contains too many observations with high propensity scores, i.e. those instances where countries are likely to default for endogenous reasons, such as having low growth rates or high debt levels. Weighting each observation in the group by the inverse of the propensity score – $1/\widehat{PD}_{i,t}$ obtained from the estimation in (1) – re-randomises the selection by giving a higher weight to countries which are underrepresented among defaulters, such as those with high growth and low debt. An equivalent operation is applied to non-defaulters, only now a high propensity score means that there is a high probability of not defaulting – hence the low growth, high debt observations are given more weight.

Once the sample is rebalanced, the cost of default is measured as its "average treatment effect": the average difference in potential outcomes of defaulters and non-defaulters across the sample. To compute potential outcomes $\Delta\hat{y}_{i,t+h}$, we estimate the following local projection forecast conditional on observable data, as in Jordà (2005):

$$\Delta y_{i,t+h} = \alpha_i + \theta_h \delta_{i,t} + \Gamma_{h,1} X_{i,t-1} + \tilde{\Gamma}_{h,1} \tilde{X}_{i,t-1} + \tilde{\Gamma}_{h,2} \tilde{X}_{i,t-2} + \varepsilon_{i,t} \quad h \in \{0, ..., 9\}; \tag{2}$$

Here $\Delta y_{i,t+h} = y_{i,t+h} - y_{i,t}$ is the conditional forecast of the outcome variable (such as GDP), taken in cumulative growth terms. This is computed for each observation in our sample, for a horizon of up to 10 years by running 10 separate regressions, hence $h = 0$ to 9. In each regression, α_i are country fixed effects, $\delta_{i,t}$ is the treatment variable – in our case a simple 0/1 sovereign default dummy – and $X, \tilde{X}$ are the control variables, again included up to two lags. We follow Jordà and Taylor (2015) and use a richer set of controls in the prediction stage (1) than in the local projection stage (2), hence $X \subset Z$ and $\tilde{X} \subset \tilde{Z}$. $\varepsilon_{i,t}$ is the constant-variance zero-mean error term. Standard errors are clustered by country. Accounting for correlation across time and in the cross-section as in Driscoll and Kraay (1998) would reduce the local projection standard errors and as a result the errors reported in the tables should be viewed collectively as an upper bound.[3]

We then compute the average difference in these potential outcomes for the rebalanced sample as follows:

$$ATE_h(\delta)^{\text{IPSWRA}} = \frac{1}{n_{\text{Def}}} \sum_i \sum_t \frac{\Delta \hat{y}_{i,t+h} * \delta_{i,t}}{\widehat{PD}_{i,t}} - \frac{1}{n_{\text{NoDef}}} \sum_i \sum_t \frac{\Delta \hat{y}_{i,t+h} * (1 - \delta_{i,t})}{1 - \widehat{PD}_{i,t}}. \quad (3)$$

Here, $\Delta \hat{y}_{i,t+h}$ is the forecast obtained by estimating (2), $\delta_{i,t}$ is the default dummy used to separate observations into the treatment and control groups (defaulters and non-defaulters). $\widehat{PD}_{i,t}$ and $1 - \widehat{PD}_{i,t}$ are the inverse propensity score weights used to rebalance the sample. We truncate these at 10 as recommended by Imbens (2004). $ATE_h(\delta)^{\text{IPSWRA}}$ is the average treatment effect of default, again computed for horizons of up to 10 years. Note that since in our setting the treatment is a dummy variable, the treatment effect is equivalent to the $\hat{\theta}_h$ coefficient in a local projection regression estimated on the rebalanced (IPS-weighted) sample.

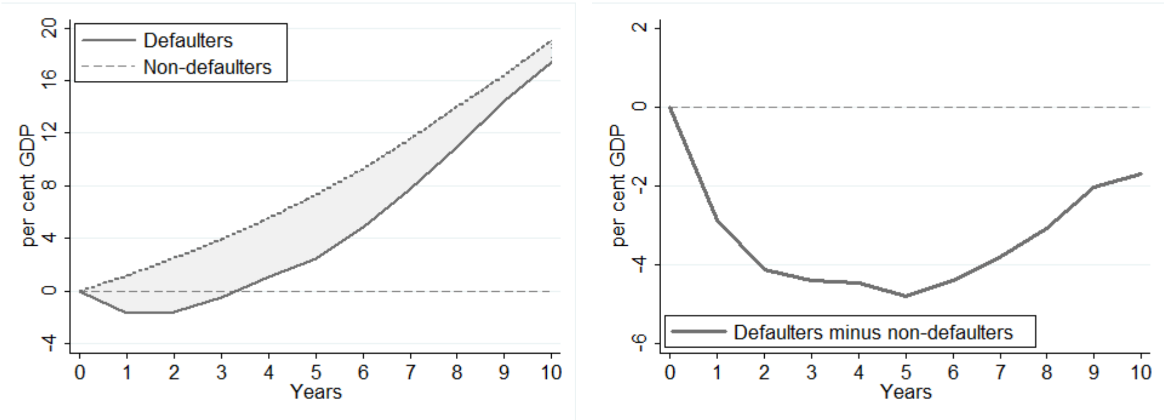
Combining the local projection methodology with inverse propensity score weighting gives us the inverse propensity score weighted regression-adjusted (IPSWRA) estimator introduced by Jordà and Taylor (2015). This estimator is "doubly-robust" to regression misspecification: it is unbiased as long as at least one of the regression stages (1) and (2) is specified correctly. We also present the results using unconditional and conditional local projections: i.e. only estimating (2) with the no-control and full-control sets.

3.3 Interpreting the estimates

In an effort to clarify what is meant by the cost of sovereign default, Figure 2 illustrates precisely how our estimate is constructed. The left panel shows the average evolution of GDP for defaulters and non-defaulters. A representative non-defaulting country is expected to grow at trend whereas if it defaults, GDP is expected to first fall and then slowly catch up.

The difference between the two expected GDP level paths is the average treatment effect – our measure of the default cost. It is the shaded area between the two curves in the left-hand panel, also plotted separately in the right-hand panel. To calculate the average treatment effect one can simply compare the cumulative difference of GDP growth rates

[3] The Driscoll and Kraay (1998) procedure is not well specified for an IPSWRA estimator. We therefore report country-clustered errors for all specifications to ease comparability.



(a) Expected GDP path for defaulters and non-defaulters

(b) Average treatment effect of default

Figure 2: Calculating the average treatment effect of sovereign default

between defaulters and non-defaulters over time. This is the figure that we present in our tables and graphs in the results section. The idea is similar to comparing the GDP growth performance of defaulters to trend, where the trend is estimated using data for the rebalanced control group. Since the cost estimate is computed for a representative country in our broad sample, it captures the "gross" cost of default – that of defaulting compared to doing nothing.

3.4 Identifying assumption

The IPSWRA methodology allows us to estimate the dynamic treatment effect of sovereign default using a highly flexible semi-parametric specification. This offers a number of advantages relative to other methods such as panel regressions and VARs. The LPs allow the year-to-year response to vary in a non-linear manner, and the propensity score weighting allows for non-linear interactions between the outcome variable, controls and the sovereign default treatment within each year. The method also allows us to compare the effects of different treatments without making any additional assumptions, by simply changing the definition of δ – to, say, a large-magnitude default.

Still, as discussed in Angrist, Jordà, and Kuersteiner (2013), the ability to causally interpret our results hinges on one crucial assumption: "selection on observables". This states that observable data actually give us enough information to model the endogenous decision-making process, and that the remaining variation in sovereign default is random. If this assumption is violated, the costs that we measure may include other shocks which could trigger both the default event and the subsequent poor output performance. Even though one can never fully eliminate this possibility when using macroeconomic data, we take several steps to ensure we come as close as possible to satisfying this assumption.

First, we use a broad range of controls and predictors in the two stages of the IPSWRA, including a large number of variables capturing the macroeconomic, financial and political state of the economy (see Section 3.5 and Appendix Table A1.1). Second, our results remain robust to including forward-looking variables, which additionally capture extra qualitative information on economic conditions: sovereign credit ratings and growth forecasts computed by the IMF. Third, the results remain unchanged under a range of alternative contemporaneous treatments such as high- and low-magnitude defaults. This suggests that our sovereign default cost estimate is largely attributable to the default decision itself rather than other confounding factors.

3.5 Putting the method into practice

To compute the default cost estimate, we require data on the treatment – sovereign default – as well as on the outcome, control and predictor variables. To best capture default costs, our treatment variable should have the following two features. First, it should be timely and refer to the start of the default episode, allowing us to capture the entirety of the cost. Second, it should be as broad as possible but remain precise and quantifiable at the same time – we therefore focus on external defaults, since domestic debt defaults are less precisely defined. The definition that fits these criteria best is provided by Standard and Poor's (2006), who define default as failure to make a principal or interest external debt payment on the due date contained in original terms of issue. We convert these default instances into a 0/1 dummy, equal 1 in the first year of the default and 0 otherwise.[4]

Furthermore we check the robustness of our baseline results to using different default definitions in Section A4-1. In some specifications, we also split the sample by augmenting the treatment variable by using other observable data – for example, by setting the dummy to equal 1 only when the default is followed by a systemic banking crisis, and zero otherwise.

We use a broad range of macroeconomic, debt, political and crisis variables as controls and predictors, all of which are listed in Appendix Table A1.1. We make use of the existing literature on predicting sovereign debt crises (see, for example Manasse and Roubini, 2009; Manasse, Roubini, and Schimmelpfennig, 2003) to select the policy score predictors and to

[4]Standard and Poor's (2006) separately record defaults on bonds and bank debt. If the country is already in default on one of these instruments, we do not treat the default on the other instrument as an additional default.

add a set of control variables typically used in the international macroeconomics literature. Altogether, our dataset is an unbalanced panel of 114 countries that spans the period of 1970 to 2010 on an annual basis and contains 88 sovereign defaults (see Appendix Table A1.2 for the full list of defaults). Note that because we estimate the IPSWRA treatment effect for a horizon of 10 years, we can only include defaults occurring before 2002 in the regression. Shortening the horizon and including more defaults does not, however, materially change any of our results.

4 The cost of default

Sovereigns have the power to decide whether to default or honour their debt obligations. In order to make this decision wisely, they need to know what happens in the event of a default – and more crucially, the associated economic cost. However, this cost is not precisely determined by the law, meaning that policymakers have to take an educated guess as to how damaging the resulting retaliation from the country's creditors, and any collateral damage to the economy may be. This makes it difficult to make well-informed policy decisions, and to model the decision-making process. In the following three sections, we seek to provide an up-to-date consolidated estimate of what this cost may be, and how exactly it might come about.

We start by establishing a credible baseline estimate of the overall sovereign default cost. This means taking the endogeneity of the default decision seriously, which we do using the IPSWRA methodology of Angrist, Jordà, and Kuersteiner (2013) and Jordà and Taylor (2015). We then further complement this by conducting a wide range of robustness checks using forward-looking predictor variables and varying default "treatment" definitions. We also recognise that the default decision is often not made in isolation – for example, the defaulting country may also be concurrently undergoing a banking crisis or be in political turmoil – both of which may be costly. To see whether this is the case, we also analyse how the cost of default varies with the concurrence of other crisis events. Finally, we decompose the cost estimates into individual components of GDP – consumption, investment, government spending and net exports – to uncover some of the underlying drivers that determine the cost of defaulting.

4.1 Baseline estimates

Our baseline default cost estimates are presented in Table 3. The figures refer to the cumulative treatment effect of sovereign default on GDP growth over a horizon of up to 10 years, for three different specifications. The treatment effect for each horizon is the cumulative average difference in per capita GDP growth performance between defaulters and non-defaulters up to that year.[5]

Table 3: Local projections: impact of sovereign default on GDP

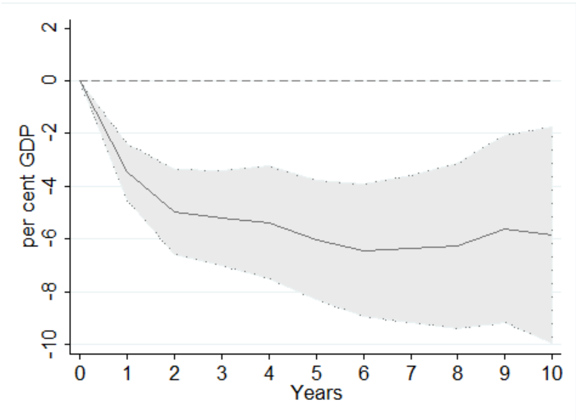
Year	1	2	3	4	5	6	7	8	9	10
Unconditional	-3.44***	-4.96***	-5.21***	-5.37***	-6.03***	-6.43***	-6.38***	-6.26***	-5.61***	-5.84***
LP	(0.65)	(0.98)	(1.10)	(1.30)	(1.37)	(1.53)	(1.70)	(1.91)	(2.17)	(2.50)
Conditional	-3.00***	-4.38***	-4.42***	-4.38***	-5.00***	-5.11***	-4.49***	-4.00**	-3.08	-2.85
LP	(0.60)	(0.95)	(1.06)	(1.20)	(1.26)	(1.41)	(1.54)	(1.81)	(2.07)	(2.33)
IPSWRA	-2.85***	-4.11***	-4.41***	-4.45***	-4.80***	-4.39***	-3.82*	-3.07	-2.03	-1.71
LP	(0.63)	(1.09)	(1.31)	(1.53)	(1.69)	(1.86)	(2.06)	(2.37)	(2.65)	(3.01)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	88	88	88	88	88	88	88	88	88	88

Notes: Expected differences in cumulative real GDP per capita growth between defaulters and non-defaulters. Clustered standard errors in parentheses. Unconditional specification controls for country fixed effects only. Conditional and IPSWRA specifications control for country fixed effects and the full list of variables in Table A1.1.
*, **, ***: Significant at 10%, 5% and 1% levels respectively

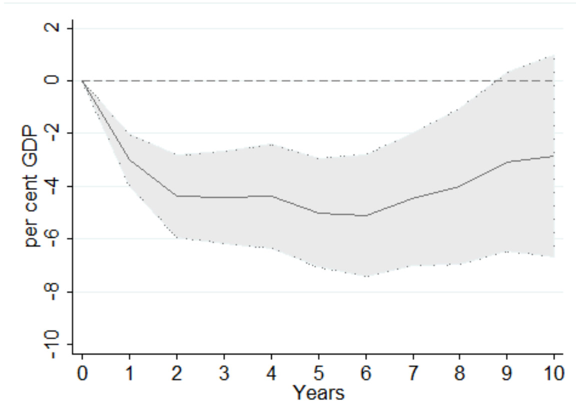
The top row is a simple unconditional local projection that only uses country fixed effects, and is akin to comparing the raw growth averages of defaulters and non-defaulters. In contrast, the middle row – conditional LP – compares conditional local projection forecasts (see Appendix Table A1.1 for the list of controls). The bottom row is our preferred IPSWRA specification, which compares conditional forecasts for the sample that is rebalanced using inverse propensity scores (see Section 3 for a more detailed description of the methodology). Figure 3 depicts the results graphically. In short, the top two panels show the unconditional and conditional LPs respectively, with the IPSWRA LP depicted in the bottom-left panel.

Our findings suggest that sovereign default is costly: the impact of default on GDP is negative, significant and highly persistent under all three specifications. Under the unconditional specification (Table 3 top row; Figure 3(a)), real GDP is 3.4% lower in the year of default, and around 6% lower in medium to long term, when compared to the counterfactual of not defaulting. The 3.4% first-year cost is roughly the same as the historical estimates calculated

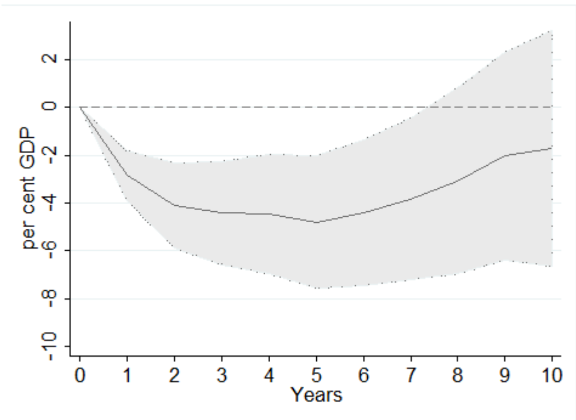
[5]For example, for Year 3 the impact is the difference in growth performance between the two counterfactual scenarios of "default" and "no default" over years 1, 2 and 3 combined. See Section 3.3 and Figure 2 for a further explanation.



(a) Unconditional LP



(b) Conditional LP



(c) IPSWRA LP

Figure 3: Impact of sovereign default on GDP

Notes: Cumulative treatment effect, GDP per capita growth. Shaded bands indicate 90% confidence intervals.

by Reinhart and Rogoff (2011a), whilst the medium-term cost estimates are similar to those found by Furceri and Zdzienicka (2012). Controlling for observed confounders in a local projection (Table 3 middle row; Figure 3(b)) does moderately lower the cost estimate, to 3% of GDP in the first year, and 5.1% of GDP at its peak, after 6 years. There is also a stronger recovery towards the end of the horizon, with GDP still 3 percentage points (ppts) below the control group's growth trend after 10 years.

As discussed in Section 3, the IPSWRA estimator is computed in two stages. The first stage estimates the policy propensity score – the likelihood of default – using a logit model with a rich set of predictors (see Appendix Table A1.1 for a complete list). The logit regression table and a ROC curve, which measures prediction quality, are shown in the Appendix Table A2.1 and Figure A2.1.[6] Most predictors enter with the expected sign – for instance, higher

[6]ROC is the "receiver operating characteristic", which compares the performance of our prediction relative to a "naive" estimate of randomly assigning observations into defaulters and non-defaulters (based on the

debt interest payments, low output growth and higher global interest rates all increase the likelihood of a future default. The logit model has a ROC of 0.88 which is indicative of high predictive power.

The second stage (Figure 3(c)) is effectively a weighted-least-squares regression, with the weights derived from inverted probabilities of treatment and non-treatment (see Section 3). The controls in the second stage generally enter with the expected sign (see Appendix Table A2.2) – for example, countries with high public debt, experiencing low growth or recovering from another crisis all have a lower predicted future growth rate, as well as a higher predicted default probability in the first stage. Correcting for this endogenous selection further attenuates our estimate of the default cost.

Our preferred IPSWRA default cost estimate reaches 2.8% GDP in the first year, gradually increases to peak at 4.8% GDP after 5 years, and – again slowly – recovers to around 1.7% GDP after 10 years. Compared to the unconditional LP estimate, the two paths differ markedly at long horizons, but are somewhat closer at short horizons. Since the unconditional and the conditional specifications are nested within the same regression, we can perform a test for the difference in LP paths. To do this, we combine the covariance matrices of our individual regression estimates using the "sandwich" estimator, and test for the differences in coefficients using these variance estimates. The two paths are statistically different over the combined horizon of 1 to 10 years, with the difference significant at the 10% level. Looking at individual horizons, the paths are statistically different in years 1, 7, 8, 9 and 10, all significant at 5% level. Since the IPSWRA path lies even further away from the unconditional LP path, these estimates are a lower bound for the differences between the IPSWRA and unconditional paths.

Based on this, we can conclude the following: controlling for endogenous selection matters. It seems to matter more for longer horizons, most likely because the sovereign default itself becomes more predictable. However, before we can evaluate our cost estimate in comparison to the rest of the theoretical literature, we want to ensure that it captures the random component of the default decision, and is not driven by other factors correlated with economic performance.

mean default probability), which achieves a ROC of 0.5. See Schularick and Taylor (2012) for a description of the methodology.

4.2 Dealing with endogeneity

The IPSWRA estimation models the endogenous default decision process, but to do this, it relies on past observable information. This means that agents' expectations and unobserved variables are not taken into account, and if some of these factors influence the default decision, the baseline IPSWRA estimate may be biased. To capture these forward-looking and unobservable factors, we have added two variables to our control and predictor set: sovereign credit ratings and GDP growth forecasts.

We use the country credit ratings provided by the *Institutional Investor Magazine* – both the absolute rating on a scale of 0 to 100 and the rating changes. This gives us the broadest possible data coverage compared to credit ratings provided by other agencies. Despite this, the resulting sample is smaller than that used for our baseline estimates. For some regression specifications we therefore complement the observed ratings with those implied by observable data in our data set by using the credit rating determinants identified by Cantor and Packer (1996). This allows for a better comparison of the ratings-based regressions with our baseline cost estimates. Further detail on the construction of the rating variable is provided in Appendix Section A3. For GDP growth forecasts, we use the dataset provided in the IMF's Historical WEO Forecasts Database. The IMF's individual country units made these forecasts over 5 years, starting in 1990.

Table 4 shows the results for three different regression specifications. Panel (a) considers the inclusion of credit ratings. Panel (b) includes forecasts and ratings – our strongest test – and panel (c) includes not only observed but also "synthetic" ratings. For each panel, we compare the specification with the baseline predictors and controls – top row – to a specification using additional ratings or forecast data.

For all three panels, the default costs computed using these two alternative specifications are very similar. This suggests that the set of observed confounders included in our baseline specification already captures much of the information that drives the default decision, and there is little to be gained by including additional forward-looking variables. When we combine the information obtained from observed ratings with that already contained in our baseline (used to predict the "synthetic" rating), we obtain a default cost estimate that is very close to baseline, shown in panel (c) bottom row. We only observe a slightly stronger recovery towards the end of the forecast horizon, which further reinforces our baseline results.

The results obtained by including rating and forecast information further support our claim that our default cost estimate is relatively uncontaminated by biases from the endogenous

Table 4: Controlling for sovereign credit ratings and growth expectations

Year	1	2	3	4	5	6	7	8	9	10
(a) Sample using ratings and rating changes:										
No Ratings	-2.69***	-3.36**	-2.38	-1.57	-1.84	-1.18	-1.38	-1.33	-0.65	-0.82
	(0.82)	(1.51)	(1.76)	(1.98)	(2.08)	(2.16)	(2.31)	(2.48)	(2.55)	(2.73)
Ratings	-2.74***	-3.52**	-2.63	-1.84	-2.17	-1.57	-1.83	-1.79	-0.94	-0.95
	(0.84)	(1.52)	(1.72)	(1.89)	(1.91)	(1.99)	(2.14)	(2.32)	(2.41)	(2.60)
Observations	1404	1404	1404	1404	1404	1404	1404	1404	1404	1404
Defaults	56	56	56	56	56	56	56	56	56	56
(b) Sample using ratings, rating changes and growth forecasts:										
No Ratings and no Forecasts	-4.75***	-5.18*	-3.84	-3.13	-2.53	-1.89	0.33	0.59	1.54	4.86
	(1.46)	(2.67)	(2.92)	(3.24)	(3.22)	(3.51)	(3.47)	(3.69)	(3.32)	(3.60)
Ratings and Forecasts	-5.09***	-5.55**	-4.42	-3.97	-3.50	-3.00	-0.79	-0.51	0.43	3.42
	(1.53)	(2.82)	(3.12)	(3.44)	(3.46)	(3.77)	(3.69)	(3.93)	(3.46)	(3.60)
Observations	837	837	837	837	837	837	837	837	837	837
Defaults	15	15	15	15	15	15	15	15	15	15
(c) Sample using synthetic ratings and synthetic rating changes:										
No Ratings	-2.84***	-3.97***	-4.22***	-4.12***	-4.47***	-4.10**	-3.51*	-2.75	-1.74	-1.45
	(0.64)	(1.09)	(1.32)	(1.52)	(1.70)	(1.86)	(2.07)	(2.38)	(2.65)	(3.00)
Ratings	-2.82***	-3.94***	-4.21***	-4.07***	-4.38***	-4.04**	-3.42*	-2.63	-1.50	-0.94
	(0.65)	(1.09)	(1.30)	(1.51)	(1.64)	(1.81)	(2.04)	(2.35)	(2.65)	(3.02)
Observations	2482	2482	2482	2482	2482	2482	2482	2482	2482	2482
Defaults	88	88	88	88	88	88	88	88	88	88

Notes: Expected differences in cumulative real GDP per capita growth between defaulters and non-defaulters. IPSWRA estimates using country fixed effects. Clustered standard errors in parentheses.
*, **, ***: Significant at 10%, 5% and 1% levels respectively.
Panels (a) and (c) include the full list of control variables from Table A1.1. Panel (b) does not include the second lag of the financial crisis dummy in the prediction stange. The "Ratings" specifications in each panel also include credit ratings and changes of credit ratings in both stages of the IPSWRA. Panels (a) and (b) use the actual ratings data provided by the *Institutional Investor Magazine*. Panel (b) additionally includes growth forecasts for the next 5 years from the Historical WEO Forecasts Database. Panel (c) uses synthetic ratings, constructed in accordance with Cantor and Packer (1996).

selection of defaulters. However, there are still a number of other potential issues, pertaining to definitions and various arguments made in the theoretical literature, which could make our estimate unreliable or uninformative. We briefly review these in the next section, with further details provided in the Appendix.

4.3 Other considerations

Our baseline estimate relies on the S & P default definition, and does not consider the effects of different types of default. This means that first, our results could be driven by the definition we use, and second, that the estimate may not be representative. The cost may be driven by a small subset of costly defaults – for example, those that have high magnitude or those that happen when the country is already experiencing substandard economic performance. We examine each of these concerns in turn to check whether our estimate provides a sufficiently representative and accurate picture of the default cost.

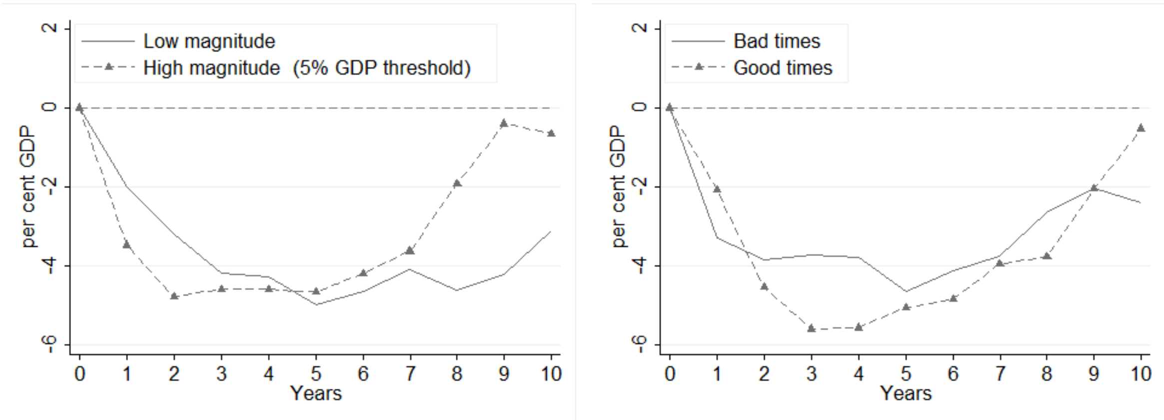
Default definition

We examine the cost of default using a number of alternative definitions. All of these consider defaults on external debt, but whilst some definitions group subsequent defaults together (Beim and Calomiris, 2000), others define default as high external debt arrears (Detragiache and Spilimbergo, 2001) or as sovereign debt crises (Laeven and Valencia, 2012). These are generally stricter than our baseline S & P definition. Detailed description of the definitions and their corresponding results are shown in the Appendix Section A4-1.

According to all definitions, sovereign default is costly and the cost is persistent. Similarly to our baseline results, the cost is attenuated by properly conditioning on observed confounders: the cost estimate under the IPSWRA specification is always lower than that for the unconditional specification. The difference is around 0.5 – 1 ppts in the initial year, generally rising to 2 – 4 ppts by the end of the horizon, depending on the definition used. Adopting the alternative definitions – which are generally stricter – does, however lead to somewhat larger costs and a higher persistence. We argue that a large part of this additional cost comes about from these alternative definitions endogenously selecting the more costly defaults, which is something that we want to avoid. However it could also be because our preferred S & P definition includes events that are not really true defaults per se, but are merely small technical repayment delays that have no real impact on sovereign debtors or creditors. To see whether this is the case, we examine how the cost of default changes with the default magnitude.

Magnitude

We measure magnitude as the amount of total sovereign debt in default during the first year of default, as recorded in the Bank of Canada CRAG database (2015). We scale this by nominal GDP. Figure 4(a) shows the IPSWRA cost estimate for two types of default: those where debt in default is below 5% GDP in the year of default (low-magnitude, solid line), and those where debt in default is above 5% GDP (high-magnitude, dashed line). It turns out that low-magnitude defaults are still costly, and the estimate remains statistically significant. Furthermore, the cost of even the highest-magnitude defaults is not much higher than that in our baseline scenario, or that of low-magnitude defaults.[7] This leads us to conclude that our main result is not driven by a subsample of high-cost, high-magnitude defaults. However, the cost could still be driven by a small subsample of countries with bad economic fundamentals, both before and after default.



(a) Default magnitude

(b) Defaulting in good and bad times

Figure 4: Impact of default magnitude and economic situation on the cost of default

Notes: Cumulative treatment effect, GDP per capita growth. Left panel shows the cost of high-magnitude compared to low-magnitude defaults. Default is classified as high-magnitude if debt in default exceeds 5% GDP in the year of default. Right panel shows the cost of defaulting during times of good, or bad economic performance. Good economic performance means that GDP growth is on average above trend during the three years preceding default.

Defaulting in good and bad times

We check whether the cost of default depends on the country's economic fundamentals. To this end, we compare the cost of default for countries growing above and below their HP-filtered trend – what we label "good" and "bad" times. Figure 4(b) compares the IPSWRA cost estimates for bad- and good-time defaults (solid and dashed lines respectively), which are classified based on growth in the three years preceding default. We find that defaulting during good times is still costly, which further allays the potential concerns about endogeneity discussed earlier. We also find that defaulting during good times is not anymore costly than

[7] The difference is both small and not statistically significant. Further details on the results are provided in Appendix Section A4-2.

defaulting during bad times, contrary to what is assumed in a number of theoretical models of sovereign default (see the discussion in Section 2). Appendix Section A4-3 provides further details.

Alternative regression specifications

In Appendix Section A4-4, we further show that our results are robust to different specifications of the method, such as using a different IPW truncation threshold or excluding countries still negotiating a past default from the control group.

4.4 Implications

Having conducted a number of robustness checks, we can be confident with our finding that there is a significant and persistent sovereign default cost. Our cost estimate is larger than the zero cost found by Yeyati and Panizza (2011), as well as the estimates of Borensztein and Panizza (2008). Still, after controlling for endogenous selection into default, the cost turns out to be lower than that found by Furceri and Zdzienicka (2012) and De Paoli, Hoggarth, and Saporta (2009). Nevertheless, it is high enough to satisfy the assumptions made in most theoretical models. Indeed, it is higher than the 2% endowment penalty typically assumed in the literature (see, for example Aguiar and Gopinath, 2006; Yue, 2010). It is smaller than the 6% output cost attributed to the endogenous reinforcement mechanism in Mendoza and Yue (2012), but those authors' calibration is based on the Argentinian default of 2001, which is more severe than the representative default in our sample.

The cost of sovereign default is comparable to that of other crisis events studied in the empirical macroeconomic literature. It is higher than that of a "normal" recession in industrialised countries, but below that of a recession accompanied by a systemic banking crisis based on the findings of Jordà, Schularick, and Taylor (2013). The fallout from sovereign default is also equivalent to a substantial amount of austerity, of around $4 - 6\%$ GDP, based on the estimates in Jordà and Taylor (2015). Focussing more on emerging market crises, the cost of default is similar to that of a currency crisis found by Cerra and Saxena (2008), but below that of a systemic banking crisis.

This suggests that the cost of default is substantial, but lies below that of the most costly crisis events throughout history (for example, systemic banking crises). It is however the case that a sovereign default is frequently accompanied by other crises, including systemic banking distress. The next section examines how the default cost changes depending on such other contemporaneous crisis events.

5 Defaults and other crises

Existing research shows that sovereign defaults frequently coincide with financial and currency crises (see, for example Reinhart and Rogoff, 2011b). The inherently political nature of sovereign decisions means that they also often coincide with political crises such as coups and wars (see Appendix Table A1.3). To again use the 2015 Greek crisis as an example, Alexis Tispras' government was facing not just the danger of default, but three other significant risks. First, the Greek banking system was highly vulnerable and heavily reliant on the central bank (and ECB) for support. Second, a default would raise the prospects of a severe currency crisis, with Greece likely being forced out of the euro altogether. And third, the prolonged economic slump was accompanied by a tense political climate, frequent street protests and a general disillusionment with the mainstream political parties. In these types of situations it is reasonable to expect that the costs of sovereign default may be different, indeed higher than in our baseline scenario, which would have a bearing on policymakers' decisions, and potentially on the way economists model them. We investigate this hypothesis further in the following section.

5.1 Defaults and systemic banking crises

The recent Eurozone sovereign crisis has reminded us of the dangerous links between the health of the sovereign and the banking sector (see, for example Bocola, 2015; Gennaioli, Martin, and Rossi, 2014a, 2014b; Jordà, Schularick, and Taylor, 2015; Reinhart and Rogoff, 2011b). Banking crises often require bailouts and activate automatic fiscal stabilisers, increasing the pressure on government finances. Further, sovereign debt write-offs harm banks, and if sovereign finances are stretched, they may not be able to act as an effective "backstop" for the banking sector. Finally, if the sovereign default is punished by autarky, as many theoretical papers suggest, domestic banks relying on foreign funding may struggle to finance their operations.

In light of this, we pose the following question: is sovereign default more costly when it leads to a systemic banking crisis? Consequently, we study defaults occurring around the time of systemic banking crises, as identified by Laeven and Valencia (2012). We try to isolate the cases where the sovereign default precedes or leads to a banking crisis, or at the very least where the default is not caused by problems in the banking sector. As a first step, we include all defaults where the banking crisis occurred in the year after, or two years after the default. Second, we add the defaults that occurred in the same year as the banking crisis,

Table 5: Sovereign default and systemic banking crises

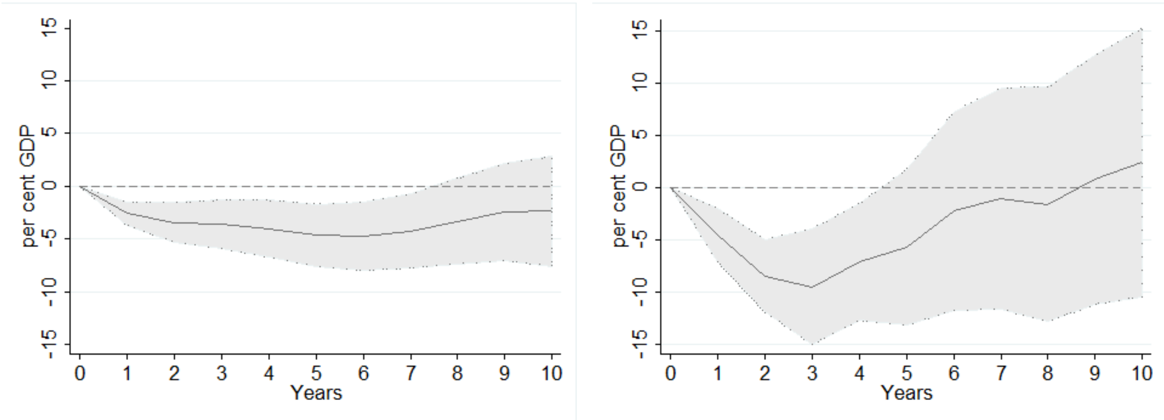
Year	1	2	3	4	5	6	7	8	9	10
(a) Banking crises occurring up to 2 years after the default										
Default + no Crisis	-2.58***	-3.42***	-3.61***	-4.03***	-4.65***	-4.72***	-4.26**	-3.29	-2.47	-2.35
(no. defaults = 78)	(0.66)	(1.14)	(1.40)	(1.65)	(1.80)	(1.98)	(2.13)	(2.47)	(2.81)	(3.17)
Default + Crisis	-4.56***	-8.48***	-9.53***	-7.15**	-5.76	-2.29	-1.08	-1.65	0.73	2.38
(no. defaults = 10)	(1.54)	(2.11)	(3.37)	(3.41)	(4.54)	(5.79)	(6.47)	(6.85)	(7.27)	(7.83)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
crisis = no crisis	0.22	0.03	0.12	0.41	0.82	0.69	0.64	0.82	0.68	0.57
(b) Banking crises occurring within ± 1 year of the default										
Default + no Crisis	-2.39***	-3.12***	-3.75***	-4.10***	-4.60***	-4.51**	-4.23*	-3.35	-2.44	-2.47
(no. defaults = 73)	(0.63)	(1.13)	(1.41)	(1.68)	(1.84)	(2.00)	(2.20)	(2.55)	(2.90)	(3.26)
Default + Crisis	-5.22***	-9.21***	-7.80***	-6.28*	-5.83	-3.75	-1.74	-1.62	0.04	2.20
(no. defaults = 15)	(1.69)	(2.18)	(3.05)	(3.35)	(4.07)	(4.95)	(5.43)	(5.76)	(6.07)	(6.54)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
crisis = no crisis	0.11	0.01	0.23	0.56	0.78	0.89	0.67	0.78	0.71	0.51

Notes: Expected differences in cumulative real GDP per capita growth for the relevant default treatment. Treatments are based on a simple sample split of our baseline default definition. All figures are IPSWRA estimates controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Panel (a) classifies a Default + Crisis event as a default, followed by a banking crisis in the same year, or the next two years. Banking crises occurring prior to default, even within the same year, are excluded. Panel (b) considers all banking crises occurring in the year of default, the year before, or the year after the default.
*, **, ***: Significant at 10%, 5% and 1% levels respectively.

but where the default was not preceded or caused by severe problems in the banking sector. Naturally, this includes cases – such as the Russian default of 1998 – where problems in the sovereign sector were the main cause of the systemic banking crisis, either creating large write-offs or causing the banks' funding sources to dry up. It also includes cases – such as the Argentinian default in 2001 – where the default and the banking panic occurred more or less simultaneously, but where the sovereign crisis was the underlying cause of the banking crisis. Finally, we include those episodes – such as Niger's default in 1983 – where the two crises occurred simultaneously, but may have both been triggered by a third unrelated event (in the case of Niger, a drop in the price of uranium, the country's key export).

It turns out that for most of the contemporaneous default banking-crisis events in our sample, the sovereign crisis was not caused or preceded by a banking crisis.[8] Presumably this is because our sample consists mainly of emerging markets with relatively underdeveloped finan-

[8] We exclude two joint events – Ecuador 1982 and Indonesia 1983 – where the banking sector problems predated default, and keep 7 other joint events.



(a) Default and no systemic banking crisis

(b) Default and systemic banking crisis

Figure 5: Treatment effect of sovereign default and systemic banking crises

Notes: Cumulative treatment effect, GDP per capita growth. Shaded bands indicate 90% confidence intervals. Sample split based on a one-year window.

cial systems. In light of this, and because the precise timing and causality of the sovereign-banking-crisis events is difficult to verify in any case, we also present our results using a broader joint event definition. For this, we simply require that the two crises occur within a year of each other; i.e. we include those events where a banking crisis started in the year before, the same year, or the year after the default.

Table 5 presents our findings. It is split into two panels: the top panel (a) presents the results using our preferred sequencing definition where the banking crisis has to follow sovereign default, whilst panel (b) double-checks the result using the broader one-year joint event window. For each joint event definition we split our defaulter sample into two groups: those where the default is followed by (or, in panel (b), broadly coincides with) a banking crisis, and those where it is not. Figure 5 presents the results in panel (a) graphically. All estimates use our preferred IPSWRA specification.

One key result stands out: sovereign defaults are significantly more costly when followed by a systemic banking crisis. Focussing on panel (a) of Table 5, the cost of default not followed by a banking crisis (top row) is very similar to our baseline results: 2.6% GDP in the first year, 4.7% GDP at the peak in year 6, and around 2.4% GDP at the end of the horizon. The cost of default followed by a banking crisis is, however, substantially higher: 4.6% in the first year, peaking at 9.5% in year 3 and persisting for several more years. A similar picture emerges when looking at the broader one-year-window joint event definition in panel (b).

The dynamic impulse response depicted in Figure 5(b) paints a picture of a crisis spiraling out of control, with costs increasing rapidly from year to year. The cost of sovereign-banking crises is higher than not only that of sovereign default (taken from our baseline estimates), but also that of most other crisis events examined in the literature: for example, financial recessions in Jordà, Schularick, and Taylor (2013), and systemic banking crises in Cerra and Saxena (2008).

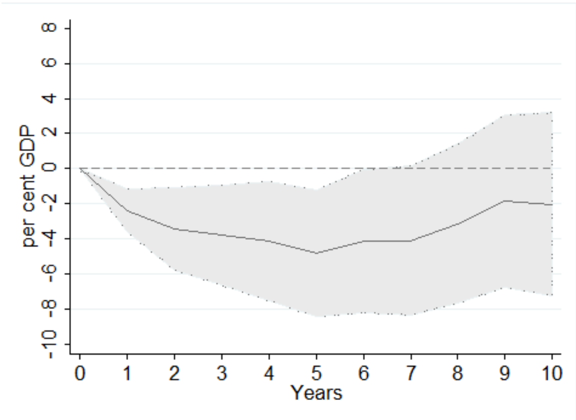
This analysis makes it clear that the policymakers would be right to worry about the potential impact of default on the domestic banking system. But should they also be concerned about the potential currency crisis, and the economic costs of any political fallout from default?

5.2 Currency and political crises

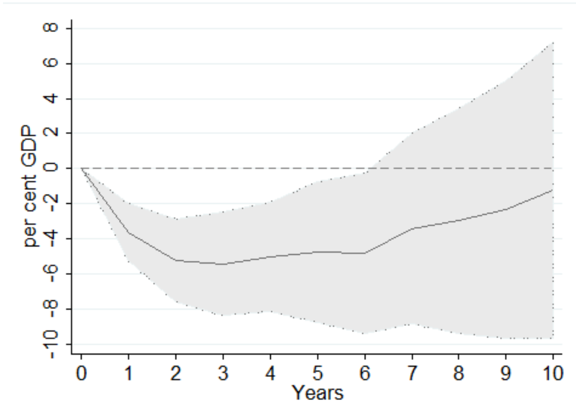
Both currency and political crises represent significant risks during the time of sovereign default. Since emerging-market sovereigns tend to denominate their external debt in foreign currency, a sharp devaluation may make the debt unsustainable. Equivalently, a sovereign default may reduce the confidence in the currency, triggering a self-fulfilling currency panic. Indeed, the strong link between sovereign default and currency crises has been well-documented in the existing literature (see for example, De Paoli, Hoggarth, and Saporta, 2009; Kaminsky, 2006; Reinhart and Rogoff, 2011b). The political environment around the time of default has not been studied as systematically, but it should not come as a surprise that defaults often coincide with times of political turmoil, as documented in the Appendix Table A1.3.

In light of these facts, we examine how the cost of default changes if the default coincides, is preceded or followed by a political or currency crisis (i.e. using a one-year joint event window). We follow the Laeven and Valencia (2012) definition of a currency crisis, and define a political crisis as a high-intensity war, a coup or a political transition. We consider the effects of currency crises and political crises separately, as represented in Figure 6. The two top panels consider the impact of defaults around currency crises, and the bottom two – around political crises. As with the systemic banking crises, the panels on the left ((a) and (c)) represent the no-crisis scenario, whilst the panels on the right ((b) and (d)) depict the impact of the joint default-crisis events. The tables with the underlying numbers are provided in the Appendix (Tables A5.1 and A5.2).

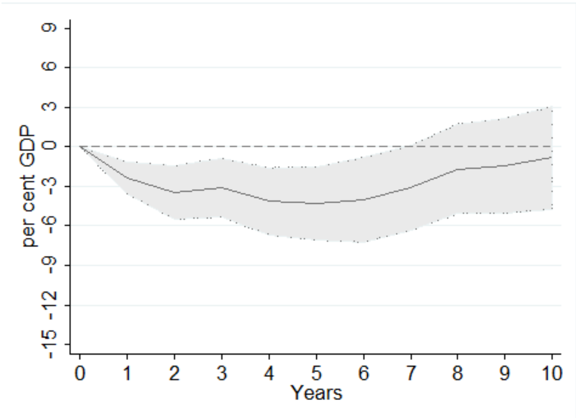
As with banking crises, the no-crisis results are similar to our baseline estimates. However unlike for banking crises, the costs of joint crisis events (Figure 6 panels (b) and (d)) are similar to those of "standalone" sovereign defaults (Figure 6 panels (a) and (c)), and far below those of the joint sovereign-banking crisis events (Figure 5(b)). Whilst – as expected – the cost of default rises slightly if it coincides with a political or currency crisis, the differences



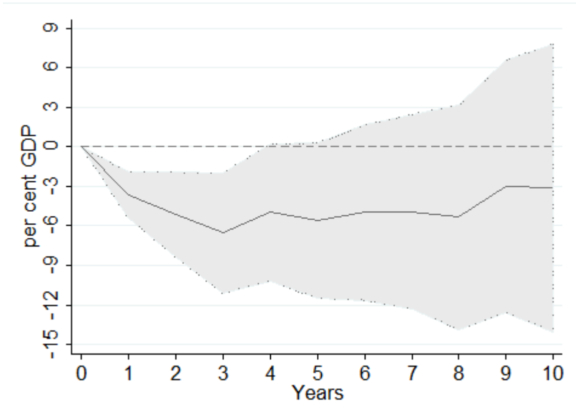
(a) Default and no currency crisis



(b) Default and currency crisis



(c) Default and no political crisis



(d) Default and political crisis

Figure 6: Treatment effect of sovereign default around currency and political crises

Notes: Cumulative treatment effect, GDP per capita growth. Shaded bands indicate 90% confidence intervals. Sample split based on a one-year window.

between the crisis and no-crisis scenarios are not large (mostly 1 – 2 percent GDP) and are not statistically significant.

Overall, the state-contingent effects of sovereign default can be summarised in one simple sentence: to paraphrase Bill Clinton's famous slogan, "it's the banks, stupid". It turns out that the costs of sovereign default, even though substantial, can be reasonably contained as long as the banking system remains operational – even if the country is experiencing a currency or a political crisis. Should the banks fail, however, the defaulting country ought to brace itself for a severe economic downturn.

Such a conclusion may seem simplistic, but it nevertheless provides some guidance for policymakers and economists. Even though there are many potential factors that could affect the decision to default and the associated economic costs, our findings suggest that one can at least start by focussing on the implications for the country's banking sector, be it in terms of write-offs or acute funding shortages. Still, when it comes to modelling the default event, our results have so far only provided limited insights into precisely why sovereign defaults are costly, and why those followed by systemic banking crises are costlier still. We aim to shed more light on this in the next section.

6 Decomposing the cost

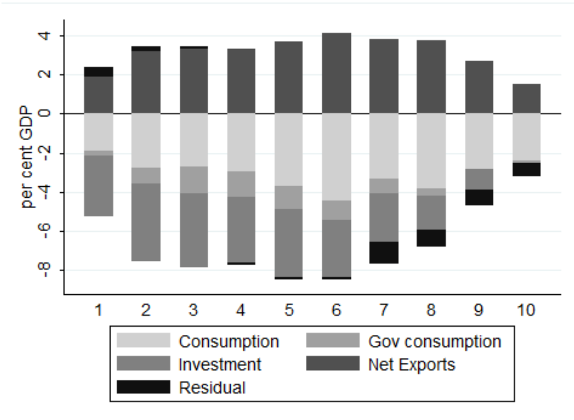
As discussed in Section 2, sovereign default can adversely impact the economy through a number of channels, including autarky and collateral damage to the banking sector. In turn, there are numerous ways of modelling these individual costs. In this section we decompose our aggregate cost estimate into individual components of GDP: consumption, investment, government consumption, exports and imports. We then use this disaggregated picture of the default cost to analyse the evidence for the existence of these various cost channels. For example, financial autarky tends to be closely linked with an increase in net exports to make up for the shortage of external borrowing, and damage to the banking sector is typically accompanied by a reduction in investment.

6.1 Baseline estimates

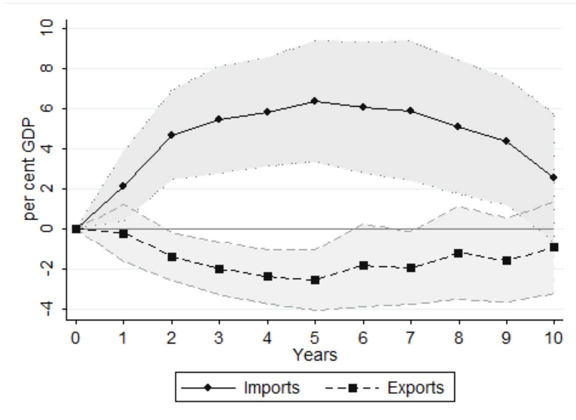
We start by analysing the default cost components for our baseline IPSWRA specification, depicted graphically in Figure 7(a). The individual bars show the contribution of each component to the total GDP treatment effect, which can be either positive (bar above zero line) or negative (bar below zero line). For example, the cumulative treatment effect in Year 1 is around –2.8% of GDP. Of that, investment contributes –3.1% of GDP, consumption –1.9% of GDP and government spending –0.3% of GDP (all shown by negative bars). In contrast net exports exert a positive contribution of +1.9% GDP.[9] Table 6 lists the actual figures underlying Figure 7. In order to interpret these figures one has to take into account the share of each component in GDP (Table A1.4). In other words, those components with the largest shares are also expected to make the largest contributions to GDP.

The fall in GDP in the aftermath of default is driven by lower domestic consumption and investment. The fall in consumption is relatively slow-paced, with its large magnitude most likely reflecting the dominant share of this component in GDP – almost 70% – rather than any specific cost transmission channel. The drop in investment, however, is sharper and

[9] The sum of all components will not exactly equal the total GDP treatment effect due to a small residual (dark bar), in the case of Year 1 roughly 0.5% of GDP



(a) Decomposition of the treatment effect



(b) Exports and Imports

Figure 7: The effect of default on the components of GDP

Notes: Cumulative contribution of individual components to GDP after a sovereign default. Calculated as the absolute change in a GDP component between t and t + h, scaled by the GDP level at t. Here t is the year before default, and h is the horizon, plotted on the x-axis. Shaded bands indicate 90% confidence intervals.

rather stark given its relatively modest GDP share of 19%. Investment falls by 3.1% of GDP in the initial year and by 4% of GDP at its peak in the second year – or around one-fifth in relative terms. In contrast, government consumption only falls by a small amount and at a rather gradual pace.

So why does investment fall so sharply? Reasons may include poor economic prospects, but also difficult borrowing conditions for firms, for example due to banking sector distress. The findings of Gennaioli, Martin, and Rossi (2014a, 2014b) seem to corroborate this with evidence that default has a negative impact on bank credit, more so for those banks with large sovereign debt exposures. Another reason why borrowing for investment may be difficult is financial autarky, which would restrict firms' foreign borrowing. The sharp increase in net exports evident in Figure 7(a) and Table 6 suggests that indeed, the domestic economy is moving closer to a current account surplus.

The overall increase in net exports, however, masks the differential picture in gross exports and imports of goods, depicted in Figure 7(b). In line with findings in the empirical literature, sovereign default appears to harm exporters (see Borensztein and Panizza, 2010; Rose, 2005), and the gross volume of exports falls. To maintain the positive net exports contribution then requires a sharp reduction in imports, which amounts to 2.1% of GDP initially and reaches 6.4% of GDP at peak. Similarly to domestic investment, this represents a drop of around one-fifth in relative terms.[10] Even though the drop in imports contributes positively to GDP (Figure 7(b), solid line), it can also be seen as the flipside of the drop in the domestic GDP

[10] 6% GDP scaled by the share of imports in GDP on the eve of default, 30%

components, which decline because of the lower consumption of imported goods, and lower imports of intermediary investment products. The sharp drop in gross trade is indicative of trade autarky, perhaps related to reputational costs or inability of firms to get trade credit.[11]

What does this reveal about the underlying mechanisms driving the cost of default? First of all, we find evidence supporting the notion that the country enters financial autarky, either as a form of punishment from creditors or a reputation as a less creditworthy borrower. This forms the backbone of the sovereign default cost mechanism in most theoretical models. However, the standard financial autarky model à la Eaton and Gersovitz (1981), in which households are simply prevented from smoothing their consumption, does not explain the drops in output, investment and gross trade that we observe in the data.

Mendoza and Yue (2012) offer a model that seems to best describe the changes in GDP components that we observe. As firms struggle to finance imports of intermediary investment goods, all three of investment, trade and output experience significant declines. Still, the drop in investment in the data is so sharp and sudden that it could indicate another underlying problem – for example, a troubled domestic financial sector. We explore this further in the next section by looking at the cost components during the banking-sovereign crisis events.

6.2 Defaults and systemic banking crises

We now turn to our analysis of the response of each GDP component to a default that is followed by a banking crisis within two years (same definition as Table 5 panel (a)). Figure 8 shows the results graphically and Table 7 provides the specific figures. Similarly to the baseline results in the previous section, there is evidence of the country moving towards financial and trade autarky: net exports increase while gross exports and imports fall. The decline in imports is particularly dramatic, peaking at some 12.3% of GDP in year 3. We also observe a sharp contraction in investment, which reduces GDP by 4.9% in the first year and 6.5% in years 2 and 3. The drop in investment alone is almost sufficient to explain the entire decline in GDP during the first three years after default, which is also when the country incurs the most of the output cost. Consumption falls more gradually, and exerts a significant drag on GDP growth in the medium term, which is consistent with its large share of GDP.

[11] We can also show that the drop in trade has a similar magnitude even when the default is not followed by a currency crisis. This suggests that it is driven by autarky rather than an exchange rate depreciation. Results are available upon request.

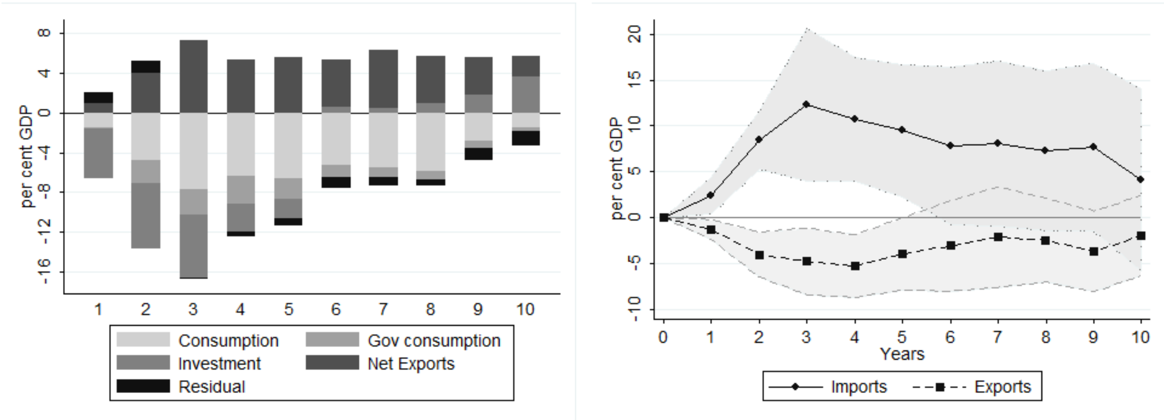
Table 6: Contribution of each component to GDP after default

Year	1	2	3	4	5	6	7	8	9	10
Investment	-3.08***	-3.99***	-3.81***	-3.32***	-3.51***	-2.87**	-2.52**	-1.75	-1.08	0.02
	(0.88)	(1.13)	(1.13)	(1.23)	(1.28)	(1.24)	(1.24)	(1.22)	(1.13)	(1.28)
Consumption	-1.89	-2.78**	-2.74*	-2.94*	-3.68***	-4.45***	-3.35	-3.85*	-2.85	-2.38
	(1.26)	(1.25)	(1.58)	(1.51)	(1.53)	(1.71)	(2.19)	(2.15)	(2.45)	(2.53)
Gov. Consump.	-0.27	-0.82**	-1.35***	-1.36***	-1.22***	-1.04**	-0.72	-0.33	0.03	-0.13
	(0.21)	(0.40)	(0.40)	(0.46)	(0.49)	(0.51)	(0.60)	(0.59)	(0.53)	(0.53)
Exports	-0.24	-1.40*	-1.99***	-2.39***	-2.58***	-1.84	-1.97*	-1.21	-1.58	-0.96
	(0.87)	(0.72)	(0.80)	(0.83)	(0.92)	(1.25)	(1.11)	(1.41)	(1.28)	(1.40)
Imports	2.12**	4.67***	5.44***	5.82***	6.35***	6.04***	5.86***	5.06***	4.34**	2.50
	(1.06)	(1.36)	(1.63)	(1.64)	(1.85)	(1.99)	(2.13)	(2.03)	(1.93)	(1.96)
Real GDP	-2.85***	-4.11***	-4.41***	-4.45***	-4.80***	-4.39***	-3.82*	-3.07	-2.03	-1.71
(total)	(0.63)	(1.09)	(1.31)	(1.53)	(1.69)	(1.86)	(2.06)	(2.37)	(2.65)	(3.01)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	88	88	88	88	88	88	88	88	88	88

Notes: Cumulative contribution of each component to GDP after a sovereign default. Calculated as the absolute change in a GDP component between t and t + h, scaled by the GDP level at t. Here t is the year before default, and h is the horizon.
IPSWRA specification, controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Effects do not sum exactly to real GDP; small residual (see Figures).
*, **, ***: Significant at 10%, 5% and 1% levels respectively

Compared to the baseline results in Figure 6.1 and Table 6, GDP components follow a similar pattern in the aftermath of a sovereign-banking crisis event. However, while the contribution of consumption and net exports to the fall in GDP is, in relative terms, similar to that in the baseline scenario, both investment and gross trade fall by relatively more. This suggests that compared to other sovereign default events, sovereign-banking crises are not fundamentally different in nature. Conversely, when the banking system breaks down in the presence of financial autarky, it is likely that firms are unable to obtain funding for investment or trade credit, either at home or abroad. Investment collapses, and together with it so do the imports of investment goods, trade credit and domestic production. The economy enters a steep decline – almost a free-fall – which, however, seems to ease after the initial three years.

The findings in this section suggest that for both standalone sovereign defaults and sovereign-banking crisis events, the cost of autarky play an important role in the transmission mechanism. At the same time, the nature of autarky cost seems far less innocent than an increase in consumption volatility: output declines, investment contracts and gross trade collapses. This asymmetric impact of default is also quite different from a standard endowment penalty,



(a) Decomposition of the treatment effect

(b) Exports and Imports

Figure 8: Default followed by a systemic banking crisis: effect on components of GDP

Notes: Cumulative contribution of individual components to GDP after a sovereign default which is followed by a systemic banking crisis within two years. Calculated as the absolute change in a GDP component between t and t + h, scaled by the GDP level at t. Here t is the year before default, and h is the horizon, plotted on the x-axis. Shaded bands indicate 90% confidence intervals.

which would impact all the components of GDP proportionately. Even though a number of mechanisms could underly this, the analysis of sovereign-banking crisis events suggests that the impact of default on the banking sector, and its interaction with autarky costs, is particularly important.

Incorporating the banking sector and the interplay between sovereign and banking distress into existing sovereign default models offers a natural way to microfound the output cost of default. This could provide an endogenous mechanism that amplifies the cost of autarky and facilitates both stronger creditor punishment and higher levels of sovereign debt. We remain ambivalent as to what precisely this mechanism could be. It could be that domestic banks are hurt by sovereign bond write-offs, as in the model of Gennaioli, Martin, and Rossi (2014a). For many defaults in our sample however, this is unlikely to be the case since these relate to external debt and direct borrowing from foreign banks. Other mechanisms such as a drying up of available funding for the domestic banking system or the absence of a lender of last resort during a sovereign default are likely to be at least equally as important. We will, however, leave the more detailed investigation of these channels to future research.

7 Conclusion

This paper provides a new "best-practice" sovereign default cost estimate by applying novel econometric methods to better deal with the endogenous selection into defaulters and non-defaulters. We find that sovereign default is costly: the economic cost is both statistically

Table 7: Contribution of each component to GDP after default followed by a systemic banking crisis

Year	1	2	3	4	5	6	7	8	9	10
Investment	-4.90***	-6.55***	-6.42***	-2.83	-1.93	0.63	0.43	0.94	1.77	3.58
	(1.83)	(2.38)	(2.56)	(2.12)	(2.39)	(2.56)	(2.88)	(3.28)	(3.17)	(3.93)
Consumption	-1.48	-4.80*	-7.67*	-6.35**	-6.58*	-5.23	-5.53	-5.85	-2.89	-1.45
	(1.60)	(2.75)	(4.00)	(2.86)	(3.77)	(4.66)	(5.78)	(5.32)	(5.53)	(5.85)
Gov. Consump.	-0.20	-2.28**	-2.62**	-2.79**	-2.13*	-1.23	-1.01	-0.84	-0.66	-0.46
	(0.56)	(1.06)	(1.22)	(1.38)	(1.28)	(1.23)	(1.16)	(1.13)	(1.14)	(1.22)
Exports	-1.29*	-4.00***	-4.73**	-5.25***	-3.94	-3.05	-2.10	-2.45	-3.66	-1.92
	(0.67)	(1.51)	(2.21)	(2.08)	(2.41)	(3.00)	(3.33)	(2.76)	(2.69)	(2.65)
Imports	2.39*	8.44***	12.33***	10.70***	9.47**	7.84	8.05	7.29	7.68	4.07
	(1.23)	(1.93)	(5.08)	(4.13)	(4.40)	(5.22)	(5.48)	(5.29)	(5.59)	(6.03)
Real GDP	-4.56***	-8.48***	-9.53***	-7.15**	-5.76	-2.29	-1.08	-1.65	0.73	2.38
(total)	(1.54)	(2.11)	(3.37)	(3.41)	(4.54)	(5.79)	(6.47)	(6.85)	(7.27)	(7.83)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	10	10	10	10	10	10	10	10	10	10

Notes: Cumulative contribution of each component to GDP after a default that is followed by a systemic banking crisis within two years. Calculated as the absolute change in a GDP component between t and t + h, scaled by the GDP level at t. Here t is the year before default, and h is the horizon.
IPSWRA specification, controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Effects do not sum exactly to real GDP; small residual (see Figures).
*, **, ***: Significant at 10%, 5% and 1% levels respectively

significant and highly persistent. Accounting for endogenous selection attenuates the cost, but its magnitude remains higher than that of a normal recession, and comparable to that of other crisis events, as well as the costs assumed in a variety of theoretical default models. This helps to explain why defaults – even though they do occur from time to time – are still considered extreme events rather than regular occurrences; at least for most countries.

What exactly then makes default costly? The impact of default on trade suggests that autarky costs do play an important role in the transmission mechanism. However, the high cost of defaults followed by systemic banking crises, and the sharp drops in investment that are observed for all types of default, show that banking sector distress most likely plays an equally important role. This suggests that theoretical models of sovereign default would benefit from focussing on sovereign-banking spillovers, and their interaction with autarky costs. When it comes to making policy decisions, it may be tempting to focus entirely on the negotiations and the potential retaliation from the country's creditors. But when a country's sovereign is going bust, it pays to keep a close eye on domestic banks.

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Appendix

A1 Data: sources and summary statistics

Table A1.1: Data sources and variables used in main regressions

Variable	Source	Description
Dependent variables		
GDP growth	Penn World Tables (PWT) 8.0	Percentage change in real GDP per capita
GDP components	PWT 8.0	Growth of investment, consumption, government spending and net exports relative to GDP
Treatments		
External default	Standard & Poor's (kindly provided by Christoph Trebesch)	Equals 1 for the first year in default and 0 otherwise. Defaults on external debt only.
Default magnitude	Beers and Nadeau (2015) CRAG database	Debt in default relative to GDP
B & C defaults	Beim and Calomiris (2000)	Equals 1 for the first year in default and 0 otherwise.
D & S defaults	Detragiache and Spilimbergo (2001)	Equals 1 for the first year in default and 0 otherwise.
L & V defaults	Laeven and Valencia (2012)	Equals 1 for the first year in default and 0 otherwise.
Controls & predictors		
Public external debt	World Bank GDF (2012) & IDS (2014)	Ratio to GDP
Total external debt	as above	Ratio to GDP
Real GDP level	PWT 8.0	GDP per capita
GDP cyclical component	PWT 8.0	Relative deviation of real per-capita GDP from HP-filtered trend
Inflation rate	PWT 8.0	Change in GDP deflator
Terms of trade	PWT 8.0	Change in terms of trade
Government size	PWT 8.0	Government consumption/GDP
Current account	PWT 8.0	Ratio to GDP
Openness	PWT 8.0	(Imports+Exports)/GDP

[continued overleaf]

Variable	Source	Description
Controls & predictors; continued		
Financial crisis	Laeven and Valencia (2012)	Equals 1 if a systemic banking crisis starts that year, 0 otherwise
Currency crisis	Laeven and Valencia (2012)	Equals 1 if a currency crisis starts that year, 0 otherwise
War	M. Marshall (2014) MEPV database	War intensity > 0 for any kind of conflict
Coup	M. Marshall and D. Marshall (2014) coups d'état	Dummy for coup or attempted coup
Governance quality	M. Marshall, Gurr, and Jaggers (2014) Polity IV	Revised combined Polity score
Political transition	Polity IV	Equals 1 in the first year of transition, 0 otherwise
Sovereign Credit Ratings	*Institutional Investor Magazine*	100-point scale, from 0 (highest credit risk) to 100 (lowest credit risk)
Growth Forecasts	Historical WEO Forecasts Database	GDP growth forecasts for the next 5 years
Predictors not used as controls (IPSW first stage only)		
Short-term external debt	World Bank GDF (2012) & IDS (2014)	Ratio to GDP
Interest payments on external debt	World Bank GDF (2012) & IDS (2014)	Ratio to GDP
US T-Bill rate	Federal Reserve	1-year constant maturity rate
Commodity Index, CCI	Thomson Reuters	Equal weight index
Years in default	Standard & Poor's	Years a country was in default since 1950
Number of past defaults	Standard & Poor's	Defaults since 1950
Continent	geonames.org	Continent dummies

Table A1.2: Defaults in the baseline sample

Algeria (1991)	Guyana (1982)	Peru (1984)
Argentina (1982)	Haiti (1982)	Philippines (1983)
Argentina (2001)	Honduras (1981)	Romania (1981)
Bolivia (1980)	Indonesia (1998)	Romania (1986)
Bolivia (1986)	Jamaica (1978)	Russia (1998)
Brazil (1983)	Jamaica (1981)	Senegal (1981)
Bulgaria (1990)	Jamaica (1987)	Senegal (1990)
Burkina Faso (1983)	Jordan (1989)	Senegal (1992)
Cameroon (1985)	Kenya (1994)	Sierra Leone (1983)
Central African Republic (1981)	Kenya (2000)	Sierra Leone (1986)
Central African Republic (1983)	Liberia (1981)	Solomon Islands (1998)
Chile (1983)	Madagascar (1981)	Sudan (1979)
Congo, Dem. Rep. (1976)	Malawi (1982)	Tanzania (1984)
Congo, Republic of (1983)	Malawi (1988)	Togo (1979)
Costa Rica (1981)	Mauritania (1992)	Togo (1982)
Cote d'Ivoire (1983)	Mexico (1982)	Togo (1988)
Cote d'Ivoire (2000)	Moldova (1998)	Togo (1991)
Dominican Republic (1982)	Morocco (1983)	Turkey (1978)
Ecuador (1982)	Morocco (1986)	Turkey (1982)
Ecuador (1999)	Myanmar (1997)	Uganda (1980)
Gabon (1986)	Nicaragua (1979)	Ukraine (1998)
Gabon (1999)	Niger (1983)	Uruguay (1983)
Gambia (1986)	Nigeria (1982)	Uruguay (1987)
Ghana (1987)	Nigeria (2001)	Uruguay (1990)
Guatemala (1986)	Pakistan (1998)	Venezuela (1983)
Guatemala (1989)	Panama (1983)	Venezuela (1990)
Guinea (1986)	Paraguay (1986)	Zambia (1983)
Guinea (1991)	Peru (1976)	Zimbabwe (2000)
Guinea-Bissau (1983)	Peru (1978)	
Guyana (1979)	Peru (1980)	

Table A1.3: Number of sovereign defaults coinciding with other crisis events

Economic crises:	
Financial crises	15
Currency crises	35
Triple crises (financial + currency + sovereign)	5
Political crises:	
Wars	15
Coups	19
Political transitions	16
All crisis events	59

A joint event is *any* of the above crises occurring concurrently, in the year before or the year after the sovereign default.

Table A1.4: Share of each component in GDP for defaulters

	GDP share
Consumption / GDP	0.67
Investment / GDP	0.19
Government Consumption / GDP	0.15
Exports / GDP	0.21
Imports / GDP	0.27
Net Exports / GDP	-0.03

The shares of the defaulters refer to the year before the default episode.

A2 IPSWRA estimation: first and second stage

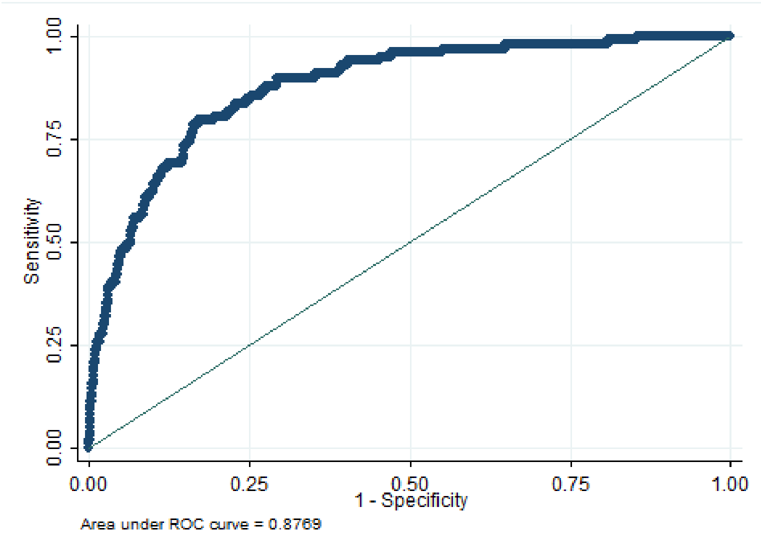

Figure A2.1: ROC graph for the IPSWRA first stage

Table A2.1: IPSWRA first stage: logit regression results

	Coefficient
External default (forwarded 1 period)	
Number of past defaults	0.3309* (0.1866)
Years in default	-0.0665 (0.0414)
External public debt to GDP	0.0061 (0.0170)
Total external debt to GDP	-0.0055 (0.0162)
Short-term external debt to GDP	0.0033 (0.0194)
Interest payments on external debt to GDP	0.1497*** (0.0577)
Log of inflation (GDP deflator)	-0.3935 (0.3096)
Openness, percent GDP	-0.0009 (0.0040)
Current account, percent GDP	0.0491*** (0.0144)
Nominal 1-year US T-Bill rate	0.2041*** (0.0384)
Change in commodity prices	-5.4737*** (1.1619)
Real GDP per capita growth	-0.1703*** (0.0398)
GDP deviation from trend	1.2647** (0.5925)
Real GDP per capital level	-0.0001 (0.0001)
Change in terms of trade	0.0440 (0.8460)
Change in nominal exchange rate	0.0005 (0.0003)
Government share, percent real GDP	-0.0045 (0.0192)
Revised Combined Polity Score	-0.0239 (0.0227)
Index of societal and interstate conflicts	-0.2017** (0.0890)
Political transition: continuous measure	-0.7414 (0.7178)
Real GDP per capita growth: 1 lag	0.0041 (0.0224)
Real GDP per capita growth: 2 lags	0.0053 (0.0244)
Political transition: first year dummy	0.6271 (0.9039)
Political transition: first year dummy, 1 lag	0.6861 (0.5014)
Financial crisis dummy	0.7030 (0.4909)
Financial crisis dummy: 1 lag	0.4047 (0.5740)
Financial crisis dummy: 2 lags	-0.6090 (1.0399)
Currency crisis dummy	0.3536 (0.4680)
Currency crisis dummy: 1 lag	0.5052 (0.4940)
Currency crisis dummy: 2 lags	0.4773 (0.4894)
Successful and Attempted Coups	0.1359 (0.4070)
Coup dummy: 1 lag	0.2993 (0.4100)
Coup dummy: 2 lags	0.1387 (0.4113)
Africa dummy	-0.1613 (0.5024)
South America dummy	0.9868* (0.5257)
North America dummy	1.1388** (0.5511)
Asia dummy	-0.7232 (0.5846)
Constant	-2.7038* (1.5763)
N	2817
Pseudo R-squared	.26

Notes: Regression coefficients on the first-stage predictors (dependent variable: external default one year ahead).
Standard errors in parentheses. *, **, ***: Significant at 10%, 5% and 1% levels respectively

Table A2.2: IPSWRA second stage: IPS-weighted regression results

	Year 1	Year 2	Year 3	Year 4	Year 5
External public debt to GDP, percent	-0.019 (0.01)	-0.069*** (0.03)	-0.103*** (0.04)	-0.136*** (0.05)	-0.158*** (0.06)
Total external debt to GDP, percent	0.021*** (0.01)	0.066*** (0.01)	0.109*** (0.02)	0.153*** (0.03)	0.185*** (0.03)
Real GDP per capita growth	0.096 (0.08)	-0.018 (0.16)	-0.144 (0.23)	-0.259 (0.29)	-0.412 (0.31)
GDP deviation from trend	1.820 (1.55)	4.115 (3.25)	5.990 (4.58)	6.904 (5.23)	7.713 (4.83)
Real GDP per capital level	-0.001*** (0.00)	-0.003*** (0.00)	-0.004*** (0.00)	-0.004*** (0.00)	-0.005*** (0.00)
Government share, percent real GDP	0.080 (0.05)	0.090 (0.10)	0.066 (0.13)	-0.012 (0.14)	-0.139 (0.14)
Log of inflation (GDP deflator)	-0.595 (0.42)	-1.107 (0.70)	-1.570* (0.86)	-2.068* (1.08)	-2.828** (1.33)
Openness, percent GDP	0.043*** (0.01)	0.096*** (0.03)	0.131*** (0.04)	0.138** (0.05)	0.138** (0.06)
Current account, percent GDP	-0.010 (0.04)	0.001 (0.07)	-0.012 (0.09)	-0.023 (0.10)	0.006 (0.12)
Index of societal and interstate conflicts	-0.060 (0.15)	-0.209 (0.25)	-0.393 (0.34)	-0.529 (0.44)	-0.515 (0.55)
Revised Combined Polity Score	0.020 (0.03)	0.073 (0.07)	0.117 (0.10)	0.158 (0.12)	0.234 (0.14)
Successful and Attempted Coups	-0.585 (0.55)	-1.775** (0.69)	-1.699 (1.04)	-2.111 (1.35)	-2.375 (1.48)
Coup dummy: 1 lag	-0.545 (0.57)	0.348 (1.07)	0.006 (1.45)	-0.521 (1.41)	-0.663 (1.47)
Financial crisis dummy	-2.369** (1.01)	-2.095 (1.28)	-2.237 (1.52)	-2.618 (1.77)	-2.956 (1.84)
Financial crisis dummy: 1 lag	0.364 (0.59)	0.020 (1.01)	-0.280 (1.25)	-1.347 (1.41)	-1.480 (1.34)
Currency crisis dummy	0.267 (0.53)	-0.262 (0.94)	-0.250 (1.26)	0.190 (1.52)	0.441 (1.71)
Currency crisis dummy: 1 lag	-1.027* (0.56)	-1.264 (0.81)	-1.261 (1.04)	-1.226 (1.17)	-1.201 (1.23)
Political transition: first year dummy	-0.565 (0.47)	-0.823 (0.78)	-1.504 (1.15)	-2.583** (1.19)	-2.884** (1.32)
Political transition: first year dummy, 1 lag	0.051 (0.49)	-0.678 (0.89)	-1.610 (1.27)	-2.276 (1.45)	-2.512 (1.58)
Real GDP per capita growth: 1 lag	-0.016 (0.03)	-0.037 (0.05)	-0.131** (0.06)	-0.243** (0.10)	-0.226* (0.12)
External default (forwarded 1 period)	-2.848*** (0.63)	-4.109*** (1.09)	-4.409*** (1.31)	-4.454*** (1.53)	-4.798*** (1.69)
Constant	1.886 (2.23)	7.031* (3.57)	9.082* (4.70)	11.956** (5.98)	16.510** (7.07)
N	2546	2546	2546	2546	2546
R-squared	.26	.33	.4	.47	.53

Notes: This table shows the estimation results for Years 1 – 5 of the IPSWRA local projection (dependent variable: cumulative real per capita GDP growth). Results for Years 6 – 10 are omitted. Clustered standard errors in parentheses.
*, **, ***: Significant at 10%, 5% and 1% levels respectively

A3 Constructing the sovereign ratings variable

We use the *Institutional Investor Magazine* country credit ratings. The ratings are based on information from sovereign risk experts in leading financial institutions and weighted to resemble the "average market expectation". They are recorded using a 100-point scale, with a low number indicating a bad rating and a high number indicating a high rating (i.e., low credit risk). Using standard credit rating measures of S&P and Moody's does not affect the results but restricts the sample substantially.

We additionally construct a "synthetic rating" variable for observations where we do not have any observed credit ratings or spread data. To do this, we first regress credit ratings on other observable data, and then predict the synthetic credit rating for the missing observations. We use the set of credit rating determinants identified by Cantor and Packer (1996) to predict the rating: real GDP growth (2 lags), GDP level, inflation, external debt to GDP and the number of past defaults. We also add the US T-Bill rate as a proxy for international borrowing conditions over time. Finally, we use continent dummies as a substitute for the indicator of economic development. Since we already use this set of predictors in our IPSWRA first stage, the "synthetic rating" results presented in Table 4 panel (c) can be best viewed as a combination of our baseline results (where we use the baseline data sample to construct a "synthetic" rating), and the additional observed ratings and spread data.

Once we have constructed the relevant measure of the ratings, we add the level and change of the rating both as an extra predictor in the first stage of the IPSWRA, and as an extra control in the second stage of the IPSWRA.

A4 Alternative treatments for the baseline specification

A4-1 Alternative default definitions

Table A4.3 presents the results using four alternative default definitions (panels (b) – (d)), with our baseline estimates from Section 4.1 shown in panel (a). The definition of Beim and Calomiris (2000) (panel (b)) groups default episodes that occur within 5 years of each other together and drops some defaults driven by political factors, such as wars and coups. Detragiache and Spilimbergo (2001) (panel (c)) classify an event as a default once the country has accumulated a large amount of arrears. The Laeven and Valencia (2012) (panel (d)) sovereign debt crisis definition focuses on more pronounced default events, and additionally excludes politically driven defaults. Finally, we use an alternative coding of the S & P dataset (panel (e)), which adds defaults that occurred while a country was still negotiating another default on past debt.

For each default definition, we compare the results under an unconditional local projection, with country fixed effects only, to those using our preferred IPSWRA specification with the full set of controls. The IPSWRA method used in panels (b) and (d) (Beim and Calomiris, 2000; Laeven and Valencia, 2012) is exactly the same as our baseline specification (panel (a), bottom row). That is, we assign countries already in default a zero probability of defaulting again. For definitions in panels (c) and (e), we do not have – or cannot use – the information on the end dates of default episodes. We therefore rely on the standard IPSWRA procedure. This implies that some of the high-weighted observations in the control group could be countries still negotiating a past default. The time period covered by datasets in panels (d) and (e) is the same as our baseline results, whereas the time period covered by panels (b) and (c) is shorter due to limited data availability.

Despite the different nature of the alternative default definitions, conditioning on observed confounders attenuates the cost of default in each of the five cases: the cost of default under the IPSWRA specification is smaller than that under the unconditional specification for each year and each default definition. In the initial year this difference is 0.5 – 1 percentage points (ppts) of GDP for all definitions, rising to around 2 – 4 ppts by the end of our horizon, depending on the definition used[12].

We find a significant drop in output under each of the five default definitions. For the alternative default definitions in panels (b) – (d), both the initial drop in output and its persistence are larger than under our baseline specification. For the Beim and Calomiris (2000) definition (panel (b)), this is largely driven by the shorter timespan of their sample: the estimation results are quite similar to our baseline definition restricted to the shorter timespan (defaults before 1992).[13] For the other two definitions, the higher cost is most likely driven by the endogenously more stringent definition of default. Laeven and Valencia (2012) focus on more pronounced "sovereign debt crisis" events. Detragiache and Spilimbergo (2001) pick out the costliest default episode episodes by using an explicit arrears threshold. Finally, we do not find any material differences between the two S & P definitions (panels (a) and (e)).

[12]This is not the case for the Beim and Calomiris (2000) defintion. In this specification unconditional and IPSWRA results are very close in the last year, but differ in the years before. This finding is largely driven by the shorter timespan of the Beim and Calomiris (2000) data.

[13]Not shown, available from authors upon request.

Table A4.3: Alternative default definitions

Year	1	2	3	4	5	6	7	8	9	10
(a) Baseline, 1970-2010										
Unconditional	-3.44***	-4.96***	-5.21***	-5.37***	-6.03***	-6.43***	-6.38***	-6.26***	-5.61***	-5.84***
	(0.65)	(0.98)	(1.10)	(1.30)	(1.37)	(1.53)	(1.70)	(1.91)	(2.17)	(2.50)
IPSWRA	-2.85***	-4.11***	-4.41***	-4.45***	-4.80***	-4.39***	-3.82*	-3.07	-2.03	-1.71
	(0.63)	(1.09)	(1.31)	(1.53)	(1.69)	(1.86)	(2.06)	(2.37)	(2.65)	(3.01)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	88	88	88	88	88	88	88	88	88	88
(b) Beim & Calomiris, 1970-1992										
Unconditional	-5.24***	-8.04***	-8.50***	-8.28***	-8.24***	-7.63***	-6.94***	-6.69***	-6.03***	-4.55**
	(1.08)	(1.84)	(1.98)	(1.89)	(1.97)	(2.07)	(2.12)	(2.16)	(2.16)	(2.25)
IPSWRA	-4.35***	-6.81***	-6.99***	-6.45***	-7.33***	-6.09***	-5.46***	-4.86***	-4.53***	-4.31***
	(1.28)	(1.93)	(2.09)	(1.99)	(2.26)	(2.15)	(1.89)	(1.82)	(1.77)	(1.76)
Observations	1157	1157	1157	1157	1157	1157	1157	1157	1157	1157
Defaults	45	45	45	45	45	45	45	45	45	45
(c) Detragiache & Spilimbergo, 1971-1998										
Unconditional	-3.97***	-6.55***	-7.81***	-7.68***	-7.39***	-7.48***	-7.64***	-9.45***	-10.04***	-10.84***
	(0.95)	(1.35)	(1.44)	(1.45)	(1.49)	(1.42)	(1.50)	(1.60)	(1.83)	(2.02)
IPSWRA	-3.18***	-5.41***	-6.49***	-6.02***	-5.30***	-5.44***	-5.04***	-6.66***	-7.16***	-8.01***
	(0.82)	(1.24)	(1.32)	(1.27)	(1.32)	(1.36)	(1.43)	(1.59)	(1.81)	(2.08)
Observations	2210	2210	2210	2210	2210	2210	2210	2210	2210	2210
Defaults	48	48	48	48	48	48	48	48	48	48
(d) Laeven & Valencia, 1970-2010										
Unconditional	-4.06***	-6.83***	-7.64***	-8.19***	-9.20***	-9.70***	-9.94***	-10.90***	-11.37***	-10.51***
	(0.66)	(1.15)	(1.30)	(1.39)	(1.62)	(1.76)	(1.88)	(1.84)	(1.97)	(2.24)
IPSWRA	-3.18***	-5.68***	-6.36***	-6.60***	-7.65***	-7.53***	-7.29***	-7.30***	-7.25***	-6.29***
	(0.68)	(1.13)	(1.32)	(1.40)	(1.76)	(1.92)	(2.02)	(2.09)	(2.16)	(2.38)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	50	50	50	50	50	50	50	50	50	50
(e) S & P alternative, 1970-2010										
Unconditional	-3.33***	-4.98***	-5.12***	-5.16***	-5.67***	-5.87***	-5.82***	-5.67***	-4.90***	-5.09**
	(0.63)	(0.95)	(1.05)	(1.22)	(1.32)	(1.49)	(1.63)	(1.85)	(2.10)	(2.43)
IPSWRA	-2.77***	-4.13***	-4.15***	-4.10***	-4.31***	-3.85**	-3.38*	-2.66	-1.64	-1.63
	(0.60)	(1.01)	(1.17)	(1.36)	(1.52)	(1.70)	(1.85)	(2.13)	(2.40)	(2.72)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	93	93	93	93	93	93	93	93	93	93

Notes: Expected differences in cumulative real GDP per capita growth between defaulters and non-defaulters. Unconditional specification controls for country fixed effects only. IPSWRA specification controls for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. For each definition, we use the longest possible sample; see panel headings for years covered. *, **, ***: Significant at 10%, 5% and 1% levels respectively

A4-2 Default magnitude

To calculate a proxy for default magnitude, we make use of the new Bank of Canada CRAG Database (2015) which records total sovereign debt in default for a given country in a given year. Using this, we first record the debt in default relative to GDP in the year of sovereign default.[14] We then split our default observations into two groups: those where debt in default was high – "high-magnitude" defaults – and those where debt in default was low. We use two different thresholds to classify defaults as "high-magnitude". The lower threshold of 5% debt-in-default-to-GDP aims to filter out those events where debt in default was relatively small, and which may thus have been ignored by both debtors and creditors. The higher threshold of 15% tries to identify the highest-magnitude defaults in our sample and see whether those are exceedingly costly in comparison.

Therefore, we ask two questions: first, is our estimate too low because it includes many low-magnitude defaults that carry almost no cost? And second, do we find that exceedingly large defaults are also exceedingly costly? Our findings rebuff each of the questions. Table A4.4 presents the estimation results, with the lower 5% threshold in panel (a) and the 15% threshold in panel (b). The findings in panel (a) correspond to Figure 4(a) in the main text.

Table A4.4: Large and small defaults

Year	1	2	3	4	5	6	7	8	9	10
(a) Debt defaulted relative to GDP: 5% threshold										
Small	-2.01***	-3.21**	-4.17**	-4.27**	-4.98**	-4.65*	-4.09	-4.61	-4.21	-3.11
(no. defaults = 39)	(0.56)	(1.44)	(1.91)	(2.14)	(2.27)	(2.42)	(2.70)	(2.97)	(3.22)	(3.45)
Large	-3.47***	-4.78***	-4.58***	-4.59***	-4.66**	-4.19*	-3.63	-1.92	-0.40	-0.66
(no. defaults = 49)	(0.98)	(1.25)	(1.61)	(1.87)	(2.16)	(2.52)	(2.73)	(3.20)	(3.56)	(4.06)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
large = small	0.19	0.34	0.86	0.90	0.91	0.89	0.90	0.51	0.38	0.60
(a) Debt defaulted relative to GDP: 15% threshold										
Small	-2.94***	-4.38***	-5.29***	-5.43***	-5.94***	-4.83***	-4.66**	-4.67*	-3.70	-3.98
(no. defaults = 60)	(0.67)	(1.37)	(1.68)	(1.86)	(1.87)	(2.01)	(2.16)	(2.39)	(2.58)	(2.79)
Large	-2.63**	-3.49**	-2.38	-2.19	-2.15	-3.37	-1.88	0.63	1.83	3.55
(no. defaults = 28)	(1.33)	(1.50)	(1.92)	(2.23)	(3.04)	(3.62)	(4.05)	(4.64)	(5.32)	(6.08)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
large = small	0.83	0.65	0.25	0.23	0.26	0.71	0.52	0.28	0.31	0.22

Notes: Expected differences in cumulative real GDP per capita growth for the relevant default treatment. Treatments are based on a simple sample split of our baseline default definition. All figures are IPSWRA estimates controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses.
*, **, ***: Significant at 10%, 5% and 1% levels respectively.

Looking at the top row of panel (a), we find that low-magnitude defaults are still costly, and not significantly different from those of higher magnitude (panel (a), bottom row). The costs

[14] The debt haircut would be a better proxy for magnitude (see, for example Cruces and Trebesch, 2013). However, since default negotiations take some time, information on haircuts is not available at the time of default, and we cannot use it in our local projection or propensity score prediction.

may be somewhat lower initially, but this is not the case over the medium and long horizon – as previously discussed, it is these horizons that drive the main difference between the baseline and alternative default definitions. Furthermore, if we isolate the highest-magnitude default events – where debt in default exceeds 15% GDP, bottom row of panel (b) – the costs are not much higher than our baseline definition or those of small-magnitude defaults. If at all, the costs are lower at longer horizons. This could be because high-magnitude defaults contain both costs and benefits, which wash out in the average. They may elicit a more severe punishment from creditors, but also provide more debt relief.

A4-3 Defaulting in good and bad times

As Tomz and Wright (2007) have noted, most countries default during bad times, i.e. periods of below-trend GDP growth. Still, our sample contains a substantial number of defaults that occur during good – or normal – times, with GDP growth at or above trend. Comparing the costs of default during good and bad times is interesting for two reasons. First, as previously mentioned, part of our default cost could be endogenous, which simply reflects the poor economic situation of countries that tend to subsequently default, regardless of whether they actually default or not. For this to not be the case, we also need default to be costly when economic fundamentals are favourable. Second, as discussed in Section 2, a number of theoretical models impose a higher default cost during good times to justify the relative rarity of defaulting when the country is doing well.

We split our sample of defaults into two subsamples – defaults in good and bad times – and compare the results between these two treatments. Good-time defaults are those that occurred when a country's GDP was above trend, and bad-times – below trend, with the trend calculated using a one-sided HP filter with a smoothing parameter of 6.25 (Ravn and Uhlig, 2002).

Table A4.5 presents the results. Panel (a) compares deviations from the trend in the year before default, and panel (b) – in the three years preceding default. Panel (b) corresponds to Figure 4(b) in the main text. We find that defaulting in good times is costly under both specifications, which suggests that our results are not driven by a subsample of defaulters who simply have poor economic fundamentals. However, defaulting in good times is no more costly than defaulting during bad times, which seems to go against the assumptions often made in theoretical literature.

A4-4 Robustness to alternative regression specifications

To check the stability of our results under different variations of the IPSWRA method, we explore alternative ways of calculating the propensity score and selecting the control group. Table A4.6 shows the results. The top row shows our baseline specification from Section 4.1. Recall that we truncate the estimated inverse propensity scores at 10 following Imbens (2004). Also the control group in the baseline includes those countries still negotiating a past default, but we give these a low weight of 1 (zero probability of default).

The second row shows the results using a larger truncation threshold of 20. This effectively makes the rebalancing stronger but less robust. The estimated effect does not differ substantially compared to baseline. The third row shows the results using the "standard" inverse propensity score weight for those observations that are negotiating a past default. This weight, $1/(1 - \widehat{PD}_t)$ implied by the logit, is greater than that in our baseline specification and gives these countries a higher prominence among the control group. The results under this specification, however, remain close to baseline. The results in the fourth row more or less do the opposite to those in the third row: rather than treating countries negotiating a

Table A4.5: Defaulting in good and bad times

Year	1	2	3	4	5	6	7	8	9	10
(a) 1 year before default										
Bad Times	-2.80***	-4.40***	-4.49***	-4.46***	-5.41***	-6.38***	-5.82***	-4.70*	-3.33	-2.56
(no. defaults = 59)	(0.74)	(1.24)	(1.57)	(1.70)	(1.94)	(2.00)	(2.21)	(2.64)	(3.03)	(3.36)
Good Times	-2.93***	-3.57*	-4.26	-4.44	-3.65	-0.64	-0.07	-0.01	0.40	-0.10
(no. defaults = 29)	(1.11)	(2.01)	(2.91)	(3.30)	(3.48)	(3.72)	(4.00)	(4.37)	(4.74)	(5.28)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
good = bad	0.92	0.72	0.95	0.99	0.67	0.17	0.20	0.34	0.50	0.68
(b) 1–3 years before default										
Bad Times	-3.31***	-3.86***	-3.71**	-3.80**	-4.64**	-4.12*	-3.76	-2.65	-2.03	-2.41
(no. defaults = 58)	(0.74)	(1.32)	(1.60)	(1.91)	(2.15)	(2.36)	(2.45)	(2.55)	(2.79)	(2.99)
Good Times	-2.08*	-4.54***	-5.59***	-5.56***	-5.06**	-4.85*	-3.94	-3.77	-2.05	-0.53
(no. defaults = 30)	(1.14)	(1.76)	(1.97)	(2.28)	(2.46)	(2.89)	(3.49)	(4.19)	(4.68)	(5.34)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
good = bad	0.36	0.75	0.44	0.54	0.89	0.84	0.97	0.81	1.00	0.73

Notes: Expected differences in cumulative real GDP per capita growth for the relevant default treatment. Treatments are based on a simple sample split of our baseline default definition. All figures are IPSWRA estimates controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Good times are defined as growth above HP-filtered trend, smoothing parameter 6.25, bad times – growth below trend, either in the year before, or over the three years before default.
*, **, ***: Significant at 10%, 5% and 1% levels respectively.

Table A4.6: Alternative propensity scores and control groups

Year	1	2	3	4	5	6	7	8	9	10	Obs.
Baseline	-2.85***	-4.11***	-4.41***	-4.45***	-4.80***	-4.39***	-3.82*	-3.07	-2.03	-1.71	2546
	(0.63)	(1.09)	(1.31)	(1.53)	(1.69)	(1.86)	(2.06)	(2.37)	(2.65)	(3.01)	
Less Truncation	-2.98***	-4.21***	-4.74***	-4.69***	-4.99***	-4.50**	-3.88	-3.03	-2.04	-1.52	2546
	(0.69)	(1.21)	(1.53)	(1.81)	(2.00)	(2.20)	(2.42)	(2.76)	(3.08)	(3.51)	
Unadjusted IPW	-2.87***	-4.06***	-4.09***	-4.13***	-4.47***	-4.11***	-3.52*	-2.72	-1.74	-1.64	2546
	(0.62)	(1.04)	(1.23)	(1.43)	(1.58)	(1.76)	(1.94)	(2.23)	(2.50)	(2.83)	
Clean Control	-3.16***	-4.54***	-4.90***	-5.05***	-5.47***	-4.87**	-4.37*	-3.71	-2.74	-2.45	1895
Group	(0.68)	(1.19)	(1.49)	(1.77)	(1.97)	(2.16)	(2.45)	(2.80)	(3.09)	(3.47)	
Defaults.	88	88	88	88	88	88	88	88	88	88	

Notes: Expected differences in cumulative real GDP per capita growth between defaulters and non-defaulters. Clustered standard errors in parentheses. Unconditional specification controls for country fixed effects only. IPSWRA specifications control for country fixed effects and the full list of variables in Table A1.1.
Less truncation: inverse propensity score weights truncated at 20 instead of 10.
Unadjusted IPW: countries negotiating a past default treated as normal in the control group.
Clean control group: countries negotiating a past default excluded from the control group.
*, **, ***: Significant at 10%, 5% and 1% levels respectively

past default as "normal" observations, they completely remove them from the control group. Even though this alters the sample substantially, it has relatively little bearing on our results.

A5 Further details on the currency- and political-crisis defaults

Table A5.1: Sovereign Default and Currency Crises

Year	1	2	3	4	5	6	7	8	9	10
Default + no Crisis	-2.37***	-3.40***	-3.77**	-4.11**	-4.82**	-4.12*	-4.09	-3.14	-1.85	-2.02
(no. defaults = 53)	(0.73)	(1.43)	(1.74)	(2.07)	(2.20)	(2.48)	(2.58)	(2.76)	(2.98)	(3.17)
Default + Crisis	-3.61***	-5.22***	-5.41***	-5.00***	-4.76*	-4.82*	-3.41	-2.97	-2.33	-1.22
(no. defaults = 35)	(0.99)	(1.44)	(1.80)	(1.89)	(2.44)	(2.78)	(3.31)	(3.91)	(4.47)	(5.14)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
crisis = no crisis	0.28	0.34	0.50	0.74	0.98	0.85	0.87	0.97	0.92	0.89

Notes: Expected differences in cumulative real GDP per capita growth for the relevant default treatment. Treatments are based on a simple sample split of our baseline default definition. All figures are IPSWRA estimates controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Sovereign default has occurred within ±1 year of a currency crisis.
*, **, ***: Significant at 10%, 5% and 1% levels respectively.

Table A5.2: Sovereign Default and Political Crises

Year	1	2	3	4	5	6	7	8	9	10
Default + no Crisis	-2.35***	-3.46***	-3.10**	-4.13***	-4.30***	-4.02**	-3.14	-1.68	-1.45	-0.84
(no. defaults = 58)	(0.74)	(1.24)	(1.36)	(1.55)	(1.70)	(1.95)	(1.96)	(2.07)	(2.18)	(2.37)
Default + Crisis	-3.65***	-5.16***	-6.54***	-4.99	-5.61	-4.99	-4.93	-5.34	-2.98	-3.12
(no. defaults = 30)	(1.05)	(1.96)	(2.78)	(3.16)	(3.59)	(4.05)	(4.51)	(5.18)	(5.84)	(6.65)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
p-value:										
crisis = no crisis	0.29	0.46	0.28	0.81	0.75	0.84	0.72	0.51	0.80	0.74

Notes: Expected differences in cumulative real GDP per capita growth for the relevant default treatment. Treatments are based on a simple sample split of our baseline default definition. All figures are IPSWRA estimates controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Sovereign default has occurred within ±1 year of a political crisis. A political crisis is defined as a coup, a political transition or a war intensity of more than 3 (MEPV total conflict variable; scale 1 – 20: sum of interstate and civil conflict, each scaled from 1 to 10).
*, **, ***: Significant at 10%, 5% and 1% levels respectively.

A6 Cost decomposition for defaults not followed by a banking crisis

Table A6.1: Contribution of each component to GDP after default not followed by a banking crisis

Year	1	2	3	4	5	6	7	8	9	10
Investment	-2.80***	-3.59***	-3.39***	-3.40***	-3.76***	-3.42***	-2.98**	-2.17*	-1.53	-0.54
	(0.96)	(1.21)	(1.19)	(1.34)	(1.41)	(1.38)	(1.35)	(1.31)	(1.22)	(1.33)
Consumption	-1.95	-2.46*	-1.97	-2.41	-3.23**	-4.32***	-3.01	-3.54	-2.84	-2.53
	(1.38)	(1.33)	(1.60)	(1.65)	(1.61)	(1.82)	(2.27)	(2.26)	(2.60)	(2.72)
Gov. Consump.	-0.28	-0.60	-1.15***	-1.14***	-1.07**	-1.01*	-0.68	-0.25	0.14	-0.08
	(0.21)	(0.41)	(0.40)	(0.44)	(0.49)	(0.53)	(0.63)	(0.64)	(0.58)	(0.56)
Exports	-0.08	-0.99	-1.56*	-1.95**	-2.36***	-1.65	-1.95*	-1.02	-1.26	-0.81
	(0.98)	(0.81)	(0.89)	(0.88)	(0.96)	(1.30)	(1.09)	(1.49)	(1.34)	(1.48)
Imports	2.08*	4.08***	4.37***	5.06***	5.87***	5.76***	5.52***	4.71**	3.82**	2.26
	(1.22)	(1.50)	(1.57)	(1.68)	(1.91)	(2.04)	(2.16)	(2.07)	(1.93)	(1.95)
Real GDP	-2.58***	-3.42***	-3.61***	-4.03***	-4.65***	-4.72***	-4.26**	-3.29	-2.47	-2.35
(total)	(0.66)	(1.14)	(1.40)	(1.65)	(1.80)	(1.98)	(2.13)	(2.47)	(2.81)	(3.17)
Observations	2546	2546	2546	2546	2546	2546	2546	2546	2546	2546
Defaults	78	78	78	78	78	78	78	78	78	78

Notes: Cumulative contribution of each component to GDP after a default that is not followed by a systemic banking crisis within two years. Calculated as the absolute change in a GDP component between t and t + h, scaled by the GDP level at t. Here t is the year before default, and h is the horizon.
IPSWRA specification, controlling for country fixed effects and the full list of variables in Table A1.1. Clustered standard errors in parentheses. Effects do not sum exactly to real GDP; small residual (see Figures).
*, **, ***: Significant at 10%, 5% and 1% levels respectively