MAGKS

Joint Discussion Paper Series in Economics

by the Universities of

Aachen · Gießen · Göttingen
Kassel · Marburg · Siegen

ISSN 1867-3678

No. 51-2011

Markus Jorra

The Heterogeneity of Default Costs: Evidence from Recent Sovereign Debt Crises

Coordination: Bernd Hayo • Philipps-University Marburg
Faculty of Business Administration and Economics • Universitätsstraße 24, D-35032 Marburg
Tel: +49-6421-2823091, Fax: +49-6421-2823088, e-mail: hayo@wiwi.uni-marburg.de

Working Paper

The Heterogeneity of Default Costs: Evidence from Recent Sovereign Debt Crises

Markus Jorra*

December 30, 2011

This paper examines the costs of recent sovereign defaults using synthetic control methods, a novel econometric technique based on comparative case studies. Evidence on the effects of debt crises is thus presented on a case-by-case basis, uncovering large variations in country-specific experiences. Our estimates of cumulated output losses, e.g., range between 8.5% and 23% depending on the considered default episode. Further differences concern the persistence and likely causes of these costs. In particular, our results are consistent with the selective use of direct trade sanctions as punishment for sovereign defaults.

Journal of Economic Literature Classification Codes: F34, H63, C21, C23

Keywords: Sovereign defaults, Default costs, Case study, Synthetic control methods

*Justus Liebig University Giessen, Department of Economics and Business, Licher Straße 66, 35394 Gießen, Germany. Telephone: +49 641 99-22172. Telefax: +49 641 99-22179. E-mail: markus.jorra@wirtschaft.uni-giessen.de.

1. Introduction

International law offers little protection to creditors of sovereign borrowers who decide to repudiate their debt. Nevertheless, sovereign defaults are relatively rare and governments are often reluctant to use this option even in situations when their debt burden is clearly unsustainable. These two seemingly contradictory characteristics of sovereign debt markets have been confirmed again during the recent, still unresolved crisis in Greece. Economic theory offers a common explanation for both of them: defaults must be a costly and thus unattractive choice for sovereign borrowers. This idea has prompted a growing literature that attempts to quantify different dimensions of default costs. We argue that most of these studies share a common shortcoming as they implicitly assume that each sovereign faces identical costs in the event of default. This seems unrealistic, given that past default episodes differed in many respects, perhaps most notably in the way creditors were treated. The recently proposed voluntary restructuring of Greece's debt, e.g., was not preceded by any missed principal or interest payments. Argentina's government, by contrast, unilaterally suspended all debt service in 2001, subsequently delayed the restructuring process and finally confronted its creditors with a take-it-or-leave-it offer which implied an average haircut of more than 75%. Foreign governments and investors are likely to take these differences in debtor behavior into account when deciding on sanctions and future investments. We would thus expect to find different economic costs for both events. Our major contribution to the literature is to provide empirical evidence for this heterogeneity in default costs.

On the methodological side, we propose the application of a novel econometric technique based on comparative case studies which is ideally suited to investigate heterogeneous responses to rare events. This method, originally developed by Abadie and Gardeazabal (2003) and refined by Abadie et al. (2010) builds on the idea that counterfactual outcomes for a unit subject to some binary treatment can be estimated as a weighted average of outcomes for similar units that have not received the treatment under study. The weights are optimally chosen in a way that minimizes selection bias and mitigates endogeneity as they ensure close affinity between the treated unit and its synthetically created counterpart. The treatment effect can then be estimated as the difference between actual and hypothetical outcomes. In macroeconomic applications of this synthetic control estimator, units refer to countries and the list of already analyzed "treatments" comprises trade liberalizations (Nannicini and Billmeier, 2011), the introduction of structural reforms (Campos and Kinoshita, 2010) and the decision to join a monetary union (Sanso-Navarro, 2011) or to follow a specific monetary strategy (Lee, 2011). We add to this literature by defining a sovereign's decision to default as the relevant treatment and the associated economic costs as our outcome variables of interest. Using this definition we then offer an in-depth analysis of five recent episodes of sovereign debt crises[1] investigating both costs in terms of GDP per capita and their likely causes.

Our results support the general notion of costly sovereign defaults and our hypothesis of heterogeneity in default costs. Country-specific estimates of cumulated output losses, e.g., range between 8.5% and 23%. Further differences emerge in the medium run when the default costs either turn out to be transitory or permanent. Taken together, these two observations imply that the welfare consequences of a specific default decision might differ markedly from those of the "average default". In fact, achieving the most favorable outcome after a default might be of similar importance to a sovereign as the decision to enter or circumvent the default status in the first place.

The findings presented in this paper also point to heterogeneity in the relative importance of the different channels through which sovereign defaults might impair economic activity. Neither of the two most popular explanations for default costs, resting either on trade sanctions or on capital market exclusion fits all of the debt crises in our sample. Evidence for a significant reduction in total exports, e.g., has only been found for one of the five defaulting countries. This result does not rule out the possibility of bilateral trade sanctions whose effects may not be detected in aggregate data. Our evidence is indeed consistent with a selective use of these sanctions as we find two incidences of a stronger than average reduction in bilateral trade with former creditor countries. Support for a punishment by international capital markets in the form of reduced FDI inflows is much weaker in comparison. Here, no significant effects are found for any defaulting country in the sample.

Our work is related to several strands of the literature. On the empirical side we add to the numerous studies that investigate different dimensions of default costs. These contributions have already documented significant default costs in terms of forgone GDP growth (Sturzenegger, 2004; Borensztein and Panizza, 2009; Furceri and Zdzienicka, 2011), reduced foreign trade (Rose, 2005; Borensztein and Panizza, 2010; Martinez and

[1]The list of analyzed events includes the default episodes of Pakistan (1998 - 1999), Ecuador (1999 - 2000), Argentina (2001), Uruguay (2003) and of the Dominican Republic (2005). See subsection 3.1 for a detailed discussion of our sample.

Sandleris, 2011) and deterred inflows of private capital (Arteta and Hale, 2008; Fuentes and Saravia, 2010).[2] Common to all of these studies is the utilization of a panel regression framework in which default costs are estimated by a single coefficient on a dummy variable indicating sovereign defaults. The focus is thus on the average default costs and not on the heterogeneous nature of the responses which is the subject of our paper. However, the wide range of results obtained by studies with similar methodology but different samples may be taken as indicative of varying default costs as noted by Furceri and Zdzienicka (2011). According to their study, estimated effects of sovereign defaults on GDP growth range between five and ten percentage points, depending on the data set used.[3]

The theoretical literature on sovereign debt crises is also relevant for our research as it highlights the mechanisms through which differences in default costs might arise. Unfortunately, cost heterogeneity is seldom modeled explicitly. The seminal work of Grossman and Van Huyck (1988) which differentiates between excusable and inexcusable default is a rare exception. In their framework, only inexcusable defaults generate costs while excusable defaults remain unpunished. Following this line of thought, one is tempted to attribute heterogeneity in default costs to different debtor actions or external circumstances that influence whether a default is perceived as more or less excusable. This consideration, however, is not completely consistent with the model, which implies that punishment is not an equilibrium outcome. Another model that does not share this feature is offered by Alfaro and Kanczuk (2005). Here, heterogeneity is introduced by the presence of different types of governments whose nature is unknown to private investors. Sovereign defaults convey information to the private sector as they increase the probability that the incumbent government is of the "bad" type and thus more likely to default again in the nearby future. Incorporating this information investors consequentially demand higher interests rates which depress production in the defaulting economy. "Good" governments for which a default is optimal thus have an incentive to signal their type, thereby reducing their default costs.[4] Further, informal arguments for heterogeneity could be made in the context of theories that rest on direct punishments. The number and severity of trade sanctions imposed after a default, e.g., are likely to be

[2] See Panizza et al. (2009) for a survey of this literature.

[3] These differences in average effects, although large in economic terms, are not statistically significant. This, however, does not rule out the existence of significant differences in the costs of individual crisis episodes.

[4] In the model of Alfaro and Kanczuk (2005) the signal takes the form of a delay in the default decision. In practice, the adaption of creditor friendly policies during the restructuring process might be viewed as an additional signal which is only chosen by benign governments.

a function of debtor behavior. Differences within the group of affected creditors might introduce additional variations in default costs as some lending countries might be more inclined to sanction delinquent debtors than others.

Finally, several studies share our interest in heterogeneity but focus on the characteristics rather than on the consequences of sovereign debt crises. Differences in investor losses have been documented by Sturzenegger and Zettelmeyer (2008) and Cruces and Trebesch (2011), among others. Wright (2011) and Trebesch (2011) also reveal large variations in the length of the debt restructuring process. Enderlein et al. (2011) classify sovereign defaults according to a composite index of government behavior, uncovering both, episodes of cooperative crises resolution and cases of highly aggressive government policies. Each dimension of heterogeneity could introduce differences in default costs according to the theories discussed above. First evidence for this idea is provided by Trebesch (2010) and Cruces and Trebesch (2011). Focusing on punishment through international capital markets, their results indicate that a harsher treatment of private creditors increases default costs.

The remainder of the paper is organized as follows. Section 2 introduces the synthetic control estimator and offers a discussion of its relationship to alternative estimation techniques. Section 3 describes the selected default episodes, the measures of default costs and our choice of control variables. The results are presented in section 4. Section 5 concludes.

2. Methodology: The Synthetic Control Estimator

2.1. Basic Idea and Estimation

The synthetic control estimator first proposed by Abadie and Gardeazabal (2003) and recently refined by Abadie et al. (2010) has its roots in the comparative case study approach to policy evaluation. This method is based on the idea that causal effects of policy interventions or other events can be estimated by comparing over time outcomes for one or few treated units with those of a control group. Implicit in this approach is the assumption that the units in the control group constitute unbiased estimates of the counterfactual, i.e. the outcome we would have observed in absence of the intervention. To see this formally, let $\text{Def}_{i,t} = \{0, 1\}$ be a dummy variable indicating the treatment status of country i at time t. In the context of this study the "treatment" refers to the occurrence of a sovereign default ($\text{Def}_{i,t} = 1$). The outcome of interest, $Y_{i,t}$, is an indicator of economic activity that is related to the channels highlighted in the literature on the costs of sovereign debt crises. The default indicator's binary nature implies that there are two potential outcomes for each country at each point of time which we denote with $Y_{i,t}^{\text{def}}$ if $\text{Def}_{it} = 1$ and with $Y_{i,t}^{\text{nodef}}$ otherwise. Observed outcomes can then be expressed in terms of potential outcomes as

$$\begin{aligned} Y_{i,t} &= Y_{i,t}^{\text{nodef}} + \left(Y_{i,t}^{\text{def}} - Y_{i,t}^{\text{nodef}}\right) \text{Def}_{i,t}, \\ &= Y_{i,t}^{\text{nodef}} + \alpha_{i,t} \, \text{Def}_{i,t} \end{aligned} \tag{1}$$

for $i = 1, \ldots, J+1$ and $t = 1, \ldots, T$. The difference $\alpha_{i,t}$ between potential outcomes measures the costs of sovereign defaults for country i at time t. To simplify the exposition we now assume that only one of the $J+1$ countries in the sample is exposed to a sovereign debt crisis from time T_0 (with $1 \leq T_0 \leq T$) onwards. This country is indexed by $i = 1$:

$$\text{Def}_{i,t} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t \geq T_0 \\ 0 & \text{else} \end{cases}$$

We thus can estimate the default costs as

$$\hat{\alpha}_{1,t} = \hat{Y}_{1,t}^{\text{def}} - \hat{Y}_{1,t}^{\text{nodef}} = Y_{1,t} - \hat{Y}_{1,t}^{\text{nodef}} \quad \text{for } t \geq T_0 \tag{2}$$

which requires an estimate of the counterfactual $Y_{1,t}^{\text{nodef}}$. In a traditional comparative case study individual observed outcomes of the J countries that have not experienced a crisis in the observation period – or simple averages of them – would be used. The success of this strategy critically depends on the characteristics of the comparison unit selected or generated from the donor pool[5]. A randomly chosen country may provide a poor estimate since sovereign defaults typically occur in countries with a worse than average macroeconomic and political environment (see, e.g., Kohlscheen, 2010 or Celasun

[5]Throughout this study the term "donor pool" is used as synonym for the group of potential comparison countries for which no sovereign default was observed in the sample period.

and Harms, 2011). A selection bias will thus be introduced if these weak fundamentals influence the future path of $Y_{1,t}$ even in the absence of default, which seems likely. The work on statistical matching techniques suggests that this bias can be eliminated by ensuring that those countries constituting the counterfactual match the relevant covariates as closely as possible. This poses a difficult problem as no single country or a simple average of countries is likely to provide a satisfying fit in terms of all confounding variables.

The synthetic control estimator improves on existing methods of generating comparison units in comparative case studies by replacing subjective judgements of similarity with a data-driven procedure that mitigates selection bias. The central idea is to use a weighted average of members from the donor pool to create a "synthetic" country without a default experience. The nonnegative weights collected in the vector $\boldsymbol{W} = (w_2, \ldots, w_{J+1})'$ with $\sum_{j=2}^{J+1} w_j = 1$ are chosen in a way to ensure that the hypothetical country resembles the defaulting country as closely as possible in the period before the outbreak of the crisis. Both pre-crises realizations of the main variable of interest $(Y_{i,1}, \ldots, Y_{i,T_0-1})$ and those of other important covariates are used to assess the degree of similarity. Given the optimal weights $(w_2^*, \ldots, w_{J+1}^*)'$, the counterfactual outcome is then estimated as

$$\hat{Y}_{1,t}^{\text{nodef}} = \sum_{j=2}^{J+1} w_j^* Y_{j,t} \quad \text{for } t \geq T_0 \; . \tag{3}$$

To see how the estimator is obtained in practice, let $\boldsymbol{X}_i$ be a $(r \times 1)$ vector of observed covariates that are not affected by the sovereign default. These variables can be time-varying although this is not indicated by an additional index. Linear combinations of pre-default realizations of the outcome variable of interest are denoted as $\bar{Y}_i^{\boldsymbol{K}_m} = \sum_{s=1}^{T_0-1} k_s^m Y_{i,s}$ with the superscript $\boldsymbol{K}_m$ referring to a specific vector of weights $\boldsymbol{K}_m = (k_1^m, \ldots, k_{T_0-1}^m)'$. We use M of these linear combinations defined by the vectors $\boldsymbol{K}_1, \ldots, \boldsymbol{K}_M$ together with the variables in $\boldsymbol{X}_i$ to assess and optimize the degree of similarity between the defaulting country and its hypothetical counterpart. In principle, each pre-default value of $Y_{i,t}$ could constitute a distinct $\bar{Y}_i^{\boldsymbol{K}_m}$.[6] However, a smaller number of linear combinations – each of them measuring for example an average value of $Y_{i,t}$ for a specific subperiod – is likely to be sufficient in most applications.

[6]This would imply $M = T_0 - 1$ with $\bar{Y}_i^{\boldsymbol{K}_m} = Y_{i,m}$ for each $m = 1, \ldots, T_0 - 1$.

The $r + M = q$ relevant characteristics of the defaulting country are then arranged into a single vector $\boldsymbol{Z}_1 = \left(\boldsymbol{X}_1', \bar{Y}_1^{\boldsymbol{K}_1}, \ldots, \bar{Y}_1^{\boldsymbol{K}_M}\right)'$ with dimension $(q \times 1)$. Each column of the $(q \times J)$ matrix $\boldsymbol{Z}_0$ contains the same variables for one of the J non-defaulting countries in the donor pool. The optimal country weights collected in $\boldsymbol{W}^*$ then minimize the distance

$$\|\boldsymbol{Z}_1 - \boldsymbol{Z}_0\boldsymbol{W}\|_{\boldsymbol{v}} = \sqrt{(\boldsymbol{Z}_1 - \boldsymbol{Z}_0\boldsymbol{W})'\,\boldsymbol{V}\,(\boldsymbol{Z}_1 - \boldsymbol{Z}_0\boldsymbol{W})} \tag{4}$$

subject to $w_j \geq 0$ $(j = 2, \ldots, J + 1)$ and $\sum_{j=2}^{J+1} w_j = 1$. The optimal vector $\boldsymbol{W}^*$ thus depends on the weights of the different explanatory variables contained in the positive definite diagonal matrix $\boldsymbol{V}$. One can think of these weights as reflecting the relative importance of the different determinants of future $Y_{1,t}$. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010) we obtain the elements of $\boldsymbol{V}$ as those weights that minimize the variable of interest's root mean squared prediction error in the pre-default period.

2.2. Relation to Alternative Estimation Techniques

Most of the empirical work on the costs of sovereign debt crises is conducted using fixed-effects panel estimation techniques. Although the synthetic control estimator is much more general, it can also be expressed in terms of a linear model that highlights the similarities between both approaches. Consider the factor model

$$Y_{i,t} = \delta_t + \lambda_t\mu_i + \boldsymbol{\beta_t}\boldsymbol{X_i} + \alpha_{it}\,\mathrm{Def}_{i,t} + \epsilon_{i,t}\,. \tag{5}$$

Here, δ_t and λ_t represent unknown common factor loadings, μ_i is an unobserved country-specific effect, $\boldsymbol{\beta_t}$ is a $(1 \times r)$ vector of coefficients and $\epsilon_{i,t}$ are random disturbances. With time-invariant values for λ_t and under the additional assumption of a common crisis effect for all countries ($\alpha_{it} = \alpha_{jt}\,\forall\, i, j = 1, \ldots, J + 1$) we get the usual fixed-effects model employed for example by Borensztein and Panizza (2009). Abadie et al. (2010) show that the synthetic control estimator is valid in the more general model of equation (5) in the sense that the difference between α_{1t} and $\hat{\alpha}_{1t}$ will be close to zero if the number of pre-default periods is large relative to the scale of random disturbances and the optimal weights collected in $\boldsymbol{W}^*$ ensure

$$Y_{1,t} = \sum_{j=2}^{J+1} w_j^* Y_{j,t} \quad \text{for } t = 0, \ldots, T_0 - 1 \quad \text{and}$$

$$X_1 = \sum_{j=2}^{J+1} w_j^* X_j \ .$$

In the realistic case of a close but non-perfect fit, the above statement will hold approximately. The synthetic control estimator can thus be seen as a generalization of traditional panel methods that allows for heterogeneous treatment effects and time-varying fixed effects. This last generalization will be non-trivial if there are important determinants of economic activity that are (1) not readily observable, (2) correlated with the occurrence of sovereign debt crises and (3) subject to changes over time which have (4) a common component for all countries. Changes in the governments' general attitude towards the private sector may be one variable that fits into this description.

Allowing for a limited amount of time variability in unobserved country-specific characteristics surely mitigates issues of endogeneity that have plagued past studies on the costs of sovereign defaults. However, we should keep in mind that reverse causality cannot be ruled out completely when interpreting our estimates which should thus be considered as an upper bound for the true default costs. This problem is common to all studies in the field since its solution would require the utilization of convincing instruments. These variables that should be correlated with defaults but not with economic activity have not yet been found and might even not exist (Borensztein and Panizza, 2009).[7]

Among the studies which use the fixed-effects estimator to assess the costs of sovereign debt crises, the one by Panizza et al. (2009) is especially close to ours as it also focuses on recent country-specific experiences. Their approach consists of estimating an equation for the relevant indicator of economic activity,

$$Y_{i,t} = \delta_t + \mu_i + \epsilon_{i,t} \ ,$$

[7] Using internal instruments in a dynamic panel framework as recently done by Furceri and Zdzienicka (2011) might be one promising way to address this issue.

followed by a visual inspection of the residuals for the defaulting countries in the years around the occurrence of the debt crisis. Negative residuals following the default event are then interpreted as reflecting the crisis' costs. This approach shares with ours the advantage of allowing for country-specific default costs. However, two problems stand out that are not present in our approach: first, the neglect of covariates and second, the lack of any method for assessing the statistical significance of the estimated effects. Since we have already discussed how we account for observed determinants of economic activity in our approach, we next turn to the discussion of statistical inference.

2.3. Aggregation and Inference

The major strength of the synthetic control estimator lies in its ability to provide country-specific estimates of default costs. This contrasts with the estimation of average effects typically conducted in the literature on the consequences of sovereign debt crises. We therefore also compute average effects to allow for a comparison between the different approaches and to highlight the importance of country-specific heterogeneity. Let G be the number of default episodes in the sample. Converting the data to event time, we now denote by $\hat{a}_{1,l,g}$ the estimated individual default costs for defaulting country g ($g = 1, \ldots, G$) l years after the outbreak of the debt crisis ($l = 0, \ldots, T - T_0$). The average effect of a debt crisis on economic activity $\bar{a}_l$ is then simply estimated as

$$\hat{\bar{a}}_l = \frac{1}{G} \sum_{g=1}^{G} \hat{a}_{1,l,g} \ . \tag{6}$$

Statistical inference is assessed in the present framework by means of placebo studies (Abadie et al., 2010). For a single default episode g this involves applying the synthetic control estimator to all countries in the donor pool as if they had experienced a debt crisis in $l = 0$. The defaulting country is returned to the pool of potential comparison units for this exercise. For each $l = 0, \ldots, T - T_0$ this results in J^g estimated pseudo-default costs $\hat{a}^{PL}_{i,l,g}$ with $i = 2, \ldots, J^g + 1$.[8] We expect to find an average value of $\hat{a}^{PL}_{i,l,g}$ that is close to zero since there is no common event for these countries. The accumulation of random country-specific events, however, obviously leads to a growing dispersion of

[8]The superscript g indicates that the number of comparison units might vary between the different case studies. We will see in subsection 3.2 that this is indeed relevant in our application as more countries become eligible to enter the donor pool for the latest default episodes.

placebo effects for increasing l. Our estimates of the true default costs $\hat{a}_{1,l,g}$ should not look exceptional when compared to these placebo estimates if the null hypothesis of no default costs were correct. The finding of abnormally negative effects for the defaulting country can thus be interpreted as evidence for significant default costs.

Cavallo et al. (2010) generalize the idea of placebo studies by applying it to the evaluation of average treatment effects. In the context of our study their approach leads to the calculation of time specific p-values for the average default costs. This involves a three-step procedure that consists of

1. Conducting for each sovereign default g all J^g placebo studies as outlined above.
2. Calculating for each $l = 0, \dots, T - T_0$ every possible average placebo effect $\hat{\bar{a}}_{l,s}^{PL}$, $s = 1, \dots, N$. These are computed by selecting one of the J^g event-time specific estimates of pseudo-default costs $\hat{a}_{i(g),l,g}^{PL}$ for each default study g which are then averaged:[9]

$$\hat{\bar{a}}_{l,s}^{PL} = \frac{1}{G} \sum_{g=1}^{G} \hat{a}_{i(g),l,g}^{PL} \ .$$

   The number of possible averages is given by $N = \prod_{g=1}^{G} J^g$.
3. Computing the p-value for each post-crisis period l as:

$$\begin{aligned} \text{p-value}_l &= \Pr\left(\bar{a}_l^{Pl} < \hat{\bar{a}}_l\right) \\ &= \frac{\sum_{s=1}^{N} 1\left[\hat{\bar{a}}_{l,s}^{PL} < \hat{\bar{a}}_l\right]}{N} \ . \end{aligned} \tag{7}$$

   The p-value thus measures the probability of observing a drop in average economic activity larger than $\hat{\bar{a}}_l$ under the null hypothesis of zero default costs.

[9]Here, the notation $i(g)$ indicates that the index number of the chosen placebo studies need not be the same for all G default episodes. In fact, averages are calculate for all possible combinations of country-specific pseudo default costs.

3. Data Issues

3.1. Case Study Selection

We investigate the costs of sovereign defaults for five emerging market economies that suffered from debt crises during the past fourteen years. Our selection covers the spectacular default of Argentina in 2001 that had not been resolved until 2005 as well as the perhaps less well known default episodes of Pakistan (1998 - 1999), Ecuador (1999 - 2000), Uruguay (2003) and the Dominican Republic (2005). We do not include the Russian (1998 - 2000), Ukrainian (1998 - 2000) or Moldovian (2002) debt crises since the economic performance of these countries is heavily influenced by their transition from centrally planned to market economies prior to default. The synthetic control estimator is not well suited to deal with such additional events in the pre-treatment period that are not shared with the majority of countries in the control group. The temporal clustering of crises in the nineteen eighties and the consequential reduction in the number of admissible comparison countries without a debt crisis also prevented us from considering earlier default episodes.[10] Our sample of defaults is thus a subset of the one analyzed by Panizza et al. (2009), allowing a comparison of the results obtained by standard panel and our synthetic control methods.

≪ insert Table 1 here ≫

Table 1 contains some key characteristics of the default events in our sample. Several differences between theses episodes stand out. First, there is considerable variation in the length of the restructuring process. While the defaults of Uruguay and the Dominican Republic were resolved relatively smoothly within the year of default, a settlement with the creditors was delayed for up to four years in the remaining cases. The length of the default period may have important implications for the crises' cost since sovereigns typically can not tap debt markets until several years have elapsed after the settlement (Gelos et al., 2011).

Second, defaulting countries also differ in terms of the harshness with which they confronted their creditors. Uruguay, e.g., reached an agreement with its bondholders before

[10] We do not include more recent debt crises like the one of Ecuador (2008 - 2009) or the Seychelles (2008 - 2010) either. Here, the reason is a lack of sufficient post-default data points.

any payment was missed and imposed only a moderate haircut of 9.8 percent. This benign stance is also reflected in the index of coerciveness developed by Enderlein et al. (2011) where Uruguay obtained the lowest possible score.[11] This contrasts most sharply with the experience of Argentina which unilaterally declared a suspension of all debt payments in 2001 and afterwards achieved a restructuring deal that left investors with compensation worth less than 25 percent of their original claims. Again, this behavior is also mirrored in the index of coerciveness where Argentina reached one of the highest scores ever recorded. The three other debt crises range between these two polar cases with the Dominican Republic acting relatively more creditor friendly than Pakistan and Ecuador while in default. It seems obvious that these differences in debtor behavior should have consequences for the costs of debt crises, especially when direct forms of punishment, e.g. through trade sanctions, are involved. The finding that higher haircuts increase both borrowing costs and the length of market exclusion documented by Cruces and Trebesch (2011) can be seen as first evidence supporting this idea.

Finally, there is also cross-country variation in the governments' fiscal balance before the outbreak of a crisis. The initial fiscal position might matter since larger deficits prior to the default increase the necessary adjustment effort when external funding dries up. Since most of the empirical evidence now supports the view that fiscal consolidations are contractionary (Pescatori et al., 2011), and especially so during financial crises (Hutchison et al., 2010), we would expect to find larger output losses for high-deficit countries like Pakistan, Uruguay and Argentina.

Given these differences and their likely implications, it seems questionable whether the cost of debt crises can be adequately captured by a common crisis dummy in a panel regression. The case study approach pursued in this study seems better suited to address issues of country-specific heterogeneity.

To estimate the case specific costs of the five recent crises mentioned above, the synthetic control estimator requires a control group of similar countries that have not been exposed to a sovereign default. We consider all emerging market economies rated by Standard & Poor's as potential members of this donor pool.[12] From these countries we discard

[11] Ranging from 1 (investor friendly) to 10 (completely uncooperative) this index provides a classification of governments actions during sovereign debt disputes. We thank Christoph Trebesch for sharing this data with us.

[12] We define those countries as emerging market economies that do not fall into the World Bank's OECD or non OECD high income classification. We further add the Central and Eastern European transition economies since their graduation to developed country status took place after most of the debt crises in our sample occurred. Major oil-exporting countries are not considered.

those with a default event as defined by Standard & Poor's in the case-study specific pre- or post-treatment period. The relevant time window starts nine years before the occurrence of the sovereign default under study and ends either in 2009 or up to seven years after this event.[13] To broaden the country base in the presence of the long-lasting defaults of the nineteen eighties and nineteen nineties we also consider those countries as eligible to enter the donor pool that had a pending default for more than five years at the beginning of the event window. Since sovereign debt crises were relatively rare in the last two decades, our criterion for exclusion leads to a growing number of comparison countries for the more recent default episodes. Appendix A illustrates the changing composition of the donor pool for our analysis of the effect of debt crises on GDP per capita. Similar trends can be observed for our other indicators of default costs although small differences are possible due to variations in data availability on our variables of interest.

3.2. Dependent and Control Variables

Our choice of dependent variables reflects the two different objectives which we pursue in this paper. We first want to provide country-specific estimates of the output losses incurred during the default episodes. These are obtained by comparing the evolution of the defaulting countries' logarithmized GDP per capita (*lngdp*) with those of their respective synthetically created counterparts.[14] The difference between both values thus approximates at each point in time the percentage output loss due to the crisis. Since our estimation window stretches several years after the resolution of each crisis we can also assess whether defaults are followed by rapid recoveries (Yeyati and Panizza, 2011) or by persistently lower levels of GDP as documented for other financial crises (Cerra and Saxena, 2008).

Second, we also try to shed some light on the relative importance of the different channels through which sovereign defaults might impair economic activity. Here, we analyze two channels that have received much attention in the literature, one operating through

[13]The length of the pre-default period reflects a compromise between the time-series and cross-sectional dimension of our sample. An increase in the length of the matching period reduces the number of comparison units in the donor pool as fewer countries meet the no-default criterion. Our results, however, are robust to alternative time windows.

[14] The analysis of GDP levels instead of growth rates is common in macroeconomic applications of the synthetic control estimator, see e.g. Abadie and Gardeazabal (2003) or Nannicini and Billmeier (2011).

a decline in trade (Rose, 2005; Martinez and Sandleris, 2011) and the other through a reduction in private capital flows (Fuentes and Saravia, 2010). More precisely, we focus on the effect of debt crises on the defaulting countries' exports and FDI inflows. Government sanctions or a reassessment of credit and expropriation risk by the private sector could provide the link between sovereign defaults and economic activity in either case.

We employ two different approaches to investigate the importance of the outlined channels. For both exports and FDI flows we use aggregate variables (*lnexports* and *lnfdi*), standardized in the same way as the GDP series since these measures should be the most important ones from the defaulting country's point of view. However, since losses due to the default are typically not shared equally among creditors of all countries coordinated sanctions also seem unlikely. Isolated actions by important creditor governments may be a more plausible outcome that does not show up in aggregate data. One way to deal with this problem in the present framework would be to analyze the fraction of total exports (FDI inflows) that is directed to (originates from) the affected creditor countries. Since information on the origins of private creditors is not available we have to resort to data on affected official creditors for these exercises. This data is available for those three countries in our sample (the Dominican Republic, Ecuador and Pakistan) that also renegotiated official debt through the Paris Club during their default period.[15] We further restrict our attention to the effects of sovereign defaults on export shares (*rel_exports*). A similar analysis was precluded for FDI flows as too many of the relevant data points are missing in standard bilateral FDI databases like the OECD's "International Direct Investment Statistics Yearbook".

Our choice of control variables has been guided by the related literature. Specifically, we follow Nannicini and Billmeier (2011) and consider the population growth rate (n), the physical investment share (s) and the average years of secondary schooling (*av2school*) as important determinants of GDP per capita. We further control for the political regime by including the popular Polity 2 variable (*polity2*) from the Polity IV database since political institutions might matter for both, the probability and the effects of financial crises (Cavallo and Cavallo, 2010).

In our export specification we try to control for the effect of changing commodity prices that might have exerted a positive influence on export performance during some of

[15] The same data has also been used in related studies by Rose (2005), Fuentes and Saravia (2010) and Martinez and Sandleris (2011).

the recent debt crises (Panizza et al., 2009). We attempt to achieve this by adding two measures of commodity dependence to our set of control variables, assuming that countries with similar export shares of agricultural (*agr*) and fossil products (*fossils*) also react similar to changing prices. Following the large literature on the gravity model of international trade we further include a measure of geographical distances. We choose the log of the average distance between the defaulting country and its creditors (*lndist*) when analyzing the effect of debt crises on the fraction of total exports directed to these countries. In our study of total exports per capita this variable is replaced by a measure of latitude (*latitude*) as an aggregate indicator of a country's geographical position.

Finally, we follow Naude and Krugell (2007) and include a measure of ethnic fractionalization (*ethnic*) as an indicator for social cohesion and conflict in addition to the already described proxies of human capital (*av2school*) and geography (*latitude*) in our FDI specification. We further add the numbers of mobile cellular subscriptions (*mobiles*) and telephone lines (*telephones*) to our matching criteria. Both variables approximate the physical capital stock as another important determinant of expected returns on FDI. Appendix B contains detailed information on the construction and the data sources for all included dependent and explanatory variables.

4. Results and Discussion

4.1. Sovereign Defaults and Overall Economic Development

We start our empirical investigation by analyzing the effect of debt crises on GDP per capita. Our set of predictor variables for future economic activity consists of pre-default averages of the control variables discussed in subsection 3.2 augmented with some linear combinations of the dependent variable. For the exogenous variables, average values are calculated over an eight year window that starts nine years before the occurrence of a sovereign default. The year that precedes the default event is thus excluded from the matching period and serves as intervention date T_0. This timing assumption suggested by Abadie et al. (2010) allows us to control for the costs introduced by the anticipation of the crisis. For the dependent variable, we use the last two values of the pre-intervention period ($T_0 - 1$ and $T_0 - 2$) together with two subperiod averages (calculated over $[T_0 - 3, T_0 - 5]$ and $[T_0 - 6, T_0 - 8]$) as additional matching criteria for the synthetic control estimator.

Table 2 shows the results of the optimization procedure.[16] A comparison of the predictor variables for the defaulting countries to those of their synthetically created counterparts depicts a quite reasonable in-sample fit for all five countries. For those measures based on the lagged dependent variable, differences are especially small and typically in the range of one to two percent. However, the achieved degree of similarity is also satisfying when judged by the other control variables, in particular when the large degree of heterogeneity in the donor pool is considered.[17] Examining the dependent variable's root mean squared prediction error (RMSPE) further supports our impression of the goodness of fit although the results are somewhat weaker for Ecuador and Uruguay than for the other three countries.

≪ insert Table 2 here ≫

The last rows in Table 2 list those countries from the donor pool that constitute the estimated counterfactual.[18] The choice of comparison units without a sufficient individual degree of similarity to the treated unit poses the risk of introducing an interpolation bias (Abadie et al., 2010) that might have been not completely absent in past macroeconomic applications of the synthetic control estimator. A glance at the countries which are chosen by the optimization routine mitigates these concerns in the present study. Most control countries are either characterized by geographical proximity to the respective defaulting country (e.g. countries from Latin America account for 60 % of synthetic Argentina's country weights, India is the second most important contributor to Pakistan's control group) or seem at least similarly exposed to the risk of sovereign debt crises.

A visual impression of the degree of similarity between the actual and synthetic defaulting countries prior and after the default can be gauged from Figure 1. Here, solid lines depict the actual time paths of the logarithmized GDP per capita series for the five crises countries. Maybe with the exception of Ecuador, these are at first closely tracked by the dashed lines corresponding to the outcome of the same variable for the synthetically created comparison countries. As it is required for admissible estimates of the counterfactual outcomes, the achieved degree of synchronization seems high even at business cycle frequencies during the pre-crises periods. Actual and synthetic outcomes

[16] All optimizations are conducted in STATA using the synth routine developed by Abadie et al. (2010).

[17] In the case of Argentina (2001) country-specific values in the donor pool ranged between −0.4 and 2.66 for the population growth rate, between 12.96 and 41.91 for the investment share, between 0.73 and 4.25 years for schooling variable and between −6.75 and 10 for the polity score.

[18] Only the five most important countries with an individual weight of a least one percent are shown.

diverge after the end of the matching period. In each of the five cases, actual GDP per capital drops below its estimated counterfactual in the year preceding the default (indicated by the first vertical line) and stays there in the default year (second vertical line).

≪ insert Figure 1 here ≫

Closer inspection of the individual graphs in Figure 1 also reveals some striking differences between the five default episodes. First, there are large variations in the level of default costs. The maximum difference between actual and counterfactual GDP per capita ranges between 8.5% (Dominican Republic) and 23.1% (Pakistan). While seemingly large, the size of these estimates are in line with those obtained by Panizza et al. (2009) using the alternative techniques described in subsection 2.2.[19] Second, differences can be observed regarding the time path of default costs. In three of the five cases the bulk of the costs was incurred in the year prior to the default. Only Argentina and Ecuador suffered more in the default year than in the period of looming debt crises. These differences may be due to the fact that both countries defaulted relatively late in the year – in the fourth and third quarter respectively – as stressed by Yeyati and Panizza (2011). However, it also seems possible that these experiences at least partly reflect the harsh treatment of creditors by the Argentinian and Ecuadorian governments. A related observation is that only the Dominican Republic and Uruguay, the two countries that were ranked the most creditor friendly in Table 1, were able to recuperate the output loss completely in the years after the crisis. Output losses seem more or less permanent in the remaining three countries although they also experienced periods of strong growth in GDP per capita starting several years after the default. These developments, however, also show up in the estimated counterfactuals suggesting that they are not related to possibly benign effects of the crises, brought about e.g., through a reduction of the debt burden.

≪ insert Figure 2 here ≫

Figure 2 serves two purposes. It facilitates the assessment of the magnitude of the default costs by displaying the difference between the actual and the estimated counterfactual

[19] See Figure 7 of Panizza et al. (2009) which shows the estimated default costs in terms of GDP growth rates. Adding up their period-specific estimates leads to results that are directly comparable to ours.

outcome for the dependent variable (thick black lines). Even more importantly, it offers a way to check whether these differences are statistically significant. As explained in subsection 2.3 this is done by means of placebo studies whose results are displayed in grey. Each line represents the estimated pseudo-default costs for one of the countries in the donor pool that had not experienced a sovereign default in the sample period. Following Abadie et al. (2010) only results for those placebo studies with a pre-intervention fit similar to the one obtained for the defaulting country are shown. More precisely, we exclude all countries that had a mean square prediction error (MSPE) of more than five times the MSPE of the respective defaulting country. This procedure is based on the reasoning that countries which could not be synthetically replicated during the matching period are also likely to suffer from the same problems out of sample. Placebo costs calculated from these studies are therefore not well suited to evaluate the likelihood that a given decline in economic activity occurs randomly in one of the better fitted defaulting countries.

Looking at the results presented in Figure 2 we can infer that the development of GDP per capita is indeed unusual after the outbreak of a sovereign debt crisis. Significant effects are always found in the default year when the estimated default costs are at the lower boundary of the range spanned by the placebo studies. Statistical significance diminishes when later dates are considered although strong effects are still found for Pakistan even eight years after the default. This has to be expected given that the effect of the debt crisis levels out or is even reversed while the dispersion of placebo effects naturally increases with the time elapsed since the end of the matching period.

≪ insert Figure 3 here ≫

Figure 3 translates the country-specific experiences into the average effects typically reported in the literature. As discussed in subsection 2.3 this requires a conversion of calendar into event time since the five defaults occurred in different years. The values on the horizontal axis in both graphs therefore refer to the number of years that have passed since the default year (*eyears*). Point estimates of the average default costs are depicted by the solid line in the left panel. According to these estimates, GDP per capita drops by 10.6% on average one year prior to a sovereign default relative to the counterfactual situation without a debt crisis. The difference between hypothetical and actual average GDP per capita continues to grow for two additional years when it reaches a maximum value of 14.1%. The "typical" debt crisis is then followed by a slow V-shaped recovery.

However, even seven years after the default average GDP per capita still falls short of its counterfactual level.

The right panel of Figure 3 tells us that the null hypothesis of zero average default costs can be rejected at conventional levels for most of the years. The reduction in GDP per capita relative to the estimated counterfactual is significant at the one percent level in the four years around the default (from eyear = -1 to 2), at the five percent level in the third, and at the ten percent level in the fourth, fifth and seventh year after the default.[20] Our evidence is therefore in line with previous studies which mostly document sizeable and statistically significant average default costs (see, e.g., Borensztein and Panizza (2009) or Furceri and Zdzienicka (2011)). The focus on average effects, however, might be misleading as it masks the country-specific heterogeneity which is apparently present in our sample. To highlight this point once more, we plot the individually estimated country-specific default costs along with their average in the left graph. The documented deviations from the average default costs are clearly not of second-order importance from the perspective of a policy maker dealing with an emerging sovereign debt crisis.

4.2. Sovereign Defaults and Exports

The documented contemporaneous decline in international trade has been repeatedly put forward as an explanation for the poor economic performance of many countries suffering from a sovereign debt crisis (Rose, 2005; Martinez and Sandleris, 2011). We therefore investigate whether this channel also operated in our sample of recent defaults by applying the synthetic control estimator to the exports per capita series as our preferred measure of foreign trade. Following Martinez and Sandleris (2011) we also contribute to the discussion on the importance of direct sanctions as explanation for the changes in trade patterns observed in the aftermath of sovereign debt crises. As these are more likely to be imposed by creditor countries, we conjecture that their bilateral trade with the defaulting country is more strongly affected than its trade with other countries. Hence, we also analyze the fraction of total exports that is directed to creditor countries. We discuss the results for each of the two specifications in turn.

[20] The increase in statistical significance of the estimated average effect in the seventh year is partly due to the fact that Uruguay and the Dominican Republic – the two countries with the fastest recovery – drop out of the calculation as they defaulted relatively late in the sample period.

Table 3 shows the optimal country weights and the resulting fit in the pre-default period for the exports per capita specification. It is evident from the first three rows that the relevant exogenous variables have not been as closely matched as those influencing GDP per capita discussed earlier. This can be partly explained by the weighting scheme employed by the synthetic control estimator which tolerates larger deviations for matching variables with relative low predictive power for the dependent variable prior to the default event.[21] This effect is intended as it is accompanied by an improved fit for variables that are likely to be strong predictors of future exports per capita. Another possible explanation points to the high degree of export specialization documented for Argentina, Ecuador and Uruguay. Their large export shares of commodity related products could not be reproduced by convex combinations of the non-defaulting countries. An imperfect matching of these variables is therefore technically inevitable.

Notwithstanding these caveats, the country weights chosen by the optimization routine and displayed in the last rows of Table 3 still seem reasonable in terms of geographic and economic proximity. For all five countries, pre-default realizations of the dependent variable are also closely matched by their synthetically created counterparts although the weakest fit is again obtained for Uruguay when measured by the RMSPE. This impression is confirmed by Figure 4 which shows for each country the difference between the log of actual and hypothetical exports per capita (thick black line). Only small fluctuations around zero can be observed prior to the years preceding the default events. This pattern changes with the outbreak of the debt crises after which exports per capita persistently fall short of their estimated counterfactual values in four of the five economies. The only exception is Uruguay whose estimated counterfactual is dominated by the poor performance of Jamaica, its most important constituent. However, heterogeneity in the country-specific reactions is evident even among the four countries for which the point estimates indicate that the effect of sovereign defaults on international trade is negative. Four years after the default the percentage deviation from the estimated counterfactual level of exports per capita ranges from 5% for Ecuador to close to 50% for Argentina.

Figure 4 also shows the output from the placebo studies for each of the five crises. The negative effects found for Ecuador, Pakistan and, depending on the considered year, also those for the Dominican Republic, turn out to be insignificant according to these results. Only Argentina's underperformance relative to its estimated counterfactual consistently appears exceptional when compared to the set of outcomes for countries not affected by

[21]Technically this corresponds to smaller weights of these variables in the weighting matrix $\boldsymbol{V}$.

a sovereign debt crisis. This result is noteworthy as it contrasts with those of Panizza et al. (2009) who find a strong *increase* in Argentina's exports starting shortly after the default.[22] Their approach, which only controls for a common time trend and country-specific effects probably confuses the commodity driven regional export boom with the effect of Argentina's debt crisis. The synthetic control estimator is much better suited to capture these confounding effects. Indeed, our results indicate that Argentina's export performance would have been even better in the absence of its default in 2001.

Considered together, our five case studies suggest that the average effect of sovereign defaults on foreign trade, depicted in Figure 5, is quite small. In the default year, average exports per capita are 6.6 percent below their estimated counterfactual level. This value increases to 14.9 percent in the third year after the default after which the gap between actual and hypothetical export per capita narrows. Only the effects in the year before and two years after the default are statistically significant at the 10 percent level. Our estimates of the trade related default costs are thus far below those reported in the literature. Martinez and Sandleris (2011), e.g., find an average impact of sovereign debt crises on trade of -6.5% in each of the first five years after the default which corresponds to an aggregate negative effect of more than 30 percent in our framework. When comparing these findings one has to consider that our results on average effects are quite sensitive to outliers as the sample of default events is admittedly small. Excluding Uruguay, whose post-default experience has clearly been exceptional, would lead to estimated average effects of similar magnitude to those in the literature.[23]

We next turn to the question whether the developments in aggregate trade patterns around the default events considered so far mask cases of bilateral punishment by creditor countries, e.g. through trade sanctions. If this were the case, we would expect the fraction of total exports directed to creditor countries to decline after a sovereign default. Table 4 documents the in-sample properties for a specification that uses this indicator as dependent variable. The export fractions prior to the default have been reproduced almost perfectly for each of the three countries in our sample.[24] In the case of the Dominican Republic, whose creditors have also been responsible for 90% of its exports, this could only be achieved by selecting a single country, Mexico, as comparison unit.

[22] This can be inferred from Figure 6 on page 37 of their publication.

[23] Without Uruguay, the estimated average effect of sovereign debt crises on exports per capita is 28.6% five years after the default with a p-value of 0.06.

[24] As discussed in subsection 3.2, our sample of default events shrinks since data on affected creditor countries is only available for those three countries which also rescheduled their debt through the Paris Club.

The weights are more evenly distributed for Ecuador and Pakistan where the largest weights have again been assigned to countries from the same region. The good fit of our distance variable also reflects this geographic proximity.

Figure 6 and Figure 7 depict our estimates of the individual and average effects of debt crises on our dependent variable. Again, no common pattern emerges for all three countries. Instead, the individual country experiences comprise (1) a persistent decline in the fraction of exports directed to creditor countries (Dominican Republic), (2) a temporary negative effect on export shares (Ecuador), and (3) the absence of any effect (Pakistan). Both the transitory and the persistent effect are found to be significant, as only 1 out of the 14 (36) placebo experiments generated negative trajectories of larger absolute size than those found for Ecuador (the Dominican Republic). The heterogeneity could have been expected given that the three countries not only differ in their own actions during the debt crises, but also defaulted on different groups of creditors. It is easy to imagine that some countries are more inclined to sanction delinquent debtors than others, thereby making different experiences after debt crises even more likely. When these considerations are ignored in favor of an emphasis on average effects, one finds a significant decline in the fraction of exports directed to creditor countries in the first four years following a sovereign default. The maximum decline of 9.7 percentage points is observed two years after the default with export market shares trending back towards their pre-crisis levels afterwards.[25] Overall, our results are consistent with the idea that direct trade sanctions are at least used selectively to punish defaulting sovereigns. This contrasts with the findings of Martinez and Sandleris (2011). Relying on estimated average effects, they do not report any evidence in favor for the sanctioning hypothesis.

4.3. Sovereign Defaults and Foreign Direct Investment

Dating back to Eaton and Gersovitz (1981) capital market exclusion is perhaps the most common form of punishment considered in the theoretical literature on sovereign debt and default. Financial autarky after a default is associated with two different types of economic costs as it implies both, forgone benefits from intertemporal consumption smoothing and a shortfall of funds needed to finance foreign inputs to domestic production. Naturally, these costs are more severe if the private sector is also cut off from

[25] The strength of the average recovery might be overstated as the jump in the export ratio in the fifth year after the default predominantly reflects the fact that Uruguay drops out of the calculation.

international capital markets after a sovereign default. Mendoza and Yue (2011) show that key features of emerging market business cycles, among them the sharp reduction in GDP observed during sovereign debt crises, can be replicated in a general equilibrium model under these assumptions. In the following, we analyze whether a punishment through international capital markets has been present during recent episodes of debt crises. We follow Fuentes and Saravia (2010) and focus on the reaction of foreign direct investment as it is presumably the most beneficial type of capital inflows (Stiglitz, 2000).

Inspection of Table 5 reveals that the defaulting countries' net FDI inflows per capita series have been much more difficult to replicate by the synthetic control estimator than our other indicators of default costs. The fit is especially poor for Argentina, the Dominican Republic and Uruguay for which the in-sample RMSPE exceeds 10 percent. We therefore focus our discussion on the experiences of Ecuador and Pakistan which have been fit reasonably well, both in terms of the dependent and control variables.

It can be inferred from Figure 8 that we do not find a significant reduction in FDI per capita for either of the two countries during the first few years after the default.[26] While predicted and actual FDI per capita virtually coincide in the case of Pakistan even after the default we merely observe a decreased post-default fit for Ecuador without a clear tendency towards lower than expected FDI inflows.[27] Given this general pattern, it seems unlikely that the significant negative effect found for Ecuador in 2006 can be attributed to its default in 1999. It seems more plausible that the unpredicted reduction in FDI inflows reflects an increase in political risk caused by Ecuador's 2006 presidential election which saw the victory of Rafael Correa who actively campaigned for a new debt restructuring (Hatchondo et al., 2009).

Unsurprisingly, the individual experiences are also reflected in the estimated average effects. Although strictly negative, the point estimates shown in Figure 9 are never statistically significant during the first 6 years after a sovereign default. This contrasts with the results of Fuentes and Saravia (2010) who find a significant negative impact of sovereign defaults on FDI inflows that is largely driven by a decline in funds from

[26]We do, however, find a consistently negative and mostly significant effect for Argentina. We do not stress this result as it might well be produced by pure chance given the weak model fit prior to the default.

[27]Obviously, the effect is also always insignificant for Pakistan. Note that the excellent fit of Pakistan's FDI per capita series in the pre-intervention period would lead to the exclusion of all placebo studies if the usual hurdle rate (5 times the defaulting countries' MSPE) were applied. We therefore use an alternative criterion for exclusion based on the average MSPE measured for Argentina, the Dominican Republic, Ecuador and Uruguay in their respective pre-intervention periods.

creditor countries directly affected by the default. This last finding might well explain the differences to our results as a lack of data availability on bilateral FDI flows precludes us from investigating host country-specific reactions to sovereign debt crises in a way similar to our analysis of export shares in subsection 4.2.

5. Conclusion

Information on the costs of sovereign defaults is clearly valuable from a policy perspective, especially during a time when looming debt crises threaten a growing number of developed economies. Previous attempts to address this issue have already documented a sizable negative impact of defaults on GDP growth, foreign trade and access to capital markets. We contribute to this literature by offering country-specific estimates of default costs using a novel econometric technique based on comparative case studies. The key advantage of this approach, its flexibility in dealing with heterogeneity, has proved especially useful in the present application, as our results reveal considerable variation in the costs of crises which has not been documented in the literature so far. Our estimates of cumulated output losses, e.g., range between 8.5% and 23% depending on the considered default episode. Further differences emerge in the medium run when the default costs either turn out transitory or permanent. This heterogeneity might reflect a varying degree of punishment by trading partners and investors who also seem to differentiate between default events.

In light of the large variation in the characteristics of the considered default episodes, heterogeneity in default costs comes as no surprise. Differences in the necessary fiscal adjustment effort and in the details of the restructuring process are among the possible explanations. A logical next step would be to investigate this issue more formally. Extending the sample of estimated default costs and analyzing their determinants econometrically thus seems a promising avenue for future research.

References

Abadie, A., Diamond, A. and Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. *Journal of the American Statistical Association*, **105** (490), 493–505.

— and Gardeazabal, J. (2003). The economic costs of conflict: A case study of the Basque country. *The American Economic Review*, **93** (1), pp. 113–132.

Alesina, A., Devleeschauwer, A., Easterly, W., Kurlat, S. and Wacziarg, R. (2003). Fractionalization. *Journal of Economic Growth*, **8**, 155–194.

Alfaro, L. and Kanczuk, F. (2005). Sovereign debt as a contingent claim: A quantitative approach. *Journal of International Economics*, **65** (2), 297–314.

Arteta, C. and Hale, G. (2008). Sovereign debt crises and credit to the private sector. *Journal of International Economics*, **74** (1), 53–69.

Barro, R. J. and Lee, J.-W. (2010). *A New Data Set of Educational Attainment in the World, 1950–2010*. NBER Working Paper 15902, National Bureau of Economic Research.

Borensztein, E. and Panizza, U. (2009). The costs of sovereign default. *IMF Staff Papers*, **56** (4), 683–741.

— and — (2010). Do sovereign defaults hurt exporters? *Open Economies Review*, **21**, 393–412, 10.1007/s11079-008-9096-2.

Campos, N. F. and Kinoshita, Y. (2010). Structural reforms, financial liberalization, and foreign direct investment. *IMF Staff Papers*, **57** (2), 326–365.

Cavallo, A. F. and Cavallo, E. A. (2010). Are crises good for long-term growth? The role of political institutions. *Journal of Macroeconomics*, **32** (3), 838–857.

Cavallo, E. A., Galiani, S., Noy, I. and Pantano, J. (2010). *Catastrophic natural disasters and economic growth*. IDB Working Paper 124, Inter-American Development Bank.

Celasun, O. and Harms, P. (2011). Boon or burden? The effect of private sector debt on the risk of sovereign default in developing countries. *Economic Inquiry*, **49** (1), 70–88.

Centre d'études prospectives et d'informations internationales (2011). dist_cepii database. http://www.cepii.fr/anglaisgraph/bdd/distances.htm.

Cerra, V. and Saxena, S. C. (2008). Growth dynamics: The myth of economic recovery. *The American Economic Review*, **98** (1), 439–457.

Cruces, J. and Trebesch, C. (2011). *Sovereign Defaults: The Price of Haircuts*. CESifo Working Paper 3604, CESifo Group Munich.

Eaton, J. and Gersovitz, M. (1981). Debt with potential repudiation: Theoretical and empirical analysis. *The Review of Economic Studies*, **48** (2), 289–309.

Enderlein, H., Trebesch, C. and von Daniels, L. (2011). Sovereign debt disputes. *Journal of International Money and Finance*, forthcoming.

Fuentes, M. and Saravia, D. (2010). Sovereign defaulters: Do international capital markets punish them? *Journal of Development Economics*, **91** (2), 336–347.

Furceri, D. and Zdzienicka, A. (2011). *How costly are debt crises?* MPRA Paper 30953, University Library of Munich, Germany.

Gelos, R. G., Sahay, R. and Sandleris, G. (2011). Sovereign borrowing by developing countries: What determines market access? *Journal of International Economics*, **83** (2), 243–254.

Grossman, H. I. and Van Huyck, J. B. (1988). Sovereign debt as a contingent claim: Excusable default, repudiation, and reputation. *The American Economic Review*, **78** (5), 1088–1097.

Hatchondo, J. C., Martinez, L. and Sapriza, H. (2009). Heterogeneous borrowers in quantitative models of sovereign default. *International Economic Review*, **50** (4), 1129–1151.

Heston, A., Summers, R. and Aten, B. (2011). Penn World Table Version 7.0. http://pwt.econ.upenn.edu/php_site/pwt_index.php, Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania.

Hutchison, M. M., Noy, I. and Wang, L. (2010). Fiscal and monetary policies and the cost of sudden stops. *Journal of International Money and Finance*, **29** (6), 973–987.

International Monetary Fund (2011a). Direction of Trade Statistics. Source: Thomson Reuters Datastream.

International Monetary Fund (2011b). World Economic Outlook Database. http://www.imf.org/external/ns/cs.aspx?id=28.

Kohlscheen, E. (2010). Sovereign risk: Constitutions rule. *Oxford Economic Papers*, **62** (1), 62–85.

La Porta, R., Lopez-de Silanes, F., Shleifer, A. and Vishny, R. (1999). The quality of government. *Journal of Law, Economics, and Organization*, **15** (1), 222–279.

Lee, W.-S. (2011). Comparative case studies of the effects of inflation targeting in emerging economies. *Oxford Economic Papers*, **63** (2), 375–397.

Martinez, J. V. and Sandleris, G. (2011). Is it punishment? Sovereign defaults and the decline in trade. *Journal of International Money and Finance*, **30** (6), 909–930.

Mendoza, E. and Yue, V. (2011). A general equilibrium model of sovereign default and business cycles. *Quarterly Journal of Economics*, forthcoming.

Nannicini, T. and Billmeier, A. (2011). Economies in transition: How important is trade openness for growth? *Oxford Bulletin of Economics and Statistics*, **73** (3), 287–314.

Naude, W. A. and Krugell, W. F. (2007). Investigating geography and institutions as determinants of foreign direct investment in Africa using panel data. *Applied Economics*, **39** (10), 1223–1233.

Panizza, U., Sturzenegger, F. and Zettelmeyer, J. (2009). The economics and law of sovereign debt and default. *Journal of Economic Literature*, **47** (3), 651–98.

Pescatori, A., Leigh, D. and Guajardo, J. (2011). *Expansionary Austerity: New International Evidence*. IMF Working Paper 11/158, International Monetary Fund.

Polity IV (2009). Political regime characteristics and transitions, 1800-2009. http://www.systemicpeace.org/polity/polity4.htm.

Rose, A. (2005). One reason countries pay their debts: Renegotiation and international trade. *Journal of Development Economics*, **77**, 189–206.

SANSO-NAVARRO, M. (2011). The effects on American foreign direct investment in the United Kingdom from not adopting the euro. *Journal of Common Market Studies*, **49** (2), 463–483.

STIGLITZ, J. E. (2000). Capital market liberalization, economic growth, and instability. *World Development*, **28** (6), 1075–1086.

STURZENEGGER, F. (2004). Tools for the analysis of debt problems. *Journal of Reconstructing Finance*, **1** (1), 201–223.

— and ZETTELMEYER, J. (2006). *Debt defaults and lessons from a decade of crises.* Cambridge, MA and London: MIT Press.

— and — (2008). Haircuts: Estimating investor losses in sovereign debt restructurings, 1998-2005. *Journal of International Money and Finance*, **27** (5), 780–805.

TREBESCH, C. (2010). *Inexcusable Sovereign Default.* Working paper, Free University Berlin, Germany.

— (2011). Debt restructuring delays: Measurement and stylized facts. In R. Kolb (ed.), *Sovereign Debt: From Safety to Default*, John Wiley & Sons, pp. 169–177.

WORLD BANK (2011). World Development Indicators and Global Development Finance Database. http://data.worldbank.org/data-catalog/world-development-indicators.

WRIGHT, M. L. J. (2011). Restructuring sovereign debts with private sector creditors: Theory and practice. In C. A. P. Braga and G. A. Vincelette (eds.), *Sovereign Debt and the Financial Crisis: Will This Time Be Different?*, World Bank, pp. 295–315.

YEYATI, E. L. and PANIZZA, U. (2011). The elusive costs of sovereign defaults. *Journal of Development Economics*, **94** (1), 95–105.

Table 1: Characteristics of Selected Sovereign Defaults

Country	Default period	Type of restructuring	Haircut (%)[a]	Index of Coerciveness[b]	Deficit (%)[c]
Pakistan	1998 - 1999	Pre- & postdefault	13.1	4.5	5.9
Ecuador	1999 - 2000	Postdefault	38.3	5.5	3.3
Argentina	2001 - 2005	Postdefault	76.8	7.2	4.3
Uruguay	2003	Predefault	9.8	1.0	4.6
Dom. Rep.	2005	Pre- & postdefault	5.6	1.0	2.9

Notes: [a] Average for bank and bond debt, weighted with the amount of exchanged debt. [b] Average over default period. [c] Average deficit in the three years prior to default.

Sources: Panizza et al. (2009) and Cruces and Trebesch (2011) for default periods, Cruces and Trebesch (2011) for the haircuts and Enderlein et al. (2011) for the index of coerciveness. Data on fiscal deficits is from Sturzenegger and Zettelmeyer (2006) and IMF (2011a).

Table 2: Synthetic Control Estimator of GDP per Capita: Pre-Default Fit and Country Weights

	Argentina (2001)		DomRep (2005)		Ecuador (1999)		Pakistan (1998)		Uruguay (2003)	
	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth
Dependent variable: lngdp										
n	1.29	1.21	1.63	1.45	2.01	2.29	2.51	2.26	0.53	1.07
s	21.71	21.59	21.42	24.93	25.88	22.79	21.64	21.11	17.50	21.07
av2school	2.06	2.20	2.07	2.28	2.17	2.16	1.30	1.64	2.22	2.18
polity2	7.13	7.18	8.00	7.34	8.88	8.86	8.00	7.70	10.00	8.11
lngdp(T_0-6,T_0-8)	9.03	9.02	8.75	8.76	8.50	8.48	7.56	7.56	9.00	8.99
lngdp(T_0-3, T_0-5)	9.11	9.09	8.88	8.88	8.54	8.51	7.59	7.59	9.07	9.06
lngdp(T_0-2)	9.18	9.14	8.94	8.91	8.54	8.52	7.63	7.62	9.06	9.07
lngdp(T_0-1)	9.14	9.13	8.89	8.92	8.56	8.57	7.62	7.64	9.02	9.05
RMSPE	0.016		0.019		0.024		0.011		0.023	
Control group[a]	MEX(0.29)		MEX(0.65)		CRI(0.54)		PHL (0.70)		TUR(0.36)	
	CZE(0.29)		IND(0.18)		PHL(0.21)		IND (0.20)		CZE(0.27)	
	COL(0.27)		BWA(0.17)		BWA(0.14)		PNG (0.08)		JAM(0.25)	
	TUR(0.10)				MNG(0.07)		BWA (0.03)		PNG(0.11)	
	BRA(0.04)				THA(0.02)					

Notes: [a] Only the five most important countries with an individual weight of at least one percent are shown.

Table 3: Synthetic Control Estimator of Exports per Capita: Pre-Default Fit and Country Weights

	Argentina (2001)		DomRep (2005)		Ecuador (1999)		Pakistan (1998)		Uruguay (2003)	
	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth
Dependent variable: lnexports										
latitude	0.38	0.38	0.21	0.16	0.02	0.09	0.33	0.27	0.37	0.17
agr	55.25	28.51	18.49	13.33	54.17	33.30	18.76	16.59	58.87	32.71
fossiles	12.33	20.35	4.64	6.87	39.64	38.91	1.33	34.41	1.62	5.09
lnexports(t-6, t-8)	6.00	6.00	6.30	6.30	5.65	5.64	3.81	3.81	6.60	6.60
lnexports(t-3, t-5)	6.44	6.44	6.35	6.36	5.86	5.88	3.98	3.97	6.68	6.68
lnexports(t-2)	6.55	6.50	6.31	6.32	6.07	6.06	4.06	4.07	6.61	6.61
lnexports(t-1)	6.42	6.46	6.36	6.41	6.12	6.10	4.13	4.12	6.50	6.52
RMSPE	0.037		0.034		0.025		0.039		0.078	
Control group[a]	CHL (0.26)		PHL(0.46)		COL(0.35)		EGY(0.67)		JAM (0.67)	
	BRA (0.25)		JAM(0.36)		PER(0.33)		IND(0.33)		CRI (0.29)	
	LTU (0.25)		MEX(0.06)		PNG(0.13)				GTM (0.03)	
	HUN (0.13)				MYS(0.11)				SUR (0.01)	
	LBN (0.12)				CHL(0.06)					

Notes: [a] Only the five most important countries with an individual weight of at least one percent are shown.

Table 4: Synthetic Control Estimator of Relative Exports: Pre-Default Fit and Country Weights

	DomRep (2005)		Ecuador (1999)		Pakistan (1998)	
	Treat	Synth	Treat	Synth	Treat	Synth
Dependent variable: rel_exports						
agr	20.39	6.09	54.17	27.41	18.76	32.31
fossiles	5.76	10.15	39.64	39.59	1.33	3.65
lndist	8.95	9.02	9.17	9.12	8.74	9.06
rel_exports(t-6, t-8)	89.02	87.06	63.53	63.48	59.17	59.17
rel_exports(t-3, t-5)	90.00	88.59	61.18	60.68	59.23	59.42
rel_exports(t-2)	88.57	88.79	57.55	58.35	58.23	57.60
rel_exports(t-1)	86.95	88.33	57.84	57.78	56.77	58.11
RMSPE	1.472		0.769		1.100	
Control group[a]	MEX (1.00)		CHL(0.45)		THA (0.44)	
			EGY(0.19)		CHN (0.27)	
			MEX(0.19)		SLV (0.24)	
			PER(0.09)		LKA (0.03)	
			FJI(0.04)		FJI (0.02)	

Notes: [a] Only the five most important countries with an individual weight of at least one percent are shown.

Table 5: Synthetic Control Estimator of Net FDI Inflows per Capita: Pre-Default Fit and Country Weights

	Argentina (2001)		DomRep (2005)		Ecuador (1999)		Pakistan (1998)		Uruguay (2003)	
	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth	Treat	Synth
Dependent variable: lnfdi										
av2school	2.06	2.72	2.07	1.90	2.14	2.06	1.30	1.11	2.22	1.86
latitude	0.38	0.28	0.21	0.35	0.02	0.12	0.33	0.22	0.37	0.37
ethnic	0.26	0.25	0.43	0.43	0.66	0.48	0.71	0.43	0.25	0.27
mobiles	3.47	3.35	9.18	18.26	0.28	0.40	0.02	0.06	6.18	8.27
telephones	16.23	12.79	9.37	20.26	5.80	4.11	1.26	1.50	23.86	23.23
lnfdi(t-6, t-8)	5.78	5.75	4.43	4.57	5.40	5.42	5.34	5.33	4.49	4.45
lnfdi(t-3, t-5)	6.02	6.07	5.22	5.16	5.52	5.51	5.35	5.34	4.58	4.65
lnfdi(t-2)	6.01	6.11	4.99	5.01	5.52	5.54	5.36	5.35	4.84	4.70
lnfdi(t-1)	6.75	6.46	4.71	4.86	5.59	5.58	5.37	5.36	4.90	4.93
RMSPE	0.151		0.179		0.024		0.002		0.114	
Control group[a]	CHL(0.78)		CZE (0.26)		LKA (0.55)		IND (0.91)		TUR(0.51)	
	PNG(0.10)		PNG (0.26)		PER (0.19)		THA (0.04)		CRI(0.29)	
	THA(0.08)		ZAF (0.25)		COL (0.11)		TUR (0.02)		POL(0.11)	
	MYS(0.05)		BGR (0.23)		FJI (0.09)		PER (0.02)		CZE(0.09)	
					HUN (0.05)		COL (0.01)			

Notes: [a] Only the five most important countries with an individual weight of at least one percent are shown.

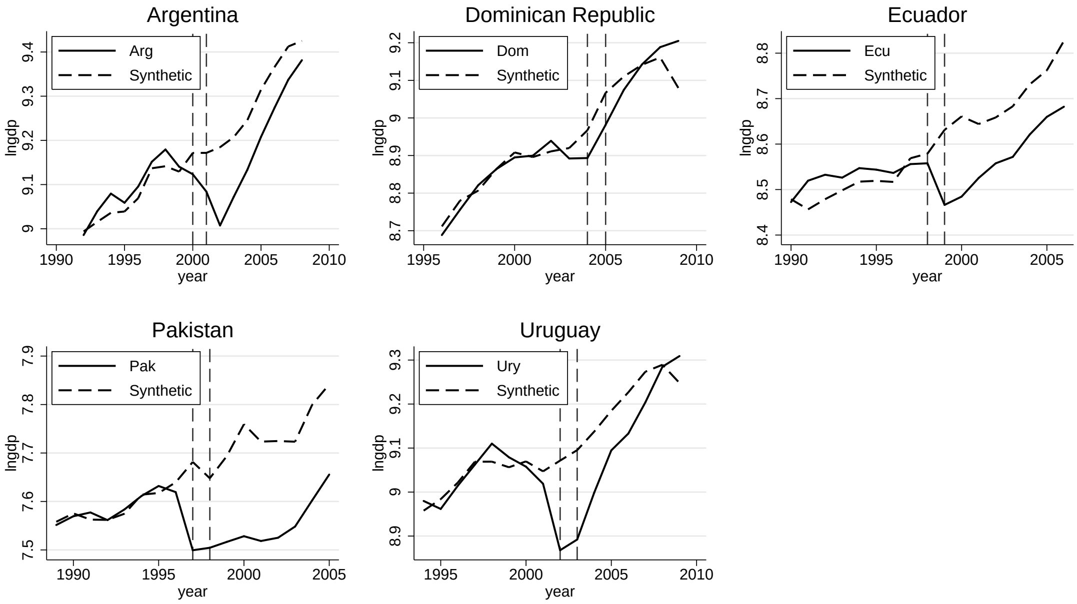

Figure 1: Evolution of GDP per capita: defaulting vs. synthetic economies

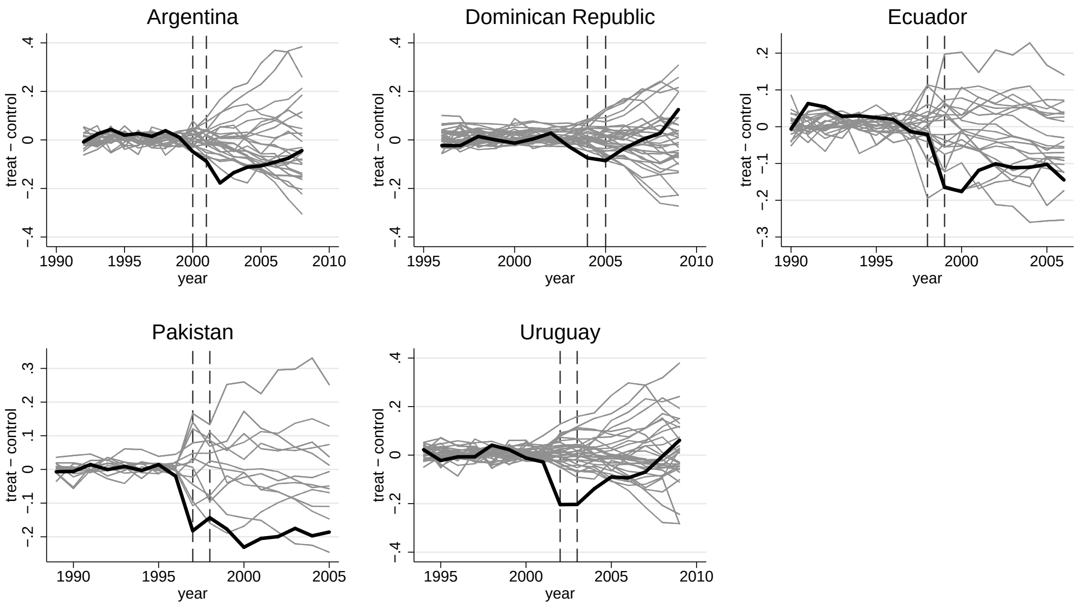

Figure 2: Country-specific costs of sovereign defaults in terms of GDP per capita

Notes: Solid line: results for defaulting country; grey lines: placebo studies.

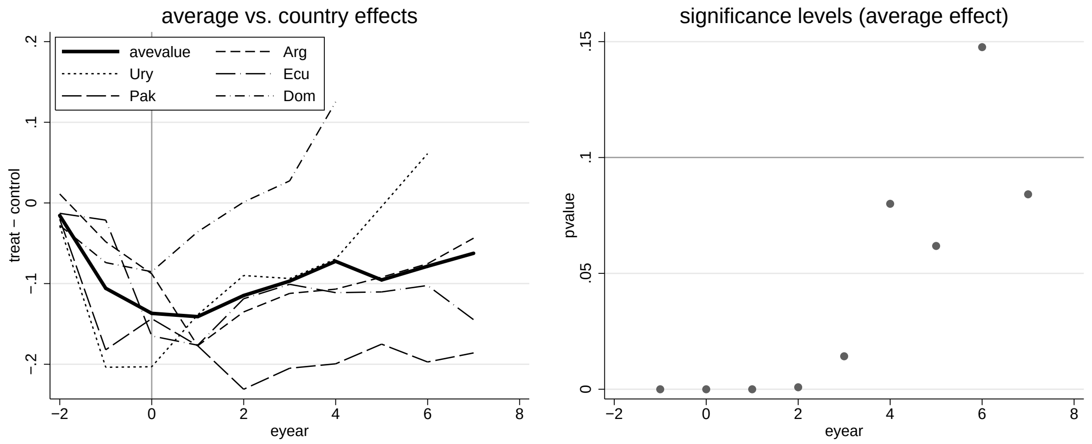

Figure 3: Average costs of sovereign defaults in terms of GDP per capita

Notes: Costs in year *eyear* after the default.

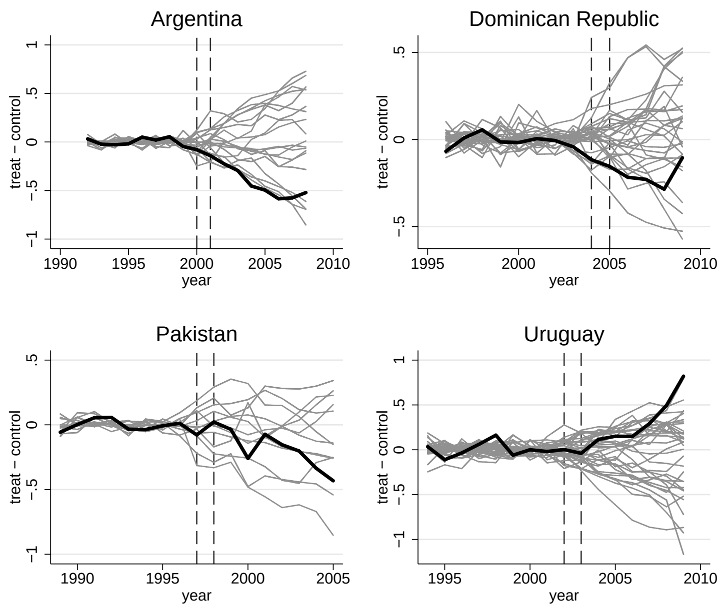

Figure 4: Country-specific costs of sovereign defaults in terms of total exports per capita

Notes: Solid line: results for defaulting country; grey lines: placebo studies.

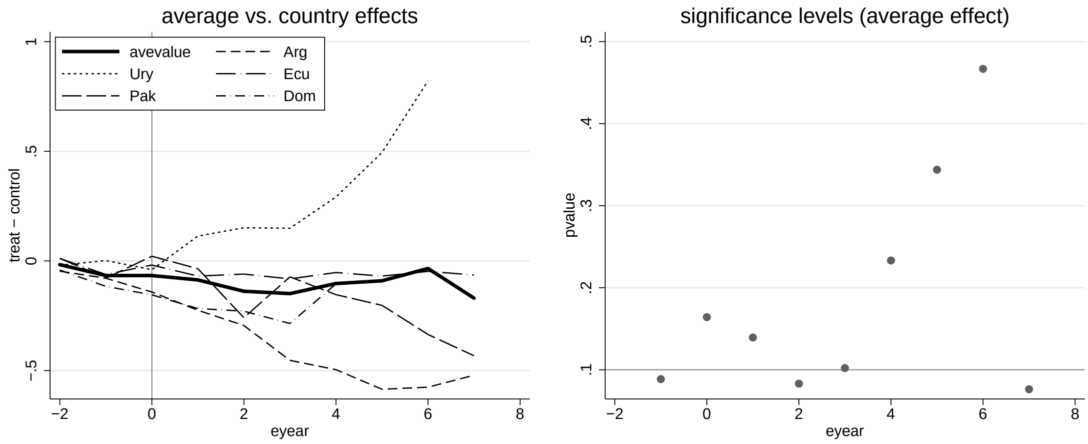

Figure 5: Average costs of sovereign defaults in terms of total exports per capita

Notes: Costs in year *eyear* after the default.

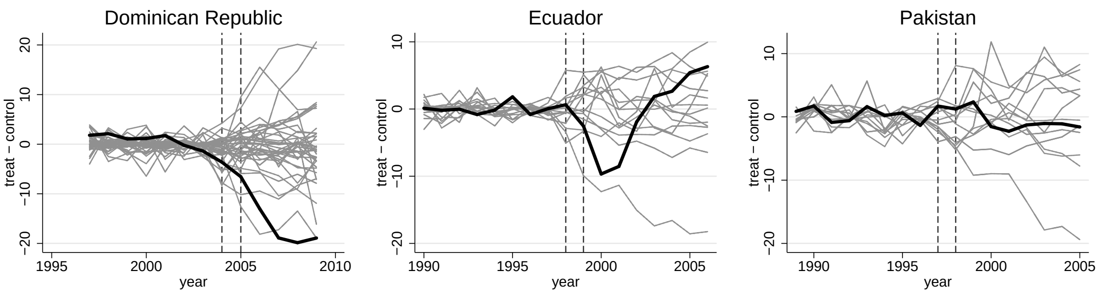

Figure 6: Country-specific costs of sovereign defaults in terms of relative exports

Notes: Solid line: results for defaulting country; grey lines: placebo studies.

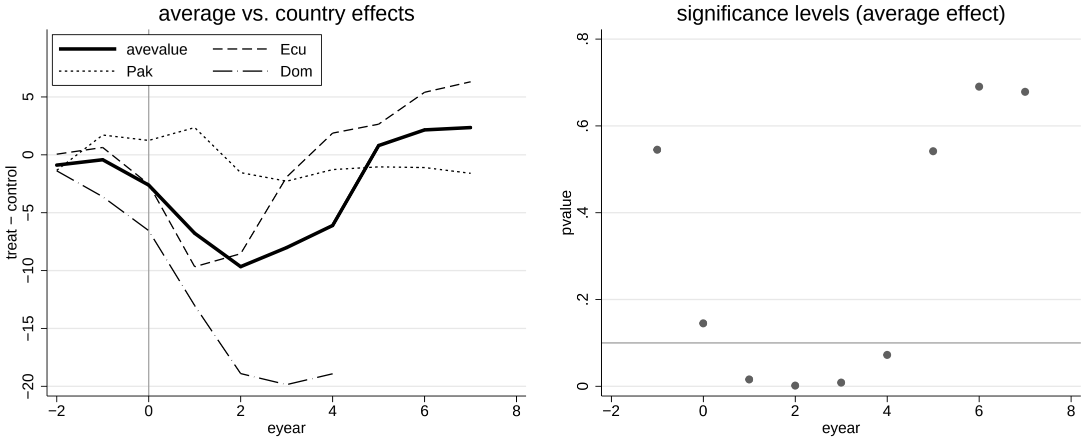

Figure 7: Average costs of sovereign defaults in terms of relative exports

Notes: Costs in year *eyear* after the default.

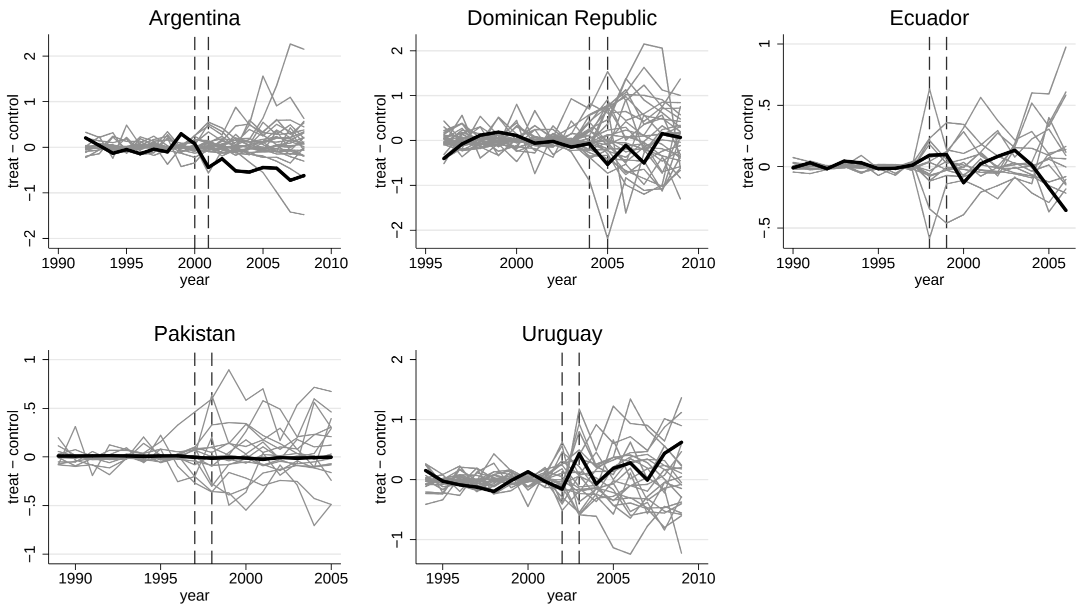

Figure 8: Country-specific costs of sovereign defaults: FDI inflows per capita

Notes: Solid line: results for defaulting country; grey lines: placebo studies.

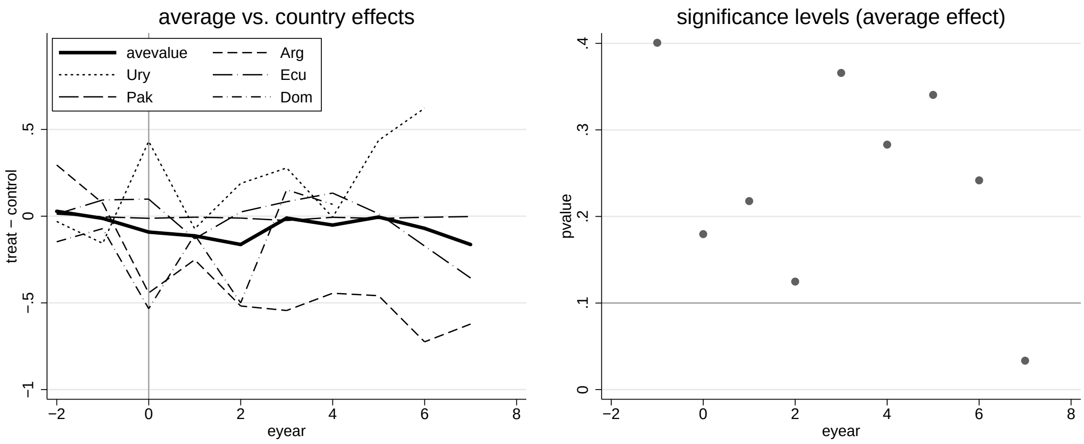

Figure 9: Average costs of sovereign defaults: FDI inflows per capita

Notes: Costs in year *eyear* after the default.

Appendix A. Country Coverage

in all samples:					
Botswana	(BWA)	Brazil	(BRA)	Chile	(CHL)
China	(CHN)	Colombia	(COL)	Egypt	(EGY)
El Salvador	(SLV)	Fiji	(FJI)	Hungary	(HUN)
India	(IND)	Malaysia	(MYS)	Mexico	(MEX)
Mongolia	(MNG)	Papua N. Guinea	(PNG)	Peru	(PER)
Philippines	(PHL)	Poland	(POL)	Romania	(ROM)
Slovak Republic	(SVK)	Sri Lanka	(LKA)	Thailand	(THA)
Tunisia	(TUN)	Turkey	(TUR)		
added for Ecuador:					
Costa Rica	(CRI)	Czech Republic	(CZE)	Estonia	(EST)
Guatemala	(GTM)				
added for Argentina:					
Cameroon	(CMR)	Morocco	(MAR)	Vietnam	(VNM)
added for Uruguay:					
Jamaica	(JAM)	Jordan	(JOR)	Lithuania	(LTU)
Panama	(PAN)	South Africa	(ZAF)		
added for the Dominican Republic:					
Bolivia	(BOL)	Bulgaria	(BGR)		

Notes: Country list refers to the analysis of default costs in terms of GDP per capita, see subsection 4.1 for details.

Appendix B. Data Sources and Definitions

Name	Source	Definition
Dependent variables		
lngdp	Heston et al. (2011)	Natural logarithm of PPP converted GDP per capita (2005 dollars)
lnexports	IMF (2011b) and Heston et al. (2011)	Natural logarithm of the ratio of total exports to population
rel_exports	IMF (2011b)	Fraction of exports destined for Paris Club creditor countries
lnfdi	World Bank (2011)	Natural logarithm of foreign direct investment (net inflows + |lowest sample value (if negative)|) per capita
Control variables for lngdp		
n	World Bank (2011)	Population growth (annual %)
s	Heston et al. (2011)	Investment share of PPP converted GDP per Capita (2005 dollars)
av2school	Barro and Lee (2010)	Average years of secondary schooling
polity2	Polity IV (2009)	Combined policy score ranging from - 10 (strongly autocratic) to +10 (strongly democratic)
Additional control variables for lnexports		
latitude	La Porta et al. (1999)	Rescaled absolute value of the latitude of each country's capital, ranging between 0 and 1
agr	World Bank (2011)	Sum of food and agricultural raw materials exports relative to total merchandize exports
fossils	World Bank (2011)	Sum of fuel, ores and metals exports relative to total merchandize exports
Additional control variables for rel_exports		
lndist	CEPII (2011)	Natural logarithm of the average physical distance to Paris Club creditor countries
Additional control variables for lnfdi		
ethnic	Alesina et al. (2003)	Estimated probability that two randomly meeting citizens belong to the same ethnic group
mobiles	World Bank (2011)	Number of mobile cellular subscriptions (per 100 people)
telephones	World Bank (2011)	Number of telephone lines (per 100 people)