FOUR ESSAYS ON CAPITAL INFLOWS AND SOVEREIGN RISK

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I. INTRODUCTION

The shock waves that originated from the burst of the US house price bubble in 2007 have put an end to the "Great Moderation", a period of economic calm in advanced economies. This is especially true for Europe, where recent developments have brought current account imbalances and sovereign risk back on the agenda of investors, policy makers and economists alike. Up to now, the so called "euro crisis" centered on five member countries of the common currency union – Greece, Italy, Ireland, Portugal, and Spain – which experienced a reversal of private capital flows as well as a substantial increase in sovereign borrowing costs. Europe even witnessed the first non-war-related sovereign default by an advanced economy since 1945 when Greece restructured its debt in March 2012. These events were accompanied by a wide range of policy responses that include a general shift towards fiscal austerity and the establishment of a new European bailout fund.

Against the background of these experiences, the recent revival of research on the determinants of international capital flows and sovereign risk comes as no surprise. The euro crisis has also sparked a renewed academic debate about the economic costs of sovereign debt crises and on the appropriate policy response. The four papers that constitute this thesis contribute to each of these topics. All of them are empirical in nature and draw on large cross-sectional time-series data sets. Hence, rather than focusing exclusively on the recent European experience, they use the fact that both, the ebb and flow of foreign capital flows and sovereign debt crises have been a recurring phenomenon in emerging market economies.

The first paper *"The Dynamics of International Capital Flows: Results from a Dynamic Hierarchical Factor Model"* coauthored with Marcel Förster and Peter Tillmann investigates the relative importance of global developments as drivers of cross-border capital flows. The analysis is motivated by the popular notion that such "push factors" have been responsible for the boom-bust cycles of capital flows characterizing many emerging market economies. In these episodes massive capital inflows have been followed by "sudden stops" which in turn were often associated with deep recessions and financial crises. To the extent that these events have indeed been caused by developments out of the realm of domestic politics, restricting international capital mobility might be an appropriate policy response. The degree to which capital flows to different countries are linked, i.e. the degree of comovement of capital flows, is thus a key question for policy makers.

Our study addresses this issue and disentangles the determinants behind international capital inflows into driving forces attributable to different levels of aggregation. In particular, we use a purely data-driven approach to decompose capital flows in a large panel of countries into (i) a global factor common to all types of inflows and all recipient countries, (ii) a factor specific to a given type of capital inflows, i.e. either foreign direct investment (FDI), portfolio investment or other kinds of investment, (iii) a regional factor driving economies in geographical proximity and (iv) a country-specific component. Using this four level structure we thus acknowledge that some push factors are likely to have a differentiated effect on the financial account of specific country groups. Technically, the decomposition rests on the estimation of a dynamic hierarchical factor model. This econometric approach, recently developed by Moench et al. (2011) and Moench and Ng (2011) is ideally suited for our purpose. Its block structure separately identifies regional and global determinants of capital inflows in a logical coherent way, i.e. it allows for the possibility that the global factor affects regional and other subordinated factors but not vice versa.

Based on a quarterly data set of 47 countries and three different types of gross capital inflows, we find that the country-specific component explains by far the largest fraction of fluctuations in capital inflows. This "pull" factor alone is responsible for around 80% of the observed volatility. The regional factor explains between 5% and 20% of fluctuations and is particularly important for emerging markets' FDI and portfolio inflows as well as bank lending to emerging Europe. The global factor, however, explains only a small share of overall variation.

The predominance of pull factors revealed by our results implies that most foreign investors carefully discriminate between different target countries. Hence, domestic policy has considerable room to affect capital inflows and, if this is deemed appropriate, also to limit their potential undesirable consequences. However, the results also suggest that most of the blame for past episodes of sudden stops has to be attributed to country-specific circumstances.

The three remaining papers of this thesis are devoted to the study of sovereign debt crises and defaults. Since these events are costly for the affected countries (see paper three) and potentially contagious, preventing them has always been a major policy objective. Although it lacks an explicitly stated mandate to fight sovereign debt crises the International Monetary Fund (IMF) has seemingly assumed this task as it intervened in many crisis-stricken countries. Its participation in the – eventually failed – efforts to rescue Greece is perhaps the best known recent example. My second paper *"The Effect of IMF Lending on the Probability of Sovereign Debt Crises"* investigates whether the IMF has been more successful in the past.

The paper starts with a review of the theoretical literature on the relationship between IMF interventions and sovereign risk. This literature identifies four channels through which the IMF's presence alters the probability of subsequent sovereign defaults. These channels focus (i) on the direct effects of liquidity provision, (ii) its influence on the governments' adjustment effort and on the role of (iii) conditionality and (iv) seniority respectively. The analysis, however, does not provide a clear-cut answer to the question whether we should expect default probabilities to rise or to decrease in the aftermath of IMF programs. Rather, the sign of the effect is disputed even at the level of the individual channels. Given this heterogeneity of theoretically plausible effects the success of IMF programs has to be assessed empirically.

The identification of the causal effects of IMF programs is complicated by the fact that their implementation is not exogenous. On the contrary, programs are often specifically targeted to countries which are already on the brink of crisis. To the degree that these common determinants of IMF interventions and sovereign debt crises are not modeled adequately any indicator of IMF actions becomes endogenous. Neglecting this endogeneity would result in biased estimates that understate the potential positive impact of the IMF's presence. To address these concerns I investigate the IMF-default nexus empirically using univariate and bivariate probit methods. Both model variants include a large set of macroeconomic and political control variables that influence the probability of sovereign debt crises and are probably correlated with the incidence of IMF interventions. The bivariate probit model further explicitly accounts for the possibility that unobserved factors affect the likelihood of crises and IMF programs simultaneously.

The results of the empirical exercises indicate that the adoption of an IMF program increases sovereign risk over the medium term. More concretely, I estimate that the probability of a sovereign default increases by approximately 1.5 to 2 percentage points in the aftermath of IMF interventions. These results can be interpreted causally as they are robust across univariate and bivariate specifications. Further analysis shows that the increase in sovereign risk cannot be attributed to a lack of compliance with conditionality but seems to reflect the effects of IMF programs per se. Furthermore, I find that the IMF's presence is especially detrimental to fiscal solvency when Fund resources are targeted to countries with already weak fundamentals. Overall, the evidence is therefore consistent with the idea that debtor moral hazard is most likely to occur in these situations as predicted by the theoretical work on the catalytic finance hypothesis. Other theoretical explanations that point to the effects of debt dilution and the possibility of IMF triggered debt runs, however, are also possible.

The final two paper of my thesis are concerned with the costs of sovereign defaults. Their analysis is interesting from both a political and a theoretical point of view. Politically, estimates of the likely consequence of a default decision provide valuable information to governments considering this step. Enhancing the understanding of the trade-off between a reduced burden of debt repayment and incurred default costs should thus lead to more deliberate policy choices. The theoretical interest in default costs is related to this argument as it already implies that sovereigns can freely decide to repudiate their debts. This is a realistic assumption since lending to sovereign entities is not enforced by any international bankruptcy law. Economic theory then predicts that optimizing governments should always repudiate if defaults were costless. It also follows that investors should rationally decline lending causing the market for sovereign debt to break down. Since we observe high quantities of sovereign debt in many countries which are generally repaid we know by contradiction that defaults cannot be costless if the theory is correct. Identifying default costs thus facilitates our theoretical understanding of the market for sovereign debt.

My third paper *"The Heterogeneity of Default Costs: Evidence from Recent Sovereign Debt Crises"* analyzes different dimensions of default costs that have recently attracted much research interest. Costs are quantified in terms of forgone GDP growth, reduced foreign trade and deterred inflows of private capital. However, I depart from the previous literature by acknowledging that past sovereign defaults have been far from homogeneous. On the contrary, country and time-specific default experiences differed in many respects from the length of restructuring process to the way creditors were treated. Different economic theories imply that these differences should affect the costs of debt crises. Unfortunately, standard panel methods – the workhorses of most empirical contributions in the field – are ill-suited to address this kind of heterogeneity. I therefore opt for a novel econometric technique based on comparative case studies that allows for a case-by-case estimation of default costs.

The 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 my application a sovereign's decision to default is defined as the relevant treatment and the associated economic costs as the outcome variables of interest. Using these definitions the paper then offers an in-depth analysis of five recent episodes of sovereign debt crises. The sample contains both the spectacular unilateral default of Argentina in 2001 and the much more cooperative restructuring of Uruguay's debt in 2003 that has been praised as role-model for future debt renegotiations.

My results support the general notion of costly sovereign defaults and the 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 results also point to differences in the relative importance of the different channels through which a default 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. Considered together with the observation that harmful effects of defaults on GDP growth have been found for all sovereign debt crises in my sample this finding suggests that not only the level but also the type of costs incurred after a default depend on country-specific circumstances. However, a competing explanation would be that all defaults are costly in a dimension that has not yet been analyzed. My final paper addresses this possibility.

The type of default costs that is analyzed in this fourth paper *"Aid Withdrawal as Punishment for Defaulting Sovereigns? An Empirical Analysis"*, coauthored with Jana Brandt, is related to the disbursement of foreign aid. More specifically, we empirically investigate whether donor countries react to sovereign defaults by reducing foreign aid flows to delinquent debtors. The assumed existence of this kind of punishment mechanism has been a cornerstone of two recent contributions to the theory of sovereign debt by Asiedu and Villamil (2002) and Asiedu et al. (2009). However, up to now, the validity of this assumption has not been tested. Our paper seeks to fill this gap in the literature.

Using bilateral data on foreign flows and sovereign defaults we are able to distinguish between two different versions of the punishment hypothesis. A strong version states that international donors as a group sanction defaults by reducing foreign aid to misbehaving sovereign debtors. The collective withdrawal of foreign aid thus represents an additional cost to the affected country that may influence its decision to default in the first place. Hence, foreign aid would serve as an enforcement mechanism as modeled by Asiedu and Villamil (2002) and Asiedu et al. (2009). However, it seems reasonable to assume that coordination among donors is too weak to ensure collective sanctioning. A reduction in aid disbursements might thus only be observed for those creditor countries to which the recipient defaulted. This is the prediction of the weaker version of the punishment hypothesis which is silent about the theoretically ambiguous reaction of the remaining donors.

Our findings – obtained by standard panel techniques – indicate that foreign aid flows are not reduced after a default. This result holds not only for the aggregate amount of foreign aid received by the delinquent country but also for the amount granted by aggrieved creditor countries. Hence, both versions of the punishment hypothesis are rejected by the data. Moreover, we even find an economically and statistically significant positive effect of defaults on aggregate aid inflows. This finding reflects additional aid flows given by non affected creditor countries which possibly react to the increased need of the recipient country in times of crises. All of these findings are robust to different empirical model specifications and several robustness checks. Foreign aid therefore seems not to work as an enforcement mechanism for sovereign debt repayment. We also conclude that the damage inflicted by a hypothetical withdrawal of foreign aid is not the explanation for the reduction in GDP growth observed in the aftermath of sovereign defaults.

The remainder of this thesis is structured as follows. Chapter II contains the dynamic factor analysis of international capital inflows. Section III provides my paper on the relationship between IMF programs and sovereign debt crises. Chapter IV and Chapter V are devoted to the empirical analysis of sovereign default costs. The papers in these four chapters constitute separate contributions to the literature and are thus presented as such. Chapter VI concludes.

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II. THE DYNAMICS OF INTERNATIONAL CAPITAL FLOWS: RESULTS FROM A DYNAMIC HIERARCHICAL FACTOR MODEL

This paper is available as

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The Dynamics of International Capital Flows: Results from a Dynamic Hierarchical Factor Model

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Abstract

The present paper examines the degree of comovement of gross capital inflows, which is a highly sensitive issue for policy makers. We estimate a dynamic hierarchical factor model that is able to decompose inflows in a sample of 47 economies into (i) a global factor common to all types of flows and all recipient countries, (ii) a factor specific to a given type of capital inflows, (iii) a regional factor and (iv) a country-specific component. We find that the latter explains by far the largest fraction of fluctuations in capital inflows followed by regional factors, which are particularly important for emerging markets' FDI and portfolio inflows as well as bank lending to emerging Europe. The global factor, however, explains only a small share of overall variation. The exposure to global drivers of capital flows, i.e. the global factor and the factor specific to each type of capital inflows, is particularly pronounced for countries with a more developed financial system. A fixed exchange rate regime does not shield countries from the ebb and flow of global capital flow cycles.

JEL Classification: F21, F32, C30

Keywords: Capital flows, Dynamic hierarchical factor model, Emerging economies, Financial crises

1. Introduction

Over the last two decades, swings in international capital flows have been a salient feature of the world economy. Both mature economies and emerging markets experienced the ebb and flow of foreign investment in domestic financial assets. Some countries, notably emerging market economies, even suffered from boom-bust cycles in capital flows, where a massive inflow is followed by a "sudden stop" and an eventual sharp reversal of cross-border flows.

The recent financial crisis in 2008/09 is only the latest incident in a series of swings in global capital flows. At the peak of the crisis following the Lehman collapse in September 2008, investors in almost all countries repatriated foreign investments. The result was a massive retrenchment of capital flows. In 2009, when many central banks around the globe started to flood financial markets with liquidity, international capital flows quickly resumed.[1]

Swings in capital inflows often appear synchronized across countries, what encouraged many observers to speculate whether global factors rather than conditions in the recipient countries dominate investors' decisions to invest abroad. For example, The Economist (2011) recently argued that flows "may have less to do with [the receiving countries'] long-term prospects than with temporary factors such as unusually loose rich-world monetary policy, over which they have no control."

The discussion of the determinants of capital flows often distinguishes between pull and push factors. If investors carefully discriminate between countries, thus sending funds as a response to the recipient countries' fundamentals such as growth prospects or return differentials with respect to advanced economies, capital is said to be driven by pull factors. If, however, investors treat emerging countries similarly irrespective of domestic fundamentals, thus responding mostly to global developments such as abundant liquidity in advanced economies, financial stress or weak growth prospects in mature economies, capital flows are said to be driven by push factors.

The extent to which capital flows to different countries are linked, i.e. the degree of comovement of capital flows, is a key question for policy makers. The reason is that the appropriate policy response to capital inflows depends on the driving forces behind

[1] See International Monetary Fund (2011b) for a detailed account of these recent episodes and a thorough analysis of international capital flows.

capital flows. Naturally, domestic economic policies may influence pull factors but have by definition no impact on the nature and the strength of push factors. Therefore, it is important to gauge the extent to which flows are correlated on a global level. Standard static or dynamic factor analysis offers valuable tools to accomplish this end.

Unfortunately, empirically distinguishing between global and local determinants of capital flows does not necessarily lead to a clear-cut categorization of push and pull factors. The reason is that such a decomposition would only identify those push factors that affect all countries simultaneously and in a similar way. A general increase in investors' home bias that causes a synchronized retrenchment of global capital flows might be an event that fits this description. Other push factors, however, are more likely to have a differentiated effect on the current account of specific country groups. The sensitivity of capital flows to interest rates in advanced economies, e.g., implies that a tightening of monetary policy in these countries risks triggering a sharp reversal of capital flows which can have large effects on emerging economies. Hence, we would observe an increased comovement of capital flows at the regional level, i.e. among industrial and emerging economies, but heterogeneous outcomes at the global level. Similarly, contagious crisis in one emerging economy may lead to "sudden stops" of capital inflows or withdrawals in neighboring or even remote countries but are less likely to affect industrialized economies.

In this paper we address this issue and disentangle the determinants behind international capital flows into driving forces attributable to different levels of aggregation. In particular, we estimate a dynamic hierarchical factor model that is able to decompose capital flows in a large panel of countries into (i) a global factor common to all types of inflows and all recipient countries, (ii) a factor specific to a given type of capital inflows, i.e. either foreign direct investment (FDI), portfolio investment or other kinds of investment, (iii) a regional factor driving economies in geographical proximity and (iv) a country-specific component. To our knowledge this paper is the first to shed light on the relative importance of these four determinants for global capital flows. The empirical approach draws on a recently developed dynamic hierarchical factor model (see Moench et al. (2011)). With its pyramidal structure, the model allows for the possibility that the global factor affects regional and other subordinated factors but not vice versa.

Based on a quarterly data set of 47 countries and three different types of gross capital inflows, we find that the country-specific component explains by far the largest fraction of fluctuations in capital inflows. This factor alone is responsible for around 80% of the observed volatility. The regional factor explains between 5% and 20% of fluctuations and is particularly important for emerging markets' FDI and portfolio inflows as well as bank lending to emerging Europe. The global factor, however, explains only a small share of overall variation.

We also relate the exposure of the economies in our sample to the global drivers of capital flows, i.e. the global factor and the factor specific to each type of capital inflows, to a set of explanatory variables which are often used to describe a country's openness to trade and financial flows as well as its financial system. It turns out that the exposure to global driving forces is particularly pronounced for countries with a large financial system. A fixed exchange rate regime does not shield countries from the ebb and flow of global capital flow cycles.

The remainder of the paper is organized as follows. Section 2 discusses the related literature and our contribution to this field of research in some detail. The data set we construct for this research project is presented in Section 3. Section 4 introduces our dynamic hierarchical factor model. The core results are discussed in Section 5. In Section 6 we relate the exposure of countries to the global factor and the flow type-specific factor to structural characteristics of the economies in our sample. Robustness analyses are carried out in Section 7. Section 8 concludes.

2. Related Literature

The present paper is related to three different branches of the literature: First, a number of papers use factor analysis to study the degree of international business cycle synchronization. Kose et al. (2003) pioneered this field and estimate a Bayesian dynamic factor model for macroeconomic aggregates from 60 countries. Their results suggest that a common global factor, i.e. a world business cycle, explains a large fraction of variation across countries. Kose et al. (2008) decompose output, investment and consumption series of more than 100 countries into a global factor, group-specific factors that drive fluctuations in industrial, emerging and developing economies as well as country-specific factors and idiosyncratic factors. They are interested in whether business cycles became more synchronized during the post-1985 period of increasing globalization. Interestingly, they find a convergence of business cycles within each group, but divergence, i.e. a decoupling of business cycles, between different country groups. Inspired by these contributions, Eickmeier et al. (2011), Helbling et al. (2011) and others examine how financial shocks originating in the U.S. affect the common component of fluctuations in the G7 economies. All these contributions model macroeconomic aggregates but are silent about capital inflows.

A second branch of the literature studies the comovement of bond spreads across emerging financial markets. McGuire and Schrijvers (2003) and Bunda et al. (2010) employ factor models to extract a global factor from bond spreads. González-Rozada and Yeyati (2008) argue that a global factor, which they attribute to investors' risk appetite, global liquidity and contagion, can explain a large fraction of movements in bond spreads. Their results thus stress the role of exogenous determinants driving emerging economies' borrowing costs. Neither of these papers, however, takes account of a regional dimension of comovement that is arguably most relevant for developing and emerging economies susceptible to contagious financial stress in neighboring countries.

A third and most relevant strand addresses the role of global determinants for international capital flows.[2] Here we briefly survey some recent studies, which were all written against the backdrop of the retrenchment and the subsequent rebound of flows observed after 2008. Milesi-Ferretti and Tille (2011) document this unprecedented collapse in international capital flows during the financial crisis. They show that the main driving force has been a risk shock that made investors more cautious about future investment prospects. The size of the capital flow reversal that precedes the current wave of inflows was tightly linked to the extent of international financial integration as well as domestic macroeconomic conditions. A second observation is that the retrenchment was highly heterogeneous across time, across types of flows and across geographic regions.[3] Forbes and Warnock (2011) study the determinants of extreme movements of capital across borders. They identify "waves" of capital flows, i.e. prolonged phases of capital flows referred to as surge, stop, flight and retrenchment periods.[4] Interestingly, they also focus on gross flows rather than net flows as capital flows initiated by foreigners are likely to be driven by other considerations than flows brought about by domestic investors. Both types of investors could also react differently to political and economic circumstances, and potentially respond by adjusting different types of capital flows. Their findings attribute a crucial role to global factors, a somewhat less important role to contagion and

[2] Early, and by now classic, contributions include Calvo et al. (1996), Chuhan et al. (1998) and Fernandez-Arias (1996).

[3] In a study prepared for the World Economic Outlook, the International Monetary Fund (2011b) also addresses the role of global factors. Estimates of time dummies and regional dummies in a simple panel of capital flows suggest that a common factor plays a minor role for capital flows.

[4] A similar classification of capital flow surges is presented by Reinhart and Reinhart (2009).

an even less prominent role to domestic pull factors. Among these global factors, global risk has the largest explanatory power. Global growth predicts surges of capital flows and sudden stops while contagion through financial linkages is a significant predictor of stops and retrenchments. In contrast to other studies, Forbes and Warnock (2011) find that liquidity conditions and global interest rates are insignificant explanatory variables. Among the pull factors domestic growth has the strongest impact on surges and stops. Finally, Zalduendo et al. (2012) identify "surges" of net capital inflows and assess the role of push and pull factors in causing these surges. They find that global push factors explain the occurrence of a surge in inflows. The size of the surge, once it occurs, is dependent on domestic pull factors.

While most of the existing studies focus on capital flows at a quarterly or even annual frequency, the recent study by Fratzscher (2011) is based on portfolio flow data at daily, weekly and monthly frequency. This is particularly interesting in the current crisis and the subsequent recovery as quarterly data wash out many of the high frequency movements of volatile portfolio inflows. He finds that common factors driving flows across countries have a highly heterogeneous impact on the 50 countries included in the study. This impact is associated with a country's strength of domestic institutions, its country risk assessment and domestic macroeconomic fundamentals. A second finding is related to the current surge in capital inflows. The author shows that idiosyncratic pull factors originating in emerging market economies dominated the driving forces during the recovery from the global crisis.

In this paper we borrow from each of these strands. We use a dynamic hierarchical factor model developed by Moench and Ng (2011) and Moench et al. (2011) that is able to decompose a country's capital inflows into three different explanatory factors. Thus, instead of looking at refinancing conditions measured in terms of bond spreads as in González-Rozada and Yeyati (2008), we use actual flow data to study the degree of comovement. Finally, rather than relating capital flows to structural determinants such as shocks to investors' risk aversion, financial conditions in advanced economies or growth prospects in emerging economies, our approach is purely data-driven in the sense that the factors we identify do not lend themselves to a straightforward economic identification. The advantage, however, is that this approach does not require us to restrict capital flows to respond to a prespecified set of explanatory variables only.

3. The Data Set

Following recent research by Forbes and Warnock (2011) and Broner et al. (2011), our focus is on gross inflows measured in percent of GDP. Gross capital inflows are more informative for our purpose as capital flows brought about by foreigners are likely to be driven by other considerations than flows initiated by domestic investors. Both types of investors could also be affected differently by policy measures and economic shocks, and potentially respond by adjusting different types of capital flows. We differentiate between portfolio, FDI and "other" flows where the last category contains residual transactions that are predominately related to bank lending activities. To this end, we augment quarterly data from the IMF's *International Financial Statistics* with additional information from a few national sources listed in Appendix A. After excluding major financial centres which could otherwise bias our estimation results we end up with a sample of 47 countries with data from 1994Q1 to 2010Q4. Our sample period thus covers the Asian crisis, the debt crises in Latin America and Russia and the recent global financial crisis.

For each country in our sample, we use data on portfolio, FDI and other capital inflows.[5] These three categories of capital flows constitute distinctive blocks in our hierarchical dynamic factor model. This specification choice allows for, e.g., FDI and portfolio inflows to react differently to changing global macroeconomic and financial conditions. To isolate the effects of regional developments we further arrange the block-specific data into geographical subblocks. Building upon the World Bank's classification we differentiate between four country groups: Asia, emerging Europe, Industrial and Latin America.[6] Appendix B describes our sample and the regional classification.

Prior to estimation, all series are transformed in order to meet the assumptions of the dynamic factor analysis. We seasonally adjust the capital flow series using the Census X12 method. The resulting series are then standardized by the recipient country's GDP to guarantee that large economies do not dominate the estimated global factors simply

[5] The exceptions are Bolivia and Nicaragua for which data on portfolio inflows are not available. Smaller gaps in two further series have been filled using data from the balance of payments' errors and omissions category. See Appendix A for details.

[6] The World Bank's geographical classification is simplified by merging the "South Asia" and "East Asia & Pacific" block into one block (Asia). Furthermore, Israel and South Africa are allocated to the emerging Europe and Asia block, respectively.

because of their size.[7] Standard unit root tests clearly reject the hypothesis that the capital flow to GDP series are integrated. Based on these results – summarized in Appendix C – we decide to estimate our factor model in levels. As a last step, all series are normalized to have a mean of zero and a variance of one.

Table 1 contains some descriptive statistics for the original capital flow to GDP series. Several aspects are noteworthy. First, some regions and income groups attract significantly more inflows relative to domestic economic activity than others. Inflows to industrial economies, e.g., averaged to 4.2% of their respective GDP across all types of flows whereas the number is only 1.65% for the typical Latin American country. Second, the geographical groups differ in the type of flow their members predominantly depend on. While portfolio inflows are the major source of finance for industrial and Asian economies, other inflows and FDI inflows are more important for countries falling into the emerging Europe and Latin America group, respectively. Third, industrialized (5 cases) as well as emerging European economies (1 case) account for all of the most extreme observations in our sample. This mainly reflects their dominant role in the run-up to and the aftermath of the recent global financial crisis. Finally, we also find some support for the notion that FDI is a more resilient source of finance than other types of capital inflows (Stiglitz, 2000). Across all regions, the FDI to GDP series have the smallest standard deviation (5.7%). Somewhat surprisingly, however, those of the portfolio inflows to GDP series are only slightly larger (5.8%).

≪ insert Table 1 here ≫

The descriptive statistics discussed so far are silent about the degree of comovement between international capital flows which is central to our analysis. A first impression of this aspect can be gauged from Table 2 which shows the average group-specific correlation coefficients of our capital flows to GDP series along with Pesaran's CD-statistic (Pesaran, 2004). This statistic – displayed in parenthesis – is based on all estimated individual correlation coefficients and offers a test of the null hypothesis of no cross section dependence.[8] Using these concepts, we find evidence for an economically weak but

[7] We use annual GDP divided by four for this exercise. Qualitative similar results can be obtained using data on quarterly GDP for reporting countries. These results are available from the authors upon request.

[8] For balanced panels the CD-statistic is calculated as $CD = \sqrt{\frac{2T}{N(N-1)}}\left(\sum_{i=1}^{N-1}\sum_{j=i+1}^{N}\hat{\rho}_{ij}\right)$, where N and T denote the number of series and time periods, respectively. The $\hat{\rho}_{ij}$ are the estimated correlation coefficients between the series i and j. Under the null hypothesis, CD follows a standard normal distribution.

statistically significant degree of comovement between capital inflows. Contrary to the notion that all capital flows tend to move together, the average correlation coefficient across all types and recipients is just 0.05. A single common factor obtained from a standard dynamic factor model is thus likely to have only limited explanatory power for the individual series of capital inflows. The average correlation coefficients are somewhat larger among specific flow types and country groups. Encouragingly, the degree of comovement is even higher for region-specific FDI, portfolio and other inflows. The average correlation between FDI flows to emerging Europe, e.g., is 0.17 compared to a value of just 0.08 for all FDI inflows. Similar tendencies can be found for other regions and types of capital flows. This observation is consistent with the hypothesis that important developments are common to groups of countries and capital flows but not to all series in our data set. The dynamic hierarchical factor model is thus an ideal tool to disentangle the relative importance of these factors.

≪ insert Table 2 here ≫

4. A Dynamic Hierarchical Factor Model for Gross Capital Flows

The econometric framework we rely on is the dynamic hierarchical factor model as presented in Moench et al. (2011). It is a four level model allowing us to split the causes of dynamics in our data into four categories, namely idiosyncratic, regional, flow-specific and global disturbances.[9], The model's hierarchical structure implies that subblock factors, i.e. factors on the most disaggregated level, hinge on superordinated factors. These interdependencies are taken into account during estimation.

Our four level factor model is build as follows. Let b and s respectively denote the specific block and subblock the observed variable n is assigned to. In our case, block b corresponds to a specific type of capital inflows whereas subblock s classifies a geographical region. Each subblock consists of N_{bs} time series different among subblocks.

[9]In another application, Moench and Ng (2011) use the dynamic hierarchical factor model downsized to three levels to analyze the U.S. housing market after the Bretton-Woods era.

For the observation Z_{bsnt} in period t, we assume the following relation between the data point and the factors:

$$Z_{bsnt} = \Lambda_{Zbsn} H_{bst} + u_{Zbsnt} \tag{1}$$

$$H_{bst} = \Lambda_{Hbs} G_{bt} + u_{Hbst} \tag{2}$$

$$G_{bt} = \Lambda_{Gb} F_t + u_{Gbt} \tag{3}$$

Here, Λ_{Zbsn}, Λ_{Hbs} and Λ_{Gb} denote the time-invariant factor loadings. The factor H_{bst} captures common movements between all the variables in subblock s of block b. All subblock factors of block b are related to the factor G_{bt} which explains joint fluctuations on the block level. In turn, G_{bt} depends on the global factor F_t collecting the part of the variance that is common to all time t observations. Thus, innovations to one factor will have an effect on all subordinated levels but not the other way round, e.g. global factors are independent of local incidents.

To address the persistence in our data set we make the assumption of autoregressive processes. This is the case for the global factor F_t:

$$F_t = \rho_F F_{t-1} + \epsilon_{Ft} \tag{4}$$

where the matrix ρ_F would contain the autocorrelation parameters. For estimation, we consider only one global factor so that ρ_F is a scalar. Moreover, we assume that:

$$u_{Zbsnt} = \rho_{Zbsn} u_{Zbsn(t-1)} + \epsilon_{Zbsnt} \tag{5}$$

$$u_{Hbst} = \rho_{Hbs} u_{Hbs(t-1)} + \epsilon_{Hbst} \tag{6}$$

$$u_{Gbt} = \rho_{Gb} u_{Gb(t-1)} + \epsilon_{Gbt} \tag{7}$$

with $\epsilon_{jt} \sim N(0, \sigma_j^2)$, $j = Zbsn, Hbs, Gb, F$. All ϵ_{jt} are uncorrelated across j and t.

Since we are interested in only one factor on each stage described by equations (1), (2), (3), restrictions necessary to ensure identification are reduced to a minimum. The first elements of Λ_i, $i = Zbs, Hbs, Gb$, take a value of unity. Moreover, as in Moench et al. (2011), the variances σ_{Hbs}^2, σ_{Gb}^2 and σ_F^2 are set to 0.1.

Estimation of the dynamic hierarchical factor model requires the consideration of the vertical connection between the factors as constituted in equations (1), (2), and (3). We do so by applying Markov Chain Monte Carlo methods. Iteratively, it first draws each factor given the parameters, the other factors and, for the subblocks, the data. In a second step, parameters are drawn based upon the obtained factors.[10] Overall, we perform 100,000 draws from which we retain every 50th of the last 50,000 draws for our analysis.

The dynamic hierarchical factor model is ideally suited for our analysis of capital inflows. Its level structure allows to separately identify regional and global factors. Furthermore, all factors are influenced by superordinated factors while subordinated effects do not spill over to global factors. A conventional non-hierarchical factor model would not take account of this one-directional relationship. Moreover, with our hierarchical model we are able to investigate how important fluctuations on different stages are for a specific time series, a feature not on hand in a simple factor analysis.

5. Results

The rich set of results of the factor decomposition is presented in two parts. In a first part, we provide a graphical analysis of the evolution of the global, the type-specific and the regional factors separately for each type of flows and for each region. These results can be found in Figures 1 to 3. In a second part, we decompose the variance of each capital inflows series into the shares attributable to either of our three factors and the idiosyncratic component. This variance decomposition is presented in Table 3.

Our estimated global factor extracted from the large set of countries closely reflects the well-known capital flow cycles of the past two decades. While the Mexican crisis of 1994, the Asian crisis of 1997 and the crises hitting Russia, Brazil and Argentina thereafter are indicated by relatively small declines in the global factor, its overall evolution is clearly dominated by the most recent financial crisis in 2008/09. At the peak of the crisis the connection between all factors intensifies suggesting that the pattern of comovement changes substantially during severe global crises.[11]

The flow-specific factors follow a similar pattern, although the similarity with the global factor differs remarkably across types of capital inflows. Whereas the portfolio and

[10]See Moench et al. (2011) for a detailed description of the specific Markov Chain Monte Carlo procedure applied in this setup. We use the MATLAB codes available on Serena Ng's website.

[11]In a companion paper (Förster et al., 2012) we show that actual capital inflows are also more closely tracked by the global factor during the recent crisis period.

other flow factors track the global factor quite closely, see Figure 2 and Figure 3, the FDI factor is considerably more independent from the global factor, see Figure 1. Flows to emerging Asia or Latin America, as characterized by their regional factors, in turn, appear only loosely connected to conditions reflected by the global factor. Likewise, the regional factors evolve differently from each other over time and sometimes even exhibit divergent dynamics. In the aftermath of the recent financial crisis, for example, the regional factors for FDI inflows to Asia reflect the regained momentum of FDI flows into this region, while FDI flows to Latin America and emerging Europe remained subdued.

≪ insert Figures 1, 2 and 3 here ≫

While the graphical analysis of the factors is interesting, it cannot reveal the extent to which capital inflows in a given region or within a given asset class are affected by different factors. To address this issue, the factors have to be discussed together with estimated factor loadings. To facilitate the interpretation, Table 3 reports a decomposition of the variance of capital inflows into the shares attributable to our different factors. This decomposition has been constructed using the mean within each subblock for every draw, from which the median and the 33% as well as the 66% percentiles over all retained draws are reported.

≪ insert Table 3 here ≫

The results show that the idiosyncratic component is by far the most important determinant of capital inflows. It explains about 80% of fluctuations in capital inflows. The regional factor is responsible for between 5% and 36% of overall variation and is more relevant for emerging economies than for capital flows to industrial countries. Flows to Latin America are particularly prone to fluctuations in the regional factor, which accounts for 17% of the variation in FDI inflows to Latin America and 18% of portfolio inflows to this region. For Asia and emerging Europe, the regional factor matters most for FDI inflows and other types of inflows, but less so for portfolio inflows. The regional factor is very important for flows other than FDI or portfolio flows to emerging European economies. This may reflect the strong dependence of those economies on bank lending from advanced European economies.

The flow type-specific factor plays an important role for FDI inflows into industrial economies. For those economies 13% of fluctuations can be traced back to fluctuations in the global FDI factor. Surprisingly, the global portfolio factor plays a small role with a share of about 5% only.

Finally, the global factor, i.e. the factor potentially affecting all countries and all types of capital inflows, has a small impact on portfolio inflows to the Asian and the industrial countries in our sample but almost no impact on FDI inflows or portfolio inflows to Latin America. The global factor seems to matter most for inflows other than FDI and portfolio inflows to industrial economies. This probably again reflects the strong impact of cross-border bank lending among global financial intermediaries in advanced economies as these lending activities might be reduced disproportionally after a global financial shock.

The sum of the variance shares explained by global and flow type-specific factors, respectively, indicates the extent to which a country is affected by forces common to all countries. The results suggest that this measure is substantially larger for portfolio flows to Asia than for FDI flows into this region. This confirms the popular view that portfolio investors are particularly affected by global conditions, whereas FDI investment is not. In Latin America, however, this measure is stronger for FDI than for portfolio flows. Taken together, we do not see a clear-cut pattern as to which type of capital flows is less affected by global forces.[12] However, the global factor explains less than 1% of variations in FDI inflows across all regions, whereas it explains a sizeable fraction of fluctuations in portfolio and other types of inflows.

In sum, our findings are consistent with the view that the bulk of swings in capital inflows is driven by country-specific components followed by the regional factors.[13] Thus, we cannot lend support to the view put forward by the The Economist (2011) arguing that capital inflows are driven by factors beyond the control of domestic policy. However, the results do also illustrate that the recent financial crisis was characterized by an extraordinarily large comovement of capital flows across regions and flow types. We address this issue again in section 7. Prior to this, the next section examines the variables that determine the extent to which a country is exposed to global drivers of capital flows.

[12]This also implies that, if a high dependency on global forces is considered detrimental to financial stability, it is not straightforward to classify one of these types of capital inflows along the lines of either "good" or "bad" or "cold" or "hot" types of inflows. This supports the results presented by, among others, Claessens et al. (1995) and Sarno and Taylor (1999).

[13]Our results are notably different from those presented by Broto et al. (2011), who argue that based on a panel of capital flows series up to 2006 global factors became increasingly more important relative to country-specific drivers after 2000.

6. Explaining the Exposure to Global Drivers of Capital Flows

The previous section revealed that the exposure to the estimated factors differs across countries and across different types of capital inflows. This leaves the question as to what structural characteristics determine whether inflows to a given country are particularly prone to global determinants. To answer this question, we proceed in two steps. In a first step, we add the variance shares accounted for by the global and the flow type-specific factor for each country. This gives us each country's exposure to factors that are global in nature in the sense that these factors potentially affect all countries in the sample. In a second step, we relate these accumulated variance shares to structural features of small open economies in a cross-sectional regression.

Four different explanatory variables are taken into account. The first is an index of capital controls *(Capital Controls)*, for which we use the indicator for financial integration developed in Schindler (2009). A high value of this index indicates tighter controls on capital inflows. The degree of trade openness *(Trade)*, our second explanatory variable, is measured by the sum of exports and imports relative to GDP. The data for this measure comes from the World Bank (2011). Since trade linkages are often thought of as transmitting contagious financial crises, a larger trade-to-GDP ratio possibly increases the sensitivity to global forces. As the third variable we use the degree of flexibility of the exchange rate regime *(Exchange Rate Flexibility)*, which we measure using the Iltzeki et al. (2008) de facto classification of exchange rate regimes ranging from one (completely fix) to six (completely flexible). Finally, we use the ratio of liquid liabilities to GDP *(Liquid Liabilities)* as a measure of financial depth. We take this measure from Beck et al. (2009), who constructed it as the interest-bearing liabilities of banks and other financial intermediaries divided by GDP. For each of these four explanatory variables we use the mean over the sample period, i.e. 1994 to 2010.[14]

Figures 4 to 7 depict scatter plots of the exposure to global factors against each explanatory variable for each type of capital inflows. As expected, we find that a higher degree of capital account restrictions reduces the variance share explained by global factors. Surprisingly, an economy more open to exports of goods and services experiences a smaller exposure to global determinants of capital inflows, although the strength of this relationship is weak. Interestingly, fixed exchange rates are associated with a larger share of FDI fluctuations explained by global factors. For portfolio inflows the exchange

[14]See Appendix A for further details on data sources and definitions.

rate regime seems to be irrelevant. Thus, fixing the exchange rate does not shield the economy from global drivers of capital flows. This is probably the most striking finding of this analysis. The strongest connection can be seen between the development of the financial system and the exposure to global factors. A larger financial sector relative to GDP increases the fraction of volatility explained by global factors.

≪ insert Figures 4 to 7 here ≫

Table 4 reports the results from a simple cross-sectional regression of the variance shares on all four explanatory variables and a constant. For portfolio flows the size of the financial sector is by far the most important determinant. A higher financial development leads to an increased exposure to global factors. For FDI and other types of capital inflows the tightness of capital controls plays the largest role. Capital controls thus significantly dampen the impact of global dynamics on a country's capital inflows. With an R^2 of more than 20% the explanatory power of this parsimonious regression is surprisingly large.

≪ insert Table 4 here ≫

These results are consistent with the "pecking order" hypothesis of cross-border investment evaluated empirically by Daude and Fratzscher (2008). These authors find evidence for some asset classes being more relevant for advancing financial integration than others. They also find that portfolio investment is more sensitive to the development of the financial system than FDI. In the present paper we show that the global and the portfolio factor translate into larger capital inflow fluctuations for a better developed domestic financial system.

7. Robustness

In this section we check the robustness of our results with respect to changes in the econometric model, the treatment of outliers and the sample period. As a first step, we want to investigate whether our results are dependent on the hierarchical modeling approach. So far, we revealed that country-specific properties to a large extent explain variations in capital inflows. One aspect to be considered is that the limited influence of the global factor may hinge on the pyramidal structure of our econometric model. Furthermore, the transmission channel depends on the pass-through of the superordinated factors to the data via subordinated factors.

To examine these concerns, we confront our data set with an alternative factor model. For that purpose we choose the latent dynamic factor approach of Kose et al. (2003) and Neely and Rapach (2011).[15] The main difference between these two approaches is the absence of the hierarchical structure in the Neely-Rapach model. Instead, the authors estimate the factors via a set of dummy variables for which no explicit interdependence is assumed.[16] The outcome of this exercise is presented in Table 5.[17] While the idiosyncratic component explains on average 80.2% of the variance of our observables in the hierarchical factor model, Neely and Rapach's (2011) method yields a value of 79.6% which is only slightly smaller than ours. Remarkably, around half of the estimated individual variance shares are identical, i.e. deviations are smaller than 1 percentage point. Furthermore, within the groups of FDI inflows and other inflows their ranking coincides with our results. Altogether, our outcomes regarding the role of the idiosyncratic components are robust since we observe only minor differences between both methodologies.

≪ insert Table 5 here ≫

Returning to our original dynamic hierarchical factor framework, we next analyze whether our results are robust with respect to the treatment of outliers. In principle, extreme values of capital inflows could be the consequence of rare economic events like balance of payments crises that are in turn caused by global, regional or country-specific developments. Hence, our previous approach would be correct and the original data should be used in the econometric analysis. However, extreme observations could also reflect measurement errors in which case an outlier adjustment would be more appropriate. Since it is a priori unclear which interpretation is more accurate, we assess the importance of the outlier treatment by reestimating our model using transformed data. Here we follow the procedure of Stock and Watson (2005) and identify outliers as those observations where

[15] We use the MATLAB code accompanying the publication of Neely and Rapach (2011) on the journal's web site for our robustness exercise.

[16] Another, third approach to estimate latent variables on different levels of aggregation is made by Beck et al. (2011) in their analysis of sectoral prices in the European Monetary Union.

[17] Since we are interested in whether idiosyncratic effects remain important, we refrain from enhancing the Neely and Rapach (2011) model with a flow-type specific factor.

the absolute median deviation exceeds the series-specific inter quartile range by a factor larger than six. These values are than replaced by the median value of the preceding five observations.

Table 6 contains the variance decomposition for the estimated dynamic hierarchical factor model with outlier correction. The results are generally close to those obtained for the unadjusted series. Most striking is the absence of any significant change in the variance share of the idiosyncratic factors. Here, one would have expected to find lower values if the eliminated outliers were the consequence of series-specific measurement errors. Using the unadjusted series thus seems to be the appropriate choice.

≪ insert Table 6 here ≫

As a final robustness exercise we investigate whether our results are subject to structural change. Unfortunately, a full-fledged subsample analysis is precluded by our relatively short sample size. However, we are able to isolate the effects of the recent global financial crisis by restricting our sample to the period 1994Q1 to 2008Q2 which ends before the Lehman collapse. Conjecturing that the degree of comovement between capital flows has been exceptionally high during the latest downturn, we expect to find a reduced importance of global factors in this subsample.

A look at Table 7 reveals that our time series are indeed less influenced by global forces during the pre-crisis period. This holds true for all types of capital inflows. Instead, regional determinants seem to be more important for foreign investors. As expected, the comovement among capital inflows has been exceptionally large during and after the global financial crisis. Thus excluding this period leads to a significant reduction in the variance explained by global forces that is matched by an increased importance of regional aspects. Furthermore, the variance share of the idiosyncratic component falls only slightly by 3% on average and is still by far the most important driving force behind capital inflows accounting for over three quarters of the observed variance.

≪ insert Table 7 here ≫

8. Conclusions

In this paper, we estimated a dynamic hierarchical factor model that is able to decompose capital flows in a large panel of countries into (i) a global factor common to all types of inflows and all recipient countries, (ii) a factor specific to a given type of capital inflows, i.e. either foreign direct investment (FDI), portfolio investment or other kinds of investment, (iii) a regional factor driving economies in geographical proximity and (iv) a country-specific component.

Our results demonstrate that the global factor tracks the overall capital flow cycles well, but leaves a large degree of heterogeneity attributable to either regional or country-specific determinants. In fact, the country-specific determinant explains by far the largest fraction of fluctuations in capital inflows. This component alone accounts for between 60% and 80% of the dynamics of international capital inflows. The regional factor explains between 5% and 20% of the fluctuations. Finally, only a small share of overall variation can be attributed to the global factor.

This suggests that domestic policy has considerable room to affect capital flows and, if this is deemed appropriate, also to limit the consequences of capital inflows such as asset price booms and a real appreciation of the domestic currency. Policymakers of small open economies are often anxious about waves of global capital flows. Inflows unrelated to country-specific economic fundamentals but instead driven by global driving forces, the argument goes, pose a threat to domestic financial stability. Curbing capital inflows by means of outright capital controls or other measures is often seen as the ultima ratio in a situation in which a country receives massive capital inflows driven by global determinants over which domestic policy has no control (see Ostry et al. (2011)). Our results, however, suggest that this is less often the case than previously thought. Thus, the primary responsibility for dealing with large and volatile capital flows remains with domestic policymakers.

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Table 1: Descriptive Statistics

	Obs	Mean	Std. Dev.	Min	Max
FDI inflows					
Industrial	1156	0.0283	0.0589	-0.3473	0.9552
Asia	612	0.0131	0.0169	-0.0397	0.2228
Emerging Europe	816	0.0434	0.0803	-1.0698	0.9206
Latin America	612	0.0364	0.0333	-0.0863	0.3096
All	3196	0.0308	0.0572	-1.0698	0.9552
Portfolio inflows					
Industrial	1156	0.0530	0.0736	-0.6166	0.5793
Asia	612	0.0143	0.0325	-0.1407	0.1992
Emerging Europe	816	0.0145	0.0459	-0.4151	0.3811
Latin America	476	0.0084	0.0310	-0.1038	0.3019
All	3060	0.0281	0.0579	-0.6166	0.5793
Other inflows					
Industrial	1156	0.0445	0.1286	-1.3225	0.9356
Asia	612	0.0056	0.0426	-0.2816	0.1551
Emerging Europe	816	0.0443	0.0808	-0.2845	0.5132
Latin America	612	0.0030	0.0617	-0.7485	0.3105
All	3196	0.0290	0.0954	-1.3225	0.9356

Table 2: Average Correlation Coefficient and Pesaran CD-statistic

	Industrial	Asia	Emerging Europe	Latin America	All
FDI	0.11	0.06	0.17	0.09	0.08
	(10.96***)	(2.74***)	(11.65***)	(4.23***)	(22.70***)
Portfolio	0.14	0.13	0.05	0.10	0.08
	(12.98***)	(6.24***)	(3.40***)	(3.94***)	(19.56***)
Other	0.16	0.15	0.25	0.03	0.10
	(15.08***)	(7.52***)	(17.04***)	(1.57)	(27.54***)
All	0.08	0.05	0.08	0.04	0.05
	(24.43***)	(7.76**)	(17.10***)	(5.21***)	(39.55***)

Pesaran CD-statistics are shown in parenthesis. ***, **, and * denote significance levels of 1%, 5% , and 10%.

Table 3: Variance Decomposition

	global	flow-specific	regional	idiosyncratic
FDI inflows				
Industrial	0.6	13.3	6.2	79.2
	[0.2, 1.1]	[12.3, 14.6]	[5.7, 6.8]	[77.9, 80.4]
Asia	0.0	0.2	17.6	82.0
	[0.0, 0.0]	[0.1, 0.3]	[16.3, 19.3]	[80.4, 83.2]
Emerging Europe	0.0	0.1	20.9	78.8
	[0.0, 0.0]	[0.1, 0.3]	[19.2, 22.6]	[77.2, 80.4]
Latin America	0.0	1.3	17.2	81.2
	[0.0, 0.1]	[0.7, 2.0]	[16.4, 17.9]	[80.6, 81.8]
Portfolio inflows				
Industrial	5.7	5.0	4.6	84.0
	[4.0, 7.4]	[4.3, 5.9]	[4.1, 5.2]	[82.9, 85.1]
Asia	4.9	4.6	10.6	77.3
	[3.5, 6.4]	[3.4, 6.9]	[8.7, 13.0]	[75.1, 79.1]
Emerging Europe	1.1	1.1	9.4	88.0
	[0.8, 1.6]	[0.7, 1.6]	[8.0, 10.2]	[87.3, 88.7]
Latin America	0.4	0.4	18.4	80.2
	[0.2, 0.8]	[0.2, 0.8]	[17.6, 19.2]	[79.4, 81.0]
Other inflows				
Industrial	12.9	5.5	5.2	76.1
	[11.9, 14.0]	[5.1, 6.0]	[4.9, 5.5]	[74.8, 77.3]
Asia	3.2	1.4	12.6	82.7
	[2.5, 4.1]	[1.1, 1.7]	[11.5, 13.7]	[81.5, 83.4]
Emerging Europe	0.4	0.2	35.5	63.6
	[0.2, 0.9]	[0.1, 0.4]	[33.2, 37.5]	[61.8, 65.3]
Latin America	1.1	0.5	15.7	82.4
	[0.7, 1.7]	[0.3, 0.8]	[14.8, 16.6]	[81.6, 83.1]

Medians, 1/3 and 2/3 percentiles (in brackets) denoted in percentage terms.

Table 4: Cross-sectional Regression, Variance Shares of Global Factors

	(1) *FDI*	(2) *Portf.*	(3) *Other*
Capital Controls	-0.10**	0.03	-0.13**
	(-2.68)	(0.75)	(-2.24)
Trade	-0.00	-0.00	-0.00
	(-0.60)	(-0.92)	(-0.25)
Exchange Rate Flexibility	-0.04**	0.01	-0.03
	(-2.11)	(0.64)	(-1.26)
Liquid Liabilities	0.08	0.15***	0.10
	(0.99)	(5.80)	(0.97)
Constant	0.17*	0.00	0.16
	(1.89)	(0.02)	(1.39)
Obs	41	40	41
R^2	0.27	0.22	0.21

t-statistics are shown in parenthesis. ***, ** and * denote significance levels of 1%, 5%, and 10%.

Table 5: Variance Decomposition for Alternative Factor Model

	global	specific to flow/region	idiosyncratic
FDI inflows			
Industrial	3.3	14.7	82.0
	[2.9, 3.6]	[14.2, 15.2]	[81.6, 82.4]
Asia	5.2	6.8	87.7
	[4.6, 5.9]	[4.9, 8.9]	[85.6, 89.7]
Emerging Europe	8.4	13.3	78.0
	[7.6, 9.3]	[12.3, 14.4]	[77.1, 79.0]
Latin America	2.0	16.8	81.2
	[1.6, 2.4]	[16.4, 17.2]	[80.6, 81.2]
Portfolio inflows			
Industrial	11.7	12.0	76.2
	[10.9, 12.5]	[11.5, 12.6]	[75.6, 76.9]
Asia	8.7	16.1	75.0
	[7.8, 9.8]	[14.9, 17.2]	[74.3, 75.9]
Emerging Europe	3.5	8.2	88.3
	[3.2, 3.9]	[7.6, 8.7]	[87.7, 88.9]
Latin America	2.8	11.1	86.1
	[2.3, 3.3]	[9.7, 12.3]	[84.8, 87.4]
Other inflows			
Industrial	11.6	14.2	74.3
	[10.7, 12.4]	[13.7, 14.7]	[73.7, 74.9]
Asia	7.8	12.5	79.3
	[7.0, 8.7]	[10.8, 14.0]	[78.2, 80.7]
Emerging Europe	15.4	21.4	63.1
	[14.2, 16.6]	[20.1, 22.7]	[62.7, 63.6]
Latin America	6.3	11.1	82.6
	[6.0, 6.7]	[10.2, 11.8]	[81.8, 83.5]

Medians, $^1/_3$ and $^2/_3$ percentiles (in brackets) denoted in percentage terms.

Table 6: Variance Decomposition with Corrected Outliers

	global	flow-specific	regional	idiosyncratic
FDI inflows				
Industrial	0.5	14.5	4.9	79.5
	[0.2, 0.9]	[13.4, 16.0]	[4.5, 5.3]	[78.0, 80.8]
Asia	0.0	0.2	17.4	82.0
	[0.0, 0.0]	[0.1, 0.5]	[15.9, 19.0]	[80.6, 83.4]
Emerging Europe	0.0	0.2	22.0	77.3
	[0.0, 0.0]	[0.1, 0.5]	[20.1, 23.7]	[75.7, 79.3]
Latin America	0.0	0.2	20.7	78.6
	[0.0, 0.0]	[0.1, 0.5]	[19.2, 22.6]	[77.1, 80.1]
Portfolio inflows				
Industrial	7.5	4.3	4.2	83.9
	[6.1, 8.7]	[3.9, 4.8]	[3.8, 4.7]	[82.7, 84.9]
Asia	5.8	3.5	13.6	76.1
	[4.4, 7.3]	[2.7, 4.5]	[11.9, 15.4]	[74.3, 77.9]
Emerging Europe	1.2	0.7	10.5	87.3
	[0.9, 1.7]	[0.5, 1.0]	[9.8, 11.1]	[86.8, 87.9]
Latin America	0.5	0.3	18.7	80.1
	[0.2, 0.9]	[0.2, 0.6]	[17.8, 19.5]	[79.2, 80.9]
Other inflows				
Industrial	13.6	5.4	5.1	75.7
	[12.5, 14.6]	[5.0, 5.8]	[4.8, 5.5]	[74.4, 76.9]
Asia	3.3	1.3	12.5	82.5
	[2.6, 4.2]	[1.0, 1.7]	[11.6, 13.6]	[81.6, 83.4]
Emerging Europe	0.4	0.2	35.4	63.6
	[0.2, 0.9]	[0.1, 0.4]	[33.0, 37.6]	[61.8, 65.3]
Latin America	1.1	0.5	15.5	82.6
	[0.7, 1.7]	[0.3, 0.7]	[14.7, 16.3]	[81.7, 83.4]

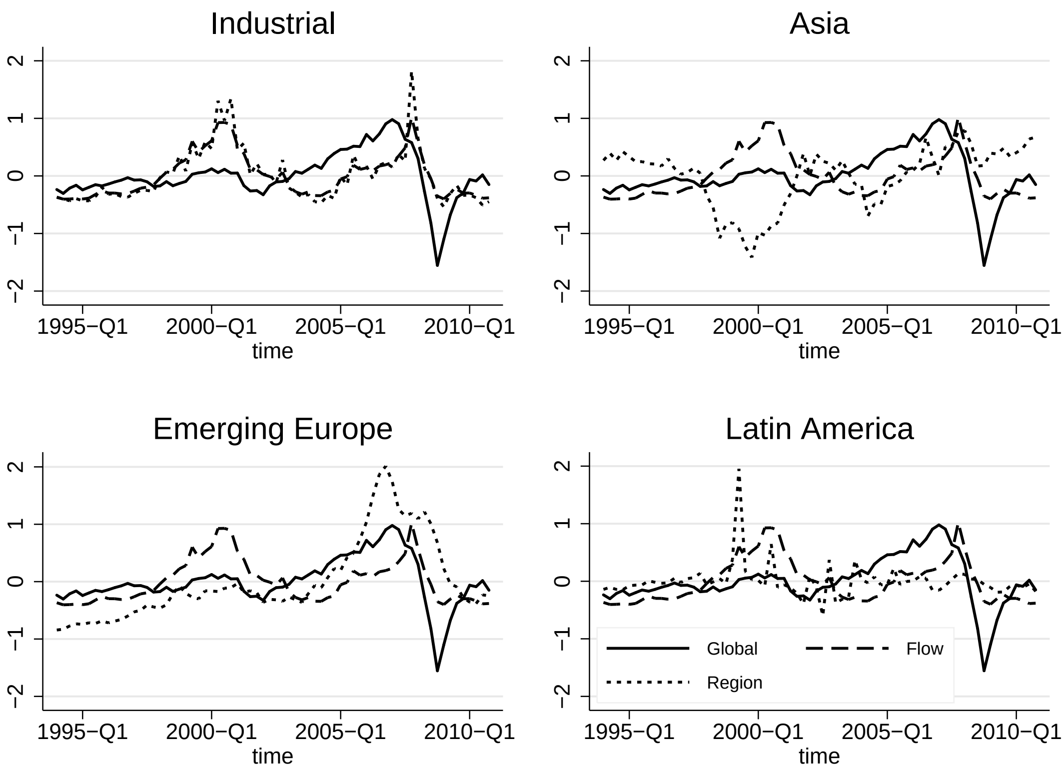
Medians, 1/3 and 2/3 percentiles (in brackets) denoted in percentage terms.

Table 7: Variance Decomposition without Financial Crisis

	global	flow-specific	regional	idiosyncratic
FDI inflows				
Industrial	0.2	14.0	8.0	77.2
	[0.1, 0.4]	[13.0, 15.0]	[7.4, 8.8]	[75.9, 78.6]
Asia	0.0	0.0	31.8	68.0
	[0.0, 0.0]	[0.0, 0.1]	[23.9, 41.3]	[58.5, 75.9]
Emerging Europe	0.0	0.1	28.0	71.7
	[0.0, 0.0]	[0.0, 0.2]	[25.2, 31.6]	[68.1, 74.4]
Latin America	0.0	0.7	18.9	80.0
	[0.0, 0.0]	[0.3, 1.4]	[18.1, 19.6]	[79.3, 80.6]
Portfolio inflows				
Industrial	0.1	18.4	3.6	77.7
	[0.0, 0.1]	[16.6, 20.6]	[3.3, 4.0]	[75.5, 79.4]
Asia	0.0	0.2	15.8	83.7
	[0.0, 0.0]	[0.1, 0.5]	[15.0, 16.5]	[82.9, 84.4]
Emerging Europe	0.0	0.4	13.8	85.6
	[0.0, 0.0]	[0.2, 0.7]	[13.0, 14.6]	[84.8, 86.3]
Latin America	0.0	1.1	17.4	81.1
	[0.0, 0.0]	[0.6, 1.8]	[16.5, 18.3]	[80.4, 81.9]
Other inflows				
Industrial	5.4	5.1	5.0	84.3
	[5.1, 5.9]	[4.9, 5.4]	[4.7, 5.3]	[83.4, 85.0]
Asia	0.1	0.1	21.3	78.4
	[0.0, 0.1]	[0.0, 0.1]	[20.2, 22.8]	[76.9, 79.5]
Emerging Europe	0.2	0.2	44.3	55.1
	[0.1, 0.3]	[0.1, 0.3]	[42.2, 46.7]	[52.6, 57.1]
Latin America	0.4	0.4	16.6	82.3
	[0.2, 0.7]	[0.2, 0.7]	[15.6, 17.4]	[81.4, 83.1]

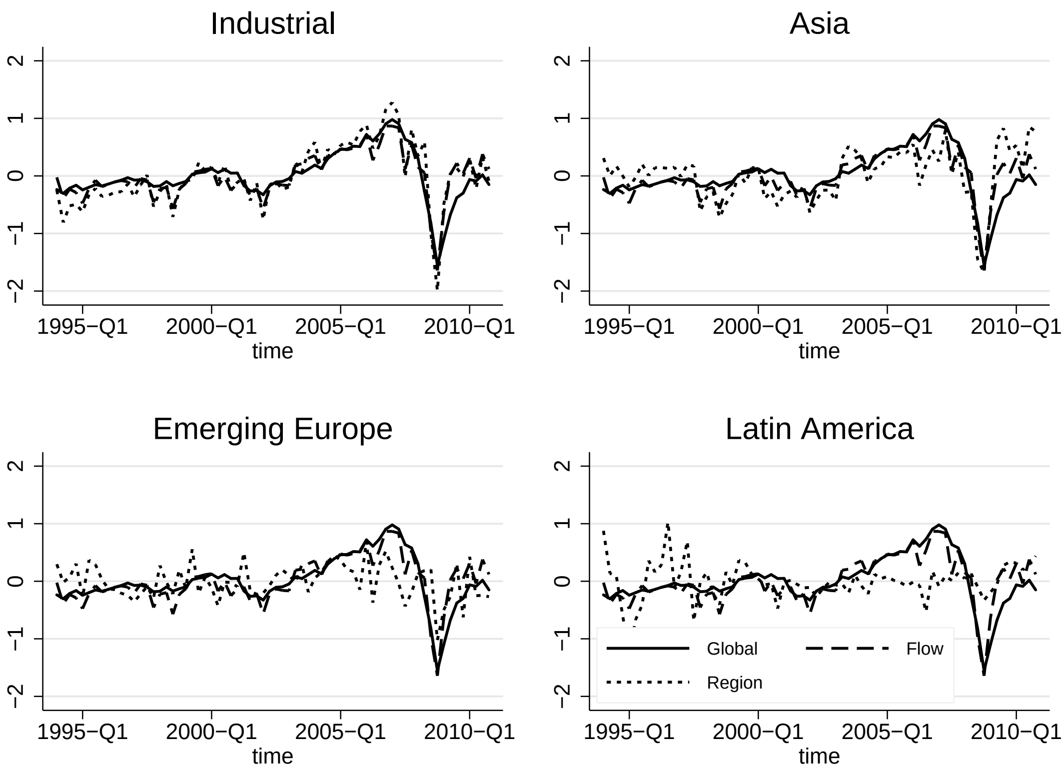
Medians, 1/3 and 2/3 percentiles (in brackets) denoted in percentage terms.

Figure 1: Decomposition of FDI Inflows



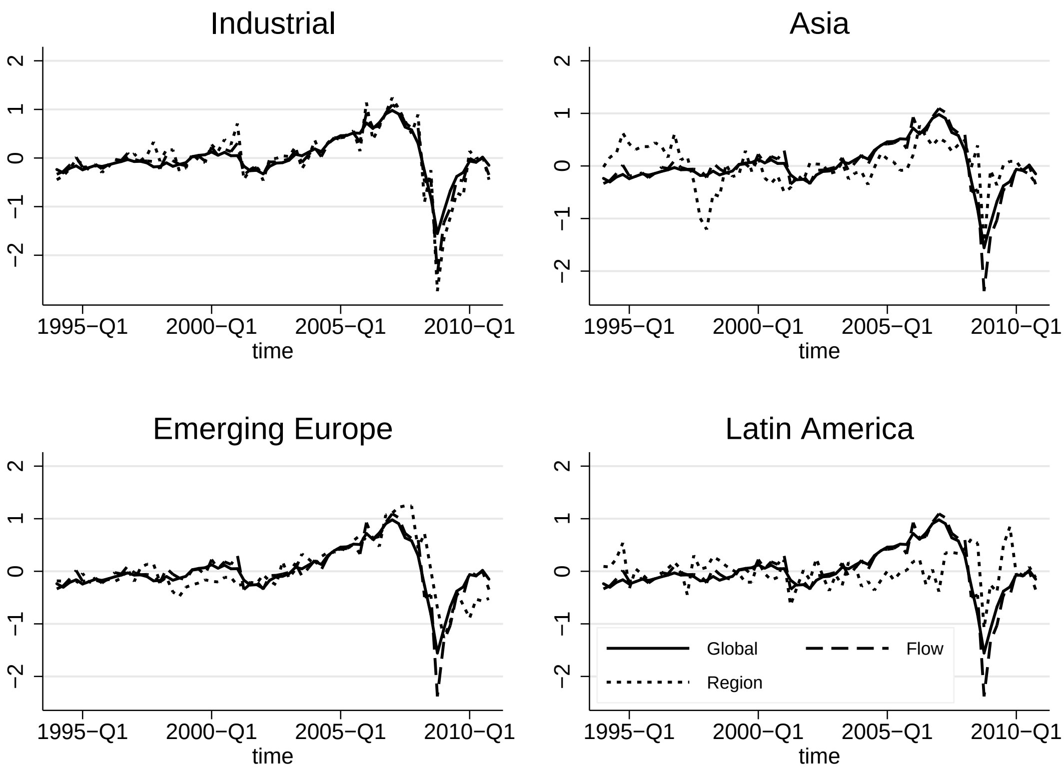
Notes: Depicted are median values of global, flow-specific and regional factors.

Figure 2: Decomposition of Portfolio Inflows



Notes: Depicted are median values of global, flow-specific and regional factors.

Figure 3: Decomposition of Other Inflows



Notes: Depicted are median values of global, flow-specific and regional factors.

Figure 4: Variance explained by global factors vs. capital controls

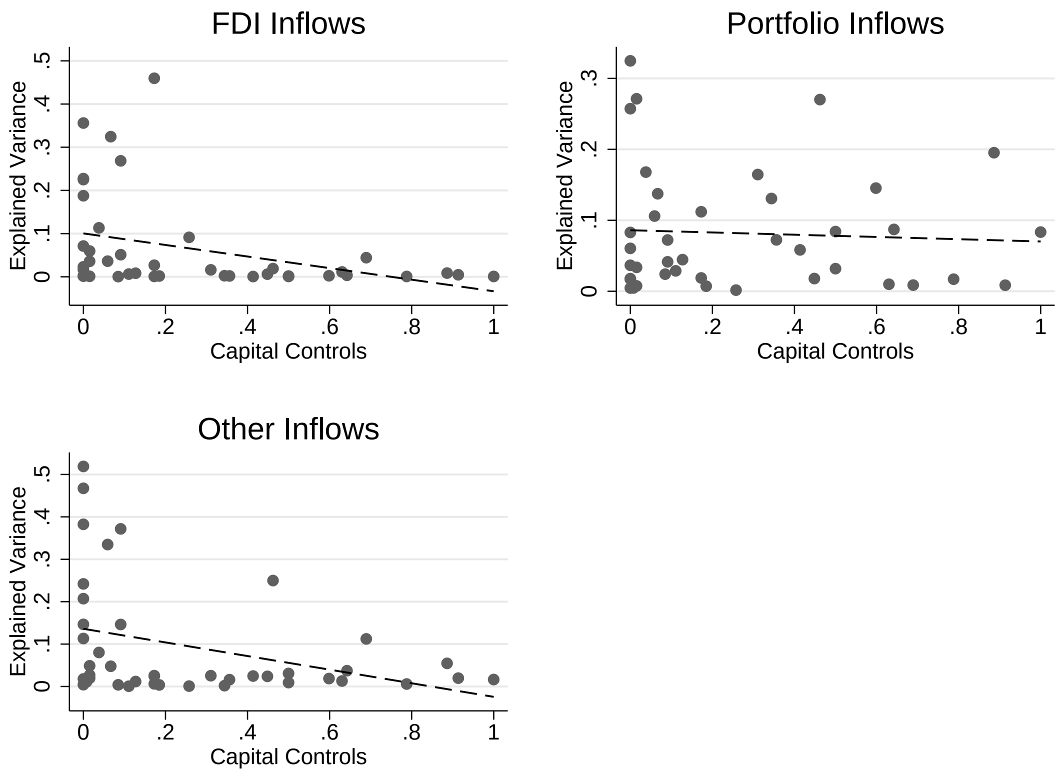


Figure 5: Variance explained by global factors vs. trade openness

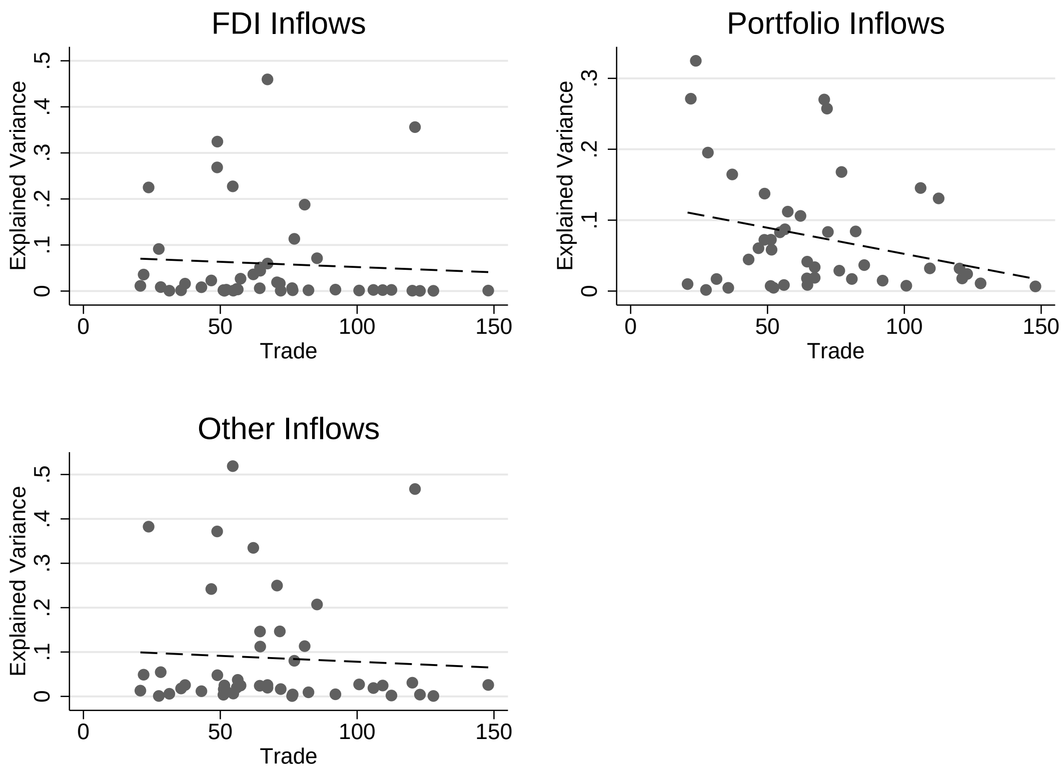


Figure 6: Variance explained by global factors vs. exchange rate flexibility

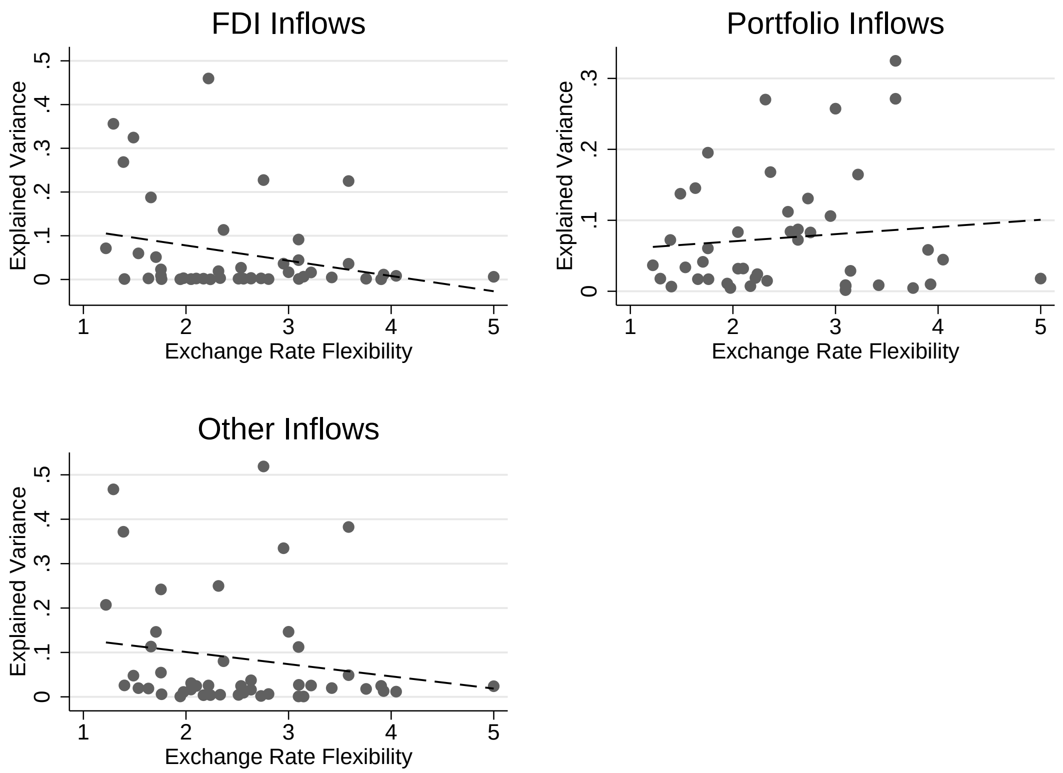
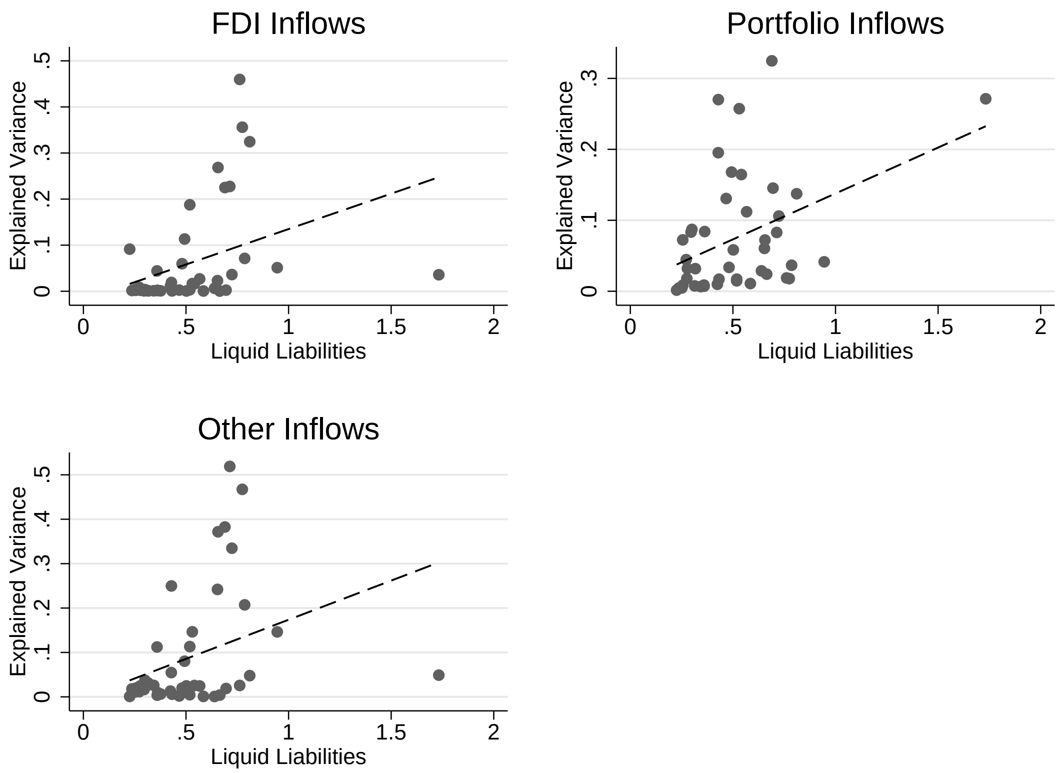


Figure 7: Variance explained by global factors vs. liquid liabilities



Appendix A. Data Sources and Definitions

Construction of the capital flow to GDP series

Data on Capital Flows:

- Primary source: IMF (2011b).
- Data on FDI, portfolio and other capital inflows measured in millions of U.S. dollars.
- Augmented with data from Taiwan (CBS (2011)) and – for 2001q1-2001q4 – from the Slovak Republic (NBS (2011)).
- Gaps in Latvia's portfolio inflows (1994Q1-1994Q4) and Slovenia's other inflows series (1994Q1-1994Q4) have been filled using data from the balance of payments' errors and omissions category as suggested by Forbes and Warnock (2011).

Data on GDP:

- Data on annual GDP expressed in national currency units from IMF (2011b).
- Augmented with data from Taiwan (using information from NSC (2011) for 1994-1996 and CBS (2011) for 1997-2010) and Nicaragua (2010) (from World Bank (2011)).
- Local currency GDP figures are converted into millions of U.S. dollars using information on exchange rates (annual period averages) from the same sources. For Euro zone members, currency conversion further requires data on official Euro conversion rates from ECB (2011).

Control Variables for Cross-sectional Regression

Capital Control Index *(Capital Controls)*:

- Index of restrictions on capital inflows
- Range: 0 (no restrictions) to 1 (fully restricted)
- Source: Schindler (2009)

Trade Openness *(Trade)*:

- Total trade (exports + imports) in percent of GDP
- Source: World Bank (2011)

Index of Exchange Rate Flexibility *(Exchange Rate Flexibility)*:

- Annual coarse classification of exchange rates
- Scale: 1 (completely fix) to 6 (most flexible)
- Source: Iltzeki et al. (2008)

Liquid-Liabilities-to-GDP Ratio *(Liquid Liabilities)*:

- Ratio of liquid liabilities to GDP
- Liquid liabilites = currency + demand deposits + interest bearing liabilities of all financial institutions
- Source: Beck et al. (2009)

Appendix B. Country Coverage and Regional Classification

Industrial countries (*Industrial*)			
Australia	Austria	Canada	Denmark
Finland	France	Germany	Italy
Japan	Netherlands	New Zealand	Norway
Portugal	Spain	Sweden	United Kingdom
United States			
Asia , Pacific Region & South Africa (*Asia*)			
Bangladesh	India	Indonesia	Korea, Republic of
South Africa	Sri Lanka	Taiwan	Thailand
Eastern Europe & Israel (*Emerging Europe*)			
Croatia	Czech Republic	Estonia	Hungary
Israel	Latvia	Lithuania	Romania
Russian Federation	Slovak Republic	Slovenia	Turkey
Latin America & the Caribbean (*Latin America*)			
Argentina	Bolivia	Brazil	Chile
Guatemala	Mexico	Nicaragua	Peru
Venezuela, Rep. Bol.			

Appendix C. Unit Root Tests

Stationarity of the capital flow to GDP series is assessed using the augmented Dickey-Fuller (*ADF*) and Phillips-Perron (*PP*) unit root tests. Table C.1 shows for each test how often the null hypothesis of instationarity is rejected at the 5 percent and 1 percent level of significance. The augmented Dickey-Fuller tests do not indicate the presence of unit roots in the majority of series irrespective of whether the regressions include only a constant (columns 3-4) or a constant and a time trend (columns 5-6).[18] The results from the Phillips-Perron tests (columns 7-8) point to the same conclusion. We therefore treat all capital flow to GDP series as I(0) and estimate the dynamic factor model in levels.

Table C.1: Unit root tests: number of stationary series

Flow Type	Number of variables	ADF[a] (5%)	ADF[a] (1%)	ADF[a] (trend) (5%)	ADF[a] (trend) (1%)	PP[b] (5%)	PP[b] (1%)
FDI	47	45	41	44	40	44	41
Portfolio	45	45	45	45	44	45	45
Other	47	47	45	45	42	45	44
Total	139	137	131	134	126	134	130

Notes: [a,b] ADF and PP denote the augmented Dickey-Fuller and Phillips-Perron tests, respectively.

[18]The augmented Dickey-Fuller tests are based on a specification with only one lag of the dependent variable. The fraction of series for which the null hypothesis is rejected decreases when a lag length of four is considered instead. The depicted specification was selected on the basis of standard information criteria.

III. THE EFFECT OF IMF LENDING ON THE PROBABILITY OF SOVEREIGN DEBT CRISES

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The Effect of IMF Lending on the Probability of Sovereign Debt Crises

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Abstract
This paper explores empirically how the adoption of IMF programs affects sovereign risk over the medium term. We find that IMF programs significantly increase the probability of subsequent sovereign defaults by approximately 1.5 to 2 percentage points. These results cannot be attributed to endogeneity bias as they are supported by specifications that explain sovereign defaults and program participation simultaneously. Furthermore, IMF programs turn out to be especially detrimental to fiscal solvency when the Fund distributes its resources to countries whose economic fundamentals are already weak. Our evidence is therefore consistent with the hypothesis that debtor moral hazard is most likely to occur in these circumstances. Other explanations that point to the effects of debt dilution and the possibility of IMF triggered debt runs, however, are also possible.

JEL Classification: F33, F34, C25, C35

Keywords: IMF programs, Sovereign defaults, Bivariate probit, International Financial Architecture

1. Introduction

When the banking panic of the years 2007/2008 endangered the stability of the worldwide financial system governments stepped in by providing a mixture of generous public guarantees and fiscal stimulus. Since then, the resulting large primary deficits and swollen debt burdens of many countries have brought sovereign risk back on the agenda of investors, policy makers and economists alike. Even among developed economies some countries - most notably Greece - experienced a dramatic loss of market confidence and saw the interest rates on their debt skyrocketing. In the search for a solution to the problem of looming debt crises politicians of the European Union (EU) turned towards the International Monetary Fund (IMF or Fund). Albeit its Articles of Agreement do not provide the IMF with an explicitly stated mandate to fight sovereign debt crises[1] the Fund's Executive Board answered the calls by approving a € 30 billion Stand-By Arrangement for Greece on May 09, 2010 which was supplemented by further EU loans. Spreads on Greece's ten year government bonds relative to Germany's, however, did not return to pre-crisis levels, a mere 0.5 percentage points in the average, measured from the introduction of the Euro in 2001 to the end of 2009. Instead, in August 2010 the spread averaged 8.5 percentage points, not far below its maximum of 10.4 recorded on the last trading day before the announcement of the rescue package.[2]

What has gone wrong? Surely, markets did not fail to realize that the exceptional large lending amount covers Greece's estimated liquidity needs for an extended period. Are there therefore other reasons to expect that IMF program participation is detrimental to fiscal solvency over a longer horizon? Have previous IMF programs exerted a positive or a negative influence on sovereign risk? Only few authors have addressed these important questions explicitly, which is surprising in face of the vast literature on the economic effects of IMF interventions.[3] Consequently, our paper aims to fill this gap by providing a first empirical study that relates program participation to actual default incidences.

Before turning to our empirical analysis it is useful to review the theoretical literature on the relationship between IMF interventions and sovereign risk. This literature identifies four channels through which the IMF's presence alters the probability of subsequent sovereign defaults. These channels focus on the direct effects of liquidity provision, its

[1] An indirect mandate may be deduced from the Fund's mission to help member countries with balance of payment needs since these often coincide with sovereign debt service problems.
[2] Data on spreads refers to Reuters' Ecowin Government Benchmarks.
[3] Bird (2007) and Steinwand and Stone (2008) offer extensive surveys on this topic.

influence on the governments' adjustment effort and on the role of conditionality and seniority respectively. Our analysis, however, does not provide us with a clear-cut answer to the question whether we should expect default probabilities to rise or to decrease in the aftermath of IMF programs. Rather, the sign of the effect is disputed even at the level of the individual channels. Liquidity provisions for example may as well prevent (Fischer, 1999) as trigger a run on sovereign debt (Zettelmeyer, 2000). Furthermore, even if emergency lending successfully fends off looming liquidity crises, it will also change the incentives of local policy makers regarding their own adjustment effort. The strength of economic fundamentals partly determines whether this results in a more prudent or a laxer macroeconomic policy with corresponding consequences for long run sovereign risk (Corsetti et al., 2006; Morris and Shin, 2006). The IMF therefore typically links the disbursement of money to conditions which are designed to guarantee a sustainable policy path, their impact, however, is often impaired by a lack of compliance. Finally, its role as a de facto senior creditor enables the IMF to lend at lower interest rates which clearly benefits the sovereign debtor and private creditors alike (Saravia, 2010). Large additional amounts of official lending, however, also increase the risk of future solvency crises in the same way private lending does. A default on more junior private debt may therefore become more and not less likely (Boz, 2011).

While the specific characteristics of IMF lending programs affect sovereign risk in several ambiguous ways even less is known on the aggregate effect of program participation on the likelihood of sovereign debt crises. We therefore investigate the IMF-default relationship empirically using univariate and bivariate probit methods. Summarizing our main results, we find that IMF programs significantly increase the risk of subsequent sovereign defaults by approximately 1.5 to 2 percentage points. This finding can not be attributed to endogeneity bias since the results for a specification that explains sovereign defaults and program participation simultaneously strengthen our conclusions. Neither can the results be explained by a lack of compliance with IMF conditionality. Further empirical exercises show that the magnitude of the effect depends on economic fundamentals in a way consistent with economic theory. However, we do not find a default-risk reducing effect of IMF interventions in any of our specifications. Hence, we conclude that the adoption of an IMF program seems to be no good news at all for private long-term creditors.

The present paper is organized as follows: Section 2 reviews the theoretical and empirical literature on the relationship between IMF programs and sovereign debt crises. Our empirical framework and our data basis are laid out in Section 3. Section 4 presents the results. Section 5 concludes this paper.

2. Review of the Literature

2.1. IMF Interventions and Sovereign Risk: Theory

A vast theoretical literature deals with the effects of international financial organizations' actions on the probability and magnitude of sovereign debt crises. This research highlights several channels through which the IMF might influence short and long-term sovereign risk in either a positive or a negative way.

The first channel focuses on the direct consequences of liquidity provision in the context of a debt run. Using models with multiple equilibria many researchers starting with Sachs (1984) show that self-fulfilling runs could lead to a default of an otherwise solvent sovereign debtor.[4] Acting as an international lender of last resort whose liquidity provision renders the search for inefficient sources of finance in the event of a run unnecessary the IMF may prevent the occurrence of those crises in the first place (Fischer, 1999). Subsequent research focuses on the question whether this conclusion still holds in a realistic setting with only limited IMF resources. As a result two starkly different positions have emerged. Zettelmeyer (2000) argues that rescue packages which cover only a fraction of the potential liquidity needs might not only fail to eliminate the possibility of a crisis but even have counterproductive effects. In the worst case, the provision of the liquidity that is demanded by short term investors can be the trigger that leads to a debt run. Contrary to this view Corsetti et al. (2006) offer a more positive assessment of limited IMF crisis lending using the framework of a global game. In their model official lending induces a greater fraction of lenders to rollover their debt which lowers the incidence of crises.

Second, programs designed to provide short term liquidity also influence the incentives of borrowing governments with regard to their policy stance. A moral hazard problem may especially arise if the IMF fails to differentiate between liquidity and solvency crises. In this case, sovereign debtors have the incentive to neglect necessary but painful policy

[4] Alesina et al. (1990) and Cole and Kehoe (1996, 2000) provide other examples for open economy debt run models.

adjustments and rely on official emergency lending instead which is often characterized by sizeable subsidy elements (Vaubel, 1983; Meltzer Commission, 2000). As a consequence an ongoing IMF program may increase rather than decrease sovereign risk in the medium and longer term if it is interpreted as a signal for further support. This effect, however, is far from clear-cut as the recent work on global games by Corsetti et al. (2006) and Morris and Shin (2006) has shown. In their models, liquidity provision can as well induce debtor countries to undertake otherwise infeasible adjustment programs, convincing short-term creditors to stay and thereby improving the fate of long-term investors. This virtuous cycle, dubbed as 'catalytic finance', is most likely to work in an environment of neither too bad nor too good fundamentals. In other circumstances country leaders will see official funding as a substitute for their own adjustment effort and moral hazard will prevail.

A third strand of the literature points to the importance of conditionality in IMF programs.[5] Policy conditions that accompany lending programs may influence economic outcomes either through their signaling function or by initiating policy improvements. Regarding the first point Marchesi and Thomas (1999) develop a model in which only productive countries choose to incur the short term costs associated with an IMF program. The participation decision therefore delivers an important signal to private investors which may respond with a debt relief or - in more general terms - with improved capital market access for the debtor country. According to this line of argument we should thus expect a lower default probability of countries which participate in an IMF program (program countries). Regarding the policy changes countries may be willing to accept constraints on their sovereignty because it is in their own best interest[6] or because they are bribed and/or forced to do so. If the conditions imposed were justified on economic grounds and enforcement is guaranteed crises should become less likely in either case. However, both qualifications have been questioned in the literature. As the argument between prominent economists on the merits of IMF induced policy changes during the Asian crisis documented by Conway (2006) shows uncertainty still surrounds optimal policy design in times of crises (Bird, 2007). Even if the medicine prescribed by IMF conditionality is the right one its effectiveness is questionable when compliance is a major problem. With official compliance rates of 54% (IEO, 2007) the effect of conditionality on default probabilities is at least uncertain.

[5]See Dreher (2009) for a survey on the economic effects of IMF conditionality.

[6]The resolution of time inconsistency problems is the leading example, see Sachs (1989), Fafchamps (1996) or Drazen (2002).

Fourth, the perceived seniority of IMF debt[7] has the potential to influence the Fund's lending decisions and the risk borne by private creditors. As Saravia (2010) points out, seniority enables the IMF to provide larger amounts of new short-term debt at lower interest rates to fill the liquidity gap of sovereign debtors without risking its shareholders' money. IMF lending thus reduces both the probability of liquidity crises and - through the effect of lower interest rates - subsequent solvency crises. Overall, this leads to an improvement of the position of private creditors despite the dilution of their claims. Policy implications differ, however, if the assumption of a fixed demand for new financial resources is dropped. Boz (2011) analyzes the effects of senior IMF lending in these circumstances invoking a 'willingness to pay' framework. Since liquidity considerations are absent in her model the only effect of a new official lending program is an increase in total debt that leads to higher debt service in the future making a subsequent default relatively more beneficial from the debtor's point of view.

To sum up, IMF lending surely affects sovereign risk through several channels. The sign of the effect, however, is disputed even at the level of the individual channels. Much less is known on the relative strength of these channels and their potential interactions.[8] Whether an IMF involvement decreases or increases the probability of a subsequent default is therefore ultimately an empirical question.

2.2. IMF Interventions and Sovereign Risk: Empirical Evidence

While an investigation of the effects of IMF interventions on sovereign default probabilities has not yet been undertaken our research builds on the large empirical literature on the economic consequences of IMF programs. Since most earlier studies do not control for the problem of self-selection into IMF programs and a complete survey of this literature is beyond the scope of this paper we focus instead on some recent contributions that are related to our own research agenda.[9]

[7]The perception of seniority can be justified empirically since according to Zettelmeyer and Joshi (2005) '. . . the Fund has virtually always been repaid in the past'.

[8]A rare example of a quantitative assessment of the combined effect of more than one channel is Boz (2011). Accounting for the effects of seniority and conditionality in a calibrated model of the Argentinean economy she concludes that sovereign risk increases after the disbursement of IMF loans.

[9]The survey articles written by Bird (2007) and Steinwand and Stone (2008) provide a more complete overview of the research on causes and consequences of IMF programs.

IMF interventions may influence sovereign risk through their impact on economic growth and macroeconomic policy. The finding of a negative (Barro and Lee, 2005; Dreher, 2006) or at best insignificant (Atoyan and Conway, 2006) causal effect of IMF programs on economic growth is troublesome in this respect since most theoretical models of sovereign debt point to a higher incidence of crises in times of economic hardship. The evidence is somewhat more encouraging for other factors potentially influencing sovereign risk like budget deficits and money growth (Dreher, 2005).

A similar objective like ours is pursued in two recent papers that analyze the success of IMF interventions in terms of crises prevention, both reaching an overwhelmingly positive conclusion. Eichengreen et al. (2008) show that countries are less likely to suffer from sudden stops in the years following their participation in an IMF program. This effect is less positive for countries with weak fundamentals which are already on the brink of crisis. Dreher and Walter (2010) demonstrate that the IMF is also quite successful in resolving currency crises. The existence of an IMF program in the previous five years reduces the probability of a subsequent currency crises by 20 percentage points. Once in a crisis, however, program countries are more likely to devalue after a shorter period of defense.

Finally and more closely related to our study some authors provide evidence on the theoretical predictions of the catalytic finance literature. Mody and Saravia (2006) and Eichengreen et al. (2006) tackle this issue by studying the impact of IMF programs on sovereign borrowing costs using data from the primary market.[10] In accordance with the theoretical literature their evidence indicates that IMF programs improve the borrowing terms of countries whose debt burdens and foreign reserves are in an intermediate range. Market conditions worsen under a program if these conditions are not met, fitting nicely into the moral hazard view of IMF lending. Mody and Saravia (2006) further show that a large part of the catalytic effects can be attributed to IMF programs that turned precautionary, which means that the agreed lending lines have not been tapped. The authors stress that this finding is in line with their preferred view that IMF interventions act as a commitment device which does not rest on the actual disbursement of money. However, a more critical interpretation that points to the omission of a relevant variable influencing both the need for IMF resources and sovereign bond spreads is also possible (Cottarelli and Giannini, 2006).

[10] Özler (1993) represents an earlier study on this subject. Ignoring the problem of self-selection into Fund programs she finds that IMF interventions are associated with increased bond spreads.

Jensen (2004), Edwards (2006) and Bird and Rowlands (2009) follow a different approach by looking at capital flows instead of spreads. A quite robust result from this research agenda is that IMF programs not only fail to encourage capital inflows but even lead to capital flight. Since this may be the result of an increased perception of sovereign risk and capital flight on its own renders successful future debt rollover less likely one would expect higher default rates in the years following an IMF program. Interestingly Van der Veer and de Jong (2010) find that catalysis seems to work for countries that do not default in the years following an IMF intervention. In conjunction with the afore mentioned results this implies that later defaulters suffer from massive capital outflows while participating in an IMF program.

Our paper extends the literature by providing a direct investigation of the relationship between IMF programs and sovereign defaults. Looking at actual default incidences is warranted since - as the discussion on the IMF loan to Greece in 2010 has shown - staving off default is a major policy objective. Information on the success of past IMF programs is therefore valuable from a political point of view. While spreads on sovereign bonds are surely informative in this dimension they always represent a mixture of the perceived default probability and the repayment conditional on default. Since both variables are likely to change after the start of a program our narrower focus on defaults allows us to disentangle those effects. Furthermore, even interest rates on bonds with longer maturities are partly driven by short term considerations. Our approach is thus better suited to analyze the long run consequences of IMF interventions.

3. Empirical Framework and Data

3.1. Empirical Framework

The goal of this paper is to analyze the medium and long run effects of IMF programs on sovereign risk. The literature on the determinants of sovereign defaults therefore provides a natural starting point for our own investigation. We follow Kohlscheen (2010) and Celasun and Harms (2011) in using a pooled probit framework for our baseline estimations employing dummy variables to indicate IMF interventions. We do not opt for a fixed effects estimation procedure since a consistent fixed effects probit estimator does not exist and the alternative use of a logistic distributional assumption still suffers from the drawback of a dramatically reduced sample size. This reduction results from the required exclusion of all countries without a default incidence in the sample period since the fixed effect is a perfect predictor of no default for these countries (Kruger and Messmacher, 2004).[11] However we do present results for a random effects specification as a robustness exercise.

We decide on the utilization of cluster robust covariance matrices on the basis of a test for dynamic completeness offered by Wooldridge (2010). This test is based on an artificial probit regression which includes the lagged residuum of the original estimation equation as an additional regressor. The null hypothesis is that the coefficient on this new variable takes the value zero. A rejection of the null hypothesis is equivalent to a rejection of the assumption of dynamic completeness. Cluster robust standard errors are required in this case.

An obvious objection against the simple univariate approach points to the long acknowledged endogenous nature of IMF programs. Since IMF programs are partly designed to avert various forms of macroeconomic crises the finding of a positive association between IMF interventions and sovereign risk could reflect causality running from the latter to the former. Although this problem should be mitigated by the adoption of a large number of macroeconomic control variables and the use of lagged IMF intervention dummies these variables may still be correlated with the error term. Given the binary nature of our two variables of interest the preferred framework in this case is the recursive bivariate probit model.[12]

The bivariate probit model explicitly specifies the endogenous nature of the binary regressor of interest in a simultaneous equations context. In our case, let D and I denote the dummy variables indicating a sovereign default and the adoption of an IMF program, respectively. The model may then be written as

$$D^*_{it} = \boldsymbol{x'_{it}\beta} + \gamma\, I_{it} + \varepsilon_{it} \qquad\qquad D_{it} = 1 \text{ if } D^*_{it} > 0 \tag{1}$$

$$I^*_{it} = \boldsymbol{z'_{it}\delta} + \upsilon_{it} \qquad\qquad I_{it} = 1 \text{ if } I^*_{it} > 0 \tag{2}$$

[11]The inclusion of institutional control variables with almost no time variation raises an additional problem for every fixed effects estimator.

[12]See Marchesi (2003) for an application of this method to the interaction of IMF programs and debt reschedulings.

with the disturbances following a bivariate normal distribution

$$\begin{pmatrix} \varepsilon_{it} \\ \upsilon_{it} \end{pmatrix} = N \begin{pmatrix} 0 & , & 1 & \rho \\ 0 & , & \rho & 1 \end{pmatrix} \quad .$$

Equation (1) describes the ability and the willingness of a sovereign borrower to honor his debt as a function of his relationship with the IMF and other determinants summarized in the vector $\boldsymbol{x}'_{it}$. Considered in isolation this equation corresponds to the univariate probit model referred to in the first part of this paragraph with the coefficient γ as our main object of interest. The desire of the sovereign borrower to participate in an IMF program and the willingness of the IMF decision-making bodies to implement one is modeled in equation (2).

Parameter identification in this type of model does not require the variables in $\boldsymbol{x}'_{it}$ and $\boldsymbol{z}'_{it}$ to be different (Wilde, 2000). Specifications with instruments for IMF programs however are preferable because of their better finite sample properties in terms of statistical inference (Monfardini and Radice, 2008). We therefore use some additional political and institutional variables in the second equation that have been proposed in the literature on the determinants of IMF program participation. Finally, the endogenous nature of IMF interventions is reflected in the correlation between the two error terms ε and υ. We would expect the estimate of the correlation coefficient ρ to be positive if countries with a higher probability of default attract more IMF interventions even after controlling for the variables in $\boldsymbol{x}'_{it}$. We estimate all model parameters simultaneously by maximum likelihood.

3.2. Data

Our empirical analysis is based on an unbalanced panel of 57 developing and emerging economies with annual data from 1975 to 2008. Although country coverage is dictated by the availability of data on our main variables of interest the country list presented in Appendix A seems to be quite representative for the population of emerging market economies.[13] We further restrict our sample by excluding the time span between the first incidence and the resolution of a default since the duration of a debt restructuring

[13]Formerly central planned economies have been included with data from 1992 onwards.

process may well be influenced by economic considerations other than the decision to enter the default status in the first place (Van Rijckeghem and Weder, 2009).

We follow Van Rijckeghem and Weder (2009), Kohlscheen (2010) and Celasun and Harms (2011) in using actual incidences of sovereign defaults on foreign currency debt as reported by the rating agency Standard & Poor's as our dependent variable. Standard & Poor's defines a sovereign default '...as the failure to meet a principal or interest payment on the due date (or within the specified grace period) contained in the original terms of a debt issue' (Standard & Poor's, 2006). This approach identifies 60 credit events in our maximum sample. We decline the attempt to enrich the data by including periods with high spreads on sovereign bonds (Pescatori and Sy, 2007), private defaults (Detragiache and Spilimbergo, 2001) or large IMF drawings (Manasse and Roubini, 2009). While the decision on the last two alternatives is obvious with regard to our research focus the utilization of a market based indicator of sovereign distress is refused on the grounds of data limitations.[14]

Our main explanatory variable of interest – the existence of an IMF program – is measured in three different ways. Our broadest indicator is a dummy variable which takes the value 1 if an IMF program was agreed on in at least one of the five preceding years. Since highly subsidized lending through the Structural Adjustment Facility (SAF) and the Poverty Reduction and Growth Facility (PRGF) is targeted at low income countries with little access to private capital markets it qualifies more as development assistance than as intervention in terms of the theoretical arguments laid out above (Barro and Lee, 2005). We therefore alternatively employ a more selective measure of IMF programs which focuses on agreed Stand-by Arrangements (SBA) and on the Extended Fund Facility (EFF). Our third indicator finally marks new SBA and EFF programs with lending lines in excess of the participating country's quota. This explicit focus on large programs can be justified in light of some of the theories discussed in Subsection 2.1. On the one hand, large programs may be particularly successful in reducing sovereign risk as most theories on liquidity crises agree that the effectiveness of IMF interventions increases with their size. Additionally one can argue that exceptional access to IMF resources renders further support in the nearby future less likely thereby reducing debtor moral hazard and sovereign risk (Dreher and Vaubel, 2004). On the other hand, the accu-

[14] The EMBI Global which covers 41 countries is the broadest sovereign debt index available. Data coverage, however, is often limited to less then 10 years.

mulation of large liabilities to the IMF may as well increase the probability of a future solvency crisis implying a default on more junior private debt.

We measure the adoption of an IMF program over a five year window because we want to account for both the direct and arguably fast working effect of liquidity provision and the more time consuming effects that influence default probabilities through changes in incentives and policy conduct. In a robustness exercise, however, we will also investigate whether our conclusions change if a different time horizon is considered.

A first impression on the relation between our different indicators of IMF interventions and subsequent defaults can be gauged from Table 1. This table shows the frequency of sovereign debt crises conditional upon the existence of an IMF program in the previous five years. The striking result from this exercise is that default frequencies of countries with an IMF involvement in the recent past exceed those without such treatment by a factor larger than two. This difference gets bigger when shifting our attention exclusively on SBA and EFF programs and especially on those where the agreed lending amount is large as defined above. The χ^2 statistics reject the null hypothesis of independence between the frequency of sovereign debt crises and IMF programs in all cases implying that the differences are statistically significant. Obviously, correlation does not necessarily imply one-way causality. In the next section we will therefore investigate whether this positive association still holds after explicitly taking other determinants of sovereign debt crises and the endogeneity of IMF programs into account.

≪ insert Table 1 here ≫

Our choice of control variables in the single-equation framework and in the default equation (1) of the bivariate probit model has been guided by the literature on sovereign debt crises and is especially close to the specification of Kohlscheen (2010). The set of covariates consists of the GDP growth rate, the ratios of debt service to exports and reserves to imports, the external debt to GDP ratio, the five year US Treasury Constant Maturity (CMT) interest rate and a policy dummy variable indicating parliamentary democracies. We also add regional dummies, an indicator of compliance with IMF programs proposed by Dreher and Walter (2010) and interaction terms in some of the specifications. To mitigate endogeneity concerns all variables except for the arguably exogenous policy variable are lagged by one year.[15] The same economic variables augmented by the short

[15]The results remain unchanged when the policy variable is also lagged by one year.

term to total debt ratio (which turns out insignificant in the default equation) are also considered as potential determinants of IMF interventions in equation (2) of our bivariate probit specification. Following the empirical literature on IMF lending decisions we further include the fraction of votes cast together with the United States in the UN General Assembly as an additional variable. We use a higher lag order in this specification to ensure that IMF programs are explained solely by already realized values of the explanatory variables. Appendix B contains information on the construction and the data sources for the included variables. The summary statistics are presented in table Table 2.[16]

≪ insert Table 2 here ≫

GDP growth should influence the probability of a default through it's impact on sovereign borrowers' willingness to pay (Arellano, 2008). We expect to find a negative relationship since borrowing constraints often tighten in recessions and a reduction of the debt burden through net repayment is a less attractive choice in times of economic hardship. The debt service to exports and reserves to imports ratios are included as measures of a country's liquidity position. Liquidity features prominently in the literature on self-fulfilling debt runs which points to a positive association between liquidity needs and the incidence of rollover crises. A similar conclusion can also be reached in a willingness to pay framework (Detragiache and Spilimbergo, 2004). The external debt to GDP ratio is the most widely used solvency indicator in the political and academic debate on debt sustainability. We opt for a broad measure of external debt that includes both private and public liabilities. This choice is motivated by data availability and the observation that private obligations often turn public through government guarantees or direct assumptions during financial and economic crises. Since variations in the risk free interest rate directly affect the demand for more risky assets like emerging market government bonds we add the five year US CMT interest rate rate as an additional regressor. Finally, political economy

[16]We also experimented with other variables proposed in the literature on debt crises and IMF interventions. In the default equation we tried indicators of real exchange rate overvaluation and banking crises, the volatility of GDP growth, the deficit to GDP ratio, an indicator of the past repayment performance and the ratio of private to total external debt. Potential explanatory variables for the IMF equations incorporated each country's share of IMF quotas, the ratio of bilateral trade with the United States relative to GDP, the fraction of times countries voted in line with major Europe in the UN General Assembly and dummy variables indicating United Nations Security Council membership. However, none of these variables turned out to be statistically significant when added to our baseline specification.

considerations surely influence the debt service decision. One particular aspect pointed out by Kohlscheen (2010) is that even heads of government that are sympathetic to a suspension of payments to international creditors may resist the temptation to do so when the consent of a polarized legislature is required. We therefore expect the coefficient on the parliamentary democracy dummy to be negative.

Countries seek the help of the IMF in times of looming crisis. Our priors on the coefficient signs for the economic variables in the IMF equation therefore coincide with our expectations laid out in the context of the default equation. As another indicator of liquidity needs we also anticipate the short term to total debt ratio to enter with a positive sign. The additional UN voting variable can be seen as a indicator for the political proximity of a country's government to the United States (Barro and Lee, 2005). Since it is often assumed that the United States use their influence as the IMF's major shareholder to favor political allies with preferred access to Fund resources this variable is expected to enter with a positive coefficient.

4. Results

4.1. Do IMF Programs Influence Sovereign Risk?

The basic results of our single equation analysis are summarized in Table 3. We present marginal effects evaluated at the means of covariates in all columns to ease the economic interpretation and the comparison between the different specifications. Since the null hypothesis of dynamic completeness cannot be rejected for any of our model variants – the p-values for the null hypothesis take the values 0.50, 0.47, 0.47 and 0.57 respectively – standard inference procedures are valid. The t-statistics of the coefficient estimates given in parenthesis are therefore based on usual Huber/White standard errors.[17]

Column (I) presents the estimates for our baseline specification that does not account for the effects of IMF interventions. Our results closely resemble those of Kohlscheen (2010) despite the larger time span covered in our analysis. Sovereign defaults become more likely in recessions, when a high debt service to exports ratio indicates pressing liquidity needs and when the debt burden is large relative to the economic size of the

[17] We use coefficient estimates instead of marginal effects for hypothesis testing as suggested by Greene (2010).

debtor country. Higher US interest rates also increase the probability of a sovereign default which is less common in parliamentary democracies even after controlling for the other covariates. Finally a large foreign reserves to import ratio is associated with a lower probability of sovereign debt crisis. The sign of all coefficient estimates are in line with our theoretical predictions. Regarding the goodness of fit our pseudo R^2 takes a value of roughly 17.5 percent. This estimate is in the range obtained in previous research on the determinants of sovereign defaults.

≪ insert Table 3 here ≫

Turning to our main variables of interest the remaining columns show that sovereign debt crises are more likely to occur in the five years following an IMF program. Importantly, the sign of this effect does not depend on the concept used to identify IMF interventions although its statistical and economic significance increases monotonically from specification (II) to (IV). According to our results the adoption of any kind of IMF program increases the probability of a subsequent default by 1.4 percentage points in a sample with an overall default frequency of just 4.8 percent. SBA and EFF programs, especially large ones, induce an even bigger surge in default probabilities by 1.7 and 2.2 percentage points, respectively. These results support the notion that moral hazard and debt dilution effects are important byproducts of IMF programs. Since the largest effects are found for lending programs that exceed a country's quota one may argue that these problems grow more severe with program size. Another explanation, however, points to an increased risk of simultaneity bias as large programs are targeted to countries with deeper structural problems. We therefore do not consider them separately in the remaining study and focus on all SBA and EFF interventions instead which also excludes programs that qualify more as development assistance than as crisis prevention lending.[18]

We next investigate whether our finding of a positive relationship between IMF interventions and default probabilities merely reflects the self selection of especially vulnerable debtors into IMF programs by means of the bivariate probit model. The parameter estimates of this specification are given in the third and fourth column of Table 4. Notice that these results are not directly comparable to the marginal effects reported earlier in this section. To allow for an assessment of the importance of endogeneity we therefore

[18]Results for the alternative IMF specifications are available upon request.

also present results for a specification that restricts the correlation of the two disturbances in the equations (1) and (2) to zero (see columns (I) and (II)). This version of the model is equivalent to an estimation of the two equations by the univariate probit methods employed earlier in this paper.

≪ insert Table 4 here ≫

Considering first the determinants of program participation the estimation results of both the univariate and bivariate specifications support the assumption that IMF resources are targeted to and demanded by countries with weak fundamentals.[19] Liquidity needs in particular seem to be an important aspect that characterizes program countries as the reserves to imports, the debt service to export and the short term to total debt ratio all enter significantly and with the correct sign. This is also true for the GDP growth rate and the UN voting variable. Here, a positive coefficient indicates a preferred access to Fund resources for countries with closer political ties to the United States. Since the UN voting variable turns out insignificant (p-value of 0.67) when included in the default equation we regard it as a valid instrument.

Most importantly from the perspective of this study a comparison of the univariate and bivariate regression results shows that our main conclusions are not altered when we account for the simultaneity of the IMF variable. The coefficient on our program indicator even increases from 0.347 to 0.748 from the first to the latter specification although its statistical significance diminishes somewhat. This result does not necessarily imply a stronger marginal effect of IMF programs which is given by the formula

$$\text{Prob}\left[D_{it}=1|I_{it}=1,\boldsymbol{x_{it}},\boldsymbol{z_{it}}\right]-\text{Prob}\left[D_{it}=1|I_{it}=0,\boldsymbol{x_{it}},\boldsymbol{z_{it}}\right].^{20} \qquad (3)$$

Evaluating equation (3) at the means of covariates and taking the negative estimate of ρ into account we obtain the result that IMF interventions increase the probability of

[19]Since the estimation results for the univariate default specification are discussed at length earlier in this section we do not regard them separately here. Differences in significance levels are due to a reduction in sample size brought about by the higher lag order employed in the IMF program equation.

[20]The effect can be computed as $\frac{\Phi_2\left(\boldsymbol{x}_{it}'\hat{\boldsymbol{\beta}}+\hat{\gamma}\,I_{it},\,\boldsymbol{z}_{it}'\hat{\boldsymbol{\delta}},\,\hat{\rho}\right)}{\Phi\left(\boldsymbol{z}_{it}'\hat{\boldsymbol{\delta}}\right)}-\frac{\Phi_2\left(\boldsymbol{x}_{it}'\hat{\boldsymbol{\beta}},\,-\boldsymbol{z}_{it}'\hat{\boldsymbol{\delta}},\,-\hat{\rho}\right)}{1-\Phi\left(\boldsymbol{z}_{it}'\hat{\boldsymbol{\delta}}\right)}$ with Φ_2 and Φ denoting the cumulative density function of the bivariate and standard normal distribution (Greene, 2008).

subsequent sovereign defaults by 1.44 percentage points. The magnitude of the effect is thus comparable to those reported for the univariate specifications.

Interestingly, the estimated correlation coefficient $\hat{\rho} = -0.276$ is not significantly different from zero according to the usual Wald test. The hypothesis of no correlation between the two error terms is also not rejected by the likelihood ratio or the lagrange multiplier test. Here the test statistics λ_{LR} and LM take the values 1.274 and 0.182 respectively, which are well below the critical values for any reasonable level of significance. Hence, the positive association between the presence of IMF programs and subsequent sovereign defaults documented throughout this study does not reflect a correlation in omitted factors which influence both program adoption and default decision. Instead, our evidence strongly supports the idea that IMF interventions increase the likelihood of future debt crises as predicted by the moral hazard, debt dilution, and default triggering theories of IMF lending.

4.2. Explanations and Robustness Exercises

Our finding of a positive causal effect of past IMF programs on sovereign risk indicates that, on balance, the negative aspects of program participation summarized in Subsection 2.1 outweigh their positive counterparts. Although an empirical separation of the effect in terms of the different theoretical explanations is challenging we now try to shed some light on the economic forces at work. Since our previous results on the bivariate probit model imply that IMF programs are not correlated with unobserved determinants of sovereign defaults we resort to the more efficient univariate estimation procedures in this paragraph.

The results presented in Table 5 show that our conclusions do not rest on the specific time horizon employed in the measurement of IMF programs. Countries are more prone to sovereign debt crises if an IMF intervention took place in the previous one, two, three or four years. These results are at odds with the idea that IMF lending prevents looming liquidity crises from unfolding in the short run while induced moral hazard becomes the dominant effect over a longer time horizon. Instead, the short run increase in default probabilities may even be seen as supportive of the idea that insufficient IMF lending programs trigger sovereign debt crises (Zettelmeyer, 2000). Finally it can be gauged from column (I) that IMF programs are particulary often accompanied by sovereign defaults in the year of their implementation. Since the case for reverse causality is obvious in this specification we do not allow for contemporaneous effects in the remainder of our paper.

≪ insert Table 5 here ≫

One objection against our results is that the IMF coefficient in our regressions just picks up a geographical clustering in both IMF programs and sovereign debt crises. We test this hypothesis in two ways. First, we add regional dummies to our baseline specification. As the estimates presented in column (I) of Table 6 show, all of the dummies turn out statistically insignificant while the sign and magnitude of our coefficient do not change. We are therefore confident that our finding of a positive effect of IMF programs on sovereign default probabilities does not merely reflect unobserved heterogeneity at the regional level. Second, we allow for random country specific effects in our default equation. The results for this specification are displayed in column (II) of the same table. Although the economic and statistical significance of the effect diminishes slightly, our main conclusions on the effect of IMF programs on sovereign risk remain unaltered.

≪ insert Table 6 here ≫

Another possible explanation for our findings points to the problem of non-compliance with IMF policy conditions. If IMF programs worked mainly through the influence conditionality exerts on policy choices, we would expect a reduction in sovereign risk to be limited to those countries that meet the conditions laid out in the lending agreement. Furthermore, governments that fall short of their promises soon after the disbursement of the first tranches of IMF credit are likely to be those with a large inclination to default on their other obligations too. A failure to differentiate between different compliance rates may therefore bias the coefficient on the IMF program variable substantially upwards. We address this problem by augmenting our estimation equation with a dummy variable that takes the value 1 if a country was compliant with its IMF program in the five preceding years.[21] Column (III) of Table 6 shows the results. While theoretically convincing, the distinction between compliers and non-compliers seems to be less important empirically. The presence of an IMF program per se increases the probability of

[21] Concretely, a country is coded as non-compliant when more than 25% of the credit amount agreed under an IMF program remains undrawn at program expiration (Dreher and Walter, 2010). The availability of the compliance variable limits our sample size in this specification.

a sovereign debt crisis for both country groups alike. Overall the evidence presented so far thus weakens the case for the importance of IMF conditionality while it is mostly in line with the moral hazard, debt dilution, and default triggering views of IMF lending.

The last part of this section puts the implications of the catalytic finance hypothesis under greater scrutiny. As emphasized above this theory implies that liquidity provisions by the IMF are most likely to induce catalytic effects when the economic fundamentals of the debtor country are neither too good nor too bad. Following Mody and Saravia (2006) we integrate this hypothesis in our empirical framework by interacting the IMF program indicator with dummy variables for a country's relative position in the empirical distribution of other factors influencing sovereign risk. More concretely, we construct three dummy variables for both the external debt to GDP and the debt service to exports ratio by using the first and second tertile of the respective distribution as threshold values. Mirroring the time window employed in the measurement of IMF interventions every country-year observation is grouped according to its mean value during the five preceding years. Marginal effects for IMF programs conditional on a specific macroeconomic environment are then computed as the difference in predicted default probabilities induced by a change in the respective program dummy variable from zero to one. We hold all other program dummies constant at zero for this calculation while the remaining covariates are evaluated at their means.

≪ insert Table 7 here ≫

Columns (I) and (II) of Table 7 display the results of this exercise. The evidence is broadly supportive to the catalytic finance theory except for one important qualification. Although the marginal effects of the IMF variables are always the lowest for the intermediate range of economic fundamentals, they never reach a significant negative sign. Instead of setting the stage for a default risk reducing effect of IMF programs a proper economic environment merely seems to limit the damage they otherwise cause. Furthermore, statistically significant positive estimates indicate that IMF programs are especially detrimental to fiscal solvency when the Fund distributes its resources to countries whose economic fundamentals are already weak. Our evidence is therefore consistent with the hypothesis that debtor moral hazard is most likely to occur in these circumstances.

5. Conclusion

Although the IMF features prominently both in the history of sovereign debt crises and in recent policy proposals aimed at the prevention of such crises, surprisingly little is known on the effects of IMF program participation on the likelihood of subsequent defaults. In light of the inconclusive findings in the theoretical literature this paper attempts to investigate this issue empirically. Using univariate and bivariate probit specifications our results indicate that the adoption of an IMF program increases sovereign risk over the medium term. More concretely, we estimate that the probability of a sovereign default increases by approximately 1.5 – 2 percentage points in the aftermath of IMF programs. These findings cannot be contributed to a lack of compliance with conditionality but seem to reflect the effects of IMF interventions per se. Financial markets' cautious reaction to Greece's rescue package described in the introduction of this paper thus seems legitimate in the light of these results.

Further analyses additionally show that IMF programs are especially detrimental to fiscal solvency when Fund resources are targeted to countries with already weak fundamentals. Overall, our evidence is therefore consistent with the idea that debtor moral hazard is most likely to occur in these situations as predicted by the theoretical work on the catalytic finance hypothesis. Other explanations that point to the effects of debt dilution and the possibility of IMF triggered debt runs, however, are also possible. The separation of the effects in terms of these different explanations surely constitutes an interesting area for future research.

Regarding the policy implications of our findings, one important qualification has to be kept in mind before concluding that debt crises would become less likely in a world without IMF interventions. Since the pure existence of the IMF as a potential international lender of last resort may deter short-run creditors from running it is possible that the Fund has prevented several debt crises without being active. This possibility, however, should not preclude the IMF from a thorough analysis of the question whether too many resources have been devoted to countries which view IMF lending as a substitute for, rather than a complement to policy reform.

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Table 1: Sovereign Debt Crises: Frequency Conditional on IMF Programs

	Frequency (%)		
	Yes	No	$\chi^{2\,a}$
New IMF program in previous 5 years?	5.02	2.40	10.64***
New Standby or Extended Fund Facility Arrangements in previous 5 years?	5.79	2.26	18.05***
New large Standby or Extended Fund Facility Arrangements in previous 5 years?	10.50	2.71	32.64***

[a] The null hypothesis of independence between the frequency of sovereign debt crises and IMF programs is distributed as $\chi^2(1)$. ***, ** and * denote significance levels of 1%, 5% , and 10%.

Table 2: Summary Statistics

	Mean	Std. Dev.	Max	Min	Obs.
Default	0.05	0.21	1.00	0.00	1256
IMF program in previous 5 years	0.57	0.49	1.00	0.00	1256
SBA/EFF program in previous 5 years	0.47	0.50	1.00	0.00	1256
SBA/EFF 100 program in previous 5 years	0.13	0.33	1.00	0.00	1252
Compliant with IMF in previous 5 years	0.70	0.46	1.00	0.00	988
GDP growth$_{t-1}$	0.04	0.05	0.27	-0.23	1256
Debt service/exports$_{t-1}$	0.21	0.15	1.29	0.00	1256
Reserves/imports$_{t-1}$	0.37	0.34	2.79	0.00	1256
External debt/GDP$_{t-1}$	0.49	0.31	3.36	0.03	1256
5-year US CMT rate$_{t-1}$	0.07	0.03	0.14	0.03	1256
Parliamentary Democracy	0.22	0.42	1.00	0.00	1256
log(UN voting)$_{t-6}$	-1.62	0.52	-0.58	-4.68	1233
Short term/total debt$_{t-6}$	0.13	0.10	0.65	0.00	1223

All statistics refer to the baseline estimation sample, Table 3, Column (II).

Table 3: IMF Programs and Sovereign Debt Crises: Baseline Estimations
(marginal effects, evaluated at means of covariates)

	(I)	(II)	(III)	(IV)
Dependent variable: Default				
IMF program in previous 5 years		0.014		
		(1.65*)		
SBA/EFF program in previous 5 years			0.017	
			(2.13**)	
SBA/EFF 100 program in previous 5 years				0.022
				(2.33**)
GDP growth$_{t-1}$	-0.223	-0.207	-0.199	-0.222
	(2.91***)	(2.71***)	(2.68***)	(2.94***)
Debt service/exports$_{t-1}$	0.109	0.102	0.093	0.091
	(4.94***)	(4.67***)	(4.25***)	(4.06***)
Reserves/imports$_{t-1}$	-0.044	-0.038	-0.036	-0.043
	(1.65*)	(1.46)	(1.42)	(1.62)
External debt/GDP$_{t-1}$	0.018	0.015	0.018	0.019
	(1.84*)	(1.60)	(1.93*)	(2.00**)
5-year US CMT rate$_{t-1}$	0.452	0.483	0.459	0.396
	(3.08***)	(3.34***)	(3.24***)	(2.75***)
Parliamentary Democracy	-0.029	-0.028	-0.028	-0.030
	(2.52**)	(2.45**)	(2.53**)	(2.58***)
Observations	1,256	1,256	1,256	1,252
Defaults	60	60	60	60
Mc Fadden R^2	0.174	0.179	0.184	0.186
Log likelihood	-199.15	-197.87	-196.80	-196.09

The absolute values of robust (Huber/White) z-statistics are shown in parenthesis. ***, ** and * denote significance levels of 1%, 5% , and 10%.

Table 4: IMF Programs and Sovereign Debt Crises: Bivariate Probit Estimation

	Probit		Bivariate Probit	
Variable	Coef.	Z-statistic	Coef.	Z-statistic
Dependent variable: Default				
Constant	−2.857	(7.74***)	−2.967	(8.59***)
GDP growth$_{t-1}$	-2.180	(1.29)	-2.022	(1.06)
Debt service/exports$_{t-1}$	1.730	(3.79***)	1.540	(2.59***)
Reserves/imports$_{t-1}$	-0.570	(1.46)	-0.541	(1.68*)
External debt/GDP$_{t-1}$	0.350	(1.69*)	0.344	(1.39)
5-year US CMT rate$_{t-1}$	9.319	(3.38***)	9.001	(3.26***)
Parliamentary Democracy	-0.884	(2.81***)	-0.872	(2.41**)
SBA/EFF program in previous 5 years	0.347	(2.11**)	0.748	(1.79*)
Dependent variable: SBA/EFF program[a]				
Constant	0.771	(3.33***)	0.744	(3.11***)
GDP growth$_{t-6}$	-5.716	(5.90***)	-5.689	(5.78***)
Reserves/imports$_{t-6}$	-1.031	(5.16***)	-1.056	(5.33***)
log(UN voting)$_{t-6}$	0.807	(6.49***)	0.792	(6.11***)
Short term/total debt$_{t-6}$	1.651	(3.81***)	1.744	(4.05***)
Debt service/exports$_{t-6}$	3.090	(7.41***)	3.103	(7.43***)
ρ			-0.276	(1.06)
Observations	1,024		1,024	
Defaults	49		49	
IMF programs	481		481	
$\lambda_{LR}{}^{b}$			1.274	
LM^{b}			0.182	

[a] Additional decade dummies used. [b] The likelihood ratio and lagrange multiplier test statistics are distributed $\chi^2(1)$. ***, **, and * denote significance levels of 1%, 5% , and 10%.

Table 5: IMF Programs and Sovereign Debt Crises: Alternative Time Horizons (marginal effects, evaluated at means of covariates)

	(I)	(II)	(III)	(IV)	(V)
Dependent variable: Default					
SBA/EFF program	0.029				
in same year	(3.45***)				
SBA/EFF program		0.021			
in previous year		(2.33**)			
SBA/EFF program			0.021		
in previous 2 years			(2.81***)		
SBA/EFF program				0.016	
in previous 3 years				(2.06**)	
SBA/EFF program					0.010
in previous 4 years					(1.28)
GDP growth$_{t-1}$	-0.181	-0.194	-0.181	-0.195	-0.206
	(2.37**)	(2.53**)	(2.44**)	(2.60***)	(2.68***)
Debt service/exports$_{t-1}$	0.090	0.100	0.093	0.096	0.101
	(4.28***)	(4.78***)	(4.44***)	(4.42***)	(4.45***)
Reserves/imports$_{t-1}$	-0.031	-0.039	-0.038	-0.038	-0.040
	(1.26)	(1.54)	(1.54)	(1.51)	(1.53)
External debt/GDP$_{t-1}$	0.018	0.017	0.016	0.017	0.018
	(2.03**)	(1.79*)	(1.79*)	(1.85*)	(1.86*)
5-year US CMT rate$_{t-1}$	0.429	0.430	0.428	0.444	0.454
	(2.97***)	(3.00***)	(3.07***)	(3.09***)	(3.14***)
Parliamentary Democracy	-0.029	-0.028	-0.028	-0.029	-0.029
	(2.66***)	(2.54**)	(2.63***)	(2.59***)	(2.55**)
Observations	1,256	1,256	1,256	1,256	1,256
Defaults	60	60	60	60	60
Mc Fadden R^2	0.197	0.186	0.191	0.183	0.177
Log likelihood	-193.53	-196.25	-195.09	-196.96	-198.26

The absolute values of robust (Huber/White) z-statistics are shown in parenthesis. ***, ** and * denote significance levels of 1%, 5% , and 10%.

Table 6: IMF Programs and Sovereign Debt Crises: Robustness
(marginal effects, evaluated at means of covariates)

	(I)	(II)	(III)
Dependent variable: Default			
SBA/EFF program in previous 5 years	0.015	0.012	0.020
	(1.99**)	(1.79*)	(1.74*)
Compliant with IMF in previous 5 years			0.008
			(0.66)
GDP growth_{t-1}	-0.156	-0.152	-0.248
	(2.28**)	(2.24**)	(2.34**)
Debt $\text{service/exports}_{t-1}$	0.077	0.078	0.133
	(3.88***)	(2.98***)	(4.15***)
$\text{Reserves/imports}_{t-1}$	-0.038	-0.034	-0.044
	(1.63)	(2.09**)	(1.29)
External debt/GDP_{t-1}	0.021	0.015	0.025
	(2.40**)	(1.49)	(1.89*)
5-year US CMT rate_{t-1}	0.432	0.356	0.590
	(3.47***)	(2.53**)	(2.97***)
Parliamentary Democracy	-0.021	-0.022	-0.036
	(1.70*)	(1.93*)	(2.33**)
Latin America & Caribbean	0.016		
	(1.45)		
Middle East & North Africa	-0.025		
	(1.62)		
East Asia & Pacific	-0.001		
	(0.10)		
Sub Saharan Africa	0.010		
	(0.79)		
Observations	1,256	1,256	988
Defaults	60	60	58
Method	pooled	re	pooled
Mc Fadden R^2	0.206	–	0.162
Log likelihood	-191.43	-193.02	-194.95

The absolute values of robust (Huber/White) z-statistics are shown in parenthesis. ***, **, and * denote significance levels of 1%, 5% , and 10%.

Table 7: IMF Programs and Sovereign Debt Crises: Interaction Effects
(marginal effects, evaluated at means of covariates)

	(I)	(II)
Dependent variable: Default		
SBA/EFF programm in previous 5 years,		
low range of External debt / GDP (I)	0.010	0.025
or Debt service / exports (II)	(1.35)	(1.37)
medium range of External debt / GDP (I)	-0.002	0.001
or Debt service / exports (II)	(0.51)	(0.10)
high range of External debt / GDP (I)	0.025	0.028
or Debt service / exports (II)	(2.63***)	(2.46**)
GDP growth$_{t-1}$	-0.210	-0.192
	(2.84***)	(2.33**)
Debt service/exports$_{t-1}$	0.096	0.080
	(4.48***)	(3.23***)
Reserves/imports$_{t-1}$	-0.037	-0.036
	(1.51)	(1.43)
External debt/GDP$_{t-1}$	0.010	0.016
	(1.05)	(1.69*)
5-year US CMT rate$_{t-1}$	0.444	0.477
	(3.13***)	(3.43***)
Parliamentary Democracy	-0.028	-0.033
	(2.54**)	(2.75***)
Observations	1,250	1,170
Defaults	60	58
Mc Fadden R^2	0.190	0.194
Log likelihood	-192.92	-186.04

The absolute values of robust (Huber/White) z-statistics are shown in parenthesis. ***, **, and * denote significance levels of 1%, 5% , and 10%.

Appendix A. Country Coverage

Argentina	El Salvador	Madagascar	Romania
Benin	Fiji	Malaysia	Russian Federation
Bolivia	Georgia	Mali	Senegal
Botswana	Ghana	Mexico	South Africa
Brazil	Guatemala	Mongolia	Sri Lanka
Bulgaria	India	Morocco	Thailand
Burkina Faso	Indonesia	Mozambique	Tunisia
Cameroon	Jamaica	Nigeria	Turkey
Chile	Jordan	Pakistan	Ukraine
China	Kazakhstan	Panama	Uruguay
Colombia	Kenya	Papua New Guinea	Venezuela
Costa Rica	Latvia	Paraguay	Vietnam
Dominican Republic	Lebanon	Peru	
Ecuador	Lithuania	Philippines	
Egypt, Arab Rep.	Macedonia, FYR	Poland	

Appendix B. Data Sources and Definitions

Name	**Source**	**Definition**
Dependent variable		
Default	Standard & Poor's (2006, 2009)	Dummy variable coded as 1 in the first year of a sovereign default.
IMF variables		
IMF program	Dreher (2006) - Extended time covering from web site	IMF program agreed, dummy variable.
SBA/EFF program	Dreher (2006) - Extended time covering from web site	IMF Standby Arrangement or Extended Fund Facility Arrangement agreed, dummy variable.
SBA/EFF 100 program	IMF (2009)	Change in total agreed SBA and EFF loans exceeding 100 percent of quota, dummy variable.
Compliant with IMF	Dreher and Walter (2010)	Dummy variable that takes the value 1 if a country was compliant with its IMF program. Non-compliance is identified as periods where at least 25% of the agreed credit amount remained undrawn at program expiration.

continued on next page

Appendix B. - continued

Name	Source	Definition
Control variables		
Debt service/ exports	World Bank (2010)	Ratio of debt service on external debt to exports of goods and services
Reserves/ imports	World Bank (2010)	Ratio of total reserves minus gold to imports of goods and services
Democratic	Polity IV (2009)	Dummy indicating democratic regimes, identified as country-year observations with non-negativ POLITY score as in Kohlscheen (2010).
Parliamentary	Keefer (2009)	Dummy signalizing a parliamentary form of government as indicated by a value of 2 for the system variable as in Kohlscheen (2010).
Parliamentary Democracy	Polity IV (2009) and Keefer (2009)	Dummy variable coded as 1 for parliamentary democracies. The construction of this variable relies on the definition of the Democratic and Parliamentary dummy variables given above.
GDP growth	World Bank (2010)	Real GDP growth rate
5 year US CMT rate	FRED (2010)	Yield to maturity of US Treasury notes with a constant maturity of 5 years
External debt/GDP	World Bank (2010)	Ratio of external debt stocks to GDP
Short term/ total debt	World Bank (2010)	Ratio of short term to total external debt
UN voting	Dreher and Sturm (2011)	Fraction of votes a country cast together with the United States in the UN General Assembly

IV. THE HETEROGENEITY OF DEFAULT COSTS: EVIDENCE FROM RECENT SOVEREIGN DEBT CRISES

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

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Abstract
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.

JEL Classification: F34, H63, C21, C23

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

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, 2012), 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 (2012). 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

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

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

$$
\begin{aligned}
Y_{1,t} &= \sum_{j=2}^{J+1} w_j^* Y_{j,t} \quad \text{for } t = 0, \ldots, T_0 - 1 \quad \text{and} \\
X_1 &= \sum_{j=2}^{J+1} w_j^* X_j \ .
\end{aligned}
$$

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,

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

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

[7] Using internal instruments in a dynamic panel framework as recently done by Furceri and Zdzienicka (2012) might be one promising way to address this issue.

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 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

[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.

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

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

   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:

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

   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 (2012)). 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.

≪ insert Table 3 here ≫

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.

≪ insert Figure 4 here ≫

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

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 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]

≪ insert Figure 5 here ≫

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

[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.

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. 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.

≪ insert Table 4 here ≫

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.

≪ insert Figure 6 here ≫

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

[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.

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.

≪ insert Figure 7 here ≫

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 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.

≪ insert Table 5 here ≫

[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.

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).

≪ insert Figure 8 here ≫

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 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.

≪ insert Figure 9 here ≫

[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.

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.

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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.

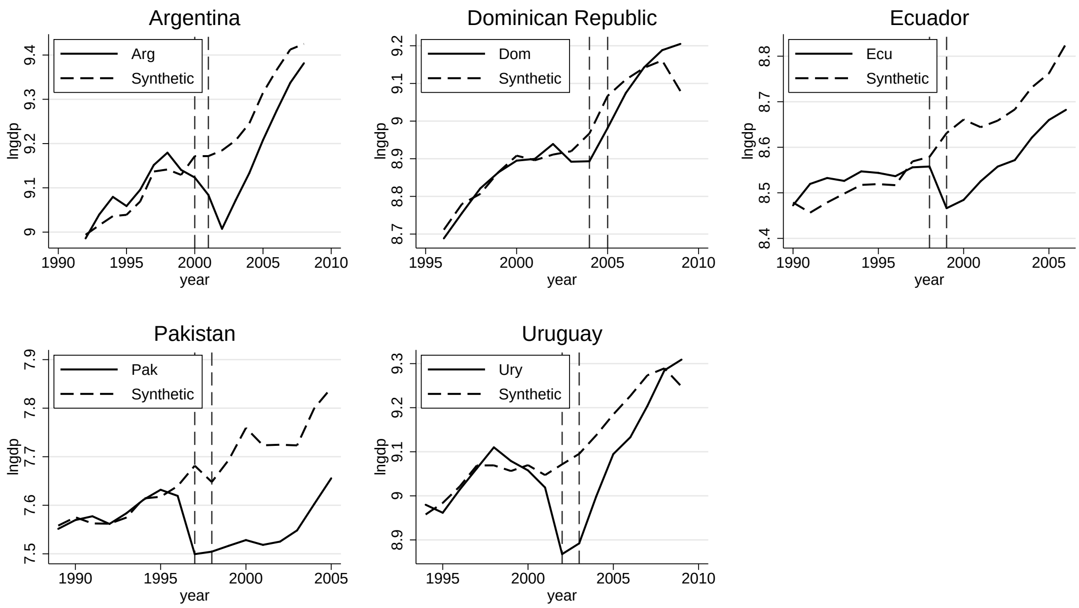

Figure 1: Evolution of GDP per capita: defaulting vs. synthetic economies

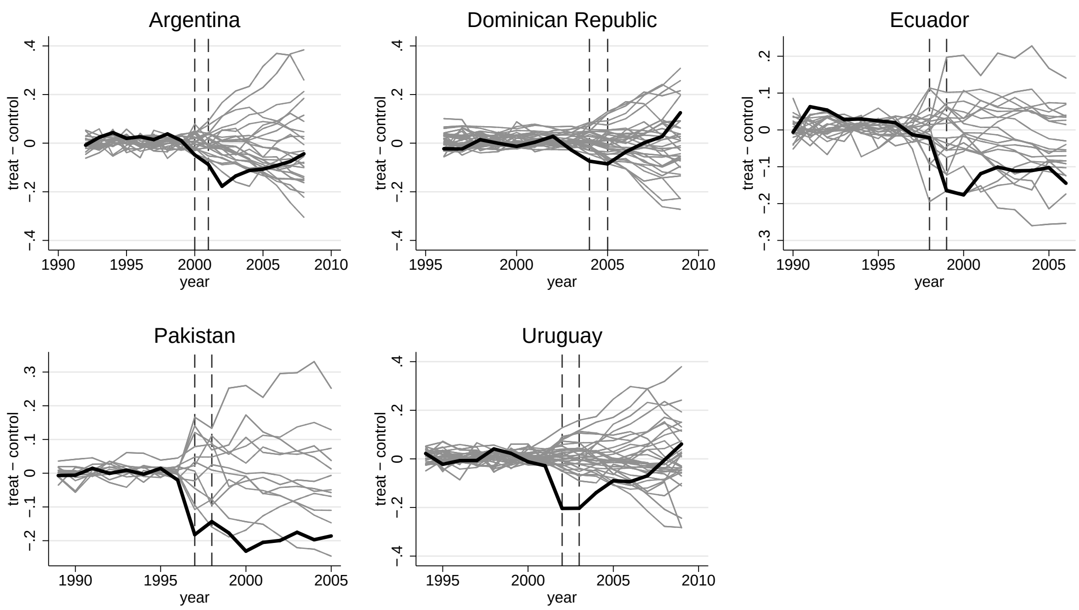

Figure 2: Country-specific costs of sovereign defaults in terms of GDP per capita

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

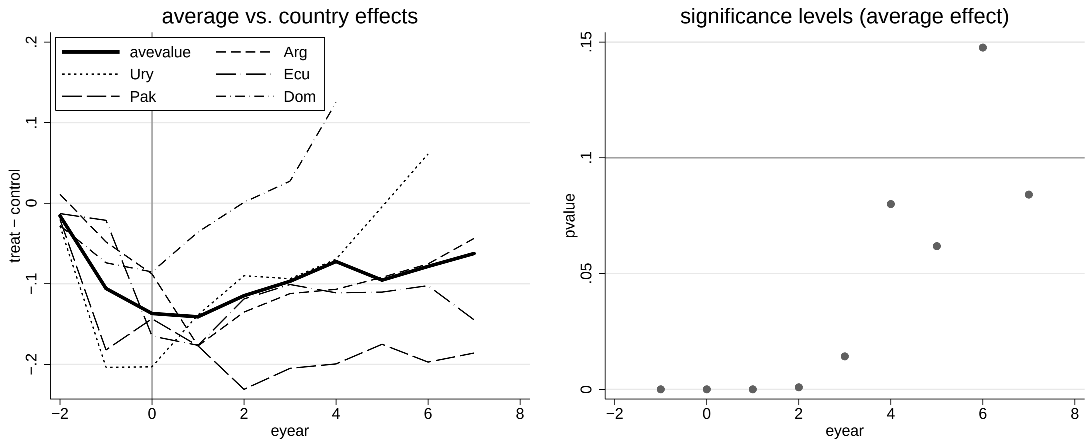

Figure 3: Average costs of sovereign defaults in terms of GDP per capita

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

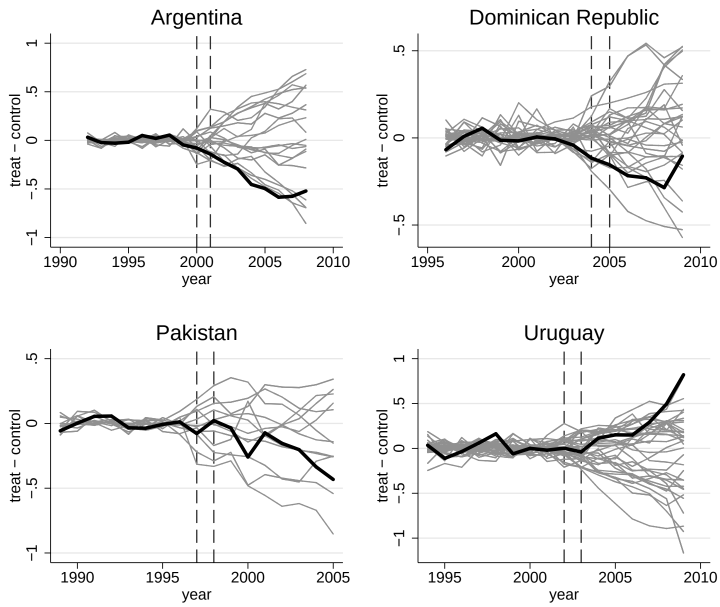

Figure 4: Country-specific costs of sovereign defaults in terms of total exports per capita

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

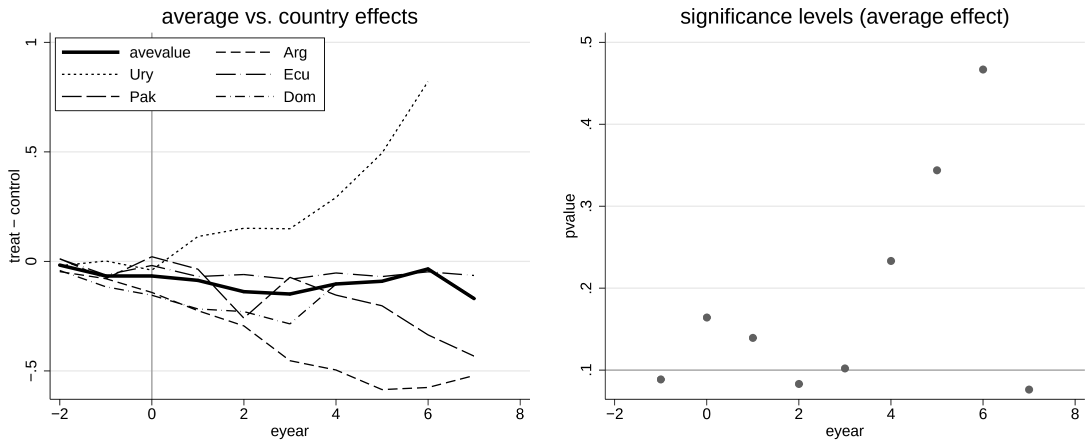

Figure 5: Average costs of sovereign defaults in terms of total exports per capita

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

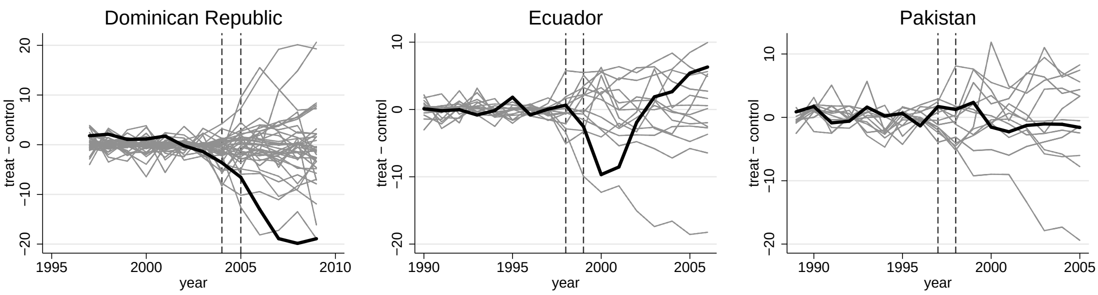

Figure 6: Country-specific costs of sovereign defaults in terms of relative exports

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

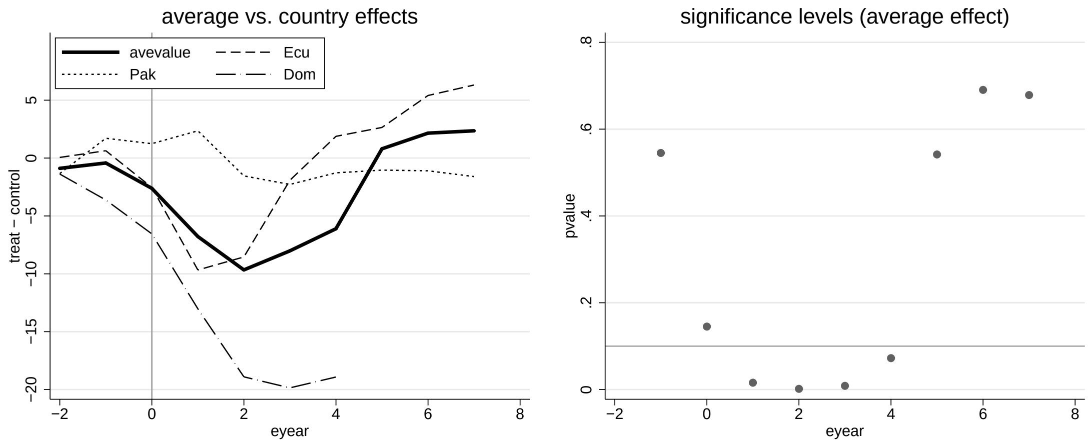

Figure 7: Average costs of sovereign defaults in terms of relative exports

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

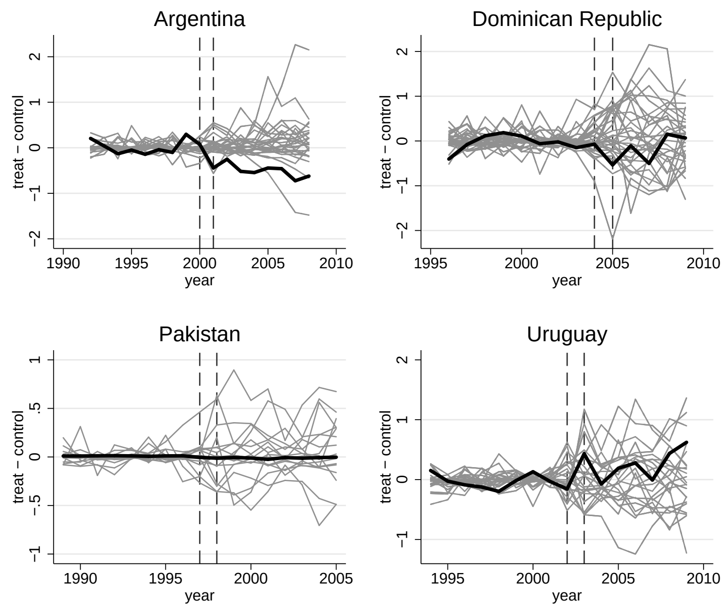

Figure 8: Country-specific costs of sovereign defaults: FDI inflows per capita

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

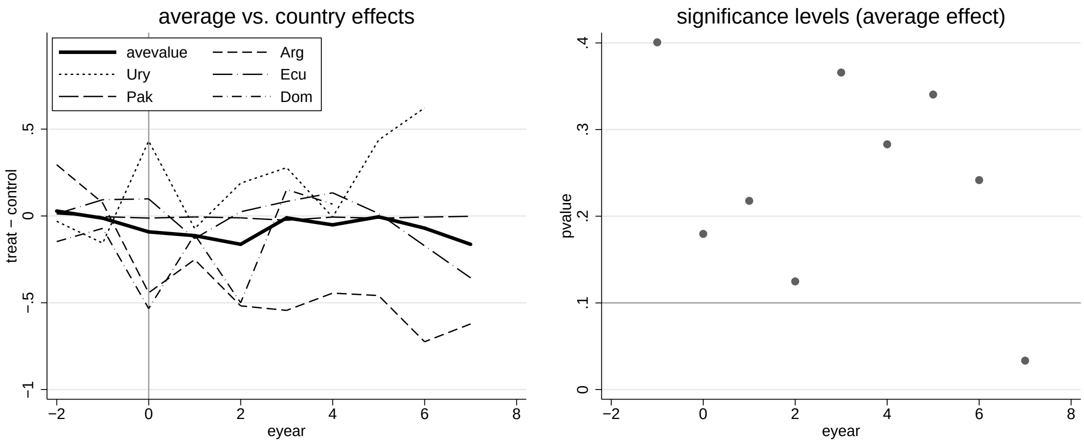

Figure 9: Average costs of sovereign defaults: FDI inflows per capita

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)

V. AID WITHDRAWAL AS PUNISHMENT FOR DEFAULTING SOVEREIGNS? AN EMPIRICAL ANALYSIS

This paper is available as

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https://www.uni-marburg.de/fb02/makro/forschung/magkspapers/20-2012_brandt.pdf.

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MAGKS course on the "Econometric Analysis of Microdata", University of Giessen, Giessen, January 12, 2012.

MAGKS Seminar, Rauischholzhausen, March 29–30, 2012.

Aid Withdrawal as Punishment for Defaulting Sovereigns? An Empirical Analysis

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Abstract
This paper empirically investigates whether donor countries punish sovereign defaults by reducing foreign aid flows. Our findings reject the hypothesis formulated in the theoretical literature that a default leads to a loss of foreign aid for the defaulting country. Creditor countries directly affected by the default do not reduce their aid disbursements. Hence, foreign aid is not used as a punishment instrument. Neither can it therefore serve as an enforcement mechanism for international debt contracts. Furthermore, other donors even raise the amount of development assistance allocated to the delinquent country by about 15% on average. Overall the amount of foreign aid given to the defaulting country increases by 6.4%.

JEL Classification: F34, F35, C23, C24

Keywords: Sovereign defaults, Default costs, Foreign aid, Sanctions

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1. Introduction

In contrast to private debt markets cross-border lending to sovereign entities is not enforced by any international bankruptcy law. If a private firm does not pay its liabilities, creditors have a legal claim to the firm's assets. In case of sovereign debt, creditors have no such tool to recoup the defaulted amount.[1] Nevertheless, we observe high quantities of sovereign debt in many countries which are generally repaid. This raises the question why this is the case. The answer is that there must be some mechanisms that make a default costly for the debtor country and thus deter sovereign defaults. In the following we investigate the existence of one specific type of default costs: a reduction in aid flows.

The literature on sovereign debt differentiates between two categories of default costs. First, a default causes a loss of reputation which in turn leads to rising borrowing costs or even to capital market exclusion. The second category covers direct sanctions. These may trigger, e.g., reductions in international trade after a default. The decline in aid flows as an additional sanctioning mechanism has been recently discussed by Asiedu and Villamil (2002). They argue that a defaulting country does not only suffer from a reduction in FDI inflows, but also from a loss of foreign aid. This raises the costs of a default and therefore makes it a less attractive option. Hence, foreign aid would reduce country risk and promote capital inflows to the debtor country. Following the theoretical argumentation of Asiedu and Villamil (2002), Asiedu et al. (2009) empirically investigate how foreign aid changes the sensitivity of FDI to country risk.[2] They show that foreign aid can in fact mitigate the adverse effect of sovereign risk on FDI. However, up to now, no study examines whether foreign aid is really used as a punishment instrument against defaulting countries. Is a default actually followed by a decline in foreign aid given to the defaulting country? The goal of this paper is to answer this question by relating aid flows to default events.

The existence of the transmission channel proposed by Asiedu and Villamil (2002) and Asiedu et al. (2009) is based on the assumption that foreign aid is granted because of strategic motives and is used for punishment in case of a default. To be specific, the

[1] It should be noted that the term default covers any change in the original debt contract leading to a loss of value for the creditor, e.g. debt rescheduling.

[2] In contrast to Asiedu and Villamil (2002), who assume that countries lose FDI and aid in case of a default, Asiedu et al. (2009) argue that a country loses both when expropriation occurs. Apart from this semantic difference, their model is identical. Furthermore, the empirical analysis does not distinguish between expropriation and default risk as it rests on a composite risk indicator that covers both concepts.

government of the donor country directs foreign aid to the debtor country to enforce debt repayment and to ensure FDI made by domestic firms. The idea that foreign aid is not solely given because of altruistic motives but is also determined by strategic and political considerations is not new. In their seminal work Alesina and Dollar (2000) highlight the importance of colonial past and political alliances as explanatory variables for foreign aid. They find that strategically important countries and former colonies receive much more foreign aid than comparable countries without one of these attributes; e.g. the U.S. gives the biggest part of its total foreign aid to Egypt and Israel and France directs most of its aid to former colonies. Using foreign aid to generate incentives for countries to pay their debt would be a further strategic motive.

If aid is used to punish a defaulting country we would expect to find a significant decrease in aid flows coming from creditor countries that are affected by the default. From the theoretical point of view, the reactions of other donor countries are not clear. To capture this heterogeneity we use data on debt rescheduled at the Paris Club and on bilateral aid flows from the OECD Development Co-operation Directorate. The information offered by the Paris Club show which countries restructured their debt and which creditor countries were affected in each case. Using bilateral data on aid flows and default events allows us to identify a differentiated default effect on affected and non-affected creditors.

Our findings indicate that foreign aid flows are not reduced after a default. This result holds not only for the aggregate amount of foreign aid received by the delinquent country but also for the amount granted by aggrieved creditor countries. On the contrary, our estimation results indicate a positive effect of a default on the aggregate amount of foreign aid received by the defaulting country. This finding reflects significantly increased aid flows given by non affected creditor countries. One intuitive explanation may be that governments of donor countries focus on foreign aid determinants other than the possibility to punish the default, e.g. the receiving country's needs. This might be especially important in times of a default since the economic situation of the debtor tends to be worse for the foreseeable future.

This paper is organized as follows. Section 2 gives a brief overview of the related literature concerning default costs and the linkage between aid and sovereign debt. In section 3 we take a closer look at the bilateral data on foreign aid flows and default events that are used in this paper. The econometric methodology is described in section 4 and section 5 presents the results of our empirical estimation. Section 6 concludes.

2. Related Literature

In this section we will review the existing literature dealing with default costs.[3] After that we will take a brief look at the literature that links foreign aid to sovereign debt.

Referring to the two categories of default costs, reputational costs and sanctions, four reasons for the repayment of international debt are typically mentioned in the literature. First, Alesina and Tabellini (1989) argue that delinquent countries may simply have their overseas assets seized by foreign creditors. This would be a direct sanction for countries that renege on their debt. However, the feasibility of this enforcement mechanism is limited, e.g. because of sovereign immunity. Second, Eaton and Gersovitz (1981) emphasize the importance of a borrower's repayment reputation when the country wants to issue further sovereign debt.[4] They suggest that a default impairs this reputation and leads to an exclusion from international capital markets. To the extend this embargo is permanent the defaulting country loses its ability to smooth consumption over time.[5]

The argument that the threat of capital market exclusion as a result of a bad reputation can effectively deter sovereign default is criticized for several reasons.[6] On the one hand, Kletzer (1994) mentions that a permanent exclusion from capital markets lacks commitment if both, creditors and donors, can benefit from interacting on capital markets after a default. On the other hand, Bulow and Rogoff (1989b) point out that a defaulting country may still be able to smooth consumption even without access to foreign borrowing, e.g. by drawing on accumulated buffer-stock savings. Both arguments indicate that there has to be at least a third type of default costs. Bulow and Rogoff (1989a) and Fernandez and Rosenthal (1990) mention reductions in international trade as consequences of defaults. Theoretically, reduction in trade could occur because creditor countries impose trade sanctions to discourage future defaults or because the defaulting country loses access to trade credit, which is needed to finance international trade.

[3] See Eaton and Fernandez (1995) for a detailed literature review on repayment incentives.

[4] For further work concerning reputation and sovereign debt see, e.g. Kletzer (1984) and Grossman and Van Huyck (1988).

[5] Eaton and Gersovitz (1981) implicitly assume that international debt is the only way to achieve consumption smoothing.

[6] Cole and Kehoe (1998) build a general model of reputation in which the government loses its trustworthiness and overall reputation in case of a default. A default therefore affects more than the ability to borrow again after a default. The model of Cole and Kehoe (1998) thereby can support large amounts of sovereign debt.

Finally, countries that renege on their debt may also lose the benefits of development assistance as the international community withdraws foreign aid. This fourth type of default costs has been recently discussed by Asiedu and Villamil (2002). The key assumption in their theoretical model is that a country that repudiates its foreign debt will lose access to FDI and aid.

Several empirical studies investigate the different types of default costs outlined above. Typically, they use bilateral data to distinguish between the reaction of countries directly affected by the default and of those countries that are not. This differentiation is highly important as the aggregate effect of a default may mask the punishment imposed by creditor countries. The necessary information is obtained from the Paris Club which provides data on the debtor countries that restructured their debt as well as information about the affected creditors.

Fuentes and Saravia (2010) use bilateral data on FDI flows and sovereign debt renegotiation to analyze weather a default leads to capital market exclusion in terms of a decline in FDI inflows. The data on FDI flows identifies the source as well as the recipient country. They find a significant decline of FDI inflows coming from the defaulter's creditor countries. FDI inflows from countries not affected by the default rise but the aggregate effect on FDI remains negative. Overall, FDI inflows of a country that renege on its debt fall by about 0.05 percentage points of its GDP. These results indicate that countries whose debt claims have not been settled impose a penalty on the defaulter in form of a reduction in FDI.

Without looking at the theoretical question why trade could be reduced in case of a default Rose (2005) and Martinez and Sandleris (2011) empirically investigate the relationship between international trade and sovereign default. Using bilateral data on trade and default events Rose (2005) analyzes trade between country pairs. His findings indicate that a default leads on average to an 8% decline in trade between the defaulting country and its creditors. This effect persists for about 15 years. Furthermore, Rose (2005) does not find strong evidence for trade diversion. Hence, trade reduction from creditor countries is not compensated by a rise in trade with other countries. A default therefore leads to an overall decline in trade for the delinquent country. Martinez and Sandleris (2011) even argue that trade reduction also occurs between the defaulting country and non-creditor countries.

Until now, no empirical analyses investigates if a default leads to a decline of foreign aid allocated to the delinquent country. Even though the empirical analysis of Asiedu et al. (2009) examines how foreign aid affects the relationship between FDI and country risk the assumption that a default is followed by a loss of foreign aid is not empirically studied. One first step to evaluate the relationship between the allocation of aid and sovereign defaults is made by Powell and Bird (2010). They analyze if a debt relief leads to an in- or decrease of aggregate foreign aid transferred to the corresponding country. In their empirical analysis Powell and Bird (2010) focus on countries in Sub Saharan Africa (SSA). They find a significant increase in aggregate aid transfers after a country received a debt relief.

At first glance, one could think of debt reliefs to be nothing else than defaults. Creditor countries might know that their debt claims will not be served and therefore decide for a voluntary debt relief. The findings of Powell and Bird (2010) would then indicate the absence of punishment in form of aid reduction after a default. However, this interpretation might be misleading. First, donor countries may not judge debt reliefs for SSA countries as a default but as a kind of aid for extremely poor and highly indebted countries. We therefore try to shift the focus to the relationship between foreign aid and *real* defaults by taking a look at a boarder set of countries and different default indicators. Second, Powell and Bird (2010) only analyze aggregate aid flows. Unilateral punishment by creditor countries might therefore remain undetected. To capture this we use bilateral data as it is common in the literature on defaults and trade or FDI. Our empirical approach is therefore related to Rose (2005) and Fuentes and Saravia (2010).

3. Data and Hypotheses

Empirical studies on the determinants of foreign aid allocation typically draw upon bilateral data from the OECD Development Co-operation Directorate. This comprehensive data base offers information on committed and actually disbursed aid flows for a large number of donors and recipients. Unfortunately, the raw data on official development assistance (*ODA*) is ill-suited for our analysis of the relationship between foreign aid and sovereign defaults. The reason is that our key explanatory variables, the default variables, affect standard indicators of foreign aid via an accounting relationship. To see this point, notice that our definition of sovereign defaults refers to the renegotiation of *official* external debt through the Paris Club. Each default event thus reflects either a postponement or an outright reduction of a country's debt service obligations owed to other sovereigns. The outcome of the renegotiation process, however, is also recorded in the OECD database as new aid payments from the affected creditors to the delinquent debtor country. This increases measured ODA although the debtor country does not receive additional financial support. The transaction enters either as a new ODA loan (subheading "rescheduled debt") in the case of a mere rescheduling or as a debt forgiveness grant. While this treatment might be sensible from an accounting point of view, it also introduces a spurious positive correlation between sovereign defaults and foreign aid.[7] As a consequence, the results from a regression of ODA on indicators of sovereign defaults would be biased against finding evidence for punishment. To address this issue we resort to the concept of "gross aid transfers" (GAT) proposed by Roodman (2011). His measure of foreign aid builds upon the official OECD statistics on actually disbursed aid but excludes all transactions that are directly related to debt renegotiations:

$$\text{GAT} = \text{ Gross ODA} - \text{debt forgiveness grants} - \text{rescheduled debt}.$$

Information on gross aid transfers is available on a bilateral basis, covering 34 different international donors and 190 recipients of foreign aid.[8] Even though the panel is unbalanced, data on some donor-recipient pairs cover the entire period from 1960-2009. Inspection of the data set further reveals that the distinction between GAT and ODA is economically important. Take US bilateral aid to the Dominican Republic as an example. After the latter country renegotiated its debt through the Paris Club in 2004 and 2005 US official development assistance in 2006 still added-up to 52.75 millions, measured in 2008 US \$. However, more than 17 % of this sum (9.02 mill. US \$) are due to the direct effects of debt forgiveness and rescheduling. Similar large discrepancies can be found for other years and country pairs. Measuring aid appropriately is thus clearly essential from the perspective of our study. In the following, we therefore use the logarithm of real GAT scaled by the recipient's population (Aid) as our dependent variable.

[7]The accounting rule introduces additional problems concerning the treatment of canceled loans that were originally meant for non-development purposes like military spending. See Roodman (2011) for an extensive discussion of this point.

[8]It is also possible to calculate a measure of *net* aid transfers that subtracts debt service on ODA loans from the GAT statistics. While the new statistic might be an even better approximation of the recipient's benefit from foreign aid (Roodman, 2011) it has the drawback of being partly determined by past aid disbursement. Since our focus is on current policy choices, we follow Dollar and Levin (2006) and opt for a measure of gross aid flows.

We follow Rose (2005), Fuentes and Saravia (2010) and Martinez and Sandleris (2011) in using information from the Paris Club to construct different indicators of sovereign defaults. The Paris Club's website is the most comprehensive data source on sovereign defaults in terms of coverage and detail. It comprises more than 400 debt restructurings that took place between 1956 and 2011. For each restructuring deal, the dataset contains information on the amount of debt rescheduled and on the type of treatment which specifies its degree of concessionality. Most important for the purpose of this study, it lists not only the defaulting sovereign but also the affected creditor countries. This allows us to test two variants of the hypothesis that aid withdrawal is actually used as punishment for sovereign defaults.

A strong version of the punishment hypothesis states that international donors as a group sanction defaults by reducing foreign aid to delinquent sovereign debtors. The collective withdrawal of foreign aid thus represents an additional cost to the affected country that may influence its decision to default in the first place. Hence, foreign aid would serve as an enforcement mechanism as modeled by Asiedu and Villamil (2002) and Asiedu et al. (2009). We test this hypothesis by adding a default indicator (*Default*) to an otherwise standard set of foreign aid determinants. This variable takes the value 1 whenever an aid recipient restructured its debt through the Paris Club. The variable's coefficient should take a negative value according to the hypothesis. As another test we also include the size of the Paris Club deal (*Amount*) in some specifications. Assuming that larger defaults are viewed as particularly inexcusable and thus deserve even more punishment, we expect to find a negative coefficient on this variable as well.

The second, weaker version of the same hypothesis allows for heterogeneous responses of donor countries. In fact, it seems reasonable to assume that coordination among donors is too weak to ensure collective sanctioning. A reduction in aid disbursements might thus only be observed for those creditor countries to which the recipient defaulted. Whether foreign aid functions as an enforcement mechanism for international debt contracts then depends on the strength of this reaction and on the behavior of the remaining donors. Their response is theoretically ambiguous. On the one hand, limited coordination might still induce those donors to cut their aid flows as well, perhaps by a smaller amount. On the other hand, aid granted to defaulting countries might even increase out of altruistic motives. The reason is that sovereign defaults typically coincide with periods of economic hardship which renders the crisis-stricken countries more needy. In either case, allowing for a differentiated reaction of donor countries depending on their role in the debt restructuring is empirically important. We do this by including a bilateral default dummy (*Bilateral Default*) as an additional regressor. This second default dummy indicates whether an aid recipient defaulted on the debt owed to a specific donor in a given year. While we do not have any priors regarding the reaction of the remaining donors, the idea of punishment implies that aid flows from the defaulter's creditor countries should decline after a sovereign default.

Our analysis includes a large set of control variables that might influence the allocation of foreign aid. We follow Hoeffler and Outram (2011) in considering variables that measure the recipient's need and merit as well as indicators of strategic motives. The need of a specific recipient is approximated by its income per capita (*GDP pc*) and by the amount of aid it receives from other donors (*Other Aid pc*). Poorer countries are expected to receive relatively more aid while the relationship between aid from different donors could either be complementary or substitutive. The merit of aid recipients is captured by three different indicators. The first, the growth rate of the recipient's GDP per capita (*Growth*), serves as a proxy for beneficial economic policies and should thus be positively related to aid inflows. The two remaining variables are an indicator of human rights violations (*Human Rights*) and the polity2 index of democracy (*Democracy*). We expect that democracies which honor human rights (low value of *Human Rights*) attract relatively more aid compared to dictatorships with a history of human rights abuses. Strategic concerns of the donor countries are proxied by two variables: bilateral trade (*Trade*) and voting allegiance in the UN General Assembly (*UN Friend*). Donors are likely to favor countries that are either important trading partner or close political allies. We thus expect to find a positive relationship between both variables and foreign aid disbursements. Random effects specifications further contain an indicator of the donor's and recipient's colonial past (*Colony*) as another time-invariant measure of political allegiances. Finally, we also include the logarithm of the recipient's population (*Population*) as an additional regressor. This variable does not fit into any of the three categories mentioned so far. Rather, it is meant to capture the stylized fact that small countries tend to attract disproportionately large amounts of foreign aid in per capita terms. Appendix A contains further information on the construction of all included variables along with their data sources.

Due to limited data availability on the UN voting variable and on some other regressors our final sample comprises 1309 different donor-recipient pairs with annual data from 1970 to 2008. The reduction in the number of observations on aid flows mainly reflects our focus on the G7 donors: Canada, France, Germany, Italy, Japan, the United Kingdom, and the United States. These countries accounted for roughly two-thirds of all bilateral aid disbursements throughout our sample period. Table 1 reports some descriptive statistics on our dependent and explanatory variables for this final sample. Notably, the fifth column of this table shows that the minimum value of our aid variable is negative. Negative gross aid transfers will occur if recipients return unspent, previously granted aid to the respective donor. With only 57 observations, these cases are quite rare. They mask, however, another important feature of the data as 5,306 of the 36,512 observations on gross aid transfers take the value zero. We address this issue in the next section.

≪ insert Table 1 here ≫

4. The Econometric Framework

The empirical analysis of bilateral foreign aid flows involves at least two key specification choices. The first choice concerns the appropriate use of the data's panel structure. Most earlier studies reduce the dimensionality of the data, which typically covers annual aid flows from multiple donors to a large number of recipients, by resorting to donor-specific estimations.[9] Alternatively, information from multiple donors could be pooled. Focusing on the average donor in this way drastically increases sample size but possibly neglects heterogeneity in individual donor behavior. These differences can be captured by dyad-specific fixed effects if they are limited to time-constant characteristics of the donor-recipient pair. Past colonial ties between countries or the USA's special relationship to Israel and other strategically important countries in the Middle East region are examples that fit into this category. We follow Berthélemy and Tichit (2004), Chong and Gradstein (2008) and Claessens et al. (2009) and use the latter approach. We do, however, check whether our findings are sensitive to this choice and also report results from donor-specific regressions.

The second important choice involves the specification of the dependent variables' data generating process. The chosen model should address the fact that bilateral aid flows – though generally nonnegative – are equal to zero for a substantial number of observations.

[9]The seminal work of Alesina and Dollar (2000) offers one prominent example for this approach.

Three different estimation strategies have been proposed in the literature that fulfill this requirement. Each of them has specific strengths and shortcomings:

1. *Two part model.* Two part models differentiate between a participation (whether aid is supplied or not) and an amount decision (aid flows given that aid is positive). The model's most critical assumption states that both decisions are independent after controlling for observed explanatory variables. In this case, the parameters of interest can be estimated from separate binary and linear models. In our application, the two estimation equations are

$$\ln\left(Aid_{ijt}\right) = \theta_t + \boldsymbol{d_{ijt}}\boldsymbol{\beta} + \boldsymbol{x_{ijt}}\boldsymbol{\gamma} + c_{ij} + u_{ijt} \quad \text{and} \tag{1}$$

$$Aid^*_{ijt} = \boldsymbol{z_{ijt}}\boldsymbol{\eta} + \zeta_{ij} + a_{ijt}; \quad Aid_{ijt} > 0\left[Aid^*_{ijt} > 0\right], \tag{2}$$

   where index i and j refer, respectively, to the donor and recipient country and Aid_{ijt} is our indicator of bilateral aid flows in year t. Vector $\boldsymbol{d_{ijt}}$ contains our main variables of interest that describe the default status of countries i and j. Equation 1 further includes a vector of control variables $\boldsymbol{x_{ijt}}$ and a time varying constant θ_t. The participation decision also depends on the covariates in $\boldsymbol{z_{ijt}}$ which contains at least one variable that is not already included in $\boldsymbol{d_{ijt}}$ or $\boldsymbol{x_{ijt}}$.[10] Furthermore, both equations include unobserved country-pair specific effects (c_{ij} and ζ_{ij}) and an idiosyncratic error term (u_{ijt} and a_{ijt}). Most studies only report results for equation 1 which is estimated on the subsample of positive observations using standard panel techniques.

2. *Selection model.* Estimation of a selection model is appropriate if the conditional independence assumption of the two part model is not met. This would be the case if the unobserved effects or the idiosyncratic error terms of equations 1 and 2 were correlated. Estimates from the two part model would be inconsistent in either case. One approach that does consistently estimate the parameters of the amount equation in a panel context is Wooldridge's (1995) variant of Heckman's two-step estimator. Details on this approach, which rests on quite restrictive assumptions, can be found in Appendix B.

3. *Tobit model.* The Tobit model resorts to a latent variable specification to account for corner solutions of the dependent variable. In our application, the latent vari-

[10] This assumption is not essential for the two part model. The related selection model, however, might be only poorly identified without this assumption.

able can be thought of as a donor's desired amount of aid for a particular recipient. If this amount is negative, no aid is distributed. Otherwise, y^*_{ijt} determines the amount actually disbursed. Again assuming that aid is lognormally distributed we get the model

$$y^*_{ijt} = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + c_{ij} + u_{ijt} \quad \text{with} \tag{3}$$

$$\ln\left(Aid_{ijt}\right) = y_{ijt} = \left\{ \begin{array}{ll} y^*_{ijt}, & \text{if } y^*_{ijt} \geq \min\left(y_{ijt}\right) \\ -, & \text{if } y^*_{ijt} < \min\left(y_{ijt}\right) \end{array} \right\} \quad .$$

The major drawback of the tobit model for panel data is that it only allows for a random effects specification of the unobserved effects c_{ij}. This requires the strong assumption that all observable covariates are uncorrelated with the c_{ij}.

Given their different limitations, we draw on all of the three approaches in our study. This procedure allows us to investigate whether our results are robust or just the reflection of a more or less arbitrary specification choice. However, following the majority of existing studies, we use the simple two part model as starting point for our analysis. A final problem common to all three approaches is that our regressors are not necessarily strictly exogenous. For some variables, even contemporaneous exogeneity seems questionable. Following Hoeffler and Outram (2011) we therefore include lagged instead of current realizations of most control variables in our regression. This procedure should at least mitigate concerns of endogeneity.[11] We do not lag the "UN Friend" and "Other Aid" variable which are clearly endogenous. Their coefficients should thus not be interpreted causally. Potential endogeneity of the default indicators as our main variables of interest is addressed separately in subsection 5.2.

[11]The fixed effects estimator, which is predominantly used in this paper, is still inconsistent in the presence of regressors that satisfy contemporaneous but not strict exogeneity. This inconsistency shrinks to zero at the rate $1/T$. Given that our sample period spans 39 years this problem should be negligible.

5. Empirical Evidence on Punishment through Aid Withdrawal

5.1. Baseline Results

We begin our empirical analysis by estimating equation 1 on the subsample of observations with positive bilateral aid flows. The classic fixed effects estimator is used for this exercise, as it seems likely that unobserved characteristics of the donor-recipient relationship correlate with our variables of interest. To account for potentially heteroscedastic and autocorrelated error terms we further employ cluster-robust standard errors. Table 2 shows the results. Column (1) starts with the estimates from a baseline specification that does not include any indicator of sovereign debt crises. The results are generally in line with our priors and with previous findings in the literature. G7 donors allocate foreign aid towards countries that are relatively poor but fast growing. Herding also seems to be an important characteristic of donor behavior as the coefficient on the variable measuring donations from other countries is positive and highly significant. This effect, however, may also reflect that some recipients are considered needy in dimensions not well captured by our other control variables and thus receive more aid from all donors. Regarding our political variables, we find only mixed evidence in favor of the hypothesis that a recipient's merit is an important determinant of aid inflows. While human right abuses tend to be sanctioned, democracies are not rewarded with significant additional financial support. At the same time, the significantly positive coefficient on the trade variable indicates that strategic concerns affect the allocation of foreign aid. The finding of a positive relationship between voting behavior in the UN and foreign aid disbursements would further support this line of argument. The coefficient on the "UN friend" variable, however, has the correct sign but is not statistically significant. Finally, we also find evidence for a small country bias meaning that less populated countries receive relatively more aid in per capita terms.[12]

≪ insert Table 2 here ≫

[12] Robustness exercises show that this effect is still present when only recipients with a population above a certain minimum threshold (e.g. 1,000,000) are included. Hence, the effect at least partly reflects that large countries like China receive relatively less foreign aid. In accordance with the literature, we nevertheless stick to the term "small country bias" although it might be a misnomer.

The lower part of Table 2 contains results from the Robust Hausman test and different measures of the model's fit. The robust and the standard Hausman test share the same null hypothesis, namely that the regressors and the unobserved effects are uncorrelated. The random effects model is consistent under this assumption. It is, however, not necessarily fully efficient since both the unobserved effects and the error terms may fail to meet the usual i.i.d. assumption. A violation of this assumption, which is used for inference in the standard Hausman test, is likely in our application.[13] We therefore use the robust version of this test which is based on an artificial random effects regression that also includes the demeaned variables as additional regressors (Arellano, 1993). Under the null hypothesis, the coefficients on these transformed fixed effects regressors should be zero. Inference can be based on clustered standard errors which are robust to violations of the i.i.d. assumptions. The results from the test strongly reject the random effects model.[14] Confirming our priors, the fixed effects model is thus the preferred one. Turning next to the three different R^2 measures, the model's explanatory power seems modest at first glance. Other studies in the field like Berthélemy (2006) or Hoeffler and Outram (2011) routinely report R^2s of more than 50%. This difference, however, can be explained by the fact that our measures focus on the explanatory power of the observed variables and ignore the contribution of the fixed effects. Adding their contribution yields a comparable R^2 of 0.74.

The remaining three columns of Table 2 are devoted to our main research question. They report results from specifications that add our two different default indicators, either separately or jointly, to the standard set of control variables. Again, all regressions also include country-pair specific fixed effects. This specification is generally supported by the Robust Hausman test.

We start with a test of the strong version of the punishment hypothesis which states that donors sanction sovereign defaulters collectively. The absolute amount of aid received by these countries should thus decrease. Our results – displayed in column (2) – clearly reject this hypothesis. The coefficient on the common default dummy is positive and statistically significant at the 5 percent level. Furthermore, the effect is also significant in economic terms. Overall, donors increase aid disbursement to delinquent borrowers by 6.4 % on average. These results are at odds with Asiedu and Villamil's (2002) idea that foreign aid may function as an enforcement mechanism for sovereign debt. On the

[13] Large differences between clustered and normal standard errors obtained for the random effects model support this notion.

[14] Results from standard Hausman tests (not shown) point to the same conclusion.

contrary, the documented surge in aid flows in the aftermath of debt restructurings even increases the attractiveness of sovereign defaults. An explanation for this finding might be that donors contemporaneously react to other objectives that dominate the strategic motive for punishment. Altruistic motives, e.g., may play a role as a sovereign default may be seen as an indicator for a persistent economic crises that renders the already poor country even more needy.

Another possibility is that only those countries that are directly affected by a default resort to sanctions. A common default dummy would fail to capture this behavior if it is counteracted by increased aid disbursements from the remaining donors. The specifications in column (3) and (4) test this weak variant of the punishment hypothesis by including a bilateral default dummy. Again, we do not find any evidence for sanctioning. The coefficient on the bilateral default dummy variable – when considered in isolation as in column (3) – is insignificant and positive. Thus, even directly affected donors fail to sanction their former debtor by reducing foreign aid. This conclusion is supported by the results in column (4). Here, the inclusion of both default variables explicitly allows for heterogenous responses from affected and not affected creditor countries. The finding of a large positive coefficient on the common default dummy confirms our previous notion that the increase in foreign aid documented in column (2) predominantly reflects additional aid disbursements from the latter group of donors. Their support increases by roughly 15 percent after a sovereign default. Less is known about the reaction of former creditors. The negative effect on the bilateral default dummy indicates that these donors tend to give less aid after a default than their peers. However, this effect is not statistically significant. Furthermore, the overall effect of a default on bilateral aid disbursements would still be positive for the countries that are involved in a renegotiation, even if the coefficient were significant.

The results presented so far strongly reject both the weak and the strong version of the punishment hypothesis. One possible explanation for this finding could be that our binary default indicators fail to differentiate between different default events. This would bias our estimates if reactions to a default differed depending, e.g., on some characteristics of the restructuring deal like the size of the haircut. Existing empirical work by Trebesch (2010), Cruces and Trebesch (2011) and Jorra (2011) support this idea. A heterogenous reaction in terms of sanctioning could also be justified theoretically. Grossman and Van Huyck (1988), e.g., differentiate between excusable and inexcusable defaults. In their model, only the latter events are punished. We try to capture this distinction by including the size of the Paris Club deal as an additional variable. The underlying hypothesis is that larger defaults are considered less excusable and are thus punished harder (Fuentes and Saravia, 2010). The first two columns of Table 3 contain the results. Again, these are not supportive for the sanctioning hypothesis. The additional variable enters with a coefficient that is correctly signed but insignificant. Moreover, none of the two coefficients on our binary default indicators is affected by the inclusion of the previously omitted variable. The insignificance of the "amount" variable does not merely reflect problems of multicollinearity. It is also present in specifications that include only one of the two other default variables (column 2).

≪ insert Table 3 here ≫

The last two columns of Table 3 investigate whether a default has harmful longer-term consequences for a recipient of foreign aid. In principal, defaults could have a lagged effect on aid disbursement as a result of a lengthy budgeting process which impairs its instantaneous reallocation. So far, evidence for such time lags has been documented for trade (Rose, 2005) and FDI related default costs (Fuentes and Saravia, 2010). To analyze this issue, we add two lagged default indicators to our baseline specification. These take the value one whenever a recipient defaulted at least once during a specified five year period ($t-1$ to $t-6$ and $t-6$ to $t-10$) on the debt owed to a particular donor. They are thus meant to capture the delayed response of former creditor countries that were directly affected by a specific default decision.

According to our results, sovereign defaults indeed have a delayed effect on the aid allocation decision. Its sign, however, is inconsistent with the idea of long-term punishment. On the contrary, directly affected donor countries only seem to lag their peers in increasing foreign aid disbursements to defaulting sovereigns. Interestingly, the coefficient on the dummy variable for a bilateral default during the most recent 5 year period is similar in absolute magnitude to the negative coefficient on the bilateral contemporaneous dummy which is now significant. A possible interpretation of this finding is that aggrieved creditors hesitate to reward a default by raising aid instantaneously. In the medium term, however, they also react to the increased need of the recipient. Column (4) of Table 3 further shows that this effect is restricted to the first five post-default years. Renegotiations that took place in any year between $t-6$ and $t-10$ do not trigger additional aid flows.

5.2. Addressing Corner Solutions and Endogeneity

This section investigates whether the sound rejection of the punishment hypothesis throughout all previously discussed regressions is related to our specification choice. We start with an analysis of the two alternative models for corner solution outcomes. As discussed in section 4, these are the tobit and selection model. Table 4 displays results from both models. For comparison purposes, column (1) further contains the findings from a random effects specification of the two part model. Such a comparison could be especially valuable for the tobit results which are also estimated from a specification that includes random effects.[15]

≪ insert Table 4 here ≫

Column (2) displays the results for the tobit model. We report marginal effects of our default variables on foreign aid disbursement, conditional on observing positive aid flows. These are calculated as the average of the marginal effects obtained for the subsample of observations where aid was actually disbursed.[16] The results should thus be directly comparable to those reported for the two part and selection models. The finding of a positive and significant marginal effect of defaults on aid from not directly affected donors can therefore be directly interpreted as further evidence against the strong variant of the punishment hypothesis. Confirming our previous findings, the reaction from donors that participated in a renegotiation tends to be more muted, although the difference is not statistically significant. Surprisingly, this changes when bootstrapped clustered instead of the usual standard errors are considered.[17] However, these results, reported in column (3), still do not support any variant of the punishment hypothesis. Rather, they imply that even aggrieved creditors increase their support to defaulting sovereigns by nearly 10 percent on average.

We next turn to Wooldridge's (1995) selection model for panel data. Although it is not necessarily required for identification we include two additional variables in the participation equation to improve the precision of our estimates. Following Koch et al. (2009)

[15] As discussed above, this specification choice is necessary as no consistent fixed effect estimator exists for the tobit model.

[16] Our calculation also takes the binary nature of the default variables into account. In both cases, the reported results thus measure the effect of a discrete change from 0 to 1.

[17] This unexpected finding of smaller clustered than normal standard errors is limited to the default variables. Standard errors for the remaining marginal effects rise markedly when switching from column (2) to column (3).

we reason that joint religion affects the decision to supply aid to a particular recipient. At the same time, we assume that the additional variable can be excluded from the allocation equation. Borrowing from the related literature on the gravity model of international trade, we further include a common language dummy as another additional regressor in the participation equation (Helpman et al., 2008). The final column of Table 4 contains the results. Again, our main conclusions remain unaltered, although many terms containing the inverse Mills ratio turn out statistically significant (not shown). The effect of selection bias thus seems at least economically negligible. In this respect, our results complement previous findings in the literature, documented, e.g., by Berthélemy and Tichit (2004) and Berthélemy (2006). We therefore resort to the simple two part model for the remainder of this paper.

The second part of this section deals with another source of biases arising from endogeneity. Problems of this kind are indeed likely to occur in our application as the relationship between sovereign defaults and foreign aid flows is potentially bidirectional. At least two different explanations for a causal link between aid disbursement and the probability of debt crises come to mind. The first explanation is related to the classical source of simultaneity bias that plagues all studies on the consequences of sovereign defaults. It states that a reduction in foreign assistance – or, more generally, in economic activity – causes debt crises as it impairs both, a sovereign's ability and willingness to serve its debt. In accordance with the punishment hypothesis, this theory thus predicts a negative correlation between aid flows and sovereign defaults. Since our results show the opposite, the case for the empirical relevance of punishment is even weaker in light of this argument. The second, alternative explanation is more troublesome for our conclusions as it implies that our coefficient estimates are biased upwards. It draws on the large literature on the economic effects of foreign aid. Studies in this field repeatedly document that aid has negative unintended side effects. Djankov et al. (2008), e.g., find a significant negative effect of aid on the quality of political institutions. Other studies summarized in McGillivray et al. (2006) document that even the effect of foreign aid on growth can be negative in some circumstances. Both effects might in turn increase sovereign risk and introduce a positive correlation between foreign aid and defaults. A careful examination of the importance of endogeneity is thus necessary since it is a priori unclear which of the two effects dominates.

Unfortunately, controlling for endogeneity turns out rather difficult empirically. The reason is a lack of variables which predict sovereign defaults but do not influence foreign aid disbursements. Similar problems have plagued previous econometric studies of default costs. Typically, it remains unclear whether the finally chosen variables in an IV approach are suitable, weak or even invalid instruments. Reviewing these past attempts Borensztein and Panizza (2009) even conclude that convincing instruments might simply not exist. These limitations in mind, we resort to three variables that have been repeatedly used for this purpose. Following Rose (2005) and Martinez and Sandleris (2011) our instrumental variables are the budget deficit (in percent of GDP), the CPI inflation rate and the ratio of the current account surplus/deficit to GDP. The results from both two-stage least-squares (2SLS) and GMM estimation techniques are shown in Table 5.

≪ insert Table 5 here ≫

Encouragingly, the results from both specifications are quite similar and generally in line with our previous findings. All coefficients of our default dummies are positive but statistically insignificant. Hence, we still do not find any evidence for punishment through a reduction in foreign aid. Furthermore and despite our initial objections, none of the reported diagnostic tests indicates misspecification. All four specifications easily pass the Kleibergen and Paap's (2006) underidentification test.[18] Testing for the likely consequences of weak instruments following the procedure of Stock and Yogo (2005) further reveals only a moderate potential bias. Finally, Hansen's J statistic does not reject the hypothesis that our instruments are valid. Although this test might fail to detect all incidences of instrument invalidity, it seems very unlikely that controlling for any remaining bias would switch the sign of the default effect. In light of the robustness of our results we are thus confident that the hypothesis that sovereign defaulters are punished through a reduction in foreign aid is empirically irrelevant.

[18] This is not true for our standard specification that includes both default indicators simultaneously. The specification is underidentified since both deficit variables are highly correlated while the predictive power of the inflation rate is very limited. This leaves us with only one effective instrument for two endogenous variables. We therefore do not report these results although they do not contradict our general conclusions.

5.3. Further Robustness Exercises

All results previously discussed rest implicitly on three assumptions that are not necessarily met. So far, we have assumed that Paris Club renegotiation dates correctly identify sovereign defaults, that the relationship between aid disbursements and defaults has not been subject to structural change, and that this relationship is the same for all donor countries. This section investigates whether our results are robust to relaxing these assumptions.

A concern with the Paris Club data is that it does not distinguish between sovereign defaults and incidences of debt relief. Both events are recorded in the data set as they imply a change in the terms of the original debt contracts that favors debtors at the expense of creditors. However, from an economic perspective defaults and debt reliefs are clearly distinct. The first type of event is typically seen and modeled as a deliberate policy choice of a debtor government. By contrast, the cancelation of loans through a debt relief is often initiated by former creditor countries. Their active role in the restructuring process does not square with the concept of punishment as envisioned by Asiedu and Villamil (2002) and Asiedu et al. (2009). Including data on incidences of debt relief might thus bias the results on the effects of sovereign defaults. This may even lead to a false rejection of the punishment hypothesis. The problem will be especially severe if donor countries view a debt relief as a complement rather than a substitute to foreign aid (Powell and Bird, 2010).

We attempt to address this issue by employing Paris Club information on the "type of treatment" to identify those renegotiations that might be better described as debt relief. Restructurings that took place under the Heavily Indebted Poor Countries (HIPC) initiative constitute a first candidate group. A broader definition of debt relief also includes other highly concessional Paris Club deals that were negotiated under "Cologne" or "Naples" terms.[19] A final categorization treats every restructuring of countries that are eligible for the HIPC initiative as debt relief. For each of these three definitions we reestimate the amount equation 1 excluding all incidences of debt relief. Table 6 contains the results which are in line with our previous findings. Sovereign defaulters attract significantly more, not less, aid in per capita terms according to the results that rest on either of the two broader default definitions (columns (1) to (4)). As before, this

[19] This definition includes renegotiations under "Toronto" and "London" terms as well as those under "Lyon" terms which were, respectively, the predecessors of "Cologne" and "Naples" treatments.

mainly reflects increased aid disbursement from countries that were not affected by the default. This effect vanishes in the specifications (5) and (6) which exclude all HIPC observations from the sample. Here, the reactions of both donor groups are statistically indistinguishable from zero. Still, we do not find any evidence for negative effects of sovereign defaults on aid inflows. Hence, both variants of the punishment hypothesis are rejected across all specifications and default definitions.

≪ insert Table 6 here ≫

We next analyze whether the impact of sovereign defaults on foreign aid flows has changed over time. In principle, such structural change seems plausible given that our sample period covers four decades and includes, e.g., the end of the Cold War. This event in particular might have affected the costs and benefits of punishments after a sovereign default. An aid withdrawal probably has never been a realistic option during the Cold War period, especially not when the allegiance of a strategically important country was at stake. Following this line of argument we would thus expect to find a more negative effect of defaults on aid starting in the 1990s. However, since previous research has documented an increased importance of altruistic motives of aid disbursements for the same period (Claessens et al., 2009), the opposite reaction also seems possible.

In light of the theoretical arguments, we tackle the issue of structural change by reestimating the simple log-linear model of positive aid disbursements separately for each decade. Table 7 shows the results. These are generally quite similar to each other and to the results that have been obtained for the complete sample period. This might either reflect that the two arguments for structural change are empirically irrelevant or that the opposing effects cancel each other out. The most noticeable change concerns the coefficient on the common default dummy which is now insignificant in all specifications. For the 1970s and 1980s this predominantly reflects the increase in standard errors brought about by the reduced sample size. In addition to this, the point estimates also decrease in the last two decades. Here, the negative coefficient of the bilateral default dummy is even larger in absolute size indicating that aid from directly affected donors might decrease after a default. This effect, however, is economically negligible and far from being statistically significant. The results thus still do not provide any evidence in favor of the punishment hypotheses.

≪ insert Table 7 here ≫

In a final step we investigate the importance of donor-specific heterogeneity that will result if individual donors are differently inclined to use foreign aid as a disciplining device. To detect these deviations from the average behavior we resort to donor-specific estimations of the amount equation 1. The upper part of Table 8 contains the regression output for a specification that includes both default dummies. At first glance, the deviations from our earlier results seem substantial as the coefficients on our two variables of interest repeatedly change signs from specification (1) to (7). However, nearly all of these differences can be attributed to a differentiated reaction to defaults which do not affect the respective donor country. By contrast, the effect of a bilateral default, measured by the sum of the two default coefficients, is nearly identical for all donors and close to zero. This again contradicts the idea that aggrieved creditor countries punish sovereign defaults by withdrawing foreign aid.

≪ insert Table 8 here ≫

One possible explanation for the puzzling variation in the donors' reaction to a recipient's default on debt owed to a third country is that these effects might be only poorly identified. This conjecture is supported by the high degree of multicollinearity between the two default indicators. In most samples, both variables take identical values for roughly 98 per cent of the observations. We therefore rerun the donor-specific regressions, each time including only one of the two dummy variables. The lower part of Table 8 shows the results for the specifications that add only the common default dummy to the standard set of control variables.[20] According to these estimates, no single donor country punishes sovereign defaults by reducing aid disbursements significantly. Japan even increases its development assistance to crisis stricken countries. Recipient countries thus do not receive less foreign aid after a default implying that aid can not work as an enforcement mechanism for sovereign debt. Furthermore, a F-test fails to reject the hypothesis that all default coefficients are jointly equal to zero, justifying our earlier focus on the pooled data set.

[20]Similar results can be obtained by including only the bilateral default dummy. These results are available from the authors upon request.

6. Conclusion

Looking at the long history of sovereign debt shows that defaults occur rather rarely. Countries typically pay their debt even if no international bankruptcy laws force them to do so. In the literature, this is explained by the existence of several kinds of default costs, e.g. exclusion from capital markets or trade sanctions. We focus on one specific reason for debt repayment, a possible decline in foreign aid allocated to the delinquent country in the aftermath of a default. The withdrawal of foreign aid would make a default more costly for the concerned country and hence function as an enforcement mechanism for sovereign debt. This type of default costs is theoretically analyzed by Asiedu and Villamil (2002). Based on the assumption that a country loses access to foreign aid and FDI in case of a default their model shows a decline in default risk for a country that receives positive aid inflows. However, to the best of our knowledge, we are the first who empirically investigate the validity of the underlying assumption, namely that a default leads to a reduction in foreign aid directed to the defaulting country.

We use bilateral data on foreign aid flows and debt renegotiations that identify the source and recipient countries of aid as well as the debtor and creditor countries involved in the debt restructuring process. We can therefore differentiate between the reaction of creditor countries directly affected by the default and of countries that are not affected. Investigating only the aggregate effect of default events on aid could mask a punishment imposed by the the first group of donors.

Our results indicate that a default does not lead to a loss of foreign aid received by defaulting countries. On the contrary, it even raises the aggregate amount of foreign aid directed to these countries by about 6.4% on average. This effect is mainly driven by the reaction of those donor countries that are not directly affected by the default. Foreign aid given by these countries rises significantly by about 15% on average. Nevertheless, creditor countries that suffer directly from the default also tend to give more aid and not less after the default. This increase is statistically insignificant and much smaller than the increase coming from the remaining donors. However, it is far from being significantly negative. These findings contradict the hypotheses of Asiedu and Villamil (2002) who assume that creditor countries punish a default by reducing the amount of foreign aid given to the defaulting country. Our findings are robust to different empirical model specifications and several robustness checks. Overall, foreign aid therefore seems not to work as an enforcement mechanism for sovereign debt repayment.

Our findings raise questions regarding the interpretation of the results of Asiedu et al. (2009). Their study indicates that foreign aid mitigates the adverse effect of country risk on FDI. Our empirical results show that this effect is not caused by the threat of losing access to foreign aid in case of a default. Providing another theoretical explanation for this effect is surely an important area for future research.

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Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Aid	36512	9.310	113.853	-11.826	15381.240
Default	51051	0.053	0.223	0.000	1.000
Bilateral Default	51051	0.041	0.198	0.000	1.000
Amount	51051	0.006	0.048	0.000	1.619
ln GDP pc	39725	8.067	1.172	4.767	11.722
ln Other Aid pc	36291	3.120	1.544	-8.504	9.680
Growth	39200	0.038	0.083	-1.101	0.827
Human Rights	29918	2.551	1.125	1.000	5.000
Democracy	33236	-0.989	6.977	-10.000	10.000
ln Trade	39737	-5.147	2.481	-16.784	1.687
UN Friend	38278	0.376	0.174	0.000	0.940
ln Population	44772	8.219	2.062	2.485	14.091
Colony	50232	0.099	0.298	0.000	1.000

Table 2: Aid and Sovereign Defaults: Baseline Results

	(1)	(2)	(3)	(4)
Dependent variable: Gross Aid per capita				
Default		0.064**		0.147**
		(2.37)		(2.42)
Bilateral Default			0.037	-0.106
			(1.29)	(-1.60)
ln GDP pc (t-1)	-0.188*	-0.190*	-0.189*	-0.188*
	(-1.71)	(-1.73)	(-1.72)	(-1.71)
ln Other Aid pc	0.455***	0.451***	0.453***	0.451***
	(10.95)	(10.83)	(10.89)	(10.84)
Growth (t-1)	0.303**	0.307**	0.305**	0.308**
	(2.47)	(2.50)	(2.48)	(2.51)
Human Rights (t-1)	-0.066***	-0.066***	-0.066***	-0.066***
	(-3.02)	(-3.01)	(-3.02)	(-3.01)
Democracy (t-1)	0.003	0.003	0.003	0.003
	(0.72)	(0.71)	(0.71)	(0.72)
ln Trade (t-1)	0.125***	0.126***	0.126***	0.126***
	(4.06)	(4.10)	(4.08)	(4.10)
UN Friend	0.279	0.275	0.277	0.274
	(1.53)	(1.50)	(1.52)	(1.50)
ln Population	-0.615**	-0.633**	-0.625**	-0.629**
	(-2.24)	(-2.30)	(-2.27)	(-2.29)
Constant	5.837**	6.016**	5.936**	5.968**
	(2.06)	(2.13)	(2.10)	(2.11)
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Robust Hausman[a]	182.542***	192.956***	203.574***	204.606***
N	22086	22086	22086	22086
R^2 overall	0.158	0.158	0.158	0.158
R^2 within	0.136	0.136	0.136	0.136
R^2 between	0.143	0.143	0.143	0.143

Note: [a] The robust Hausman statistic is distributed as $\chi^2(N)$ where N denotes the number of explanatory variables.

Table 3: Aid and Sovereign Defaults: Default Size & Reputation Effects

	(1)	(2)	(3)	(4)
Dependent variable: Gross Aid per capita				
Default	0.159**	0.081***	0.150**	0.151**
	(2.55)	(2.58)	(2.47)	(2.48)
Bilateral Default	-0.102		-0.122*	-0.119*
	(-1.55)		(-1.88)	(-1.82)
Amount	-0.147	-0.162		
	(-1.07)	(-1.18)		
Default between			0.108***	0.100***
t-1 and t-5			(2.71)	(2.64)
Default between				0.041
t-6 and t-10				(0.99)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Robust Hausman[a]	212.487***	200.918***	201.234***	200.839***
N	22086	22086	22086	22086
R^2 overall	0.158	0.158	0.157	0.157
R^2 within	0.136	0.136	0.137	0.137
R^2 between	0.143	0.143	0.142	0.142

Notes: [a] The robust Hausman statistic is distributed as $\chi^2(N)$ where N denotes the number of explanatory variables. Additional control variables included, but not reported.

Table 4: Aid and Sovereign Defaults: Tobit and Selection Models

	(1) *Linear, RE*	(2) *Tobit, RE*	(3) *Tobit, RE*	(4) *Selection*
Dependent variable: Gross Aid per capita				
Default	0.142**	0.241***	0.241***	0.157**
	(2.35)	(2.70)	(2.79)	(2.09)
Bilateral Default	-0.097	-0.143	-0.143*	-0.151*
	(-1.48)	(-1.46)	(-1.68)	(-1.86)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Standard errors	Clustered	Standard	Clustered (Boot)	Clustered (Boot)
N	22086	24051	24051	18285

Notes: Additional control variables included, but not reported. Selection equation includes *Joint Religion* and *Common Language* as additional variables.

Table 5: Aid and Sovereign Defaults: Endogeneity

	(1) *2SLS*	(2) *GMM*	(3) *2SLS*	(4) *GMM*
Default	0.318	0.385		
	(0.48)	(0.59)		
Bilateral Default			0.348	0.451
			(0.44)	(0.58)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
N	5639	5639	5639	5639
Underid. Test	39.15***	39.15***	39.01***	39.01***
Weak Id. Test	10.59^{+}	10.59^{+}	9.90^{+}	9.90^{+}
Hansen J statistic	3.06	3.06	3.07	3.07

Notes: $^{+}$ denotes maximum bias due to weak instruments $\leq$ 10% of the bias of OLS according to critical values calculated by Stock and Yogo (2005). *Budget Deficit*, *Current Account* and *Inflation* are used as instruments. Additional control variables included, but not reported.

Table 6: Aid and Sovereign Defaults: Different Default Definitions

	No HIPC Terms		*No HIPC, Cologne or Naples Terms*		*No HIPC Countries*	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Gross Aid per capita						
Default	0.066**	0.144**	0.071**	0.123*	0.038	0.108
	(2.37)	(2.23)	(2.41)	(1.79)	(0.93)	(1.14)
Bilateral Default		-0.099		-0.065		-0.082
		(-1.41)		(-0.88)		(-0.84)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	21933	21933	21679	21679	15489	15489
R^2 overall	0.160	0.159	0.162	0.162	0.169	0.169
R^2 within	0.136	0.136	0.136	0.136	0.131	0.131
R^2 between	0.145	0.145	0.147	0.147	0.156	0.156

Note: Additional control variables included, but not reported.

Table 7: Aid and Sovereign Defaults: Different Decades

	(1) *70s*	(2) *80s*	(3) *90s*	(4) *00s*
Dependent variable: Gross Aid per capita				
Default	0.120	0.139	0.034	0.002
	(0.17)	(1.19)	(0.54)	(0.03)
Bilateral Default	-0.090	-0.089	-0.038	-0.020
	(-0.13)	(-0.72)	(-0.52)	(-0.24)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
N	1211	5922	7741	7212
R^2 overall	0.078	0.191	0.117	0.045
R^2 within	0.028	0.077	0.133	0.040
R^2 between	0.088	0.176	0.099	0.029

Note: Additional control variables included, but not reported.

Table 8: Aid and Sovereign Defaults: Donor Specific Estimates

	(1) *CAN*	(2) *DEU*	(3) *FRA*	(4) *GBR*	(5) *ITA*	(6) *JPN*	(7) *USA*
Dependent variable: Gross Aid per capita							
a) Both default dummies							
Default	0.074	-0.104*	0.251***	-0.056	0.515***	0.276**	-0.042
	(1.03)	(-1.74)	(2.75)	(-0.33)	(3.46)	(1.99)	(-0.41)
Bilateral Default	-0.104	0.149**	-0.264**	0.148	-0.509***	-0.208	0.125
	(-1.16)	(2.09)	(-2.60)	(0.86)	(-3.04)	(-1.30)	(1.07)
b) Only unilateral default dummy							
Default	0.016	0.020	-0.001	0.065	0.088	0.131*	0.058
	(0.35)	(0.43)	(-0.03)	(0.93)	(1.00)	(1.81)	(0.96)
Specifications a) and b):							
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3019	3442	3313	3025	2923	3351	3013

Note: Additional control variables included, but not reported.

Appendix A. Data Sources and Definitions

Name	Source	Definition
Dependent variable		
Aid	Roodman (2011) and Heston et al. (2011)	Gross aid transfers (GAT) over recipient's population with GAT = Gross ODA − debt forgiveness grants − rescheduled debt.
Default variables		
Default	Rose (2005) and Paris Club (2011)	Default indicator. 1 whenever an aid recipient restructured its debt through the Paris Club.
Bilateral Default	Rose (2005) and Paris Club (2011)	Bilateral default indicator. 1 whenever an aid recipient defaulted on the debt owed to a specific donor.
Amount	Rose (2005), Paris Club (2011) and World Bank (2011)	Amount of rescheduled debt over recipient's GDP (both variables in current US$).

continued on next page

Appendix A. - continued

Name	Source	Definition
Control variables		
ln GDP pc	Heston et al. (2011)	Natural logarithm of the recipient's PPP converted GDP per capita.
ln Other Aid pc	Roodman (2011) and Heston et al. (2011)	Natural logarithm of Aid (see def. above) disbursed by other G7 donors.
Growth	Heston et al. (2011)	Growth rate of the recipient's GDP per capita.
Human Rights	Cornett et al. (2011)	Index of human rights violations based on US State Department human rights reports. Ranging from 1 -5 with higher values indicating more human insecurity.
Democracy	Polity IV (2009)	Policy score ranging from - 10 (strongly autocratic) to +10 (strongly democratic)
ln Trade	IMF (2011a) and World Bank (2011)	Natural logarithm of bilateral trade measured in percent of the donor's GDP.
UN Friend	Dreher and Sturm (2012)	Voting inline with donor in the UN General Assembly. Definition accoding to Kegley and Hook (1991)
ln Population	Heston et al. (2011)	Natural logarithm of the recipient's population.
Colony	CEPII (2011)	Dummy for common colonial past. 1 for pairs that were ever in a colonial relationship.
Additional variables for IV and selection models		
Joint Religion	Helpman et al. (2008)	Index for common religion. Higher values indicate more similar country pairs.
Common Language	CEPII (2011)	Dummy for common language. 1 if donor and recipient share the same official language
Budget Deficit	World Bank (2011)	Recipient's budget deficit (cash) in percent of GDP
Current Account	World Bank (2011)	Recipient's current account balance in percent of GDP
Inflation	World Bank (2011)	Recipient's CPI inflation rate

Appendix B. A Selection Model for Panel Data

The starting point for Wooldridge's (1995) selection model for panel data are equations 2 and 1, repeated here for convenience:

$$\ln\left(Aid_{ijt}\right) = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + c_{ij} + u_{ijt} \quad \text{and} \tag{1}$$

$$Aid^*_{ijt} = \boldsymbol{z_{ijt}\eta} + \zeta_{ij} + a_{ijt}; \quad Aid_{ijt} > 0\left[Aid^*_{ijt} > 0\right]. \tag{2}$$

As in every fixed effects model, ζ_{ij} might be correlated with $\boldsymbol{z_{ijt}}$. Furthermore, a_{ijt} is independent of $\boldsymbol{z_{ijt}}$ with $\mathrm{E}(a_{ijt}) = 0$ and (ζ_{ij}, a_{ijt}) is jointly normally distributed. Aid flows are positive $(Aid_{ijt} > 0)$ and $\ln\left(Aid_{ijt}\right)$ is defined if $Aid^*_{ijt} > 0$. The correlation between the unobservables in the participation $(\zeta_{ij} + a_{ijt})$ and allocation equation $(c_{ij} + u_{ijt})$ then introduces the selection problem. Wooldridge's (1995) solution for this problem rests on four assumptions:

1. The correlation between ζ_{ij} and $\boldsymbol{z_{ijt}}$ can be described by the equation

$$\zeta_{ij} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + c_{ij},$$

   where $\boldsymbol{\bar{z}_{ij}}$ denotes the time average of $\boldsymbol{z_{ijt}}$.[21] Equation 2 therefore simplifies to

$$Aid^*_{ijt} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + \boldsymbol{z_{ijt}\eta} + c_{ij} + a_{ijt}. \tag{4}$$

2. The new reduced form probit model has a random effects representation, i.e. $\nu_{ijt} = a_{ijt} + c_{ij}$ are independent of $\boldsymbol{z_{ij}} = (z_{ij1}, \ldots, z_{ijT})$. Furthermore, $\nu_{ijt} \sim \mathrm{No}(0, \sigma^2_t)$.

3. The two error terms of the participation (ν_{ijt}) and amount equation (u_{ijt}) are jointly normal distributed:

   $\mathrm{E}(u_{ijt}|\boldsymbol{z_{ij}}, \nu_{ijt}) = \mathrm{E}(u_{ijt}|\nu_{ijt}) = \rho_t\nu_{ijt}.$

4. The conditional expectation of c_{ij} is given by

   $\mathrm{E}(c_{ij}|\boldsymbol{z_{ij}}, \nu_{ijt}) = \boldsymbol{\bar{z}_{ij}\psi} + \phi_t\nu_{ijt}.$

[21] Wooldridge (1995) actually proposes to use all leads and lags of the explanatory variables in this equation. Replacing the non-contemporary values of these variables with their time averages has been suggested by Dustmann and Rochina-Barrachina (2007) and Wooldridge (2010).

These assumptions imply

$$\mathrm{E}\left[\ln\left(Aid_{ijt}\right)|\boldsymbol{z_{ij}}\right] = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + \boldsymbol{\bar{z}_{ij}\psi} + \kappa_t \nu_{ijt}$$

with $\kappa_t = \phi_t + \rho_t$. Conditioning on observations with positive aid flows we then get

$$\mathrm{E}\left[\ln\left(Aid_{ijt}\right)|\boldsymbol{z_{ij}}, Aid_{ijt} > 0\right] = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + \boldsymbol{\bar{z}_{ij}\psi} + \kappa_t \lambda\left(H_{ijt}\right) \tag{5}$$

where $H_{ijt} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + \boldsymbol{z_{ijt}\eta}$ denotes the index value from the selection equation and $\lambda(\cdot)$=$\phi(\cdot)/\Phi(\cdot)$ is the inverse Mills ratio.

We then estimate Equation 5 using a two step procedure:

1. Estimate Equation 4 using a pooled probit model and calculate the estimated values $\lambda\left(\hat{H}_{ijt}\right)$.
2. Estimate Equation 5 with pooled OLS using the estimates for $\lambda(\cdot)$ obtained in step 1. These are interacted with time dummies to account for the fact that their influence is not restricted to be constant across time.

As suggested by Wooldridge (2010), bootstrapped standard errors are used to account for cluster specific autocorrelation and the first sage estimation of $\lambda(\cdot)$.

VI. CONCLUSION

Advanced economies have long been thought immune against the perils of volatile capital flows and sovereign risk. The recent financial turmoil in peripheral European economies shattered this belief. Learning the right lessons from the experiences of emerging market economies which have repeatedly suffered from both sudden stops and sovereign debt crises has thus become even more important.

The four papers of this thesis attempt to contribute to this task. Their results are generally quite strong, partly going against conventional wisdom. For example, it is shown that past swings in private capital inflows were predominantly driven by "pull factors", i.e domestic developments. "Push factors" which are often pictured as culprits for boom-bust cycles were far less important by comparison. The conclusion that IMF interventions increased rather than decreased the risk of a sovereign default in the past might also come as a surprise.

The results of the two papers of this thesis that study the costs of sovereign defaults are perhaps the least controversial. The finding of varying costs for different default episodes, e.g., complements several theoretical studies that make this prediction. However, the extent of this heterogeneity in default costs has not yet been empirically documented. Finally, this thesis also rejects the hypothesis that donor countries punish defaults by withdrawing foreign aid contradicting an important assumption of two recent papers. The results are nevertheless well anchored in the economic literature as they support the common notion that development assistance is not solely determined by strategic concerns.

At the time of writing, whether and which of these results carry over to the recent European experiences remain open questions. The crises-stricken countries exhibit a much closer economic and financial proximity than any other group of countries. By sharing a common currency with a group of more stable economies they also lack the possibility to tailor monetary policy to their specific needs. These characteristics might well render common shocks and even contagious crises more likely. Hence, the "euro crisis" may turn out as one of the rare examples of synchronized retrenchments of capital inflows.

With regard to the determinants and consequences of sovereign defaults, the restructuring of Greece's debt has enriched the sample of default events by a single new data point. In face of the prolonged recession in Greece it seems safe to predict that a thorough analysis of this event will again confirm the notion that default decisions are costly. Given its status as advanced economy and, accordingly, as a donor country loosing access to foreign aid probably again did not contribute much to these costs. As the debt restructuring occurred despite an earlier IMF intervention – supported by further rescue funds from the European Union – Greece's default also provides another example of an unsuccessful rescue attempt. However, this obviously does not necessarily imply that the relationship between the IMF program and the subsequent debt restructuring has been causal. To make this claim, further analysis of the "euro crisis" is required that also takes new IMF loans to other crisis countries like Ireland and Portugal into account which have not yet been followed by a default.

Overall, the simultaneous crises in several European economies provide an ideal opportunity to test existing theories and reevaluate previous empirical findings. The theory and empirics of capital flows and sovereign debt are thus likely to remain at the frontier of economic research for some time to come.

VII. AFFIDAVIT

Ich erkläre hiermit, dass ich die vorgelegten und nachfolgend aufgelisteten Aufsätze selbstständig und nur mit den Hilfen angefertigt habe, die im jeweiligen Aufsatz angegeben oder zusätzlich in der nachfolgenden Liste aufgeführt sind. In der Zusammenarbeit mit den angeführten Koautoren war ich mindestens anteilig beteiligt. Bei den von mir durchgeführten und in den Aufsätzen erwähnten Untersuchungen habe ich die Grundsätze guter wissenschaftlicher Praxis, wie sie in der Satzung der Justus-Liebig-Universität Gießen zur Sicherung guter wissenschaftlicher Praxis niedergelegt sind, eingehalten.

Markus Jorra

Markus Jorra, Giessen, den 15.5.2012

SUBMITTED PAPERS

Förster, M., Jorra, M. and Tillmann, P. (2012). *The dynamics of international capital flows: results from a dynamic hierarchical factor model.* MAGKS Discussion Paper 21-2012, University of Giessen.

Jorra, M. (2012). The effect of IMF lending on the probability of sovereign debt crises. *Journal of International Money and Finance* 31 (4), 709-725.

Jorra, M. (2011). *The heterogeneity of default costs: evidence from recent sovereign debt crises.* MAGKS Discussion Paper 51-2011, University of Giessen.

Brandt, J. and Jorra, M. (2012). *Aid withdrawal as punishment for defaulting sovereigns? An empirical analysis.* MAGKS Discussion Paper 20-2012, University of Giessen.