(At Least) Four Theories for Sovereign Default: An Empirical Evaluation

Markus Eberhardt*

School of Economics, University of Nottingham, UK
Centre for Economic Policy Research, UK

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Abstract: Why do some sovereigns repay their debts while others default? I empirically investigate four prominent theories for default (or its avoidance): (i) reputation, (ii) punishment, (iii) domestic politics, and (iv) international spillovers. Adopting an early warning system (EWS) approach for a large sample of developing and emerging economies (1970-2015) I find that reputation and spillover effects dominate in terms of economic significance. In robustness checks I allow for the transmission of each theory strand through macro-fundamentals, account for capital controls, debt relief, and capital flow bonanzas, investigate domestic, private and present-value external debt, and conduct sample splitting exercises (by exchange rate arrangement, political regime, financial development, and time period). Though they provide more refined insights into the differential mechanisms at work, none of these exercises substantially alter the above conclusions.

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1 Introduction

Are sovereign defaults accidents brought about by precarious macroeconomic fundamentals and triggered by unanticipated shocks, or do governments chose to default as the result of strategic calculation? Are sovereigns kept in check by threats and implicit gunboat tactics, by concerns over their reputation and future lending conditions, or by democratically elected governments worried about the next (opinion) polls? What about sovereign default in a globalised world, where macroeconomic vulnerability may spill across economies, or where the presence of common lenders entices sovereigns to default and renegotiate when they observe other economies doing so? No doubt *multiple* factors such as these are at play, but which are the empirically dominant mechanisms?

The existing empirical literature on sovereign default and bond spreads with reference to emerging and developing economies is very rich, but typically focuses on a relatively small set of factors related to macroeconomic fundamentals (Manasse and Roubini, 2009), foreign liabilities (Catão and Milesi-Ferretti, 2014), concerns over loss of reputation (Catão and Mano, 2017), political regime and institutions (Archer et al., 2007; Van Rijckeghem and Weder, 2009; Beaulieu et al., 2012), or economic reprisals (Rose, 2005; Fuentes and Saravia, 2010) among others. Even measures for the two dominant theoretical explanations in the economics literature (reputation and punishment, see Bulow and Rogoff, 2015) are rarely entered into the same empirical model to gauge their comparative explanatory power.[1]

In this study I use an 'early warning system' approach to empirically investigate four theories for sovereign default or its avoidance: (i) concerns over a sovereign's international reputation, i.e. being branded as a 'serial defaulter,' with implications for future lending conditions (Eaton and Gersovitz, 1981; Tomz, 2007); (ii) the deterrence effect of external threats, sanctions and other forms of reprisal (Bulow and Rogoff, 1988, 1989); (iii) the domestic political system: differential behaviour of autocracies versus democracies, and within the latter the impact of change in political stability (Archer et al., 2007; Beaulieu et al., 2012); and (iv) a 'domino effect' of international crisis spillovers with default either an inevitability or a strategic choice (Elliott et al., 2014; Acemoglu et al., 2015; Arellano et al., 2017).

These four strands comprise the two explanations dominating the economics literature (reputation and punishment), one favoured in political science (political system), and a more recent explanation reflecting on the embeddedness of sovereign lending in an international network of lenders and borrowers (spillovers).[2] In addition to these four strands I include macroeconomic fundamentals (e.g. debt stock and debt service flows, inflation, foreign reserves) and

[1] A notable exception is Benczúr and Ilut (2016).

[2] It should be noted that Bulow and Rogoff (1988) already consider a broader *international relations* framework which leads to enforcement mechanisms other than the direct punishment approach. See Section 2.4 for other early investigations of spillovers.

consider a large number of additional factors (e.g. conflict, financial crises, exchange rate arrangements). As will become clearer when I motivate robustness checks and alternative data series employed, there are a great many more theories of default and its avoidance underpinning these additional analyses, and I attempt to do them justice while maintaining the focus on the four considered most prominently by the existing literature.

I carry out this investigation with a common empirical framework so as to compare and contrast the salience and (in the statistical sense) power of the above factors in predicting sovereign defaults. After introducing each theory I operationalise them for empirical investigation alongside standard macroeconomic controls in a panel of up to 114 developing countries for 1970 to 2015. I adopt and extend existing empirical approaches from the bond spreads and financial crisis literatures to proxy reputation, political economy and spillover effects, while introducing a number of novel empirical measures for the threat of direct punishment, centred around the evolution of trade costs and of membership in regional trade agreements.

My empirical implementation, a Mundlak-Chamberlain extension to the random effects logit model (recently applied in Caballero, 2016, for the analysis of banking crises), also known as a 'correlated random effects' model, allows me to predict sovereign default events while keeping observations from countries which *never* defaulted in the sample. This is in contrast to the standard conditional logit model widely applied in empirical crisis prediction (e.g. Kaminsky and Vega-Garcia, 2016): excluding non-crisis countries is an awkward starting point (clearly subject to concerns over sample selection) if the aim of the exercise is to quantify the effects of different variables on the *propensity* of default.[3]

Critics will be swift to point out that treating a set of macroeconomic fundamentals as seemingly unconnected to the alternative theories for default is naïve and likely a source of bias. Furthermore, reputation, threats and incentives, domestic politics, and spillovers may affect *all* crisis predictors in some countries but not in others, with a pooled analysis merely including these measures as covariates likely to miss these heterogeneities. Alternatively, jointly controlling for factors like political regime and reputation may be introducing a 'bad control', where one factor is causally downstream from another and hence blocking their direct effect on default. This aside, the recent literature on financial crises more generally has pointed to a number of 'external' variables related to global commodity price fluctuations and capital inflows, among others, which arguably have a significant bearing on the vulnerability to debt crises as well (e.g. Caballero, 2016; Eberhardt and Presbitero, 2021). Finally, measures of public indebtedness are likely misleading if they do not account for domestic debt or private debt or

[3]Does this empirical choice matter? I find that a number of covariates are statistically insignificant in the smaller defaulter-only sample. Comparing effect sizes (marginal effect of a 1sd increase in x relative to the unconditional default probability in the full and defaulters-only samples, respectively) results differ substantially for reputation, punishment, and political effects, as well as macro-fundamentals, but *not* for international spillovers. *Relative* effect size between full and defaulters-only samples vary between 33% and 240% for individual covariates.

if external debt is measured at face value.

I attempt to address these concerns by (i) allowing for interaction effects between some macroeconomic fundamentals and measures for the four default theories;[4] (ii) analysing the changes in results when selective strands are omitted (to ward against bad controls) while also studying minimalist specifications to establish whether plausible proxies offer any predictive power on their own; (iii) conducting a number of sample splitting exercises based on a country's political regime, exchange rate arrangement, level of financial development, as well as by time period; (iv) adding additional controls to the benchmark model to capture changes in capital controls, commodity price movements, the HIPC (highly indebted poor countries) debt relief initiative, hyper-inflation, or capital inflow bonanzas; and (v) conducting my analysis with data for total debt (Abbas et al., 2010), private debt (Mbaye et al., 2018), and external debt stock in present value terms (Dias et al., 2014), respectively.

My findings for each theory strand in isolation suggest that reputation, threats and incentives, domestic politics, and spillovers can play an important role in (self-)disciplining governments. When I analyse these strands in an early warning system approach alongside macroeconomic fundamentals I find that reputation as well as spillover effects dominate in terms of economic magnitudes, followed by a number of macroeconomic fundamentals. Empirical evidence for domestic politics and threats/incentives is weaker.[5] These findings are robust to a battery of additional controls, as well as the use of alternative measures for indebtedness. While much of the economics and political science literature has focused on its own preferred explanations — reputation and punishment for the former, (domestic) politics in the latter — it would appear that the relatively less appreciated fourth theory strand on spillovers is of great significance and thus warrants the recent focus in theoretical work (Cole et al., 2016; Arellano et al., 2017).

The remainder of the paper is organised as follows: in Section 2 I review the literature on the four theories for sovereign default. In Section 3 I introduce the data and carry out basic descriptive analysis. The empirical model and implementation are introduced in Section 4. Results are presented and discussed in Section 5, Section 6 concludes.

[4]This setup can for instance account for a situation where 'bad reputation' or 'direct punishment' work through restricted access to international financial markets, thus affecting debt stock.

[5]I cannot rule out that these aspects are adversely affected by the inclusion of the spillover variables, which may wash out subtler effects. However, alternative models adopting a split by political regime, in contrast to the evidence for splits by exchange rate regimes or level of financial development, do not show any significant differences in crisis determinants between autocracies and democracies.

2 Four Theories of Sovereign Default

Sovereign lending is conducted in an environment of limited enforcement: given the very nature of sovereignty, creditors are limited in their ability to seize assets inside a debtor country when the latter defaults on a loan. Sovereign default has been taking place for centuries, but to date no dominant "clear and coherent answer" (Bulow and Rogoff, 2015) to the question what foreign creditors can do when sovereigns default — and relatedly, why some countries default and others do not — has emerged. At least four broad strands of explanations have been proposed in the literature, reviewed in turn below, followed by variations and 'other' determinants.

2.1 International Reputation: 'Serial Defaulters'

This strand of theory is most closely associated with the seminal paper by Eaton and Gersovitz (1981). Sovereign borrowing and repayment is *only* related to consumption smoothing (the mechanism) and concerns over reputation (the motivation), whereas external threats and the governing law of the debt is irrelevant. This setup also implies that political regime and institutions do not play any role in the model. As a result, debtors repay their debt in good times and scramble to borrow more in bad times; default only occurs in good times, since the consumption smoothing mechanism of sovereign lending necessitates continued borrowing in 'bad states' — this theoretical prediction makes for a nice link with the empirical question about 'default in bad times' raised by Tomz and Wright (2007) among others.

Mike Tomz' (2007) book *Reputation and International Cooperation* provides a detailed motivation for a variation on the reputation theme, categorising borrowers into 'stalwarts' (goodie two-shoes), 'fair-weather' (repay in good times) and 'lemons' (default in bad times and occasionally in good times as well). On the basis of observed actions creditors may form and update beliefs about a sovereign's default probability, which constitutes the latter's reputation. Types and hence reputations are not fixed and Tomz (2007) acknowledges that domestic politics may change priorities and thus the perceived importance of reputation. The rich empirical evidence he presents in support of the reputation theory (and against the punishment theory) is based on historical subperiods and geographical subsamples dealing with each motivation to repay *separately* — Tomz (2007) is thus unable to empirically test both theories jointly.

Alfaro and Kanczuk (2005) provide a theoretical model in which *some* equilibria imply countries delay default (they 'muddle through') due to concerns over their reputation: a 'good' government can signal its type by enduring a recession and thus securing favourable conditions for future lending.

Gelos et al. (2011) empirically study access to international credit markets and find little

evidence for detrimental reputation effects from default frequency, provided renegotiations are completed relatively swiftly. Catão and Mano (2017) study the 'default premium puzzle,' the phenomenon that countries with a history of, and hence a reputation for, default only seem to pay small interest rate premia for subsequent borrowing. They construct a number of 'memory variables' which capture the historical prevalence and recency of default for each country and encompass the existing metrics for default premium estimation. Equipped with these proxies they are able to establish a substantial reputational default premium for historical and modern-day samples (250 and 350 basis points in the first years of market re-entry, respectively).

2.2 Default and (the Threat of) Direct Punishment

This strand is closely associated with the theoretical work by Bulow and Rogoff (1988, 1989). Here, creditors have rights in foreign creditor-country courts, i.e. if country A borrows from bank i in country B, defaults on i's loan and proceeds to borrow from bank j in the same country, then bank i can stake a claim on being repaid from the loan by bank j (Bulow and Rogoff, 2015). This incentivises the sovereign to repay old debts before borrowing again. Creditors are thus afforded legal rights to interfere in the commercial dealings of the borrowers, e.g. by sanctioning trade outright, seizing shipments, or creating serious trade frictions by regulatory means.

Jayachandran and Kremer (2006) highlight the inefficiency of trade sanctions (due to incentives for evasion, self-harm to the creditor, harm to the target country's population rather than its politicians) and with an eye on the 'regime type' explanation discussed below suggest a mechanism of 'loan sanctions' to break a spiralling 'odious debt' cycle by a repressive regime. Bulow and Rogoff (2015) however argue that the potential for self-harm and other considerations are "precisely the kind of issue that a bargaining theoretic framework [like theirs] allows one to approach."

The analysis in Tomz (2007) offers a variety of arguments and empirical evidence *against* the direct punishment explanation — most notably his use of text search to show that any mention of 'sanctions' is virtually absent in tens of thousands of newspaper clippings compiled by the *British Corporation for Foreign Bondholders* for the period of the 'First Globalisation' (1870-1914). This and other findings make it somewhat harder to argue the direct punishment case. Detailed historical analysis for the same period in Mitchener and Weidenmier (2010) however suggests that extreme sanctions were far from isolated incidents (concluding a one in three chance of 'supersanctions') and that these measures disciplined defaulters effectively.

In modern times the notion of explicit trade sanctions in response to sovereign default seems — at least in the period prior to the Trump presidency — somewhat far-fetched. However, while sanctions may not be explicit, a focus on outcomes (trade, FDI flows) can ignore the

rhetoric of diplomacy (a criticism raised against Tomz' (2007) text search) and provide insights into what direct consequences default has had for all parties involved. While the self-harm argument against sanctions is intuitive, it nevertheless appears that firms in creditor economies have reduced or delayed their commercial activities with/in defaulter economies: Rose (2005) suggests a significant decline in trade between creditor and debtor economies when renegotiating sovereign debt. Fuentes and Saravia (2010) study consequences of default for FDI flows and find a significant decline in investment flows from creditor nations after default. It does however remain unclear *why* these changes in trade and FDI patterns occurred: Martinez and Sandleris (2011), for instance, find that the 'trade punishment' effect is stronger for non-creditors. Borensztein and Panizza (2009) argue that one potential channel through which default could affect creditor trade is via the deterioration in trade credit terms and they find empirical evidence to that end.

2.3 Political Economy: Regime Type and Domestic Politics

The 'regime type' argument has its origins in the work of North and Weingast (1989) and was further developed in Schultz and Weingast (2003): countries with constitutions which limit executive discretion should see a 'democratic advantage' in terms of their credibility for debt repayment and thus also in the spreads and official bond ratings they receive (Archer et al., 2007). Since such constitutional aspects are typically subject to little change over time the question of a 'democratic advantage' is at times viewed as an aspect of institutional quality, with serial default tied to poor institutions (Van Rijckeghem and Weder, 2009). The proposed mechanism implicitly assumes that domestic voters — the actions of autocrats are less likely to be driven by concern over the views of the populace — care about commitments to international agreements such as sovereign loan arrangements and would remove leaders if these agreements were broken. As Tomz (2002) convincingly argues, this assumption is weak as it presumes that voters understand complex agreements, care enough about them to sway their vote, and that compliance is always favoured by voters on the grounds of personal and/or national interest.

The 'democratic advantage' in sovereign lending has greatly occupied the political science literature (Archer et al., 2007; Van Rijckeghem and Weder, 2009; Beaulieu et al., 2012). Beaulieu et al. (2012) argue that the manifestation of a 'democratic advantage' is closely linked to a selection problem, whereby autocratic regimes do not enter international bond markets because they know it is futile since nobody will lend to them: the democractic advantage is primarily driven by *access* to credit, rather than better credit *conditions*.[6] Scholl (2017) argues for dynamic interaction of sovereign default risk and political turnover (i.e. in her model po-

[6]The statistical selection problem comes to bear in empirical studies which only consider sovereigns which have received credit ratings in the past, thus assuming that entry into bond markets is ignorable.

litical turnover is endogenous, not some random switch). The basic narrative here is that the incumbent (democratic) government accumulates debt to win over the electorate. This line of reasoning points to the study by Herrera et al. (2020) to argue that political booms (increased popularity of the incumbent government) are important predictors of financial crises. The political stability argument is also taken up in Hatchondo and Martinez (2010, and references therein). Borensztein and Panizza (2009, Table 10) provide evidence for rather stark implications of defaults on the fate of incumbent democratic governments: of the 19 events between 1980 and 2003 for which electoral results are available prior to and in the aftermath of external default, the ruling coalitions lost votes in all but one case, with the median decline 52% of the pre-default vote-share. This provides a very strong political motivation to avoid default.

2.4 The World as a Network: Spillovers[7]

Theoretical foundations for this approach can be provided by Elliott et al. (2014), who model the interdependence of financial institutions in its impact on 'cascades of failures' (spillovers). They find non-monotonic effects of integration (greater dependence) and diversification (more network members) on the extent of cascades at different points in time of the network genesis. Acemoglu et al. (2015) also study the extent of financial spillovers but argue for a differential effect of small versus large shocks to a financial network: if shocks are small, a denser network will enhance stability, but when they are larger dense interconnections act to propagate these shocks, thus creating a more fragile system. This would imply that the limited financial development of developing countries may have positive effects (a form of 'de-coupling') in times of global upheaval (e.g. during the 1970s oil crises or the recent global financial crisis) but comparatively detrimental effects in 'calmer' periods. Countries may thus default because they are helpless to avoid the spillover effects of debt crises in 'connected' economies, but they may also avoid default if a shock they experienced can be smoothed out via the membership in a dense financial network.

Cole et al. (2016) study what could be termed 'information cascades' in the context of bond yields, whereby changes in the risk profile of one sovereign can lead investors to research alternatives, with repercussions for seemingly unrelated economies and the pricing of default risk globally. This channel can help explain sudden fluctuations in risk premia for countries without obvious changes in their fundamentals. Again, this model thinks of spillovers as happening to an unsuspecting and helpless sovereign.

[7] In this paper I follow the definition of Kaminsky et al. (2003) who distinguish 'fast and furious' contagion from more gradual effects which they term 'spillovers', although the two may be difficult to disentangle empirically. Including a large number of controls for domestic and external phenomena (e.g. recessions, commodity price movements, capital inflow bonanzas) can go some way of capturing common shocks, so that there is a stronger case that the dedicated variables for spillovers capture the propagation of shocks *across* countries.

Reinhart and Rogoff (2009, Chapter 5) provide a host of motivations for the significance of spillovers for sovereign default in the context of banking crises in advanced countries: the resulting weakening in global growth can manifest itself in reduced demand for exports (less forex for developing countries), a decline in commodity prices (dto), 'sudden stops' of financial flows, along with the systemic effect of investors reducing their exposure to risk as described in the previous paragraph.[8] Although strategic behaviour is always considered relevant by these authors, the channels described appear to affect an unwitting sovereign in the periphery caught up with the financial turmoil in the economic core.

Arellano et al. (2017) instead argue that the presence of common lenders lead countries to act *strategically*, defaulting when they observe a foreign default so as to be given the opportunity to renegotiate sovereign debt simultaneously and thus pay lower recoveries while otherwise facing more expensive new borrowing. In contrast to the inevitability of default through the 'cascades of failures' here sovereigns act on strategic motives to improve their overall external position. A simple empirical illustration shows strong positive (negative) correlation between domestic default and the global share of countries in default (renegotiating) as predicted by their model.

Kaminsky and Vega-Garcia (2016) remark on the empirical regularity of debt crises 'clustering' at certain periods of time[9] and note that existing theoretical models typically only include *country*-specific factors, thus failing to address this 'systemic' aspect of sovereign default. Empirical investigation for Latin America in the 19th and early 20th centuries demonstrates that crises in the financial centres can have a strong adverse effects on access to finance, economic performance and ultimately the ability to avoid defaults in the periphery. According to their classification almost two thirds of Latin American debt crises during this period are of a systemic nature.

Benczúr and Ilut (2016) augment their empirical model of bond spreads with a variable capturing a regional spillover effect in their analysis of 37 developing economies. This measure, based on the share of countries in arrears within a geographic region, turns out to be insignificant in specifications including country fixed effects.

2.5 Other Factors For Sovereign Default

Boz (2011) and Dellas and Niepelt (2016) highlight the impact of 'heterogeneous creditors,' i.e. International Finance Institutions (IFIs) and private lenders, with the former more enforce-

[8] Some of these phenomena are at times described as 'external economy' variables of default prediction and as far as they are quantifiable (e.g. commodity price booms or capital inflow bonanzas) are not typically regarded as spillovers.

[9] In their analysis of financial crises over two centuries Reinhart and Rogoff (2009) repeatedly highlight the clustering of (banking, currency, debt, inflation) crisis events at specific points in time. Sturzenegger and Zettelmeyer (2007) note that defaults typically cluster at certain periods in the aftermath of lending booms.

able, leading to smaller, countercyclical borrowing. Shea and Poast (2018) study the relationship between war and default, which is weak since the likelihood of default predetermines the decision to avoid war. Levy-Yeyati and Panizza (2011) suggest that defaults can mark the 'beginning of economic recovery'. They argue that the observed output contraction prior to defaults (detectable with higher frequency data) points to an anticipation effect, rather than poor economic fundamentals. Government guarantees of its national (private) financial sector can provide the link between credit booms, banking crises and sovereign default (Diaz-Alejandro, 1985; Reinhart and Rogoff, 2011b). Foreign exchange reserves have been recognised in the literature for their function as a precautionary device (Catão and Milesi-Ferretti, 2014). Since developing and emerging market debt is denominated in *foreign* currency (contrary to US, UK and Japanese debt), sovereigns cannot inflate away the real burden of nominal debt. Capital flow bonanzas coming to an end can trigger sovereign defaults, especially in combination with booming commodity prices (a 'double bonanza:' Reinhart et al., 2016) — see also Eichengreen and Portes (1986) on terms of trade. Finally, a sizeable literature studies twin or triple crises (Reinhart and Rogoff, 2009, 2011b) which seem to appear with great regularity — the inclusion of recent banking and currency crises is common in the literature.

3 Data and Descriptive Analysis

3.1 Sample

My sample covers up to 114 developing economies for the period 1970-2015 (raw data prior to variable transformations; unbalanced panel). These economies experienced 174 debt crises, but due to data coverage for other variables the regression samples only capture at most 171 of these — for the same reason the empirical analysis is conducted for a sample of 102 countries which experienced 141 crises. Details on sample makeup and descriptive statistics are relegated to an Appendix. Figure 1 gives an idea of the distribution of crisis events and the distinction between S&P-defined default and large IMF financing (in the upper panel), as well as of the share of sample countries in default (left axis) along with the sample evolution over time (in the lower panel) — in both cases I highlight the differences between the S&P definition of default and that of large official financing.

Figure 2 gives some insights into the evolution of median debt stocks (official, private) and debt service flows (relative to GNI) over time. Official debt stock (solid blue line, right scale) appears hump-shaped with a maximum in the early eighties, though followed by a persistently high level for a period of almost 20 years. From 2007 onwards *levels* of official debt were substantially reduced to pre-1978 oil crisis magnitudes (G8 multilateral debt relief initiative in 2005), and have remained relatively stable since. Debt stocks from private lenders (yel-

low line) are most significant in the late 1970s/early 1980s, and have recently made a comeback. The median of debt service *flows* (dashed black line, left scale) largely shares the evolution of the official debt stock, though it has a more pronounced early peak in 1988 and a second, albeit much lower, peak in the early noughties.

3.2 Variable Construction

In the following I describe the dependent and independent variables in broad brushes — a detailed discussion of variable construction and sources is contained in Appendix A. Appendix Figure D-1 shows the results of an event analysis study for a number of candidate predictors of sovereign default.

My dependent variable is an indicator for the first year of default, adopting the Standard & Poor's definition of a credit event (about 1/3 of events, largely taken from Reinhart and Rogoff, 2009) and/or that of exceptionally large financing from the IMF (Medas et al., 2018).[10]

I construct the reputation proxies based on the memory variables in Catão and Mano (2017): the rolling share of years spent in default, using 1950 as a starting point (bad reputation); and the rolling count of years since the last default (also, variations with different discounting).

The direct punishment variables are intended to capture the exposure of sovereigns to the potential for trade sanctions (whether explicit or implicit) and with this the incentives for sovereigns to avoid or defer default.[11] I hypothesise that *lower* membership in formal trade networks (especially those of economic significance as evidenced by actual trade flows) deters sovereigns from entering into default. I construct RTA (regional trade agreements) counts and trade-weighted RTA counts based on the Regional Trade Agreements Database (Egger and Larch, 2008). I further hypothesize that a sovereign with *higher* trade costs will fear trade-related reprisals and thus have incentives not to default. I compute trade costs following the methodology introduced in Novy (2013) and Milner and McGowan (2013) using bilateral trade data from IMF DOTS and GDP data from the World Bank WDI. One variant here is to focus on trade costs with major creditors (France, the US, Germany, the UK, and Italy). All of the trade cost measures are *relative* measures, i.e. they benchmark the trade costs in country i against the *average* trade costs in all 114 countries, to highlight deviations.

Measures related to domestic politics are taken from Polity IV: the revised polity score (ranging from -10 to $+10$), and regime durability (in years). I also construct a democracy dummy for Polity IV scores above zero. From POLCON I use polcon III and polcon V which measure the feasibility of policy change. Following Herrera et al. (2020) I adopt 'government

[10] Following the standard in this literature (e.g. Catão and Milesi-Ferretti, 2014) the observations for countries *in default* following the first crisis year are omitted.

[11] Trade of debtor countries is known to decline after default (Rose, 2005), especially so with debtor countries; in the present dataset merchandise trade drops by around 1-2% following default.

stability' from ICRG.

I pursue a number of strategies to capture international spillovers: following Arellano et al. (2017) I construct the share of sample countries currently in default (excluding country i) and the alternative 'number of defaults elsewhere.' Inspired by a recent study on banking crises investigating the importance of *foreign* credit booms for *domestic* busts (Cesa-Bianchi et al., 2010) I compute the cross-section averages of macro-fundamentals (debt stock and service flow, inflation, reserve ratio, real growth) and include these alongside their country-specific versions. In alternative specifications these cross-section averages are interacted with time-invariant (base year) measures of trade openness (imports plus exports divided by GDP, taken from the WDI) or financial development (proxied by bank credit to bank deposits from the World Bank *Global Financial Development Database* (GFDD). The latter is motivated by the recent literature on 'too much finance' and the potential trade-off between growth and crisis vulnerability. I provide an *econometric* rationale for this setup in Section 4 below. I further construct these 'spillover' variables for OECD countries, which are *not* part of my sample, capturing the 'core-periphery' notion of Kaminsky and Vega-Garcia (2016).

A great deal of other variables have been suggested in the existing literature. Measures of debt stock and debt service flow are quite common, and I adopt the external debt to GNI measures (stock, service flows) from GFDD — these are also available split into private and official creditors. In robustness checks I adopt total public debt and the external/total debt ratio from Catão and Mano (2017) to follow up on the insights from recent work on domestic debt (Panizza, 2008; Reinhart and Rogoff, 2011a); private and public debt from the 2018 GDD (Mbaye et al., 2018); and the present-value external debt data from Dias et al. (2014).

Additional macroeconomic fundamentals include real GDP growth from the WDI, inflation (I adopt the GDP deflator from WDI for improved coverage), the foreign reserves to GDP ratio from Catão and Mano (2017) augmented with data from the WDI, the current account balance as a measure of external balance and the terms of trade gap (all taken from the same study). The 10-year US Treasury constant maturity rate to capture the state of the global economic environment is taken from FRED. In robustness checks I use an update to the Chinn and Ito (2006) capital account openness index (standardized), as well as country-specific commodity price growth and volatility measures adopted from Eberhardt and Presbitero (2021); capital flow bonanzas are constructed following Caballero (2016) using net capital inflows as a share of GDP from the IMF Financial Flows Analytics database.

I use data from Ilzetzki et al. (2019) to classify countries with fixed exchange rate regimes as well as a finer disaggregation. Dummy variables for banking and currency crises (start year) come from a 2017 update to Laeven and Valencia (2013). From Marshall (2017) I take 'major episodes of political violence' involving at least 500 deaths.

The above empirical measures treat the manifestation of each theoretical strand as separate from macroeconomic fundamentals. As the discussion in Panizza (2013) suggests, this separation is clearly not always warranted, given that reputational motives, direct punishment/incentives, domestic politics, and also spillovers affect the 'choice' and thus evolution of the level of public indebtedness and foreign exchange reserves, among others: bad reputation or punishment may prevent a country's access to international financial markets, thus directly affecting the public debt to GDP ratio. I try to capture these channels by interacting some of the above 'theory strand' variables with the two most relevant macro fundamentals 'controlled' by the domestic government: the external debt to GDP ratio and the foreign reserve ratio.

3.3 Variable Transformations

Outliers can at times drive empirical results, especially when analysing macro variables such as inflation or indebtedness which cover a vast range of values. I therefore follow standard empirical practice and wisorise the top and bottom one percent of observations for each continuous variable, respectively.

One important aspect of the empirical modelling of default and more generally financial crises is how to take account of the pre-crisis dynamics of macroeconomic variables in the construction of an 'early warning' approach. In this context, the standard practice in the literature is to lag the regressors, typically by just a single time period (e.g. Catão and Mano, 2017). This choice seems somewhat *ad hoc* and may fail to adequately capture the prevailing dynamics in the run-up of a crisis. In their seminal contribution on *banking* crises Schularick and Taylor (2012) employ lag polynomials of length five in their analysis of advanced economies over a 140-year time period. Given the comparatively short time series dimension of my panel data (on average 23 years) along with the large number of candidate determinants included in the model, I adopt moving averages to capture pre-crisis dynamics, as practiced by Reinhart and Rogoff (2011b) and Jordà et al. (2011, 2016) — based on the below event analysis I select an MA(3) process, for $t-1$ to $t-3$. I further present results for a single lag as well as MA(2) to MA(5) transformations for robustness.

4 Empirical Model and Implementation

I follow the majority of studies in the financial crisis literature and estimate a latent crisis model, where the observed variable (the debt crisis) is a realized event when the latent variable exceeds some threshold. I code the crisis variable as equal to one in the year the debt crisis starts, and zero otherwise.

A key issue to confront in order to obtain robust estimates is unobserved cross-country

heterogeneity. I adopt an empirical implementation which deals with this issue by allowing for country-specific fixed effects, which give all coefficients the interpretation of 'within' country estimates and thus come closer to a plausibly causal interpretation of the results, but at the same time are not subject to the incidental parameter problem. One disadvantage of the standard practice in the literature, where fixed effects are simply included in a pooled logit or probit model (e.g. Gelos et al., 2011; Catão and Milesi-Ferretti, 2014), is that the regression sample is limited to those countries which experienced a crisis *at one point* during the sample period. Since this implementation excludes countries which avoided crises it may run the risk of distorting the results due to a sample selection effect.[12]

To overcome this limitation, I follow the practice in Caballero (2016), who adopts a well-established empirical approach to get around the incidental parameter problem in nonlinear models, dating back to Mundlak (1978) and a generalisation by Chamberlain (1982). The implementation (henceforth RE-Mundlak Logit) builds on a random effects logit model, where the strong assumption of no correlation between the individual (in my case country-specific) effects and the covariates can be relaxed by separately including the country-specific means of each variable. Further, the statistical significance of accounting for country-specific effects can easily be tested. The empirical setup is based on a binary choice model:

$$Y_{it}^{*} = \boldsymbol{\alpha}_i' \boldsymbol{d}_t + \boldsymbol{\beta}' \boldsymbol{X}_{it} + e_{it}, \tag{1}$$

where Y_{it}^{*} is a latent variable relating to the observed response variable, Y_{it} (the default start year), via the indicator function Y_{it}=$\mathbf{1}(Y_{it}^{*})$. Thus $Y_{it} = 1$ if $Y_{it}^{*} > 0$ and zero otherwise. d_t may include observed common factors and country fixed effects (when $d_t = 1\,\forall t$).

The estimated model simply adds the within-country averages of all time-varying variables, $\widetilde{\boldsymbol{X}}$, to the random effects logit model to capture the between-country variability in the propensity of default, which can be argued to arise from factors related to (colonial) history, geographic location and relatedly natural resource environment, as well as legal system and other determinants. If the true model does contain such individual effects and these are correlated with the other covariates in the model, the resulting coefficients of interest $\hat{\boldsymbol{\beta}}$ are asymptotically biased if these 'fixed effects' are ignored by the econometrician. Standard errors are clustered at the country-level in the raw logit regressions, and computed from these via the Delta method in the marginal effects.

The inclusion of cross-section averages in the empirical model (following Cesa-Bianchi et al., 2010) to capture spillovers can be given a more formal justification from the macro panel

[12] In Appendix H I present the results for such a specification (logit with country fixed effects). Estimated marginal effects follow quite similar patterns of signs and statistical significance, although coefficient estimates are typically inflated whle external debt plays a greater role in these specifications.

econometric literature. A recent contribution by Boneva and Linton (2017) adopts the common factor error structure from the *linear* macro panel literature (see Chudik and Pesaran, 2015, for a detailed survey) and extends this concept to the *nonlinear* panel setup. With reference to equation (1) above they specify:

$$e_{it} = \boldsymbol{\lambda}_i' \boldsymbol{f}_t + \varepsilon_{it}, \tag{2}$$

where $\boldsymbol{f}$ is a set of unobserved common factors, the $\boldsymbol{\lambda}_i$ are associated unknown factor loadings and ε is white noise. These factors are further assumed correlated with the independent variables $\boldsymbol{X}_{it}$ in (1), such that their omission induces omitted variable bias in the estimates of $\boldsymbol{\beta}$. A widely-cited paper by Pesaran (2006) establishes for the linear model that the unobserved common factors can be proxied by the inclusion of the cross-section averages of all dependent and independent variables, while the country-specific factor loadings are captured by estimating the model separately for each country, or by interacting each cross-section average with N country dummies (see Eberhardt and Presbitero, 2015, for a recent empirical application). The contribution by Boneva and Linton (2017) establishes that this idea extends to nonlinear models where the cross-section averages are based only on the independent variables, and where the model in (1) augmented with cross-section averages is estimated separately for each country.

This 'common correlated effects' estimator is however not feasible in the present panel of relatively modest time series dimension. The elaborate econometric excursion above can however give some insights into the assumptions made (and relaxed) when cross-section averages (CA) are included in a pooled logit model as is suggested here: if the CA are simply added to the model, we are implicitly assuming that the factor loadings are *identical* across countries, hence $e_{it} = \boldsymbol{\lambda}' \boldsymbol{f}_t + \varepsilon_{it}$. This is a strong assumption. Since the $\boldsymbol{\lambda}_i$ parameters cannot be freely estimated in my model I experiment with two forms of *country-specific* spillover variables by interacting the cross-section averages with time-invariant measures for trade openness and financial development, respectively.[13] These interactions relax the assumption that $\boldsymbol{\lambda}_i = \boldsymbol{\lambda}$ and mimic the country-specific nature of the factor loadings with country-specific values: $e_{it} = \boldsymbol{\lambda}_i^{*\prime} \boldsymbol{f}_t + \varepsilon_{it}$. At the same time they restrict the 'spillover channels' to be proxied by a country's openness to trade or level of financial development.

Finally, in my empirical analysis I use the Receiver Operating Characteristic (ROC) curve along with the associated AUROC (area under the ROC curve) statistic, which has become a prominent feature of the empirical literature on financial crises (see Jordà et al., 2011; Schularick and Taylor, 2012), to study the predictive power of the model. A higher AUROC statistic (bounded between 0 and 1) indicates better predictive power (a value of 0.5 represents the

[13] This is motivated by existing work on crisis propagation and international spillovers (Glick and Rose, 1999; Van Rijckeghem and Weder, 2001). I use country-specific averages for openness and findev as weights following Ciccone (2018).

benchmark for any informative model, where predictive power is equivalent to the flip of a coin), and statistical tests to compare the predictive power of different models can be constructed given the availability of AUROC standard errors.

5 Results and Discussion

5.1 Results by Theory Strand

The results for separate regressions by theory strand are discussed in details in an Appendix section. In all cases candidate variables for each strand are tested for predictive power in a benchmark model of macro-economic determinants of default. The selection of the latter is also discussed in the Appendix.

5.2 Main Results

In Table 1 I bring together results from models including the empirical interpretations of the four theory strands alongside macroeconomic fundamentals. The estimates presented in this table are marginal effects, which I have multiplied by one hundred times the standard deviation of the covariate in question — for ease of discussion I will refer to these as 'marginal effects'. Column (1) presents the benchmark specification, all other models are tested against this specification in terms of their predictive power: the final row of the table presents p-values for the statistical comparison between the benchmark in (1) and each other model. Models (2) and (3) provide results for alternative spillover proxies, where the former represents a single variable (count of default events elsewhere), whereas the latter is a more elaborate specification — there is the potential that the cross-section averages may pick up the impact of other omitted variables, such that we can think of the results for the default count in (2) as the lower bound and those for the CA specifications in (1) and (3) as the upper bound of the spillover effects. The remaining four models each drop one of the theory strands for comparison with the benchmark results.[14] Note that for all results presented in this and the following section, model comparison tests (ROC comparison) always rejected the joint omission of all theory strands or of all macro-fundamental variables.

Reputation acts as a disciplining device: having a higher share of years in default since 1950 ('Bad Reputation') and/or a more recent history of default are associated with lower default rates — the latter result is only significant in the parsimonious spillover specification in (2) or when spillovers are excluded in (7). The trade cost measures are statistically insignificant

[14] If this exercise in (4)-(7) is repeated with the parsimonious model in (2) — see Appendix Table G-1 — the relative magnitude of spillover versus reputation coefficients is similar, and the ROC comparison indicates the same patterns of significance for the reputation, politics, and spillover theory strands.

throughout except when spillovers are excluded in (7). A higher Polity IV score is associated with higher default propensity, but this measure is only significant in the parsimonious spillover specification in (2) and again when spillovers are excluded, while regime durability flips signs across the three spillover specifications and is never statistically significant. As is already clear from the above, the specification of the spillover variable(s) has significant bearings on the results for all other covariates: the benchmark model in (1) particularly highlights the role for foreign reserves elswhere for domestic default, both in sample countries and the OECD, which is also reflected in the results for the more elaborate model in (3) — the effects of all spillover variables are *in addition to* the global economic climate captured by the risk free rate, which in turn is highly significant and positive in the benchmark model but not in the two alternatives. Notable among the macro fundamentals are the strong significant associations of the reserve ratio (negative — the opposite sign to the sample spillover variable) as well as the insignificance of GDP growth, inflation and debt stock effects (except when spillovers are excluded), respectively. Debt service flows, in support of the notion expressed in Sturzenegger and Zettelmeyer (2007) that debt *payments* rather than *stock* enter into a sovereign's consideration, have a positive association with default though this is only statistically significant in the parsimonious spillover model in (2).

The signs of these measures in either models (1) or (2)[15] are in line with the raw logit estimates from the separate strand regressions discussed in Appendix E. The marginal effects computed and presented allow for direct comparison of the *economic magnitudes of the effects* across strands and measures. Quantitatively the largest effects are associated with 'bad reputation' and spillover effects, depending on the specification choice for the spillovers. As was suggested above, if we worry about the conflation of spillovers and omitted variables in the results for the various cross-section averages, then the parsimonious 'default count' specification in (2) can perhaps provide a narrower definition of spillover effects, which still highlights the economic significance of this theory strand. Among the macro-fundamentals the global economic environment (risk-free rate) and the foreign reserve ratio have the highest economic significance, with debt service somewhat more moderate in effect.

The remainder of the table presents two sets of statistics alongside empirical results testing the robustness of the benchmark model in (1), while Table G-1 in the Appendix does the same for the model in (2): the final table row compares the predictive power with restricted models where in (4)-(7) the set of variables associated with each theory are dropped, respectively (see 'Theory omitted' near the top of the table for orientation). In all but the threat/incentive case in (5) these restricted models are rejected by this measure of empirical

[15] It is somewhat academic to discuss consistency of signs between various models when coefficients are estimated imprecisely such as the domestic politics proxies.

fit, implying that the omitted theory 'matters' in terms of predictive power. Investigating the joint (in)significance for each strands of covariates (row marked 'Theory' toward the bottom of the table) indicates that the threat/incentive and domestic politics measures are statistically insignificant ($p = .42$ and $p = .58$) — these tests were carried out for the benchmark model in (1). Country fixed effects (implemented via time-series averages) are significant in all models presented, vindicating the choice of the RE-Mundlak approach over a logit fixed effects model.

In addition to the specifications presented a number of alternative or additional measures were considered for each theory strand (detailed results available on request), which resulted in identical or inferior predictive power and statistically insignificant coefficients, so that these covariates were dropped/dismissed in favour of those presented.[16] Similarly, the patterns of my results are qualitatively unchanged (all results are available on request) when I (i) include the first difference of the Chinn and Ito (2006) standardized measure for capital account openness (2017 update) or run a split sample regression by level of capital account openness, to gauge the effect of levels or changes in capital controls (Bai and Zhang, 2012);[17] (ii) include the annual growth of an aggregate index for primary commodity price movements along with its 12-month rolling standard deviation (see Eberhardt and Presbitero, 2021);[18] (iii) exclude the full time series, post-decision point observations or decision-point to completion date observations in the 34 countries in my sample considered for the Highly Indebted Poor Countries (HIPC) initiative of debt relief (Cassimon et al., 2015); I also estimate a split-sample regression for non-HIPC and HIPC countries;[19] (iv) include a squared term for inflation in an effort to capture the hyper-inflation-default nexus of Reinhart and Rogoff (2011b);[20] (v) add in controls

[16]None of the following alternatives changed/improved the predictive power of the benchmark results: exponentially discounted years since default (statistically significant at the 1% level), the addition of trade-weighted RTA counts (insignificant), measures of political constraints (insignificant), interactions between trade openness or financial development and the cross-section averages of inflation and reserves (mixed significance), additional cross-section averages for real growth, debt stock or service flows (insignificant).

[17]Following their cautious endorsement by the IMF in certain circumstances, the merits and perils of capital controls in emerging economies are currently subject to debate (Montecino, 2018). In my sample the inclusion of the change in the Chinn-Ito index of capital account openness leads to a negative significant coefficient on this variable, but no improvement in predictive power ($p = .57$). A 1 sd change in capital account openness is associated with a 0.85% decline in default propensity — this is a modest effect compared with that associated with reputation or spillover effects and other macro fundamentals. The split sample regressions yield similar patterns across theory strands as the pooled analysis, though forex/GDP spillovers and the risk-free rate have statistically significantly larger effects in economies with more capital constraints.

[18]The commodity price growth and volatility terms have the expected signs but only the latter is statistically significant in a reduced sample from 1981-2015 (114 defaults in 99 countries), in line with the findings in Eberhardt and Presbitero (2021). The exclusion of the 1970s reduces the magnitudes of the spillover variables in the benchmark model without commodity prices; upon their inclusion the relative magnitudes of reputation, punishment, politics, spillover, and macro-fundamentals remain qualitatively identical.

[19]Note that other debt relief initiatives, such as the Baker and Brady plans studied in Reinhart and Trebesch (2016), were dealing with countries debt restructing following default. Since I *exclude* ongoing crisis years these policy interventions do not come to bear on the prediction of (future) crises in these countries. Relative magnitudes of the theory strand proxies and macro-fundamentals are qualitatively unchanged in the reduced sample regressions. In the split sample regression the economic magnitudes are larger in the HIPC sample though only statistically significantly so for the spillover variables, the debt stocks and the risk-free rate. Patterns across theory strands are again very similar to those reported above.

[20]The coefficient on the squared term has the expected positive sign but neither term is statistically significant.

for capital inflow bonanzas (Caballero, 2016) and 'double bonanzas' (Reinhart et al., 2016).[21] and (vi) exclude 'new sovereigns' emerging from the breakup of the Soviet Union during the early 1990s.

Additional analysis employs alternative measures for sovereign debt from Catão and Mano (2017) for domestic debt, Dias et al. (2014) for external debt in net present value, and Mbaye et al. (2018) for private debt. These results are presented in Appendix F and confirm the general patterns of statistical significance and effect magnitude in the main analysis.

Although I point to the event analysis to justify the MA(3) transformation of the explanatory variables one might argue that this choice is to a certain extent arbitrary. In Table 2 I present the benchmark results from Table 1 when all independent variables are lagged by one period only, or are MA-transformed with two to five lags; in Appendix Table G-2 the more parsimonious spillover specification is studied in the same fashion. Note that selecting just a single lag runs the risk of conflating the predictor variable with the anticipation of the imminent default event, while selecting a much longer lag specification may wash out short-lived but important spikes in the lead-up to the debt crisis. A number of aspects of the results for these exercises are noteworthy: (i) reputation and spillover effects are economically and statistically significant throughout at any lag specification in either setup for spillovers, as is the case for foreign reserves; (ii) debt service flows and GDP growth are only significant if we consider short lag lengths, which may highlight the crisis trigger aspect of these variables; (iii) in contrast the trade-cost proxies for threats and punishment seem only to be captured when longer lag-lengths are considered, and then only for the default count specification for spillovers; (iv) the effects of domestic politics are somewhat fragile, though less so in the default count specification where their economic significance increases with longer lags.

What about twin and triple crises (Reinhart and Rogoff, 2009, 2011b)? Given the lag structure employed for my crisis determinants the above results do not account for the impact of a *contemporaneous* banking or currency crisis on the propensity of default. In Appendix Figure C-1 I study the timings of twin and triple crises, which suggests that most banking or currency crises in my sample occur in the years *prior* to a default, not thereafter. Nevertheless, a substantial number of twin crises occur (13 for banking-default, and 20 for currency-default

The inclusion of indicator variables for periods of hyperinflation as suggested by Reinhart and Rogoff (2011b) is infeasible in this empirical setup since these perfectly predict defaults — a vindication of these authors' arguments.

[21] Capital inflow bonanzas, defined as deviations 1sd or 2sd above the HP-filtered trend of net capital inflows (of these there are 215 and 18 in 101 countries, respectively, but *only 15 and 5 were in 80 defaulting economies*; since the bonanza dummies are short-run triggers I lag them by a single time-period only) have a positive significant effect on default propensity: the 1sd version yields an odds ratio of 2.37, which implies the average bonanza raises the unconditional default probability from 6.1% to 14.3%. The 2sd specification yields an odds ratio of 4.61. Recall that in the banking crisis analysis of Caballero (2016) the effect is typically a three to four-fold increase in a sample of pre-dominantly advanced and emerging economies. Inclusion of both commodity price growth and capital inflow bonanzas alongside their interaction does not provide any empirical support for the 'double bonanza' hypothesis in this particular sample and time period.

events) and are therefore not captured by my analysis. In a robustness check I include dummies for *contemporaneous* banking and currency crises in addition to the MA-transformed crisis dummies (results available on request). Although both these dummies are positive significant their inclusion does not lead to an increase in predictive power.[22] The patterns of economic magnitudes for the various theory strands and macro-fundamentals described above also do not change in any substantial way.

A qualitative conclusion from this analysis is that there is empirical evidence for the significance of *all but the direct punishment theory* for sovereign default, while it appears that *reputation and spillover effects trump all other factors* in terms of economic significance. While there is some tentative evidence for domestic politics affecting default, the direct punishment strand produces no statistically significant results and fails to increase the predictive power of the model. Foreign reserves, debt service and the global economic environment (risk-free rate) are additional macro controls which statistically significantly affect the default propensity.

5.3 Extension: Heterogeneities across Countries and over Time

Thus far the analysis has treated hypothesised crisis determinants as covariates. This section presents a number of extensions to the above preferred specification, intended to speak to the concern that different 'types' of countries, where type is defined by political regime, ER regime, etc., are characterised by entirely different default determinants. In each case I devise a sample split which maintains both a substantial number of observations *and* defaults in each of the two subsamples. Based on *kmeans* clustering of time-series averages of each country — meaning countries cannot change clusters with the exception of the democracy/autocracy split where failure of model convergence forced me to adopt a classification based on the time-varying Polity IV score, and, naturally, the temporal split into early and late periods — I investigate whether empirical results are significantly different (i) between democracies and autocracies, (ii) between countries with fixed and flexible exchange rate regimes (based on the classification in Ilzetzki et al., 2019), (iii) between countries with high and low financial development (using private bank credit to deposits as a proxy), and (iv) over time (taking 1999 as the cut-off year). In each case I add additional terms for all model variables interacted with a cluster dummy to the benchmark specification in column (1) of Table 1.

Table 3 presents the pooled benchmark results ('marginal effects' defined as above) alongside the models for sample splits as indicated. The shaded coefficient pairs rejected a simple Wald test for equality in the underlying RE-Mundlak logit regression, where darker shading indicates rejection at the 10% level, and lighter shading at the 15% level. Since sample splits

[22] Using again odds ratios to express the coefficient magnitudes on these contemporaneous terms suggests 2.9 for banking and 3.16 for currency crises.

essentially double the number of parameters estimated it is not surprising that (a) the predictive fit is, with one exception, better than for the pooled model (see ROC comparison in final table row), and (b) many covariate coefficients are estimated much less precisely. The number of default events in each cluster group is not necessarily the same or even close to the same, while the number of parameters to estimate push the estimator to the limits of its feasibility.

With these caveats in mind I limit the discussion to two broad confirmations and a number of novel insights. First, regarding the former, across all sample splits (with the exception of the temporal one) the marginal effects for bad reputation and the foreign reserves spillover variables consistently have the same sign and in terms of economic magnitudes are significantly larger compared with the marginal effects for any macro-fundamentals or alternative theory strand proxies. Second, the trade cost proxies for the direct punishment theory strand are insignificant throughout, and of the domestic politics proxies only regime durability shows some signs of significance (in statistical and economic terms). The first novel insight relates to the differential significance of spillovers, which in line with network arguments appears to be higher in economic terms in more financially integrated economies. These economies are much more susceptible to default if they have high debt services, but they can also reduce default risk via a growth spurt, a tool which is not available to countries with low levels of financial development. Second, the effect of reputational proxies varies substantially over time, with the negative effect of a bad reputation massively larger in the more recent period, and the memory effect (years since default) fading into insignificance in this period as well.[23] Third, countries with more flexible exchange rates differ significantly in the effects of their macro-fundamentals from countries with fixed ER regimes: debt stock and service flows have more substantial effects on default propensity, same goes for inflation, though GDP growth has a stronger moderating effect. Although the moderating effect of foreign reserves is economically significant in the flexible ER economies, this pales against the safeguard provided by reserves in fixed ER regimes. Fourth, there is very little evidence for a 'democratic advantage' if this is interpreted as differential effects for democracies versus autocracies, with the impact of banking crises the sole covariate significantly different across political regimes. Finally, the spillover variables provide some very stark coefficient magnitudes for the foreign reserve cross-section averages in the more recent time period, suggesting that perhaps the significance of this theory strand has increased vis-à-vis the earlier period.

These novel insights aside the sample split exercise primarily serves to confirm the patterns of statistical significance and economic magnitudes concluded from the pooled analysis

[23] With reference to the graphs in Figure 1 this pattern may be explained by the relative absence of defaults following the S&P definition from the turn of the century: 82% of defaults in this period were due to exceptionaly large official financing, whereas during 1970-1999 this type made up only 60% of defaults. 83% of defaults following the S&P definition were before 2000.

in Table 1. The conistency of major factors across specifications as described is a very strong indication of the pervasiveness and robustness of reputation, spillover and macro-fundamental effects on sovereign default.

6 Concluding remarks

This paper asked which factors dominate statistically and economically in predicting sovereign default events in developing and emerging economies over the past forty years. Standard approaches in economics have traditionally emphasised either reputational concerns or the threat of punishment, while a more recent literature has modelled international finance as a global network which sovereign borrowers either exploit strategically or which subjects them to higher default risk due to spillovers and 'cascades of failures'. Much of the political science literature in contrast has focused on the role of political regime and domestic politics with less concern for punishment and reputation (Michael Tomz being a notable exception).

In this paper I provide a wide range of empirical proxies for the four primary theoretical strands alongside standard macroeconomic controls. These proxies come in the form of constructed variables, but additional interactions with macro fundamentals and sample splits by political regime or exchange rate arrangement among others can cover a wide range of theoretical prescriptions on the possible manifestation of or channel of impact for reputation, punishment, political, and spillover effects.

The main contribution of this paper is the pursuit of an empirical early warning system approach including all four main theory strands, which in the existing literature are analysed separately. This analysis yields a number of novel and important insights: first, reputational and international spillover effects quantitively play the most significant role in predicting default events in a large sample of developing and emerging economies. Second, sound empirical evidence for the significance of direct punishment is less forthcoming, while the impact of domestic politics is fragile and comparatively modest in economic terms. Third, foreign reserves consistently provide moderating effects on default propensity which in economic terms are large though not necessarily on par with the reputation and spillover effects. Fourth, these findings are qualitatively robust to (i) sample splits by a variety of measures including ER arrangements and political regime, (ii) the use of alternative definitions for public debt as well as the consideration of private debt, (iii) alternative variable transformations to capture pre-crisis dynamics, as well as (iv) a host of additional covariates related to commodity price movements, hyper-inflation, or capital controls.

While this study focused on sovereign default in developing and emerging economies the economic environment in the world at large is captured in at least two ways: first, by the

inclusion of the risk-free rate, and second, by the inclusion of cross-section averages for macro fundamentals in OECD countries.

An empirical study of a comparatively small number of default events (6% of observations) with a large number of candidate predictors is always subject to concerns over a range of technical issues, starting from overfitting bias,[24] to empirical conflation of theoretically separate phenomena, to technically insurmountable problems such as constructing meaningful marginal effects for interaction effects in nonlinear models, to overinterpretation of results for flawed proxies to theoretical concepts. It was for instance not possible here to disentangle strategic behaviour from inevitable 'fate' in the analysis of international spillovers; many nuances of ongoing theoretical work on the reputation and direct punishment channels also had to be ignored, while the difficulty of modelling unobserved threats and incentives may have contributed to the failure to elucidate any significant effects for the punishment theory strand; and finally, while the split into early and late time periods yielded some stark differences and thus indicative evidence, the cross-country empirical approach applied here is not best-suited to speak to the dramatic changes seen in, for instance, the shift from external to domestic debt or the rise of private debt among developing countries over the past decade.

While all of these caveats and shortcomings must not be ignored, they cannot detract from the economic significance of international spillovers, to date not widely recognised in the sovereign default literature, and the confirmation of substantial reputation effects established in this study.

[24] Stripped-down models of just individual proxies for each theoretical strand yield encouraging results for the analysis of reputation and spillovers, and somewhat less enthusiastic support for punishment and domestic politics.

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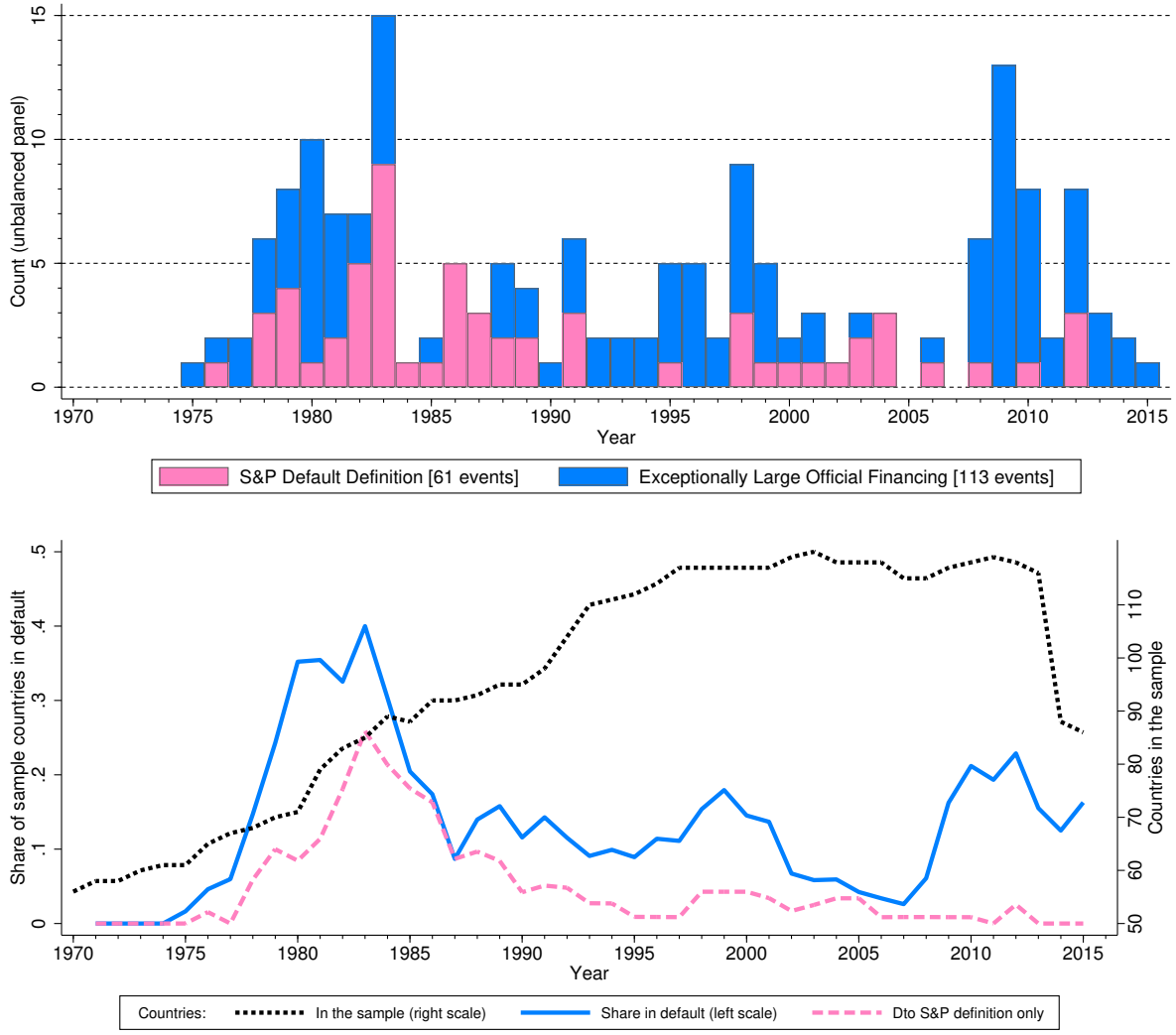
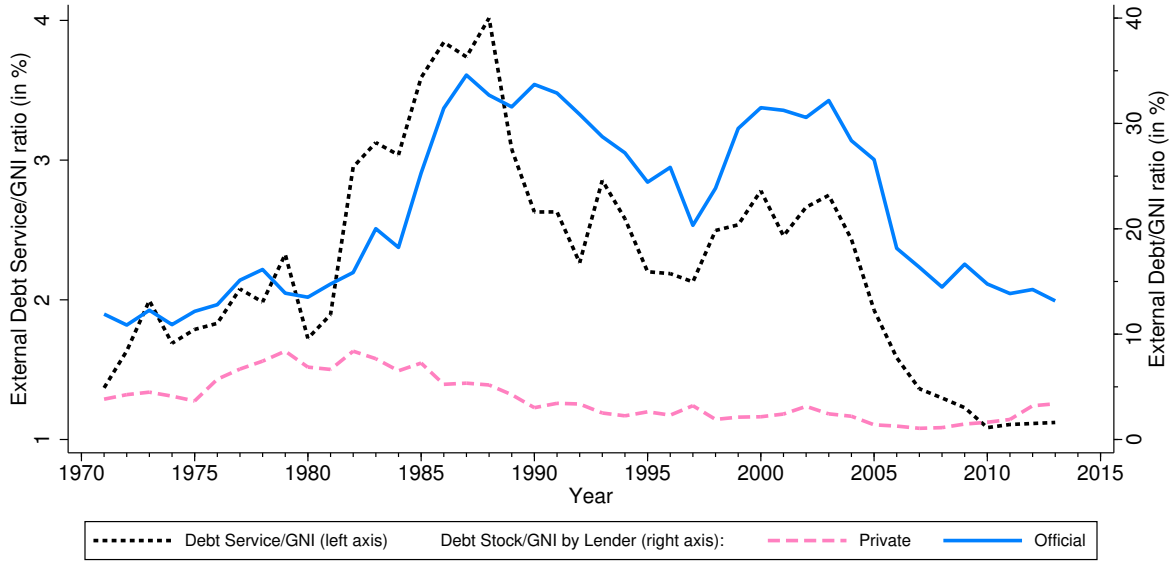
Figures and Tables

Figure 1: Default Event Frequency (top), Share of Sample in Default (bottom)



Notes: $N = 114$. The sample covers the period 1970-2015; 88 countries experienced 174 default events (in the regression samples the maximum number of defaults covered is 171 due to data availability for the RHS variables). Both graphs report annual frequencies/values, the top graph highlights the difference between defaults defined by S&P and by exceptional official financing focusing on default start years, the bottom graph countries *in default* (also indicating the unbalancedness of the sample).

Figure 2: Evolution of External Debt/GNI (stock, service flow)



Notes: The solid lines shows the median external public debt stock/GNI by lender (grey for private, black for official), the dashed line the median service/GNI ratio for my sample of 114 countries over the 1970-2015 time horizon. All values presented are in percent.

Table 1: Main Results

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-7.932	-4.783	-10.048		-8.089	-7.685	-3.280
	(5.61)***	(4.14)***	(5.19)***		(5.80)***	(4.72)***	(3.14)***
Years since default	0.517	1.522	0.120		0.558	0.680	2.576
	(0.60)	(1.88)*	(0.14)		(0.67)	(0.69)	(3.14)***
Average trade costs	-0.673	-0.542	-0.793	-0.917		-0.427	-0.746
w/ top-5 creditors	(0.51)	(0.47)	(0.58)	(0.68)		(0.28)	(0.71)
Relative trade costs	1.370	1.395	1.261	1.723		1.494	2.086
	(1.30)	(1.23)	(1.08)	(1.47)		(1.25)	(1.90)*
Polity Score	1.090	2.046	1.227	-0.051	1.176		2.563
	(0.88)	(1.87)*	(0.96)	(0.04)	(0.95)		(2.23)**
Regime durability	-0.317	0.640	-0.803	0.207	-0.191		0.902
	(0.30)	(0.63)	(0.77)	(0.17)	(0.19)		(0.99)
CA Inflation	0.862		2.722	0.776	0.843	0.928	
	(1.77)*		(2.49)**	(1.35)	(1.73)*	(1.82)*	
CA Foreign Reserves/GDP	9.709		7.676	7.427	9.699	9.778	
	(6.93)***		(2.46)**	(5.53)***	(7.23)***	(5.89)***	
CA External debt stock/GNI			1.084				
			(0.29)				
CA External debt service/GNI			4.260				
			(1.35)				
CA Real GDP growth			3.134				
			(2.12)**				
OECD CA Inflation	-1.780		7.011	2.402	-1.854	-2.051	
	(1.32)		(3.14)***	(1.82)*	(1.37)	(1.44)	
OECD CA Foreign Reserves/GDP	-2.780		-2.027	-2.275	-2.856	-2.835	
	(2.59)***		(1.40)	(1.91)*	(2.65)***	(2.46)**	
OECD CA Total debt/GDP			8.513				
			(2.11)**				
OECD CA Real GDP growth			-0.061				
			(0.05)				
Count of default		2.898					
events elsewhere (DC)		(5.51)***					
External debt stock/GNI	0.471	0.889	0.673	0.953	0.426	0.615	1.705
Private Creditors	(0.60)	(1.21)	(0.88)	(1.20)	(0.53)	(0.68)	(2.45)**
External debt stock/GNI	0.518	0.101	0.706	-0.983	0.651	0.480	-0.503
Official Creditors	(0.68)	(0.13)	(0.81)	(1.28)	(0.85)	(0.51)	(0.69)
External debt service/GNI	1.481	1.919	0.065	1.963	1.427	1.343	1.327
	(1.56)	(2.18)**	(0.07)	(1.93)*	(1.53)	(1.34)	(1.64)
Real GDP growth	-0.838	-0.799	-0.765	-0.826	-0.790	-0.868	-0.837
	(1.37)	(1.24)	(1.18)	(1.21)	(1.28)	(1.38)	(1.29)
Inflation	-0.597	0.340	-0.899	0.137	-0.420	-0.431	0.296
	(0.67)	(0.38)	(0.98)	(0.16)	(0.52)	(0.46)	(0.37)
Foreign Reserves/GDP	-3.827	-3.271	-3.698	-3.657	-4.057	-3.678	-1.810
	(3.20)***	(2.98)***	(2.97)***	(2.25)**	(3.13)***	(2.56)**	(1.72)*
Risk-Free Rate	4.977	0.128	1.661	2.783	4.906	5.101	2.644
	(5.00)***	(0.15)	(1.49)	(2.96)***	(4.87)***	(4.54)***	(3.74)***
Standard errors	Cluster	Cluster	Cluster	Jack	Cluster	Jack	Cluster
LogL	-420.29	-434.72	-403.21	-449.51	-421.65	-423.81	-451.43
AUROC (se)	0.83 (.017)	0.81 (.019)	0.85 (.016)	0.79 (.019)	0.82 (.018)	0.82 (.018)	0.78 (.021)
Theory: Wald χ^2 p-value ‡				0.000	0.418	0.576	0.000
FE: Wald χ^2 p-value ♯	0.000	0.000	0.000	0.001	0.000	0.000	0.000
AUROC = (1): p-value ♮		0.137	0.004	0.002	0.150	0.042	0.002

Notes: All results are for 102 countries with 2,329 observations and 141 crises. The estimates presented are marginal effects multiplied by 100 times the variable standard deviation. Banking and Currency crisis dummies included but not reported. Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit clustered at country level; unless jackknife where indicated). Models (4)-(7) highlight the significance of each of the four theories. ♯ Wald test for the insignificance of the within-country averages. ‡ This indicates the joint significance of each 'theory' in model (1). ♮ This test compares the ROC with that of Model (1); in (4)-(7) rejection indicates that the benchmark model is preferred and that the theory variables omitted 'matters'.

Table 2: Alternative MA-transformation of the Covariates

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1st.d. × 100)				
	(1)	(2)	(3)	(4)	(5)
Variable Transformation	1 lag	MA(2)	MA(3)	MA(4)	MA(5)
Bad Reputation	-8.165	-8.343	-7.932	-7.121	-6.239
	(5.40)***	(5.48)***	(5.61)***	(5.33)***	(4.54)***
Years since default	0.004	0.101	0.517	1.053	1.314
	(0.00)	(0.13)	(0.60)	(1.31)	(1.67)*
Average trade costs	0.220	1.088	1.370	1.364	1.200
w/ top-5 creditors	(0.22)	(1.09)	(1.30)	(1.30)	(1.08)
Relative trade costs	-0.344	-0.802	-0.673	-1.388	-0.998
	(0.43)	(0.65)	(0.51)	(1.06)	(0.78)
Polity Score	1.595	0.894	1.090	1.316	1.761
	(1.66)*	(0.71)	(0.88)	(1.26)	(1.73)*
Regime durability	0.421	-0.364	-0.317	-0.265	0.517
	(0.44)	(0.35)	(0.30)	(0.26)	(0.52)
CA Inflation	0.598	0.837	0.862	1.080	1.029
	(1.18)	(1.71)*	(1.77)*	(1.91)*	(1.64)
CA Foreign Reserves/GDP	7.520	9.310	9.709	8.911	7.864
	(5.49)***	(6.25)***	(6.93)***	(6.43)***	(6.01)***
OECD CA Inflation	-5.428	-2.332	-1.780	-0.385	2.068
	(2.67)***	(1.70)*	(1.32)	(0.29)	(1.52)
OECD CA Foreign Reserves/GDP	-4.385	-1.876	-2.780	-2.944	-1.848
	(2.60)***	(2.23)**	(2.59)***	(2.48)**	(1.51)
External debt stock/GNI	0.485	0.500	0.471	0.936	0.894
Private Creditors	(0.69)	(0.66)	(0.60)	(1.22)	(1.36)
External debt stock/GNI	0.873	0.849	0.518	0.224	0.200
Official Creditors	(1.13)	(1.08)	(0.68)	(0.29)	(0.25)
External debt service/GNI	1.237	1.363	1.481	1.139	1.092
	(1.80)*	(1.49)	(1.56)	(1.02)	(1.08)
Real GDP growth	-1.208	-1.023	-0.838	-0.801	-0.128
	(2.53)**	(1.88)*	(1.37)	(1.36)	(0.24)
Inflation	0.258	-0.396	-0.597	-0.366	0.369
	(0.41)	(0.47)	(0.67)	(0.39)	(0.55)
Foreign Reserves/GDP	-3.657	-3.875	-3.827	-3.768	-3.886
	(2.77)***	(3.27)***	(3.20)***	(3.31)***	(3.44)***
Risk-Free Rate	3.862	5.190	4.977	3.680	2.260
	(5.76)***	(5.79)***	(5.00)***	(3.57)***	(2.09)**
Observations	2,264	2,304	2,329	2,389	2,419
Countries	102	102	102	102	102
Crises	133	137	141	143	147
Log L	-401.95	-409.84	-420.29	-432.38	-449.24
AUROC	0.824	0.828	0.826	0.830	0.822
se(AUROC)	0.018	0.018	0.017	0.016	0.017
FE: Wald χ^2 ♯	125.29	143.77	149.17	139.79	145.80
p-value	0.000	0.000	0.000	0.000	0.000

Notes: The Table presents results for the specification in Column (1) of Table 1, varying the dynamic transformation of all right hand-side variables from a single lag in (1), to moving average transformation for $t-1$ to $t-k$ with $k = 2, 3, 4, 5$ in the remaining columns. For all other details see notes to Table 1.

Table 3: Extensions — Sample Splits

Estimator	RE-Logit Mundlak (Results reported: Cluster-Specific Margins × 1 st.d. × 100)								
	(1)	(2) Polity		(3) ER regime		(4) FinDev		(5) Time Period	
	Pooled	Low	High	Fixed	Flex	Low	High	Early	Late
Bad Reputation	-7.932	-9.099	-7.921	-7.385	-11.243	-6.650	-13.782	-3.868	-18.117
	(5.61)***	(4.12)***	(3.00)***	(3.88)***	(4.59)***	(3.92)***	(3.03)***	(1.12)	(5.83)***
Years since default	0.517	0.528	1.303	0.163	1.738	0.569	3.849	6.978	-0.833
	(0.60)	(0.46)	(0.59)	(0.11)	(1.31)	(0.51)	(2.19)**	(2.20)**	(0.68)
Average trade costs	1.370	1.452	1.760	0.291	0.678	1.361	0.032	-1.231	-0.308
w/ top-5 creditors	(1.30)	(0.73)	(1.23)	(0.16)	(0.40)	(0.96)	(0.01)	(0.57)	(0.09)
Relative trade costs	-0.673	0.451	-1.460	0.799	-1.587	-1.574	0.724	-0.122	-0.379
	(0.51)	(0.20)	(1.17)	(1.13)	(0.68)	(0.75)	(0.47)	(0.21)	(0.11)
Polity Score	1.090			0.287	0.479	0.163	-1.360	0.165	3.251
	(0.88)			(0.16)	(0.24)	(0.12)	(0.37)	(0.11)	(1.11)
Regime durability	-0.317	0.569	-0.983	1.080	-1.753	1.070	-7.299	-1.881	3.313
	(0.30)	(0.31)	(0.62)	(1.05)	(0.72)	(1.35)	(2.94)***	(1.31)	(1.98)**
CA Inflation	0.862	0.798	1.761	1.673	-0.603	0.973	1.857	1.869	-1.398
	(1.77)*	(1.27)	(1.64)	(2.29)**	(0.54)	(1.61)	(1.68)*	(2.30)**	(0.64)
CA Foreign	9.709	10.837	8.386	9.406	14.882	8.567	15.397	1.939	22.424
Reserves/GDP	(6.93)***	(4.82)***	(3.46)***	(4.19)***	(6.31)***	(4.55)***	(2.75)***	(0.83)	(4.85)***
OECD CA Inflation	-1.780	-1.624	-3.006	-0.672	-4.255	0.113	-11.541	3.967	2.728
	(1.32)	(0.86)	(1.16)	(0.38)	(1.41)	(0.07)	(2.53)**	(4.15)***	(0.70)
OECD CA Foreign	-2.780	-3.697	-1.504	-1.711	-5.820	-2.229	-4.865	1.333	1.264
Reserves/GDP	(2.59)***	(2.56)**	(0.95)	(1.36)	(2.49)**	(1.84)*	(1.83)*	(0.96)	(0.63)
External debt stock/GNI	0.471	0.647	0.284	0.764	-0.094	0.792	2.036	-0.915	2.152
Private Creditors	(0.60)	(0.45)	(0.25)	(0.75)	(0.06)	(0.88)	(1.01)	(1.08)	(0.88)
External debt stock/GNI	0.518	-2.399	0.365	0.267	3.438	1.464	0.454	0.382	5.278
Official Creditors	(0.68)	(1.29)	(0.34)	(0.23)	(2.46)**	(1.39)	(0.23)	(0.36)	(2.35)**
External debt	1.481	3.120	1.456	-0.198	3.799	0.022	5.307	1.835	0.169
service/GNI	(1.56)	(1.75)*	(1.18)	(0.16)	(2.01)**	(0.02)	(2.50)**	(1.63)	(0.05)
Real GDP growth	-0.838	-0.618	-1.533	-0.112	-2.472	-0.013	-2.873	-1.579	-0.873
	(1.37)	(0.63)	(1.53)	(0.16)	(2.13)**	(0.02)	(2.44)**	(2.17)**	(0.46)
Inflation	-0.597	-1.083	-0.854	-1.942	1.165	-0.138	-2.040	-0.315	-3.481
	(0.67)	(1.18)	(0.53)	(1.93)*	(2.03)**	(0.14)	(1.01)	(0.49)	(0.92)
Foreign Reserves/GDP	-3.827	-5.161	-1.376	-7.227	-3.883	-4.463	-2.150	-0.801	-8.408
	(3.20)***	(1.63)	(1.37)	(3.55)***	(1.67)*	(2.30)**	(0.82)	(0.68)	(2.13)**
Risk-Free Rate	4.977	4.684	5.512	5.174	6.889	4.292	9.489	-1.592	8.738
	(5.00)***	(3.46)***	(3.55)***	(4.38)***	(3.26)***	(3.20)***	(3.75)***	(0.84)	(3.97)***
Banking crisis	0.404	1.242	-0.981	0.042	1.088	0.320	0.369	0.434	0.608
	(1.10)	(2.53)**	(1.10)	(0.08)	(1.42)	(0.73)	(0.45)	(1.40)	(0.58)
Currency crisis	0.670	0.781	0.539	0.835	0.860	0.471	0.903	0.337	1.402
	(1.46)	(1.04)	(1.00)	(1.62)	(0.85)	(0.80)	(1.01)	(0.79)	(1.09)
Observations	2,329	1,311	1,018	1,130	1,199	1,561	748	1,166	1,163
Countries	102	53	49	47	55	68	33	100	88
Crises	141	75	66	70	71	92	49	45	96
LogL	-420.29	-407.17		-396.41		-395.25		-363.01	
AUROC	0.826	0.837		0.852		0.854		0.887	
seAUROC	0.017	0.017		0.016		0.016		0.015	
AUROC = (1): p-value		0.309		0.013		0.001		0.000	

Notes: This Table presents results for sample splits by (2) political regime, (3) fixed vs flexible exchange rate (following the classification in Ilzetzki et al., 2019, outlined in the data section), (4) level of financial development (proxied by private bank credit to bank deposits), (5) time — the early period ends with 1999. The estimates presented are marginal effects multiplied by 100 times the variable standard deviation — for the cluster, not the full sample except in model (1). Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit results clustered at the country level). Column (1) is the benchmark (pooled) model from Table 1, (2)-(5) report results for models split by democratic regime, exchange rate regime, financial development, and time. ♮ This test compares the ROC with that of the benchmark in Model (1); rejection indicates that the cluster-based model is preferred in terms of predicted power, which (naturally given the doubling of parameters) is the case for three of the four splits investigated. The shaded pairs indicate statistically significant differences between the two clusters (darker shading at the 10% level, lighter shading at the 15% level).

Appendix — not intended for publication

A Data Construction, Sample Makeup, and Descriptives

Dependent variable I follow the standard in the literature to define a sovereign default as indicated by either the Standard & Poor definition of a 'credit event' or exceptionally large official financing from the IMF. The S&P data beginning from 1824 are collated by Reinhart and Rogoff (2009, datasheet for the Preface Figure P.1) and updated to 2012 by Carmen Reinhart during May 2015 (see `carmenreinhart.com`); I added to these based on the 2016 S&P *Annual Sovereign Default Study And Rating Transitions* report covering 2012-15. In the S&P definition a default is either a unilateral decision on behalf of the debtor (a sovereign government) to interrupt (re)payment of interest or principal, or a 'swap' of new for existing debt with less favourable terms for the lender. Large IMF financing events are characterised by Medas et al. (2018, 10) as "any year under an IMF financial arrangement with access above 100 percent of quota and fiscal adjustment as a program objective." The analysis here adopts a dummy for the first year of a default as the dependent variable and following the standard in the literature excludes ongoing default years from the sample. In the main specifications below there are up to 174 default events, of which 61 are credit events as defined by S&P.

Reputation I construct the 'memory' variables following Catão and Mano (2017): primarily, the rolling share of years spent in default using the 1950s as a starting point (MEM1, above 'Bad Reputation') and the rolling count of years since the last default (MEM2). In robustness checks I adopt alternative functional forms in the construction of the latter variable to provide a notion of discounting.

Threats and Incentives The intention for these variables is to capture the exposure of sovereigns to the potential of some form of trade sanctions (whether explicit or implicit) and alongside this the incentives for sovereigns to defer default. I operationalise this using two main concepts: (i) the existince of formal preferential trade agreements, and (ii) bilateral trade costs. The former is based on Mario Larch's Regional Trade Agreements Database from Egger and Larch (2008) which I use to compute simple counts of country i's regional trade agreements (RTAs) at time t as well as an export-share weighted version of the same indicator, capturing RTAs which 'really matter' to country i. I hypothesise that *lower* membership in formal trade policy networks (especially those of economic significance as evidenced by actual trade flows) deters sovereigns from entering into default.

Bilateral trade costs are constructed following the strategy described in Novy (2013, adopting $\sigma = 8$) and using data for goods exports in nominal US$ from the IMF *Direction of Trade Statistics* (DOTS) along with GDP data in nominal US$ and the US GDP deflator from the World Bank *World Development Indicators* (WDI, via `Stata` command `wbopendata`). I follow Milner and McGowan (2013) to create country-specific trade cost measures from these bilateral estimates. In Figure B-1 the top panel presents the evolution of average country-specific trade cost for all countries featured in the combined DOTS and WDI databases (shaded 90% confidence interval), while the middle panel shows the average evolution specific to my 114 country

sample adopting the weighted *country-specific* cost estimates using estimated frictionless trade for weighting (see Data Appendix in Milner and McGowan, 2013). The bottom panel shows the trade cost evolution of my sample countries only accounting for costs with the top-5 creditors (US, UK, Germany, France and Italy) as highlighted in data on involvement in Paris Club renegotiation associated with Rose (2005).[25] The wide confidence intervals[26] in the latter two indicate wide variation across countries but also the comparatively smaller sample vis-à-vis the bilateral data analysis in the top panel for the whole world. All of the trade cost measures presented in the regression analysis below are *relative* measures, i.e. they benchmark the trade costs in country i against the *average* trade costs in all 114 countries, to highlight deviations.

Domestic politics From *Polity IV* (Marshall et al., 2016) I adopt the revised polity score (ranging from -10 to $+10$), and regime durability (in years). I further construct a democracy dummy (scores above zero) from the Polity data. From the *Political Constraint Index* (POLCON: Henisz, 2002, available from a dedicated website at Wharton, UPenn) I adopt polcon III and polcon V which measure the feasibility of policy change and take into account the "number of independent veto points over policy outcomes and the distribution of preferences of the actors that inhabit them" (ibid). Finally, I follow Herrera et al. (2020) in adopting data from the *International Country Risk Guide* (ICRG) database of the Political Risk Service Group: the 'government stability' measure captures a government's ability to remain in office and push through its declared political agenda. Coverage of this variable is however substantially worse than for all other data.

Spillovers From Arellano et al. (2017) I adopt the strategy to enter the share of sample countries currently in default (excluding country i), and construct the alternative 'number of defaults elsewhere' which is independent of the sample size.

In a recent study on banking crises investigating the importance of foreign credit booms for domestic busts Cesa-Bianchi et al. (2010) add GDP-weighted cross-section averages of credit growth for all $N-1$ countries to the covariates of country i in their pooled logit regressions. This notion of a global dimension of financial crises and the high level of interconnectedness of all types of markets in advanced economies is very close to what I focus on in this paper, augmenting the model with cross-section averages of the macro-fundamentals (debt stock and service flow, inflation, reserve ratio, real growth). Based on insights from the recent panel time series literature (e.g. Boneva and Linton, 2017; Chudik and Pesaran, 2015) the inclusion of cross-section averages with *common* coefficients — i.e. in a pooled regression model — implies that the spillovers captured by these variables are assumed to be on average *identical* across countries. This is a very strong assumption (and likely false), although in the context of pooled logit analysis, as is pursued here, this restriction is likely not to be taken too literally since cross-country heterogeneities are likely averaged out in this form of model. Nevertheless, I

[25] (Rose, 2005, see Rose's personal website for the data) reports the creditors in Paris Club renegotiations for 1956-97 the top-5 creditors (not necessarily in volume terms) are France, Germany, Italy, the UK and the US, involved as creditor in 82% of negotiations or more (96% for France).

[26] All graphs are based on the coefficients on year dummies in a regression of country-specific (in the case of the top-panel: bilateral) trade costs which also includes country dummies (in the case of the top-panel: exporter and importer dummies) and clusters the standard errors at the country-level (in the case of the top-panel: country pair-level).

experiment with two forms of 'country-specific' spillover variables by interacting the cross-section averages with a measure of trade openness (imports plus exports divided by GDP, taken from the WDI) and financial development (proxied by 'bank credit to bank deposits' from the World Bank *Global Financial Development Database* (GFDD) via `wbopendata`). Note that a number of 'external shocks' as raised by Eichengreen and Portes (1986) are represented by macroeconomic fundamentals discussed in the following section.

Macroeconomic Fundamentals and Other Controls A great deal of other variables have been suggested in the existing literature, some of which will be analysed and then dumped from the set of controls due to limited impact on predictive power and the requirement to be quite parsimonious.

Measures of debt stock and debt service flow are quite common — for instance in their analysis of spreads in advanced and developing countries Catão and Mano (2017) use total public debt to GDP in combination with external to total public debt. In the present case of developing countries there is perhaps less significance attached to domestic debt, and in the main analysis I therefore adopt the external debt to GNI measure from GFDD which further improves coverage. In an extension I adopt total public debt and the external/total debt ratio from Catão and Mano (2017) to follow up on the insights in recent work on domestic debt (Panizza, 2008; Reinhart and Rogoff, 2011a); I also adopt external debt in present value terms from Dias et al. (2014). Sturzenegger and Zettelmeyer (2007) argue that debt stocks may not be an ideal measure for analysis due to hidden debt and quasi-fiscal liabilities as well as ignoring liquidity concerns. In a similar vein Bulow and Rogoff (2015) argue that "sovereign debt renegotiations focus very much on the flow of repayments, and much less on how the stock of debt evolves…because all sides realise that any future promises can be renegotiated." See also the discussion in Bassanetti et al. (2019) on debt dynamics. Below I adopt total reserves/total external debt (liquidity) from the WDI in robustness checks but primarily focus on both external debt stocks and external debt service flows (both public and publically guaranteed and expressed as a share of GNI) from the GFDD. These are also available and applied split into private and official creditors.

Alternative measures of indebtedness are the net present value of external debt stock taken from Dias et al. (2014) and measures for private debt from the recent Global Debt Database (Mbaye et al., 2018).

Economic performance is clearly significant in light of the prediction that defaults only occur during 'good times' from the canonical Eaton and Gersovitz (1981) reputation theory. I adopt real GDP growth from the WDI.

A variety of studies emphasise inflation and also hyper-inflation (defined by Reinhart and Rogoff, 2011b, as inflation in excess of 500%pa); for improved coverage I adopt the GDP deflator from the WDI.

It is common practice in the literature (e.g. Bassanetti et al., 2019; Catão and Mano, 2017) to include a measure of the global risk-free rate in the model to capture the state of the global economic environment. The 10-year US Treasury constant maturity data is taken from FRED. I select the year-end value on the final trading day of each year from this daily dataset.

Reinhart and Rogoff (2011b) emphasise the occurance of dual or triple crisis and closely

associate currency crises with sovereign defaults. For my analysis I take banking and currency crises data (crisis start year) from a 2017 update to Laeven and Valencia (2013) — note that the Reinhart and Rogoff (2009) definition of banking crises is much looser, which may account for the link they establish in their analysis between banking crises and sovereign default. Relatedly, if banking crises are associated with defaults (Arellano and Kocherlakota, 2008; Reinhart and Rogoff, 2011b; Jordà et al., 2016), then the most significant determinant of banking crises, private credit growth, may also explain default patterns: I adopt the growth rate of the ratio of private bank credit to GDP from the WDI.[27]

A number of studies (including Eichengreen and Portes, 1986; Reinhart and Rogoff, 2011b; Catão and Mano, 2017) emphasise the significance of the exchange rate regime for default cost and the potential to erode the value of debt indirectly. I use recent data from Ilzetzki et al. (2019) to classify countries with fixed regimes (regimes 1 and 2) as well as a finer disaggregation (adopting the 'coarse' classification and gathering 1, 2, 3, 4-5, 6 into separate dummies) into hard pegs, crawling pegs, managed floats, free floats and parallel markets. Additional data on anchor currencies was used but did not yield additional insights and is therefore omitted here.

The current account balance as a measure of external balance is adopted in terms of GDP from Catão and Mano (2017). Furthermore I employ the foreign reserves to GDP ratio (as precaution against external shocks) from these authors augmented with data from the WDI.

Many countries in my sample are reliant on exports of natural resources and primary commodities, such that exogenous terms of trade shocks are deemed important for default prediction. Catão and Mano (2017) provide data on the terms of trade gap.[28]

Concerns over the significance of commodity price movements lead me to adopt the methodology described in Eberhardt and Presbitero (2021) to construct country-specific annual commodity price growth and its 12-month standard deviation from data by Gruss (2014) and the IMF Primary Commodity Prices database.

Probing further the significance of capital inflow bonanzas I construct dummies for deviations in excess of one (or two) standard deviation(s) from the HP-filtered trend following Caballero (2016), using data on net capital inflows as a share of GDP from the IMF Financial Flows Analytics (FFA) database. More specifically, this refers to the 'Total Net Nonofficial Inflows,' in percent of GDP (in US$).

Finally, I adopt the measure of 'major episodes of political violence' involving at least 500 deaths from Marshall (2017) to relate to the literature on war and default (Shea and Poast, 2018).

Variable descriptives are presented in Table A-1, where I adopt the sample of the macro fundamentals regression in column (2) of Table E-1. The sample makeup and further information about defaults (S&P, IMF financing), default start years, and ongoing default years is presented in Table A-2 for the same regression sample.

[27] This is not real credit growth as in the seminal Schularick and Taylor (2012) study but it is conceptually not obvious that one is clearly preferred to the other against the background of improved coverage for the ratio measure.

[28] External terms of trade relative to a backward-looking 3-year moving average.

Table A-1: Variable descriptives

Variable	Obs	mean	median	sd	min	max
Dependent variable						
Sovereign default (start year)	2,826	0.061	0.00	0.24	0.00	1.00
Control variables						
External Debt/GDP (in %)	2,826	35.12	26.65	30.15	1.37	166.77
External debt service/GDP (in %)	2,826	2.65	1.98	2.30	0.07	13.02
External Debt/GDP (in %) - private creditors	2,826	6.61	3.24	9.16	0.00	50.64
External Debt/GDP (in %) - official creditors	2,826	28.44	19.20	29.26	0.24	157.27
External debt service/GDP (in %) - private creditors	2,826	1.09	0.48	1.61	0.00	10.16
External debt service/GDP (in %) - official creditors	2,826	1.53	1.22	1.23	0.02	6.43
Real GDP growth (in %)	2,826	4.57	4.47	3.66	-11.70	18.87
Inflation (in %)	2,826	13.09	7.81	23.81	-7.26	197.70
Foreign reserves/GDP (in %)	2,826	13.22	10.21	12.90	0.23	82.46
Risk-free rate (in %)	2,826	5.97	5.62	2.50	2.24	12.26
Banking crisis (dummy)	2,826	0.01	0.00	0.07	0.00	0.33
Currency crisis (dummy)	2,826	0.02	0.00	0.08	0.00	0.33
Political Violence (dummy)	2,826	0.18	0.00	0.36	0.00	1.00
Fixed Exchange Rate (ER) regime	2,826	0.74	1.00	0.41	0.00	1.00
ER: peg	2,826	0.42	0.00	0.48	0.00	1.00
ER: crawl	2,826	0.32	0.00	0.45	0.00	1.00
ER: managed float	2,826	0.18	0.00	0.37	0.00	1.00
ER: parallel markets	2,826	0.01	0.00	0.10	0.00	1.00
Current account/GDP	2,826	-0.09	-0.04	0.38	-7.61	0.27
Terms of trade gap	2,422	0.01	0.00	0.12	-0.46	0.55
Private credit growth	2,701	3.92	3.76	10.16	-42.22	41.31
Liquidity - total reserves/external debt (in %)	2,626	53.34	27.45	85.46	0.79	625.62
Capital flow bonanza (1sd) dummy	2,329	0.05	0.00	0.23	0.00	1.00
Capital flow bonanza (2sd) dummy	2,329	0.01	0.00	0.09	0.00	1.00
Commodity price growth (annual)	1,819	0.11	0.00	1.81	-21.75	25.39
12-month SD: Commodity Price Growth	1,819	0.14	0.02	0.72	0.00	16.09
Change in capital account openness (Chinn-Ito)	2,261	0.00	0.00	0.05	-0.41	0.25
Alternative Measures for Debt						
Total Debt/GDP (in %)	2,154	0.53	0.44	0.38	0.06	2.26
External/Total Debt (in %)	2,141	0.65	0.68	0.23	0.11	1.00
External Debt (Present Value)/GDP (in %)	967	25.23	22.52	15.57	2.37	102.77
Private Debt/GDP - GDD (in %)	1,954	29.84	20.72	26.73	1.75	142.94
Public Debt/GDP - GDD (in %)	1,954	45.30	36.47	34.92	3.60	204.04
Reputation						
Bad Reputation	2,826	0.10	0.05	0.13	0.00	0.46
Years since default	2,826	3.45	0.67	5.26	0.00	24.00
dto w/ exponential discounting (5yr MA)	2,261	0.94	0.98	0.11	0.00	1.00
dto w/ exponential discounting (1.2%)	2,261	0.68	0.80	0.34	0.00	1.00
dto w/ exponential discounting (15%)	2,261	0.55	0.42	0.42	0.00	1.00

(continued overleaf)

Table A-1: Variable descriptives (continued)

Variable	Obs	mean	median	sd	min	max
Threats and Incentives						
Relative trade costs	2,797	1.15	1.19	0.38	0.15	2.21
Relative trade costs (5 creditors)	2,797	0.96	0.68	0.97	0.20	8.54
Relative trade costs (FRA)	2,797	0.98	0.68	1.24	0.15	13.80
Relative trade costs (DEU)	2,797	1.03	0.73	1.33	0.13	17.85
Relative trade costs (ITA)	2,797	0.88	0.61	1.15	0.14	13.00
Relative trade costs (GBR)	2,797	0.84	0.57	1.08	0.12	11.88
Relative trade costs (USA)	2,797	1.13	0.75	1.62	0.23	20.91
Sum of RTAs	2,741	20.75	14.00	20.97	0.00	110.00
Export-weighted sum of RTAs	2,741	0.28	0.20	0.28	0.00	0.99
Domestic Politics						
Democracy	2,578	0.50	0.50	0.49	0.00	1.00
Polity IV (revised)	2,554	0.54	1.00	6.58	-9.00	10.00
Regime durability	2,577	14.58	10.33	14.68	0.00	77.00
Years since independence	2,762	73.01	39.00	106.51	-4.00	777.00
ΔPOLCON III	2,476	0.00	0.00	0.05	-0.37	0.44
ΔPOLCON V	2,415	0.00	0.00	0.06	-0.43	0.52
Annual SD government stability	1,382	0.30	0.23	0.27	0.00	2.12
Spillovers						
Sample share in default (in %)	2,714	15.78	16.18	11.73	0.00	54.84
Default count elsewhere	2,826	3.60	3.33	2.59	0.00	15.00
CA External Debt/GDP (in %)	2,611	35.92	38.44	11.47	18.19	51.64
CA External debt service/GDP (in %)	2,611	2.72	2.95	0.66	1.59	4.04
CA Real GDP growth (in %)	2,611	4.59	4.51	0.92	2.25	6.37
CA Inflation (in %)	2,611	13.21	10.35	7.46	5.39	37.92
CA Foreign reserves/GDP (in %)	2,611	12.58	12.92	5.18	4.94	21.70
Bank credit to deposits (in %) ('FinDev')	2,740	97.61	83.85	56.50	20.68	360.42
CA External Debt/GDP (in %) $\times$ FinDev	2,544	3361.94	2954.91	2192.06	453.75	18323.50
CA External debt service/GDP (in %) $\times$ FinDev	2,544	258.17	225.12	160.82	43.08	1399.43
CA Real GDP growth (in %) $\times$ FinDev	2,544	450.58	379.37	287.09	47.59	2176.15
CA Inflation (in %) $\times$ FinDev	2,544	1263.72	927.77	1071.76	151.98	12593.44
CA Foreign reserves/GDP (in %) $\times$ FinDev	2,544	1168.58	994.64	815.62	134.95	7747.14
CA OECD Total debt/GDP (in %)	2,826	58.56	64.21	15.29	31.24	84.98
CA OECD Real GDP growth (in %)	2,826	3.07	3.35	1.42	0.12	6.05
CA OECD Inflation (in %)	2,826	5.55	2.87	4.37	1.26	14.60
CA OECD Foreign reserves/GDP (in %)	2,826	11.51	11.55	0.84	9.22	12.97
CA OECD Total debt/GDP (in %) $\times$ FinDev	2,740	5547.62	4955.04	3391.07	675.41	30628.88
CA OECD Real GDP growth (in %) $\times$ FinDev	2,740	310.77	247.49	261.66	2.53	1978.63
CA OECD Inflation (in %) $\times$ FinDev	2,740	590.29	279.59	686.87	35.03	5263.92
CA OECD Foreign reserves/GDP (in %) $\times$ FinDev	2,740	1116.93	975.46	639.49	209.29	4675.97

Notes: All variables are winsorized and transformed into MA(3) processes. 'IA' indicates the interaction between variable cross-section averages and the measure of financial development (FinDev). xx% of all observations are default years (ongoing default years are omitted by convention), in the sample of defaulters this amounts to xx%. $*$ SD indicates the standard deviation, constructed from monthly data.

Table A-2: Sample Makeup

wbcode	Country	Obs	Defaults	S&P	>100%	Coverage	Default Start Year(s)	Ongoing Default Years
AGO	Angola	7	1	0	1	2004-2015	2009	2010-12
ALB	Albania	13	2	0	2	1997-2014	1998, 2014	1999-2011, 2015
ARG	Argentina	22	2	1	1	1970-2015	1982, 1998	1983-5, 1999-2001
ARM†	Armenia	13	2	0	2	1993-2015	1996, 2009	1997-9, 2010-2
AZE	Azerbaijan	22	0	0	0	1993-2015		
BDI	Burundi	41	1	0	1	1970-2015	1991	1992-4
BEN	Benin	36	2	0	2	1970-2010	1993, 2010	1994-6, 2011-3
BFA	Burkina Faso	22	2	1	1	1974-2010	1983, 2010	1984-6, 2011-3
BGD	Bangladesh	33	3	0	3	1973-2012	1980, 1990, 2012	1981-2, 1991-3, 2013-5
BGR	Bulgaria	16	1	0	1	1995-2015	1998	1999-2001
BIH†	Bosnia and Herzegovina	10	1	0	1	1999-2009	2009	2010-2
BLR	Belarus	20	1	0	1	1994-2015	2009	2010
BLZ†	Belize	38	2	2	0	1976-2015	2006, 2012	2007
BOL	Bolivia	28	2	2	0	1970-2015	1980, 1986	1981-3, 1987-9
BRA	Brazil	29	2	1	1	1970-2015	1983, 1998	1984-6, 1999-2001
BWA	Botswana	39	0	0	0	1976-2015		
CAF	Central African Republic	15	2	2	0	1970-2013	1981, 1983	1984-6
CHN	China	34	0	0	0	1981-2015		
CIV	Cote d'Ivoire	16	2	0	2	1970-2009	1985	1982-4, 2010-2
CMR	Cameroon	26	1	1	0	1970-2015	1985	1986-8
COG	Congo, Rep.	19	1	1	0	1970-2013	1983	1984-6
COL	Colombia	41	1	0	1	1970-2015	1999	2000-2
COM	Comoros	28	1	0	1	1981-2009	2009	2010-2
CPV	Cabo Verde	18	0	0	0	1981-2015		1982-4*
CRI	Costa Rica	34	1	0	1	1970-2015	1980	1981-3
DJI	Djibouti	9	1	0	1	1991-2005	1999	2000-2
DMA†	Dominica	26	1	1	0	1981-2013	2003	1982-4*, 2004-5
DOM	Dominican Republic	22	2	1	1	1970-2015	1982, 2003	1983-5, 2004-6
DZA	Algeria	37	1	1	0	1970-2015	1991	1992-4
ECU	Ecuador	29	3	3	0	1970-2015	1982, 1999, 2008	1983-5, 2000, 2009
EGY	Egypt, Arab Rep.	41	2	1	1	1970-2015	1978, 1984	1979-81
ERI	Eritrea	16	0	0	0	1995-2011		
ETH	Ethiopia	21	1	1	0	1983-2013	1991	1992-4
FJI	Fiji	45	0	0	0	1970-2015		
GAB	Gabon	18	2	1	1	1970-2013	1978, 1986	1979-81, 1987-9
GEO	Georgia	12	2	0	2	1995-2015	1996, 2008	1997-9, 2009-11
GHA	Ghana	32	3	1	2	1970-2013	1983, 1987, 2009	1984, 1988-90, 2010-12
GIN	Guinea	14	2	1	1	1989-2012	1991, 2012	1992-4, 2013-5
GMB	Gambia, The	38	2	0	2	1970-2013	1982, 1988	1983, 1989-91
GNB	Guinea-Bissau	12	3	1	2	1980-2010	1983, 2000, 2010	1983-6, 2001-3, 2011-3
GRD†	Grenada	27	3	2	1	1978-2013	1983, 2004, 2012	1984, 2005-7, 2015
GTM	Guatemala	44	3	2	1	1970-2015	1983, 1986, 1989	1984
GUY	Guyana	18	1	1	0	1970-2015	1979	1980-2
HND	Honduras	18	1	0	1	1970-2015	1979	1980-2
HTI	Haiti	10	1	0	1	2000-2015	2006	2007-9
IDN	Indonesia	38	1	0	1	1970-2015	1997	1998-2000
IND	India	41	1	0	1	1970-2015	1981	1982-4
IRN	Iran, Islamic Rep.	24	1	1	0	1971-2013	1978	1979-81
JAM	Jamaica	24	3	2	1	1970-2010	1977, 1987, 2010	1978-80, 1988-90, 2011-3
JOR	Jordan	25	2	1	1	1976-2012	1989, 2012	1990-2, 2013-5
KAZ	Kazakhstan	18	1	0	1	1993-2015	1996	1997-9
KEN	Kenya	16	3	0	3	1970-2011	1975, 1989, 2011	1976-8, 1990-2, 2012-4
KGZ	Kyrgyz Republic	15	1	0	1	1992-2015	1994	1995-7
KHM	Cambodia	16	0	0	0	1998-2014		
KNA†	St. Kitts and Nevis	26	0	0	0	1984-2010		2012-3*
LAO	Lao PDR	30	0	0	0	1985-2015		
LBN	Lebanon	26	0	0	0	1989-2015		
LBR	Liberia	18	3	1	2	1979-2015	1980, 1987, 2008	1981, 1988-90
LCA†	St. Lucia	32	0	0	0	1981-2013		

(continued overleaf)

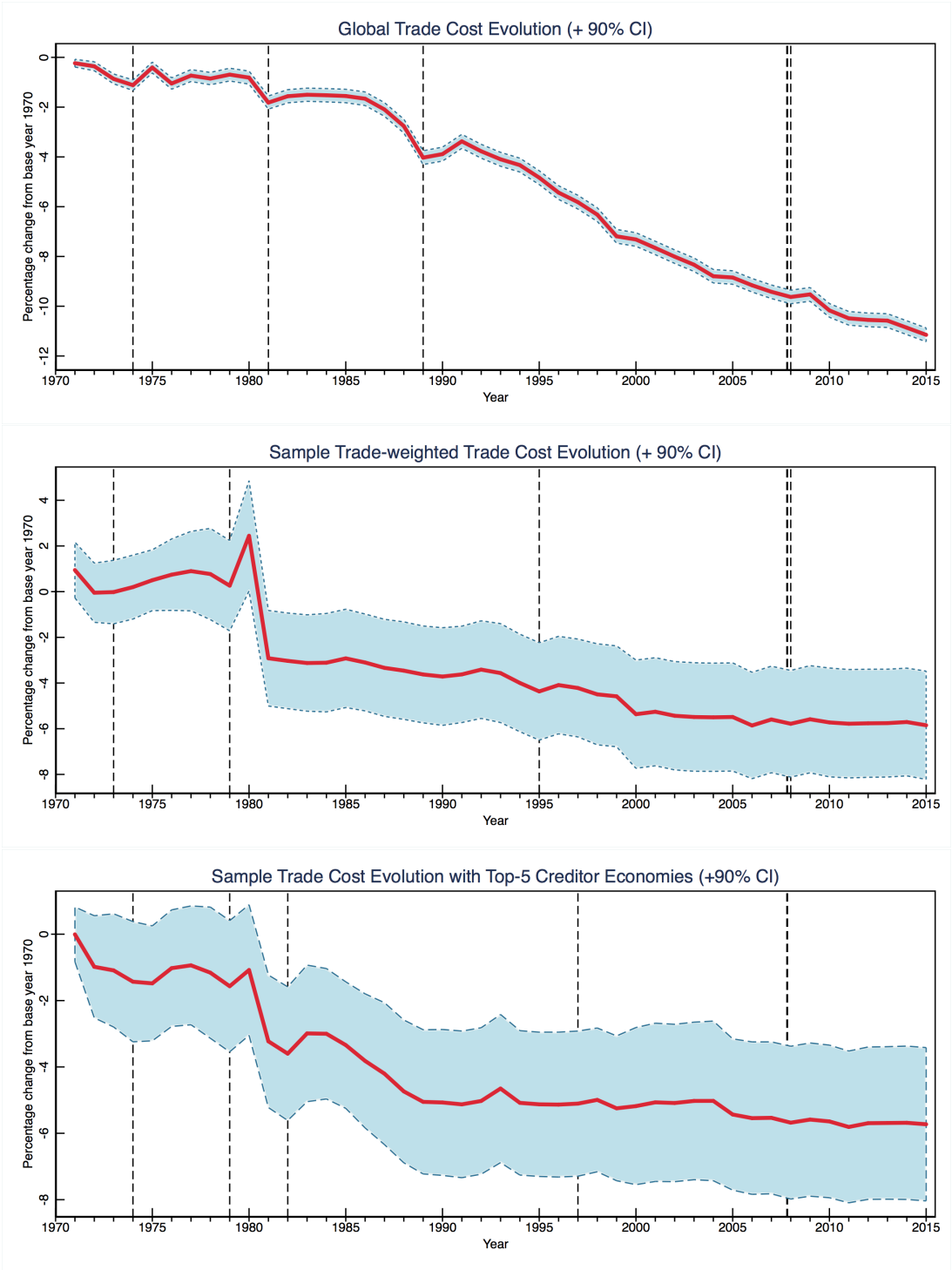
Table A-2: Sample Makeup (continued)

wbcode	Country	Obs	Defaults	S&P	>100%	Coverage	Default Start Year(s)	Ongoing Default Years
LKA	Sri Lanka	34	3	0	3	1970-2015	1979, 1991, 2009	1980-1, 1992-4, 2010-2
LSO	Lesotho	32	2	0	2	1975-2015	1991, 2010	1992-4, 2011-3
MAR	Morocco	34	1	0	1	1970-2015	1980	1981-3
MDA	Moldova	10	2	1	1	1996-2015	2002, 2010	1997-9, 2011-3
MDG	Madagascar	20	2	1	1	1970-2013	1980, 1986	1981-3, 1987-9
MDV†	Maldives	11	1	0	1	2002-2015	2009	2010-1
MEX	Mexico	28	4	1	3	1970-2015	1977, 1982, 1995, 1999	1978-9, 1983-5, 1996-7, 2000
MKD	Macedonia, FYR	12	1	0	1	2001-2015	2011	2012-3
MLI	Mali	36	3	1	2	1970-2013	1983, 1992, 2012	1984-5, 1993-5
MNE	Montenegro	9	0	0	0	2006-2015		
MNG	Mongolia	18	2	0	2	1992-2015	1983, 2009	1994-6, 2010
MOZ	Mozambique	18	2	0	2	1996-2015	2009, 2015	2010
MRT	Mauritania	30	3	0	3	1970-2010	1980, 1989, 2010	1981-2, 1990-2, 2011-3
MUS	Mauritius	35	2	0	2	1977-2015	1979, 1983	1980-1, 1984
MWI	Malawi	29	4	1	3	1970-2012	1979, 1988, 1995, 2012	1980-2, 1996-8, 2013-5
MYS	Malaysia	45	0	0	0	1970-2015		
NER	Niger	29	3	1	2	1970-2012	1983, 1996, 2012	1984-6, 1997-9, 2013-5
NGA	Nigeria	33	3	3	0	1970-2015	1982, 2001, 2004	1983-5, 2005
NIC	Nicaragua	20	1	1	0	1970-2015	1979	1980-2
NPL	Nepal	45	0	0	0	1970-2015		
PAK	Pakistan	30	5	1	4	1970-2013	1980, 1998, 2001, 2008, 2013	1981-3, 1999, 2002-4, 2009-11, 2014-5
PAN	Panama	30	2	1	1	1970-2015	1980, 1983	1981, 1984-6
PER	Peru	27	3	2	1	1970-2015	1976, 1978, 1982	1979-80, 1983-5
PHL	Philippines	22	3	1	2	1970-2015	1976, 1983, 1994	1977-9, 1984-6, 1995-7
PNG	Papua New Guinea	41	0	0	0	1973-2014		
PRY	Paraguay	20	1	1	0	1995-2015	2003	
RUS	Russian Federation	14	0	0	0	2001-2015		
RWA	Rwanda	40	1	0	1	1970-2015	1998	1999-2001
SDN	Sudan	11	1	1	0	1970-2015	1979	1980-2
SEN	Senegal	28	2	0	2	1970-2013	1980, 1988	1981-3, 1989-91
SLB	Solomon Islands	9	1	1	0	1991-2015	1998	1999-2001
SLE	Sierra Leone	24	4	1	3	1970-2013	1981, 1986, 2001, 2013	1982-4, 1987-0. 2002-4, 2014-5
SLV	El Salvador	45	0	0	0	1970-2015		
STP†	Sao Tome and Principe	13	0	0	0	2002-2015		
SWZ	Swaziland	28	0	0	0	1971-2015		
TCD	Chad	38	1	0	1	1970-2013	1995	1996-8, 2015*
TGO	Togo	22	3	2	1	1970-2013	1979, 1988, 2008	1980-2, 1989-91, 2009-11
THA	Thailand	38	3	0	3	1970-2015	1981, 1985, 1997	1982-3, 1986, 1998-2000
TJK	Tajikistan	10	2	0	2	1997-2015	1998, 2009	1999-2001, 2010-12
TKM	Turkmenistan	20	0	0	0	1993-2013		
TON†	Tonga	28	0	0	0	1985-2013		
TUN	Tunisia	38	2	0	2	1970-2013	1988, 2013	1989-91, 2014-5
TUR	Turkey	29	2	1	1	1970-2015	1978, 1999	1979-81, 2000-2
TZA	Tanzania	10	1	0	1	2004-2015	2009	2010
UGA	Uganda	20	0	0	0	1995-2015		
UKR	Ukraine	11	4	1	3	1992-2014	1995, 1998, 2008, 2014	1996, 1999-2001, 2009-11, 2015
UZB	Uzbekistan	21	0	0	0	1992-2013		
VCT†	St Vincent & Grenadines	37	0	0	0	1976-2013		
VEN	Venezuela, RB	29	3	3	0	1970-2013	1983, 1995, 2004	1984-6, 1996-7, 2005
VNM	Vietnam	16	0	0	0	1999-2015		
VUT†	Vanuatu	33	0	0	0	1981-2014		
YEM	Yemen, Rep.	9	1	0	1	2002-2013	2010	2011-2, 2015*
ZAF	South Africa	21	0	0	0	1994-2015		
ZMB	Zambia	28	1	0	1	1970-2015	1978	1979-81
ZWE	Zimbabwe	26	3	1	2	1981-2015	1983, 1992, 2000	1984, 2001-3

Notes: † indicates the countries which are not included in the main regressions (typically due to missing data on domestic politics). For Defaults I separately indicate the number of defaults based on the S&P criterion and large official financing from the IMF. Coverage indicates the nominal start and end year of the country series (not accounting for gaps in the data or additional years in default), the final two columns indicate the default start year and ongoing default years (not included in the sample), respectively. * highlights periods which were not in the sample (due to missing control variables) in the first place.

B Trade cost evolution

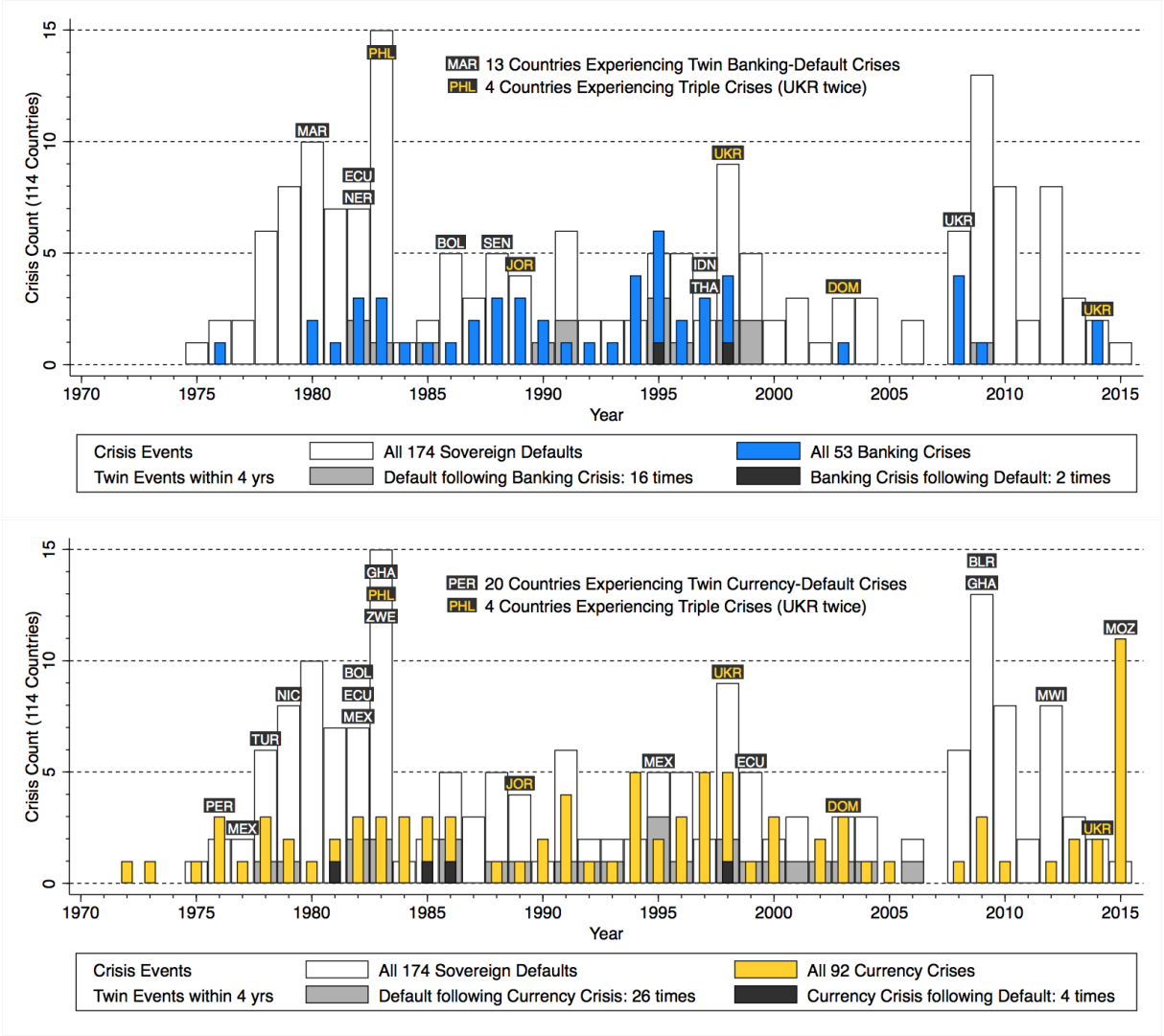
Figure B-1: Global/Sample Trade Cost Evolution



Notes: Following the methodology laid out in Novy (2013) and the construction of weights in Milner and McGowan (2013) these graphs present the *global* trade cost evolution from the base year 1970 using country averages and weighted country averages (weights determined by share of frictionless trade — see maintext for details) in the upper and lower panels, respectively.

C Twin and Triple Crises

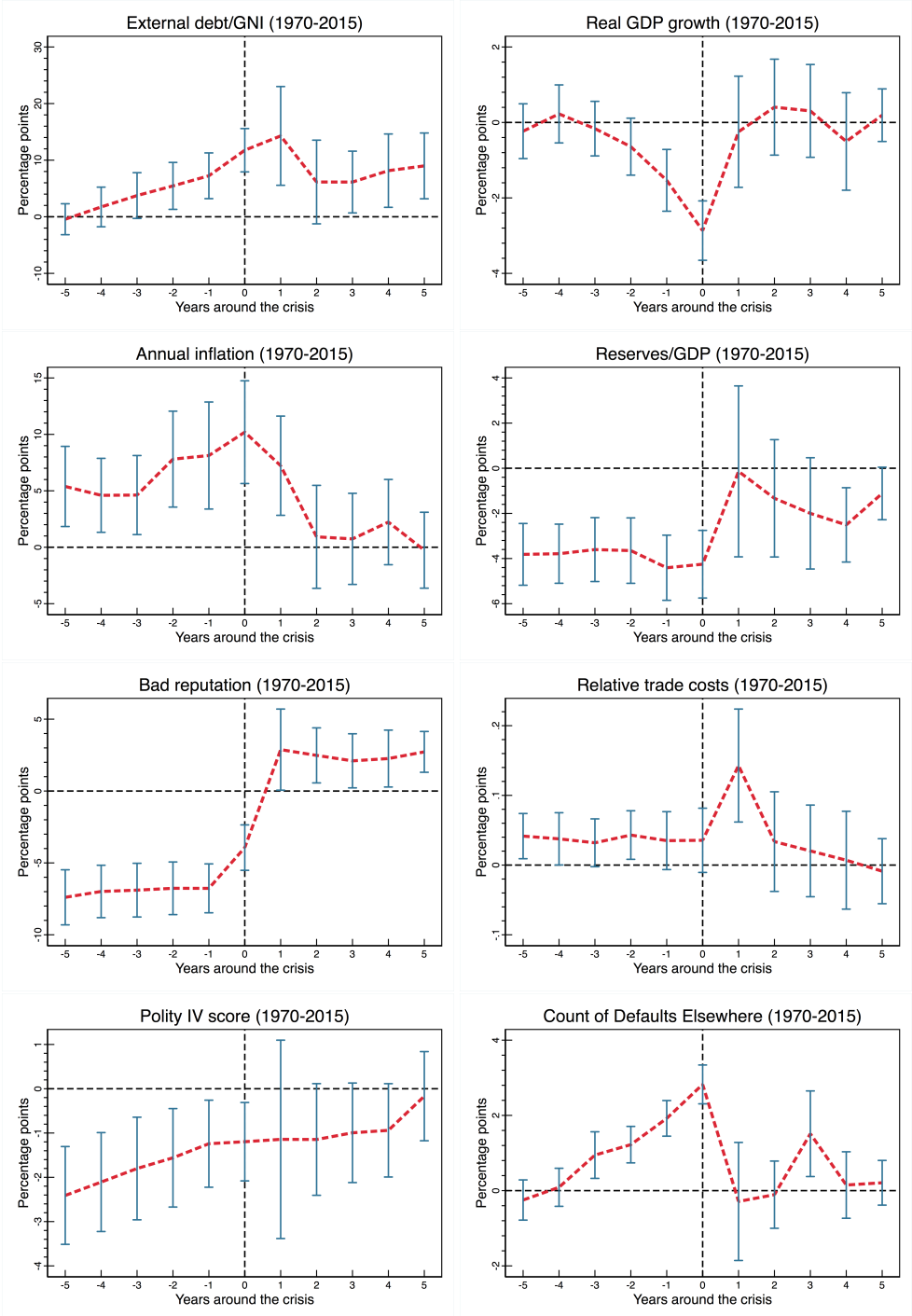
Figure C-1: Twin and Triple Crises – Default with Banking Crises (top) and Currency Crises (bottom)



Notes: $N = 114$. The sample covers the period 1970-2015; 88 countries experienced 174 default events (in the regression samples the maximum number of defaults covered is 171 due to data availability for the RHS variabes). In the top graph default events (start years) are compared to banking crises, in the bottom graph with currency crises. Each plot highlights (a) twin crises (country code), (b) triple crises (dto.), and (c) twin crisis events within four years of each other (using grey and black bars to distinguish the order of events: defaults typically take place *in the same year or after* banking or currency crisis events). Taken together, 16% (29/174) of the defaults in the sample are also associated with a banking crisis (crises within four years of each other), for currency crises this figure is 26% (46/174). The unconditional propensity of a twin crisis (between $t-3$ and $t+3$) is 1.1% for banking crises and 1.6% for currency crises — compared with an unconditional propensity of default of 5.7%.

D Event Analysis

Figure D-1: Event Analysis



Notes: The sample for this event analysis is based on a logit regression including all of the above covariates along with country fixed effects ($N = 87$, $n = 2,112$ and 171 default events) — years when a country is in default (other than the start year) are excluded from the sample. The event analysis itself also includes country fixed effects, thus giving the plots a 'within-country' interpretation. The first two rows of plots cover macro-economic fundamentals (debt service flows have similar patterns to debt stock), the bottom two rows represent variables for the reputation, direct punishment, domestic politics, and spillover theory strands, respectively.

E Empirical Results by Theory Strand

This section presents the empirical results from each individual theory strand. I begin with the set of macro-fundamentals I chose to constitute a benchmark specification for default prediction, with results presented in Table E-1. The subsequent paragraphs turn to proxies for reputation, threats, domestic politics and spillovers, with empirical results presented in Tables E-2 to E-6.

Macro-Fundamentals Table E-1 presents results for macro fundamentals, where here and in the following the sample size is held approximately constant across specifications. Table footnotes provide additional information for each theory strand. Reported results are raw logit coefficients, so in this initial analysis by strand the focus is on sign and significance of the covariates as well as (change in) predictive power. A first notable aspect of *all* specifications is the high AUROC statistic, around .7, suggesting sound predictive power: macroeconomic fundamentals *on their own* clearly matter for default proponsity; second, all models (here and in all following results presented) highlight the importance of the country fixed effects as operationalised using the Mudlak-Chamberlain methodology.

The combination of disaggregated debt stocks with aggregate service flows in model (3) provides the most parsimonious setup with the highest predictive power. In this specification (without theory variables) neither growth nor inflation nor foreign reserves are statistically significant, while financial crises and the global risk-free rate (a measure of the overall 'health' of the global economy) have some bearings on default propensity. Additional results (available on request) suggest that the inclusion of (i) a broad measure of political violence, (ii) a dummy for fixed exchange rate (ER) regimes, and (iii) separate dummies for finer definitions of ER regimes do not improve the predictive power of the model, while their coefficients are statistically insignificant. In the following all theory strands are tested against the macro-fundamentals model of column (3), Table E-1. In the following I further report predictive power for a 'bare-bones' model only considering the theory strand proxies.

Reputation The reputation theory is tested in Table E-2 using the measures introduced by Catão and Mano (2017). The inclusion of reputation variables increases predictive power in almost all specifications considered: 'bad reputation' (share of years in default) is highly significant on its own and in combination with variations of the 'years since default' measure. Reputation clearly matters as a disciplining devide for sovereigns: a higher share of years in default is associated with a lower propensity to default, but sovereigns feel less constrained the further in the past their last default occurred. The predictive power of the bare-bones theory variable(s) reported in the final row of the table is consistently high at around .7.

What if (bad) reputation manifests itself through the (in)ability to borrow funds abroad or to accumulate foreign reserves? In separate analysis (results available on request) I interact the debt stock and foreign reserve ratio variables with 'bad reputation' and 'years since default', corresponding to the specifications in columns (2) to (5) of Table E-2. None of these interaction terms between reputation and macro-fundamentals are statistically significant, and none of the augmented models increases predictive power. Reputational effects thus appear to be primarily

affecting the default 'decision' *directly*, rather than through macroeconomic fundamentals.

Threats/Incentives Table E-3 investigates the 'direct punishment' theory strand, relying on measures of preferential trade networks and relative trade costs. All of the models tested have at least some statistically significant covariates, and the signs for the RTA count measures are in line with the hypothesis that higher trade integration makes countries less worried about post-default reprisal. Only the trade cost variable for the top-5 creditors has the hypothesised negative sign. Models combining the two measures in (6)-(8) have the highest predictive power when macro fundamentals are included, note however that the AUROC for these models without controls ('bare bones models') is substantially lower than in the reputation strand analysis.

With regards to interaction effects between punishment variables and macrofundamentals to capture alternative channels through which this theory strand may affect default, none of the models tested increased the predictive power and none of the interactions (private and official debt, foreign exchange ratio) were statistically signifcant (results available on request).

Domestic Politics The importance of political regime and domestic politics is tested in Table E-4 where the availability of Polity IV data constrains the sample to around 100 countries and 163 defaults. Predictive power is lower here and not all models improve on the macro-fundamentals benchmark in column (1). Model (5) including the Polity score as well as the measure for regime durability has the best predictive power for the full sample models: a higher Polity score and longer regime durability are associated with higher default propensity, though the estimate for the former is not statistically significant. Predictive power is even better for the change in government stability variable in (8) but data availability reduces the sample by almost 50%.

Analogously to the previous analysis I considered whether the influence of domestic politics could be detected in the relevances of macroeconomic fundamentals, interacting indicators with the debt stock and reserve ratio variables: a repressive political regime may pursue an odious debt cycle (Jayachandran and Kremer, 2006), whereas democratic regimes cannot adopt a 'devil-may-care' attitude to run-away public borrowing since the electorate have the power to vote out the government. These extensions to the models presented in columns (6) and (7) however did not yield any new insights (results on request): the variable of interest and its interaction terms were all jointly insignificant and while predictive power improves over the simpler models in (2) and (3) it does not improve over the more parsimonious model in (5).

The predictive power of the 'bare-bones' theory variables for domestic politics is comparable to that of the direct punishment variables, thus substantially below the reputation proxies.

Spillovers Tables E-5 and E-6 offer results for proxies of spillovers in the prediction of debt default — it should be noted that a distinction between strategic and nonstrategic default in response to events initiated outside a sovereign's economy cannot be determined here. Following the macro-fundamentals benchmark in column (1) the next four models are variations on default counts and sample share in default — in all cases the variable constructed excludes country i. The final three columns instead present results where spillovers are modelled using

cross-section averages. All models presented have significantly improved predictive power compared with a macro-fundamentals model in (1).

Higher count of defaults or higher sample share of defaults — in both cases the variables are MA(3) transformed like all other covariates — is associated with higher default propensity. Interactions between these two theory variables and macro-fundamentals in (3) and (5) do not improve predictive power and therefore the simpler specifications are preferred. Columns (6)-(8) present specifications augmented with cross-section averages (CA) of the five macro-fundamentals with cross-section variation (the risk-free rate is common to all countries) following the methodology in Cesa-Bianchi et al. (2010). As was suggested in the discussion of the variable construction, the models in (7) and (8) are in principle preferable to that in (6) since they allow potential spillovers to *vary* across countries, albeit requiring that spillover channels are linked to trade and financial development, respectively. The improved predictive power of model (6) however suggests that this specification captures the spillovers better than the alternative models. The model in (6) is also preferable to those in (2) and (4), suggesting the 'global' cross-section averages provide a better proxy for spillovers — note that 'global' in this case refers to the sample of 114 developing countries.

The latter point raises the question whether it is appropriate to ignore the goings-on in the rich OECD countries which are not included in the sample. In the following I allow for core-periphery spillovers in the spirit of Kaminsky and Vega-Garcia (2016) by augmenting the model with OECD country cross-section averages of the macro-fundamentals.[29] In Table E-6 columns (2) and (3) reprint the columns (6) and (8) from the previous table analysing within-sample spillovers (i.e. possibly ignoring any core-periphery effects), columns (4) and (5) focus on the exclusive effect of OECD spillovers, and columns (6) and (7) combine the two sources of spillovers. This analysis suggests that considering OECD-spillovers alongside those from countries in the sample does significantly improve predictive power and highlights the significance of GDP growth, inflation, and foreign reserves in developing countries and inflation in OECD countries for default prediction. Debt stock and service flows on the other hand do not seem to have significant bearings on default. Again all models presented in the Table improve on the macro-fundamentals benchmark specification, but only the specification with within-sample CA in (2) and the two final models adopting OECD and witin-sample CA improve predictive power over the much simpler model in Column (2) of Table E-5. All bare-bones theory model have high AUROCs, which in case of the preferred CA model in (6) reaches .75.

[29] Note that the use of the default count or share variables from models in Table E-5 alongside OECD-only equivalents would not be a fruitful exercise due to the sparsity of defaults in OECD economies.

Table E-1: Macroeconomic Fundamentals

Estimator	RE-Logit Mundlak					
Results Reported	Untransformed Logit Results					
	(1)	(2)	(3)	(4)	(5)	(6)
External debt stock/GNI	-0.004			-0.003	-0.005	-0.005
	(1.09)			(1.00)	(1.38)	(1.38)
Private Creditors		0.038	0.045			
		(2.77)***	(3.36)***			
Official Creditors		-0.004	-0.006			
		(0.91)	(1.91)*			
External debt service/GNI	0.203		0.093	0.202	0.225	0.204
	(4.77)***		(1.96)**	(4.81)***	(4.73)***	(4.23)***
Private Creditors		0.162				
		(2.16)**				
Official Creditors		0.024				
		(0.25)				
Real GDP growth	-0.036	-0.031	-0.030	-0.035	-0.060	-0.049
	(1.24)	(1.12)	(1.08)	(1.21)	(1.75)*	(1.62)
Inflation	0.006	0.006	0.006	0.006	0.003	0.005
	(1.12)	(1.20)	(1.21)	(1.12)	(0.41)	(0.85)
Foreign Reserves/GDP	-0.021	-0.012	-0.015	-0.021	-0.015	-0.021
	(1.59)	(0.84)	(1.05)	(1.63)	(1.28)	(1.59)
Risk-Free Rate	0.159	0.146	0.149	0.159	0.165	0.170
	(3.84)***	(3.67)***	(3.74)***	(3.85)***	(3.56)***	(4.03)***
Banking Crisis	1.601	1.894	1.814	1.582	1.517	1.388
	(1.55)	(1.91)*	(1.85)*	(1.53)	(1.25)	(1.24)
Currency Crisis	1.599	1.380	1.335	1.610	1.597	1.975
	(2.07)**	(1.70)*	(1.63)	(2.08)**	(1.78)*	(2.48)**
CA Balance/GDP				-0.054		
				(0.23)		
Terms of Trade gap					0.231	
					(0.31)	
ΔPrivate credit/GDP						0.012
						(1.48)
Observations	2,852	2,826	2,826	2,852	2,446	2,727
Countries	114	113	113	114	105	112
Crises	171	171	171	171	152	162
Log L	-585.01	-574.62	-575.41	-584.68	-518.66	-556.52
AUROC	0.708	0.724	0.727	0.709	0.696	0.708
se(AUROC)	0.023	0.022	0.022	0.023	0.025	0.023
FE: Wald χ^2 p-value ♯	0.002	0.000	0.000	0.002	0.001	0.006
AUROC = Model (1): p-value		0.055	0.012	0.491	0.229	0.091

Notes: Absolute t-statistics in parentheses, based on standard errors clustered at the country level. ♯ Wald test for the insignificance of the within-country averages (fixed effects). ♮ This test compares the ROC with that in Model (1). Inclusion of sets of dummies for (i) fixed ER regimes and conflict, (ii) a quadratic inflation term, and for (iii) a finer disaggregation of ER regimes, respectively, are rejected by the data. Variables are winsorized (except dummies) and transformed into MA(3) processes.

Table E-2: Reputation

Estimator	RE-Logit Mundlak						
Results Reported	Untransformed Logit Results						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rolling share of years in default		-1.824 (2.11)**		-1.852 (1.80)*	-2.181 (0.19)	-2.587 (0.63)	-3.532 (3.43)***
Years since default (a) Untransformed			0.102 (4.84)***	0.098 (3.71)***			
Years since default (b) Exp discount 1.2%					1.306 (1.00)		
Years since default (c) Exp discount 15%						-0.807 (0.25)	
Years since default (d) Quasi-hyp discount							-0.732 (2.21)**
Controls	×	×	×	×	×	×	×
Observations	2,826	2,826	2,826	2,826	2,826	2,826	2,826
Countries	113	113	113	113	113	113	113
Crises	171	171	171	171	171	171	171
Log L	-575.41	-563.55	-564.45	-553.12	-558.49	-559.86	-556.49
AUROC	0.727	0.747	0.743	0.762	0.751	0.758	0.763
se(AUROC)	0.022	0.022	0.021	0.021	0.022	0.021	0.020
FE: Wald χ^2 ♯	35.82	74.76	38.17	76.50	63.08	76.18	97.03
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = Model (1): p-value		0.019	0.108	0.006	0.009	0.001	0.000
AUROC (no controls)		0.696	0.557	0.709	0.683	0.692	0.686

Notes: Absolute t-statistics in parentheses, based on standard errors clustered at the country level. ♯ Wald test for the insignificance of the within-country averages (fixed effects). ♮ This test compares the ROC with that in Model (1). Column (1) reports the sample and diagnostics for a model consisting only of a parsimonious set of macro control variables (private and official external debt stock/GNI, External debt service/GNI, Real GDP growth, Inflation, Foreign Reserves/GDP, Risk-Free Rate, banking crisis dummy, currency crisis dummy). Variables are winsorized (except dummies) and transformed into MA(3) processes. ♯ Wald test for the insignificance of the within-country averages (fixed effects). Adopting a test of equality between ROC areas all models augmented with 'reputation' variables in columns (2)-(7) reject this null with reference to the benchmark macro fundamentals model in column (1). By the same measure/test, Model (4) improves on Model (3), but not on Model (2); model (7) in contrast improves on (2) and (3) (respective p-values .007, .059) and is the preferred model. Models interacting the reputation variables with private and official debt stock as well as foreign reserves do not improve the model fit and yield statistically insignificant coefficient on the interaction terms. The final row of the table reports the AUROC for the reputation models without any macroeconomic controls.

Table E-3: Threats and Incentives (Direct Punishment)

Estimator	RE-Logit Mundlak							
Results Reported	Untransformed Logit Results							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RTA count		0.010 (1.79)*						
Trade-weighted RTA count			0.932 (2.55)**		0.980 (2.69)***		1.117 (2.95)***	1.101 (2.90)***
Relative trade costs				0.850 (1.73)*		1.045 (1.95)*	1.202 (2.35)**	1.235 (2.37)**
Relative trade costs w/ top-5 creditors					-0.034 (0.62)	-0.083 (1.92)*		-0.051 (0.92)
Controls	×	×	×	×	×	×	×	×
Observations	2,798	2,726	2,719	2,798	2,719	2,798	2,719	2,719
Countries	112	112	112	112	112	112	112	112
Crises	167	166	166	167	166	167	166	166
Log L	-563.34	-555.88	-554.36	-559.73	-550.74	-556.53	-548.28	-546.22
AUROC	0.727	0.730	0.730	0.737	0.741	0.744	0.742	0.747
se(AUROC)	0.022	0.022	0.023	0.022	0.022	0.021	0.022	0.021
FE: Wald χ^2 ♯	35.92	38.49	48.11	45.80	60.40	56.41	65.98	81.97
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = (1): p-value		0.397	0.528	0.077	0.036	0.012	0.050	0.012
AUROC (no controls)		0.534	0.549	0.565	0.591	0.587	0.600	0.609

Notes: Absolute t-statistics in parentheses, based on standard errors clustered at the country level. Column (1) reports the sample and diagnostics for a model consisting only of a parsimonious set of macro control variables (private and official external debt stock/GNI, External debt service/GNI, Real GDP growth, Inflation, Foreign Reserves/GDP, Risk-Free Rate, banking crisis dummy, currency crisis dummy). Variables are winsorized (except dummies) and transformed into MA(3) processes. ♯ Wald test for the insignificance of the within-country averages (fixed effects). The models in (5) and (6) increase predictive power relatitve to the simpler versions in (3) and (4) and are therefore the preferred models, whereas (8) does not improve on either of them; (7) is an improvement on (3) but not on (4). Models interacting the punishment variables with private and official debt stock as well as foreign reserves do not improve the model fit and yield statistically insignificant coefficient on the interaction terms. The last row of the table reports the AUROC for the direct punishment models without any macroeconomic controls.

Table E-4: Domestic Politics

Estimator	RE-Logit Mundlak							
Results Reported	Untransformed Logit Results							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy		0.237				0.354		
		(0.89)				(0.84)		
Democracy × Private Debt Stock/GNI						-0.006		
						(0.38)		
Democracy × Official Debt Stock/GNI						-0.002		
						(0.28)		
Democracy × Foreign Reserves/GDP						0.008		
						(0.31)		
Revised polity score			0.023		0.029		0.023	
			(1.03)		(1.31)		(0.83)	
Polity score × Private Debt Stock/GNI							-0.000	
							(0.23)	
Polity score × Official Debt Stock/GNI							-0.000	
							(0.08)	
Polity score × Foreign Reserves/GDP							0.002	
							(0.74)	
Regime durability				0.011	0.015			
				(1.26)	(1.71)*			
ΔGovernment stability								0.581
								(0.83)
Controls	×	×	×	×	×	×	×	×
Observations	2,554	2,554	2,554	2,554	2,554	2,554	2,518	1,317
Countries	103	103	103	103	103	103	100	77
Crises	163	163	163	163	163	163	162	81
Log L	-541.88	-540.46	-539.61	-539.61	-535.89	-537.60	-528.84	-260.49
AUROC	0.721	0.728	0.729	0.728	0.741	0.736	0.743	0.762
se(AUROC)	0.023	0.022	0.022	0.022	0.021	0.022	0.022	0.027
FE: Wald χ^2 ♯	33.55	35.24	35.93	41.13	45.59	22.15	53.93	37.80
p-value	0.000	0.000	0.000	0.000	0.000	0.014	0.000	0.000
Interaction: Wald χ^2 ♯						0.34	0.67	
p-value						0.953	0.881	
AUROC = Model (1): p-value		0.097	0.074	0.167	0.007	0.020	0.013	0.312
AUROC (no controls)		0.549	0.562	0.557	0.606	0.653	0.596	0.616

Notes: Absolute t-statistics in parentheses, based on standard errors clustered at the country level. Column (1) reports the sample and diagnostics for a model consisting only of a parsimonious set of macro control variables (private and official external debt stock/GNI, External debt service/GNI, Real GDP growth, Inflation, Foreign Reserves/GDP, Risk-Free Rate, banking crisis dummy, currency crisis dummy). Variables are winsorized (except dummies) and transformed into MA(3) processes. ♯ Wald test for the insignificance of the within-country averages (fixed effects). The model in (5) improves on both (3) and (4) in terms of predictive power — this is the preferred model. The last row of the table reports the AUROC for the domestic politics models without any macroeconomic controls.

Table E-5: Spillovers

Estimator	RE-Logit Mundlak							
Results Reported	Untransformed Logit Results							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CA-Interaction						none	open$_{it}$	findev$_{it}$
Count of default events elsewhere (DC)		0.170 (6.12)***	0.207 (3.99)***					
DC × Private Debt Stock/GNI			-0.001 (0.69)					
DC × Official Debt Stock/GNI			-0.001 (1.38)					
DC × Foreign Reserves/GDP			0.002 (0.63)					
Sample share in default (DS)				0.019 (2.26)**	0.046 (3.71)***			
DS × Private Debt Stock/GNI					-0.000 (0.55)			
DS × Official Debt Stock/GNI					-0.000 (1.68)*			
DS × Foreign Reserves/GDP					-0.002 (2.24)**			
CA External debt stock/GNI						-0.067 (1.31)	-0.043 (0.85)	-0.000 (0.62)
CA External debt service/GNI						1.276 (1.55)	0.271 (0.33)	0.003 (0.34)
CA Real GDP growth						0.683 (2.96)***	0.468 (2.89)***	-0.001 (0.77)
CA Inflation						0.057 (2.61)***	0.006 (0.23)	0.000 (0.66)
CA Foreign Reserves/GDP						0.204 (2.92)***	0.041 (0.77)	0.001 (1.22)
Controls	×	×	×	×	×	×	×	×
Observations	2,529	2,529	2,529	2,529	2,529	2,529	2,062	2,529
Countries	112	112	112	112	112	112	111	112
Crises	148	148	148	148	148	148	133	148
Log L	-511.55	-491.11	-484.73	-505.57	-500.41	-475.58	-423.55	-487.68
AUROC	0.716	0.756	0.770	0.727	0.739	0.785	0.770	0.771
se(AUROC)	0.023	0.020	0.020	0.022	0.022	0.020	0.022	0.019
FE: Wald χ^2 ♯	29.75	73.98	64.02	42.09	29.61	54.43	55.18	77.48
p-value	0.000	0.000	0.000	0.000	0.001	0.000	0.000	0.000
AUROC = (1): p-value		0.000	0.000	0.001	0.000	0.000	0.003	0.000
CA: Wald χ^2 ♯						15.76	20.31	23.50
p-value						0.008	0.001	0.000
AUROC (no controls)		0.676	0.737	0.639	0.715	0.710	0.699	0.683

Notes: Absolute t-statistics in parentheses, based on standard errors clustered at the country level. A test for the null of equality of the AUROC statistic between (2) and (4) is statistically significant, $p = .01$, thus in favour of (2). Model (6) is however preferred to model (2) by the same measure, while (7) and (8) are not. Taking all of this into account the model in (6) is preferred. Interaction models: A ROC comparison test suggests the two interaction models in (3) and (5) do not improve on the simpler models in (2) and (4). ♯ Wald test for the insignificance of the within-country averages (fixed effects). ♮ Wald test for the insignificance of the 5 cross-section average terms. 'CA' indicates cross-section averages across across the 114 sample countries — the row 'CA-Interaction' near the top of the table highlights whether these CA have been interacted with trade openness or financial development, if at all. A separate Wald test indicates joint insignificance of the CA terms (rejected in all three models). The last row of the table reports the AUROC for the spillover models without any macroeconomic controls.

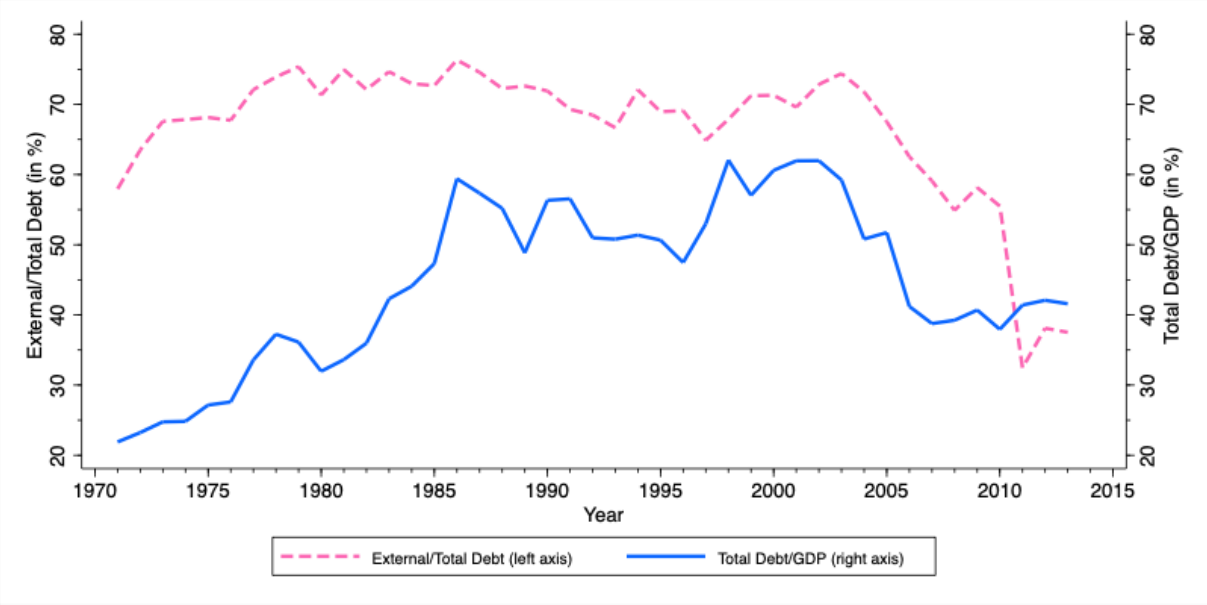
Table E-6: Spillovers (Two Sources)

Estimator	RE-Logit Mundlak						
Results Reported	Untransformed Logit Results						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CA-Interaction		none	findev$_{it}$	none	findev$_{it}$	none	findev$_{it}$
Within-Sample (Developing Economies) CA							
CA External debt stock/GNI		-0.067	-0.000			-0.033	-0.000
		(1.31)	(0.62)			(0.51)	(0.77)
CA External debt service/GNI		1.276	0.003			1.369	0.004
		(1.55)	(0.34)			(1.48)	(0.56)
CA Real GDP growth		0.683	-0.001			0.574	-0.002
		(2.96)***	(0.77)			(2.05)**	(0.89)
CA Inflation		0.057	0.000			0.056	-0.000
		(2.61)***	(0.66)			(1.90)*	(0.36)
CA Foreign Reserves/GDP		0.204	0.001			0.264	0.002
		(2.92)***	(1.22)			(1.69)*	(2.53)**
OECD-Country CA							
CA Total debt/GDP				0.064	0.001	0.045	0.000
				(2.62)***	(3.35)***	(0.99)	(1.27)
CA Real GDP growth				-0.057	0.000	0.086	0.002
				(0.44)	(0.21)	(0.49)	(1.29)
CA Inflation				0.128	0.001	0.325	0.001
				(2.16)**	(2.23)**	(3.14)***	(1.22)
CA Foreign Reserves/GDP				-0.679	-0.006	-0.235	-0.007
				(2.45)**	(2.47)**	(0.69)	(2.41)**
Controls	×	×	×	×	×	×	×
Log L	-511.55	-475.58	-487.68	-491.82	-491.46	-468.58	-478.97
AUROC	0.716	0.785	0.771	0.772	0.763	0.798	0.791
se(AUROC)	0.023	0.020	0.019	0.019	0.020	0.019	0.018
FE: Wald χ^2 ♯	29.75	54.43	77.48	54.69	86.01	82.33	135.67
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = Table E-5 (2): p-value		0.044	0.183	0.275	0.602	0.009	0.010
AUROC (no controls)		0.710	0.683	0.713	0.700	0.748	0.725

Notes: The sample for all models is 148 defaults in 112 countries (529 observations). Absolute t-statistics in parentheses, based on standard errors clustered at the country level. Column (1) reports the sample and diagnostics for a model consisting only of a parsimonious set of macro control variables (External debt stock/GNI, External debt service/GNI, Real GDP growth, Inflation, Foreign Reserves/GDP, Risk-Free Rate). CA indicates cross-section averages across panels: in the upper part all variables are averaged across the 114 sample countries, in the lower part all variables are averaged across OECD countries (which do *not* form part of the sample). In the latter case it was necessary to adopt 'total debt/GDP' instead of the two external debt variables due to data availability. The row 'CA-Interaction' near the top of the table highlights whether these respective CA series have been interacted with a measure of country-specific financial development or not. The addition of the OECD CA in (6) and (7) statistically (significant at the 5% level) improves on the AUROC for the respective models in (2) and (3) as well as (4) and (5): inclusion of *both* within-sample- and OECD-spillovers produces the best predictions. All models presented have improved predictive power over the model in (1), I therefore present a ROC model comparison with the parsimonious model (2) from the previous table (count of default events elsewhere) — here the models in (2), (6) and (7) represent improvements. Note further that model (6) is the only model considered here which also improves on model (5) in Table E-5 ($p = 0.02$) — it is thus the preferred spillover model. ♯ Wald test for the insignificance of the within-country averages (fixed effects). The last row of the table reports the AUROC for the spillover models without any macroeconomic controls.

F Robustness Checks — Alternative Debt Definitions

Figure F-1: Evolution of Total Debt/GDP and External/Total Debt



Notes: The solid line shows the evolution of median total debt stock/GDP, the dashed the external/total debt ratio for my sample of 112 countries over the 1970-2015 time horizon. All values presented are in percent.

Domestic debt Recent work by Reinhart and Rogoff (2011a) has highlighted the relative ignorance in the literature on default with regards to the relevance of domestic debt, while Panizza (2008) has pointed out the recent shift away from external and toward domestic sources of public finance among developing and emerging economies. In historical analysis over several centuries Reinhart and Rogoff (2011a) suggest that accounting for domestic debt can help solve the puzzle of sovereign defaults at relatively low levels of external debt, especially for the case of emerging economies.[30] In the analysis in the maintext, the (in)significance of indebtedness may thus be distorted if substantial (and/or shifting) proportions of public debt are domestic and hence ignored in that setup. Panizza (2008) discusses the difficulties arising in classifying public debt as domestic or external, and against the background of the recent switch from external to domestic borrowing cautions against complacency: "the real source of vulnerabilities are maturities and currency mismatches...and countries that are switching from external to domestic debt could be trading a currency mismatch for a maturity mismatch" (2). Although there are a number of additional threats arising from this observed switch, he views the general trend as having a positive effect on reducing the potential for sovereign defaults. While I do not attempt to capture a structural break in the default predictors after 2004 (the year when the external/total debt ratio starts dipping), these arguments again underline the potential bias arising from a narrow focus on external borrowing.

I adopt total public debt/GDP and the external/total debt ratio from Catão and Mano (2017) which in turn is based on data from the IMF, World Bank and Abbas et al. (2010). As

[30] For instance, in my sample around 22% of default events are characterised by a lower median external debt/GDP ratio for the defaulters than non-defaulters in that year, conspicuously so in the late 1990s — see also Reinhart and Rogoff (2009).

Panizza (2008) points out there are a number of shortcomings with the definition of domestic versus external debt associated with these data, but as a simple extension to my analysis and in the interest of maintaining a large sample of developing and emerging countries I adopt these measures here and acknowledge the caveats raised. Figure F-1 charts the median evolution of total public debt (solid line, right scale) and the external to total debt ratio (dashed line, left scale) in the sample of (now) 112 countries. Total debt evolution displays the same hump-shaped pattern as the median *external* debt stock series presented in Figure 2 in the maintext. The external debt ratio measure however provides some interesting patterns: having accounted for between 60 and 70% of total debt for three decades, the external debt share began to decline in the early 2000s, with the aftermath of the Global Financial Crisis accounting for a particularly dramatic drop. The latest available data for 2013 suggest that external debt accounts for *less than 40%* in the average developing country in my sample.

Table F-1 presents results for the specification when I include total debt and the external to total debt ratio instead of external debt and debt service flows. Diagnostic tests suggest that the punishment and domestic politics strands are only borderline significant, while reputational and spillover effects have significant predictive power. An increase in the total debt stock is associated with increased default propensity, but the magnitude of this effect is moderate compared with the aforementioned theory strands. At least in this specification the split between external and domestic debt bears no economic or statistical significance, possibly due to the relatively recent shift in proportions. It bears emphasising that the distinction between domestic and external debt adopted in the data used is far from perfect, and that further analysis with alternative data is necessary to confirm these findings.

External Debt in Net Present Value A different concern about the debt burden relates to the fact that the face value of debt as applied above can be misleading since sovereigns can borrow at different maturities and contractual forms. One prominent concern here is that low-income countries typically borrow from official lenders at low long-term rates, whereas middle-income countries borrow from private lenders at market rates (Dias et al., 2014) — as a result, the debt burden of the latter relative to the former may be seriously understated. As was emphasised in the previous paragraphs, mismatched maturities can be a significant source of sovereign vulnerability (Panizza, 2008) and I therefore adopt the external debt stock data at 10% coupon-equivalent face value from Dias et al. (2014), equivalent to the present value of debt cashflows discounted at 10%. These data are available for 100 developing countries for 1979-2006 — in my analysis, due to availability of other data, this reduces to 74 countries experiencing 71 defaults; the number of observations at 967 is just over 40% of that in the main results for external debt in Table 1, so that comparability of results is somewhat hampered.

Table F-2 presents results for present value debt: in the benchmark model in (1) and the alternative in (2) the spillover effects are now all statistically insignificant, though the more elaborate spillover model in (3) provides similar patterns to earlier findings: bad reputation and spillovers matter most, while foreign reserves are the most significant macro-fundamental. Overall, however, evidence for the statistical significance of spillover effects is weaker: the p-values for dropping spillover variables from models (1), (2) or (3) are .165, .836 and .158, respectively. Only the reputation effect emerges from this exercise with a very robust confir-

mation of economic and statistical significance. I emphasise again the difficulties arising for comparison with earlier results from the substantially reduced sample in this analysis.

Private Debt With the rise in private sector debt in total external debt over the past decade and a half, the economics literature has also considered the link between private sector debt and sovereign default. Typically two major mechanisms are suggested: first, a negative effect of private debt on the propensity of default since governments may take on substantial debt from private banks in the wake of financial distress or outright crisis, thus undermining their own solvency (Arellano and Kocherlakota, 2008; Reinhart and Rogoff, 2011b).[31] Second, excessive public debt can undermine the private sector, perhaps due to a nonlinear relationship between public debt and economic growth (Reinhart and Rogoff, 2009), or because recessions are unnecessarily prolonged if the public sector does not have sufficient 'fiscal space' to pursue macro-economic stabilisation policies when the financial sector is in distress (Jordà et al., 2016).

Until recently the sparsity of private debt data prevented empirical analysis beyond a small number of advanced economies (e.g. the Jordà-Schularick-Taylor Macrohistory Database). With the recent publication of the IMF Global Debt Database (GDD: Mbaye et al., 2018) a much wider sample of countries is now available for analysis. In order to maximise coverage I adopt the 'loans and debt securities' definition of private debt along with the 'central government debt' definition of public debt from GDD — both stock variables are defined in terms of GDP. My sample is reduced to 91 countries with 2,113 (1,954) observations, experiencing 126 (112) crises (lower values are for the full spillover specification).

Table F-3 presents results for the regressions taking the model with the default count spillover variable as the benchmark in column (1) and the full spillover specification in (3) as alternative.[32] Column (2) omits all macro-fundamentals and thus confirms their significance by comparison of the resulting AUROC with that for the benchmark model in (1). As before the specifications in (4)-(7) drop one theory strand at a time — in contrast to the previous analysis this suggests that the direct punishment strand is (marginally) significant, whereas the domestic politics strand is (marginally) insignificant. This is reflected in the economic and statistical significance of relative trade costs in all models presented, while results for the domestic politics proxies are mixed. Spillovers measured via default event counts are more moderate in their economic size, though the cross-section averages specification re-establishes the economic supremacy of bad reputation and spillover effects. Regarding the substantial innovation in these regressions of the addition of private debt, it is notable that in contrast to public debt stock this variable is statistically and economically significant in predicting default, with the exception of the elaborate spillover model in (3).

[31] See also the classic narrative of this mechanism for Chile 1977-85 in Diaz-Alejandro (1985).

[32] Note that the model adopting the more parsimonious set of cross-section averages as in all the previous regressions did not converge in the present empirical setup.

Table F-1: Robustness — Total Debt Burden

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-8.677	-4.866	-10.813		-8.768	-8.513	-3.205
	(6.71)***	(4.95)***	(6.62)***		(6.80)***	(5.51)***	(3.39)***
Years since default	0.144	0.941	-0.248		0.262	0.257	1.790
	(0.18)	(1.10)	(0.30)		(0.32)	(0.24)	(1.85)*
Average trade costs	-0.926	-1.014	-0.692	-1.885		-0.848	-0.793
w/ top-5 creditors	(0.83)	(0.88)	(0.71)	(1.21)		(0.65)	(0.79)
Relative trade costs	1.829	1.922	1.875	2.335		1.853	1.911
	(1.73)*	(1.68)*	(1.69)*	(1.72)*		(1.53)	(1.67)*
Polity Score	0.676	1.912	0.836	-0.402	0.659		2.749
	(0.55)	(1.89)*	(0.66)	(0.32)	(0.53)		(2.67)***
Regime durability	-0.204	0.934	-1.014	0.549	-0.112		1.813
	(0.17)	(0.82)	(0.82)	(0.40)	(0.09)		(1.70)*
CA Inflation	1.247		2.429	1.226	1.279	1.331	
	(2.55)**		(2.12)**	(1.97)**	(2.69)***	(2.30)**	
CA Foreign Reserves/GDP	10.032		8.415	7.511	9.963	10.022	
	(7.58)***		(2.78)***	(4.59)***	(7.55)***	(5.66)***	
CA External debt stock/GNI			0.761				
			(0.19)				
CA External debt service/GNI			4.017				
			(1.25)				
CA Real GDP growth			2.634				
			(1.74)*				
OECD CA Inflation	-2.371		6.778	2.986	-2.448	-2.469	
	(1.78)*		(3.06)***	(2.32)**	(1.83)*	(1.62)	
OECD CA Foreign	-2.374		-2.320	-1.818	-2.483	-2.434	
Reserves/GDP	(2.19)**		(1.57)	(1.50)	(2.27)**	(2.10)**	
OECD CA Total debt/GDP			8.818				
			(2.34)**				
OECD CA Real GDP growth			0.297				
			(0.24)				
Count of default		3.065					
events elsewhere (DC)		(6.23)***					
Total public	0.990	1.372	0.634	0.682	1.064	1.080	0.652
debt/GDP	(1.67)*	(2.32)**	(0.90)	(0.88)	(1.77)*	(1.34)	(1.02)
External/Total	-0.072	-0.500	-0.113	-0.123	-0.066	-0.111	-1.345
public debt	(0.07)	(0.56)	(0.11)	(0.11)	(0.06)	(0.10)	(1.48)
Real GDP growth	-0.543	-0.574	-0.523	-0.478	-0.422	-0.555	-0.814
	(0.86)	(0.77)	(0.73)	(0.63)	(0.68)	(0.83)	(1.14)
Inflation	-1.057	-0.018	-1.351	-0.193	-0.781	-0.946	-0.039
	(1.43)	(0.02)	(1.78)*	(0.25)	(1.14)	(1.13)	(0.05)
Foreign Reserves/GDP	-3.752	-2.893	-3.678	-3.811	-3.837	-3.582	-1.585
	(2.78)***	(2.44)**	(2.77)***	(2.10)**	(2.60)***	(2.36)**	(1.49)
Risk-Free Rate	5.587	0.348	1.880	3.049	5.498	5.609	3.101
	(6.19)***	(0.45)	(1.80)*	(3.38)***	(6.06)***	(5.30)***	(4.22)***
Standard errors	Cluster	Cluster	Cluster	Jack	Cluster	Jack	Cluster
LogL	-420.29	-434.72	-403.21	-449.51	-421.65	-423.81	-451.43
AUROC (se)	0.821 (.019)	0.799 (.020)	0.851 (.017)	0.779 (.020)	0.814 (.020)	0.816 (.019)	0.768 (.022)
FE: χ^2 or F p-value ♯	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = (1): p-value		0.164	0.002	0.002	0.152	0.121	0.002

Notes: All results are for 100 countries with 2,122 observations and 129 crises. The estimates presented are marginal effects multiplied by 100 times the variable standard deviation. Indicators for banking and currency crises are included but not reported. Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit clustered at country level; jackknife; jackknife where indicated where indicated). Models (4)-(7) highlight the significance of each of the four theories. ♯ Wald test for the insignificance of the within-country averages. ‡ This indicates the joint significance of each 'theory' in model (1). ♮ This test compares the ROC with that of Model (1); in (4)-(7) rejection indicates that the benchmark model is preferred and that the theory variables omitted 'matters'.

Table F-2: Robustness — External Debt in Present Value Terms

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-13.701	-11.817	-18.088		-13.102	-12.898	-11.481
	(4.18)***	(4.82)***	(4.45)***		(4.47)***	(3.11)***	(4.95)***
Years since default	-0.273	-0.375	-0.465		-0.040	0.030	-0.293
	(0.21)	(0.31)	(0.35)		(0.03)	(0.02)	(0.24)
Average trade costs	-3.776	-3.886	-2.916	-3.035		-3.542	-3.535
w/ top-5 creditors	(2.28)**	(2.61)***	(1.81)*	(1.89)*		(1.08)	(2.43)**
Relative trade costs	1.555	1.723	1.129	0.756		1.922	1.331
	(0.75)	(0.85)	(0.58)	(0.29)		(0.84)	(0.64)
Polity Score	0.944	1.568	1.165	0.792	1.227		1.661
	(0.57)	(1.02)	(0.71)	(0.35)	(0.75)		(1.10)
Regime durability	0.147	0.918	-2.103	1.043	0.681		0.541
	(0.06)	(0.36)	(0.75)	(0.36)	(0.29)		(0.21)
Count of default		-1.625					
events elsewhere (DC)		(0.88)					
CA Inflation	1.866		7.784	3.038	2.111	2.282	
	(1.32)		(3.39)***	(1.85)*	(1.58)	(1.28)	
CA Foreign Reserves/GDP	-0.256		4.628	-2.461	-1.052	-0.487	
	(0.11)		(1.33)	(0.74)	(0.42)	(0.16)	
CA External NPV debt stock/GDP			14.461				
			(3.08)***				
CA External debt service/GNI			-3.493				
			(1.13)				
CA Real GDP growth			2.472				
			(1.41)				
OECD CA Inflation	-8.038		7.517	4.060	-9.232	-6.508	
	(1.50)		(0.71)	(0.86)	(1.75)*	(1.11)	
OECD CA Foreign	1.587		2.196	8.514	0.717	2.987	
Reserves/GDP	(0.28)		(0.28)	(1.25)	(0.13)	(0.50)	
OECD CA Total debt/GDP			8.938				
			(1.56)				
OECD CA Real GDP growth			-3.008				
			(2.21)**				
External debt	2.912	2.376	3.159	1.674	3.346	3.087	2.653
stock (PV)/GDP	(1.63)	(1.39)	(2.08)**	(0.62)	(1.86)*	(1.38)	(1.59)
External debt service/GNI	-0.310	0.257	-1.553	2.259	-0.332	-0.110	0.508
	(0.13)	(0.11)	(0.78)	(0.87)	(0.14)	(0.04)	(0.24)
Real GDP growth	-0.126	-0.575	-0.717	-0.059	-0.393	-0.152	-0.565
	(0.08)	(0.38)	(0.43)	(0.04)	(0.29)	(0.09)	(0.40)
Inflation	-1.199	-0.613	-1.049	-0.656	-0.360	-1.107	-0.498
	(0.97)	(0.46)	(0.69)	(0.53)	(0.31)	(0.63)	(0.37)
Foreign Reserves/GDP	-7.976	-6.841	-7.225	-9.077	-7.132	-8.671	-6.945
	(2.24)**	(2.24)**	(2.65)***	(1.73)*	(2.21)**	(1.52)	(2.47)**
Risk-Free Rate	5.266	-0.387	5.795	3.757	4.960	4.749	-1.343
	(1.27)	(0.19)	(1.15)	(0.75)	(1.19)	(1.02)	(0.81)
Standard errors	Cluster	Cluster	Cluster	Jack	Cluster	Jack	Cluster
LogL	-182.74	-188.73	-171.36	-201.01	-185.69	-186.58	-190.03
AUROC	0.875	0.858	0.885	0.817	0.864	0.870	0.857
se(AUROC)	0.018	0.022	0.019	0.025	0.020	0.018	0.021
FE: χ^2 p-value ♯	0.000	0.000	0.000	0.048	0.000	0.011	0.000
AUROC = (1): p-value		0.221	0.462	0.001	0.116	0.464	0.165

Notes: All results are for 74 countries with 967 observations and 71 crises (1980-2006). The estimates presented are marginal effects multiplied by 100 times the variable standard deviation. Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit results clustered at the country level; jackknife where indicated). Column (1) is the benchmark (full) model, (2) and (3) report results for alternative spillover variables, models (4)-(7) highlight the (in-)significance of each of the four theories: reputation, threats and incentives, domestic politics, and spillovers. See Table F-1 for further details.

Table F-3: Robustness — Private and Public Debt

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-3.448	-4.760	-12.628		-3.117	-2.744	-2.421
	(3.41)***	(5.88)***	(7.36)***		(2.98)***	(2.45)**	(1.88)*
Years since default	3.314	1.118	0.405		2.840	3.865	4.213
	(2.16)**	(0.86)	(0.22)		(1.85)*	(2.23)**	(2.81)***
Average trade costs	-0.517	-0.522	0.720	-0.715		-0.381	-0.311
w/ top-5 creditors	(0.48)	(0.53)	(0.74)	(0.49)		(0.30)	(0.24)
Relative trade costs	4.000	4.264	2.972	3.447		3.779	3.580
	(3.59)***	(3.16)***	(2.18)**	(2.94)***		(2.75)***	(3.07)***
Polity Score	2.072	0.850	0.415	1.422	1.961		2.013
	(1.97)**	(0.76)	(0.31)	(1.26)	(1.86)*		(1.75)*
Regime durability	0.842	1.260	-1.479	1.827	1.254		0.346
	(1.20)	(1.54)	(1.26)	(2.42)**	(1.43)		(0.16)
Count of default	1.467	3.077		1.115	1.601	1.376	
events elsewhere (DC)	(1.94)*	(7.20)***		(1.78)*	(2.14)**	(1.58)	
CA Private Debt/GDP			10.796				
			(2.27)**				
CA Public Debt/GDP			5.749				
			(4.11)***				
CA Real GDP growth			1.777				
			(1.54)				
CA Inflation			3.985				
			(2.85)***				
CA Foreign Reserves/GDP			-7.704				
			(0.84)				
OECD CA Private Debt/GDP			9.920				
			(1.35)				
OECD CA Public Debt/GDP			2.238				
			(0.38)				
OECD CA Real GDP growth			0.617				
			(0.48)				
OECD CA Inflation			3.279				
			(0.96)				
OECD CA Foreign Reserves/GDP			0.548				
			(0.27)				
Private Debt/GDP	3.369		0.624	4.441	2.600	2.774	3.901
	(2.49)**		(0.44)	(2.91)***	(1.89)*	(1.59)	(2.50)**
Public Debt/GDP	0.969		0.327	0.320	0.991	0.969	0.625
	(1.24)		(0.36)	(0.40)	(1.20)	(1.09)	(0.62)
Real GDP growth	-0.708		-0.837	-0.605	-0.638	-0.796	-0.787
	(1.26)		(1.17)	(0.88)	(1.10)	(1.24)	(1.15)
Inflation	0.662		-0.329	0.571	0.829	0.442	0.590
	(0.85)		(0.57)	(0.73)	(1.17)	(0.43)	(0.74)
Foreign Reserves/GDP	-2.988		-3.194	-3.310	-3.092	-2.544	-2.431
	(2.35)**		(2.51)**	(1.98)**	(2.12)**	(1.73)*	(1.58)
Risk-Free Rate	2.027		5.940	2.269	1.947	1.971	3.138
	(1.81)*		(3.36)***	(2.42)**	(1.71)*	(1.59)	(3.59)***
Standard errors	Cluster	Jack	Cluster	Jack	Cluster	Jack	Jack
LogL	-387.95	-418.21	-323.55	-402.55	-394.82	-394.97	-398.56
AUROC	0.826	0.753	0.857	0.800	0.812	0.815	0.806
se(AUROC)	0.018	0.021	0.017	0.020	0.019	0.018	0.019
FE: χ^2 p-value ♯	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = (1): p-value		0.000	0.012	0.045	0.091	0.113	0.034

Notes: All results are for 91 (in model (3) 90) countries with 2,113 (1,954) observations and 126 (112) crises. The estimates presented are marginal effects multiplied by 100 times the variable SD. Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit results clustered at the country level; jackknife where indicated). Column (1) is the benchmark model, (2) omits the macro controls, and (3) reports results for a set of alternative spillover variables; models (4)-(7) highlight the (in-)significance of each of the four theories: reputation, threats and incentives, domestic politics, and spillovers. See Table F-1 for further details.

G Empirical Results using alternative Spillover Definitions

Table G-1: Robustness check using default counts

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-3.774	-8.040	-10.185		-3.817	-2.843	-2.642
	(3.36)***	(5.61)***	(5.19)***		(3.38)***	(2.69)***	(2.53)**
Years since default	1.737	0.504	0.118		1.687	2.030	2.630
	(2.24)**	(0.60)	(0.14)		(2.21)**	(2.31)**	(3.54)***
Average trade costs	-1.584	-0.688	-0.811	-1.422		-1.488	-1.529
w/ top-5 creditors	(1.24)	(0.51)	(0.58)	(1.07)		(0.87)	(1.37)
Relative trade costs	2.052	1.357	1.248	1.898		2.003	2.486
	(1.46)	(1.30)	(1.08)	(1.35)		(1.31)	(1.89)*
Polity Score	1.890	1.087	1.223	0.507	1.871		2.269
	(1.73)*	(0.88)	(0.96)	(0.51)	(1.68)*		(2.05)**
Regime durability	0.827	-0.314	-0.795	0.839	0.962		0.959
	(0.80)	(0.30)	(0.77)	(0.60)	(0.90)		(1.14)
Count of default	2.588			2.269	2.622	2.591	
events elsewhere (DC)	(4.54)***			(4.10)***	(4.58)***	(4.25)***	
CA Inflation		0.862	2.722				
		(1.77)*	(2.49)**				
CA Foreign Reserves/GDP		9.709	7.676				
		(6.93)***	(2.46)**				
CA External debt stock/GNI			1.084				
			(0.29)				
CA External debt service/GNI			4.260				
			(1.35)				
CA Real GDP growth			3.134				
			(2.12)**				
OECD CA Inflation		-1.761	6.935				
		(1.32)	(3.14)***				
OECD CA Foreign Reserves/GDP		-2.979	-2.173				
		(2.59)***	(1.40)				
OECD CA Total debt/GDP			8.556				
			(2.11)**				
OECD CA Real GDP growth			-0.062				
			(0.05)				
External debt stock/GNI	1.385	0.473	0.676	1.383	1.093	1.498	1.948
Private Creditors	(1.90)*	(0.60)	(0.88)	(1.63)	(1.39)	(1.70)*	(3.12)***
External debt stock/GNI	-0.168	0.527	0.718	-1.554	0.144	-0.318	-0.692
Official Creditors	(0.22)	(0.68)	(0.81)	(2.28)**	(0.19)	(0.38)	(0.94)
External debt service/GNI	1.785	1.495	0.066	1.728	1.809	1.782	1.535
	(2.06)**	(1.56)	(0.07)	(1.95)*	(2.08)**	(1.94)*	(2.03)**
Real GDP growth	-0.781	-0.893	-0.815	-0.674	-0.560	-0.944	-0.924
	(1.29)	(1.37)	(1.18)	(1.01)	(0.94)	(1.42)	(1.55)
Inflation	0.743	-0.661	-0.997	0.857	0.907	0.847	0.495
	(0.73)	(0.67)	(0.98)	(0.87)	(1.03)	(0.76)	(0.55)
Foreign Reserves/GDP	-3.082	-3.758	-3.631	-2.879	-3.189	-2.881	-1.874
	(3.05)***	(3.20)***	(2.97)***	(2.28)**	(3.03)***	(2.36)**	(1.97)**
Risk-Free Rate	0.796	4.918	1.642	1.419	0.738	0.636	2.819
	(0.93)	(5.00)***	(1.49)	(1.69)*	(0.84)	(0.67)	(4.12)***
LogL	-481.53	-420.29	-403.21	-500.20	-484.73	-487.38	-497.93
AUROC(se)	0.81 (.018)	0.83 (.017)	0.85 (.016)	0.78 (.019)	0.81 (.018)	0.80 (.019)	0.79 (.020)
FE: Wald χ^2 p-value ♯	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = (1): p-value ♮		0.054	0.002	0.003	0.227	0.005	0.002
Dto. same sample		0.137	0.004				

Notes: Results for $2,526$ observations ($2,329$ in (2) and (3)) — in 102 countries with 159 (141) crises. Models in (4) and (6) are derived from logit regressions with jackknifed standard errors. See Table 1 in the main text for all other details.

Table G-2: Alternative MA-transformation for alternative spillover specification

Estimator (Results Reported)	RE-Logit Mundlak (Margins × 1st.d. × 100)				
	(1)	(2)	(3)	(4)	(5)
Variable Transformation	1 lag	MA(2)	MA(3)	MA(4)	MA(5)
Bad Reputation	-3.380	-3.197	-3.774	-3.867	-3.933
	(3.06)***	(2.70)***	(3.36)***	(3.69)***	(3.81)***
Years since default	1.053	1.598	1.737	2.086	2.120
	(1.49)	(2.22)**	(2.24)**	(2.82)***	(2.97)***
Average trade costs	1.523	2.099	2.052	2.454	2.401
w/ top-5 creditors	(1.09)	(1.46)	(1.46)	(1.81)*	(1.88)*
Relative trade costs	-1.607	-1.623	-1.584	-2.056	-1.531
	(2.03)**	(1.50)	(1.24)	(1.72)*	(1.65)*
Polity Score	1.734	1.752	1.890	2.050	2.181
	(1.78)*	(1.69)*	(1.73)*	(1.95)*	(2.11)**
Regime durability	0.897	0.737	0.827	0.687	0.853
	(0.89)	(0.70)	(0.80)	(0.66)	(0.92)
Count of default	2.518	2.179	2.588	2.348	1.782
events elsewhere	(5.25)***	(4.27)***	(4.54)***	(4.34)***	(3.37)***
External debt stock/GNI	0.485	0.500	0.471	0.936	0.894
Private Creditors	(0.69)	(0.66)	(0.60)	(1.22)	(1.36)
External debt stock/GNI	1.396	1.593	1.385	1.608	1.420
Private Creditors	(2.19)**	(2.23)**	(1.90)*	(2.32)**	(2.50)**
External debt stock/GNI	0.424	-0.021	-0.168	-0.313	-0.575
Official Creditors	(0.62)	(0.03)	(0.22)	(0.38)	(0.75)
External debt service/GNI	1.726	1.652	1.785	1.487	1.313
	(2.64)***	(1.99)**	(2.06)**	(1.53)	(1.53)
Real GDP growth	-1.157	-1.064	-0.781	-0.685	-0.293
	(2.28)**	(1.85)*	(1.29)	(1.16)	(0.54)
Inflation	0.640	0.511	0.743	0.828	1.206
	(0.76)	(0.51)	(0.73)	(0.92)	(2.01)**
Foreign Reserves/GDP	-3.432	-3.341	-3.082	-2.825	-2.626
	(3.08)***	(3.41)***	(3.05)***	(2.90)***	(3.08)***
Risk-Free Rate	0.571	1.031	0.796	1.169	1.181
	(0.75)	(1.21)	(0.93)	(1.39)	(1.43)
Observations	2,491	2,512	2,526	2,557	2,573
Countries	102	102	102	102	102
Crises	159	159	159	159	159
Log L	-479.23	-482.77	-481.53	-489.48	-497.53
AUROC	0.818	0.815	0.813	0.801	0.795
se(AUROC)	0.017	0.017	0.018	0.019	0.019
FE: Wald χ^2 ♯	153.33	160.74	201.69	208.38	201.01
p-value	0.000	0.000	0.000	0.000	0.000

Notes: The Table presents results for the specification in Column (2) of Table 1, varying the dynamic transformation of all right hand-side variables from a single lag in (1), to moving average transformation for $t-1$ to $t-k$ with $k = 2, 3, 4, 5$ in the remaining columns. For all other details see notes to Table 1.

H Empirical Results using simple Logit estimator

Table H-1: Main Results using Logit with Country FE

Estimator (Results Reported)	Logit with Country FE (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
Bad Reputation	-12.083	-7.501	-15.807		-12.168	-11.345	-4.730
	(4.93)***	(3.63)***	(4.99)***		(5.05)***	(4.66)***	(2.54)**
Years since default	0.062	2.285	-0.776		0.200	0.105	4.096
	(0.04)	(1.76)*	(0.50)		(0.14)	(0.08)	(3.24)***
Average trade costs	-1.303	-0.776	-1.218	-1.706		-1.332	-0.711
w/ top-5 creditors	(0.90)	(0.59)	(0.84)	(1.00)		(0.83)	(0.54)
Relative trade costs	2.159	1.947	2.104	2.466		2.304	2.220
	(1.69)*	(1.58)	(1.63)	(2.14)**		(1.78)*	(1.64)*
Polity Score	2.177	3.528	2.128	0.253	2.224		4.027
	(1.15)	(1.90)*	(1.09)	(0.16)	(1.14)		(2.06)**
Regime durability	-0.482	0.497	-1.586	-0.184	-0.568		0.923
	(0.28)	(0.29)	(0.89)	(0.10)	(0.33)		(0.54)
CA Inflation	0.935		2.850	1.068	0.903	1.006	
	(1.30)		(1.87)*	(1.30)	(1.25)	(1.41)	
CA Foreign Reserves/GDP	14.268		12.593	10.050	14.162	14.488	
	(6.25)***		(2.90)***	(6.54)***	(6.46)***	(6.78)***	
CA External debt stock/GNI			2.284				
			(0.44)				
CA External debt service/GNI			5.262				
			(1.22)				
CA Real GDP growth			3.477				
			(1.73)*				
OECD CA Inflation	-2.308		9.950	4.035	-2.378	-2.604	
	(1.13)		(3.37)***	(2.28)**	(1.17)	(1.28)	
OECD CA Foreign Reserves/GDP	-3.526		-2.970	-2.386	-3.624	-3.531	
	(2.56)**		(1.56)	(1.62)	(2.59)***	(2.53)**	
OECD CA Total debt/GDP			12.104				
			(2.29)**				
OECD CA Real GDP growth			0.091				
			(0.06)				
Count of default		3.931					
events elsewhere (DC)		(5.02)***					

(Table is Continued Overleaf)

Table H-1: Main Results using Logit with Country FE (continued)

Estimator (Results Reported)	Logit with Country FE (Margins × 1 st.d. × 100)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theory omitted				Bad Rep	Threats	Politics	Spillover
External debt stock/GNI	0.828	1.527	1.057	1.459	0.782	0.746	2.433
Private Creditors	(0.81)	(1.40)	(1.04)	(1.38)	(0.73)	(0.68)	(2.21)**
External debt stock/GNI	0.873	0.371	0.674	-1.197	1.171	0.613	-0.520
Official Creditors	(0.75)	(0.33)	(0.53)	(1.19)	(1.00)	(0.53)	(0.50)
External debt service/GNI	2.509	2.609	0.834	2.388	2.416	2.335	1.615
	(2.08)**	(2.09)**	(0.64)	(1.82)*	(2.01)**	(1.86)*	(1.29)
Real GDP growth	-1.158	-1.228	-1.229	-1.125	-1.055	-1.169	-1.482
	(1.30)	(1.35)	(1.29)	(1.30)	(1.19)	(1.30)	(1.51)
Inflation	-0.655	0.327	-1.085	0.022	-0.550	-0.566	0.164
	(0.54)	(0.27)	(0.94)	(0.02)	(0.52)	(0.47)	(0.14)
Foreign Reserves/GDP	-3.939	-3.059	-3.679	-3.528	-3.944	-3.677	-2.208
	(3.16)***	(2.53)**	(2.76)***	(2.59)***	(2.74)***	(2.93)***	(1.70)*
Risk-Free Rate	6.401	-0.052	2.119	3.659	6.431	6.512	3.766
	(4.46)***	(0.04)	(1.31)	(2.84)***	(4.41)***	(4.65)***	(3.61)***
LogL	-382.78	-399.65	-368.49	-406.39	-384.51	-384.50	-412.21
AUROC (se)	0.84 (.02)	0.81 (.02)	0.85 (.02)	0.80 (.02)	0.83 (.02)	0.83 (.02)	0.80 (.02)
Theory: Wald χ^2 p-value ‡				0.000	0.205	0.404	0.000
FE: Wald χ^2 p-value ♯	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AUROC = (1): p-value ♮		0.061	0.023	0.015	0.415	0.349	0.005

Notes: All results are for 80 countries with 1,784 observations and 141 crises. The estimates presented are marginal effects multiplied by 100 times the variable standard deviation. Banking and Currency crisis dummies included but not reported. Absolute t-statistics in parentheses, based on standard errors computed via the Delta method (st.errors for RE-Mundlak logit clustered at country level; unless jackknife where indicated). Models (4)-(7) highlight the significance of each of the four theories. ♯ Wald test for the insignificance of the within-country averages. ‡ This indicates the joint significance of each 'theory' in model (1). ♮ This test compares the ROC with that of Model (1); in (4)-(7) rejection indicates that the benchmark model is preferred and that the theory variables omitted 'matters'.