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Michael Zabel

Referent:	Prof. Dr. Gerhard Illing
Korreferent:	Prof. Dr. Uwe Sunde
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Namen der Berichterstatter: Gerhard Illing, Uwe Sunde, Christoph Trebesch

To Jakob.

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Introduction

Over the past years, “real life” as well as academic macroeconomics has been under the overwhelming influence of the world financial crisis. One only needs to recall that it has been just ten years ago that Robert E. Lucas (in)famously declared the “central problem of depression prevention” as “solved, for all practical purposes” (Lucas, 2003) to get an impression of how profoundly the advent of the financial crisis shocked the economics profession. However, as Socrates famously stated, “wonder is the beginning of wisdom” (Plato, 1987), and if the crisis came as a shock to economists, it certainly has been a healthy one, as it directed attention to important research questions previously underappreciated.

The financial crisis also provides the background for the research questions treated in this dissertation. While chapter one contributes to the literature on the origins of the financial crisis, chapters two and three are motivated by one of its most significant consequences, namely the dramatic rise in public debt. Over the course of the last years, costly bank rescue packages and debt-financed stimulus programmes boosted government debt levels, while the repercussions of the financial crisis on the real economy depressed growth and led to declining tax revenues. At now 108%, the debt to GDP ratio of advanced economies increased by almost 50% compared to 2007 (International Monetary Fund, 2013). While, back then, sovereign debt of advanced economies was perceived as basically risk-free, the increase in public debt led to a re-evaluation of risks which prompted rating agencies to massively downgrade the sovereign debt of some countries, especially in the Eurozone. Against this background, chapter two analyses whether and under which conditions a rating agency's announcement on one country leads to spillover effects on the refinancing costs of other countries. As the Greek debt restructuring in 2012 exemplified, even sovereign defaults — unimaginable for advanced economies before the start of the crisis — can no longer be excluded as an option. As even conservative macroeconomists like Kenneth Rogoff state that "any realistic strategy for dealing with the eurozone crisis must involve massive write-downs (forgiveness) of peripheral countries' debt" (Rogoff, 2013), this may not have been the last incidence of a default. Chapter three therefore studies the costs that a sovereign default entails for a country's GDP growth. The chapters are arranged chronologically in the order of their inception and can be read independently. In the following, I will give a brief outline of each chapter of this dissertation.

The first chapter sheds new light on the origins of the financial crisis, which emanated from the investment behaviour of financial institutions. Even today, many people shake their head in disbelief when they reflect on how it could come about that those big banks with their huge and sophisticated risk management departments ended up investing so much money in assets that turned out to be basically worthless. To account for this investment behaviour, great importance is attached to the so-called risk-taking channel of monetary policy, which states that low central bank policy rates increase the risk-hunger of financial institutions.

This chapter contributes to the literature by proposing a new transmission mechanism for the risk-taking channel that highlights the role of the central bank's interest rate as a signal for its preferences. In the model, the central bank cares both about macroeconomic and financial stability but possesses only one instrument, its interest rate, to pursue its policy objectives. While the (private) banks are generally aware of the central bank's policy goals, they are only imperfectly informed about

(i) the central bank's preference for financial stability and about (ii) its assessment of the macroeconomic situation. Since the importance the central bank attaches to financial stability can be interpreted as the degree to which the banks are insured against the risk of a financial crisis, banks try to infer the degree of insurance by assessing the economic situation themselves and by coming up with a "counterfactual" interest rate. The problem, however, is that should the banks' assessment of the economic situation differ from the one of the central bank, banks misinterpret the policy preferences of the central bank and can erroneously overinvest in risky assets.

As I illustrate, this concept of "monetary policy misperception" can provide new insights into the build-up of financial sector risk during the years preceding the crisis. Thus, a striking particularity of the pre-crisis years consists in the dramatic divergence of inflation expectations by the Fed and the private sector. Over the period from 2002 to 2006, the inflation expectations of the private sector persistently turned out much higher than the inflation forecasts by the central bank. Based on the theoretical model, I argue that this divergence in economic outlooks may have given rise to a dangerous misinterpretation of monetary policy that might have played a significant role for the build-up of financial risk during the pre-crisis period. Hence, I contribute to the literature on the risk-taking channel by stressing that it is not only the interest rate of the central bank *per se*, but also the interpretation of that interest rate that matters for the banks' attitude towards risk. Thus, my study lends further support to the notion that a clear and transparent central bank communication policy has to be a central element of any successful monetary policy.

The second chapter analyses spillover effects across sovereign debt markets in the wake of sovereign rating changes.[1] Ever since the start of the Eurozone debt crisis,

[1] This chapter is based on the article "Credit Ratings and Cross-Border Bond Market Spillovers", which is joint work with Benjamin Böninghausen from the University of Munich (see Böninghausen and Zabel, 2013).

announcements of credit rating agencies on the creditworthiness of Eurozone member states have been one of the main driving forces for developments in the sovereign bond market. This has drawn considerable attention to the impact and potential side effects of the rating agencies' actions. In particular, the idea that an agency's rating action on one country might affect the refinancing costs of other countries alarmed policymakers and provided one of the main rationales for the European Commission to just recently set up stricter rules for credit rating agencies.

While spillovers are thus highly relevant from a policy perspective, their presumed existence is not straightforward to identify in financial markets where confounding events are ubiquitous and hamper the establishment of clear counterfactuals. We therefore make a methodological contribution to the literature in proposing a novel empirical strategy to cleanly identify the existence of cross-border spillover effects of sovereign rating announcements. This is made possible by collecting an extensive dataset of the complete history of rating actions by the "Big Three" (Standard & Poor's, Moody's, and Fitch) and daily sovereign bond market movements for up to 73 countries between 1994 and 2011. Exploiting substantial variation across crisis and non-crisis periods as well as developed and emerging economies, we perform an explicit counterfactual analysis. This pits bond market reactions to small revisions in an agency's assessment of a country's creditworthiness against reactions to all other, more major changes. Importantly, we demonstrate that this helps to avoid the problems associated with a classic event-study approach in a spillover context, and that it relieves us of having to make additional assumptions as a number of other papers.

Our findings suggest that rating downgrades indeed trigger significant negative spillovers which turn out to be highly robust to a number of tests. On the other hand, evidence for positive spillovers emanating from upgrades is much more limited at best. This points to an important asymmetry in the processing of positive and negative information by the sovereign debt market. Regarding potential channels of spillovers, we find that spillovers from downgrades tend to be significantly more pronounced for countries within the same region. Strikingly, however, we find that bilateral trade linkages, financial integration, or fundamental similarities between countries cannot explain why belonging to a common region amplifies negative spillover effects. This is particularly interesting in view of the notion inherent in many policy discussions and proposals that spillovers are in some sense unwarranted, so as to merit an intervention by the state to constrain the agencies' scope of action. While the amount of measurable fundamentals is naturally limited, our findings do not suggest that concerns over countries being found "guilty by association" in financial markets can be easily dismissed.

Chapter three finally studies the impact of sovereign default and debt renegotiation on a country's GDP growth. Given the dramatic increase in public debt levels, recent years have seen interest in the topic of sovereign debt and default resurface. A particularly relevant question in this context is, to what extent a sovereign default depresses economic activity in the defaulting country. Many empirical studies have shown that sovereign defaults tend to go along with substantial contractions in output. Yet, a central shortcoming of the existing empirical literature is that it typically categorises debt crises as dichotomous events, which hides enormous variation in crisis characteristics.

Therefore, the main contribution of chapter three is to take the diversity in sovereign debt crises seriously and to empirically assess whether and to what extent the output costs of sovereign defaults differ depending on the severity of a default. Specifically, we distinguish between "hard" and "soft" defaults by building on two distinct empirical measures on the heterogeneity of debt crisis events. The first measure is the index of debtor coerciveness, which is procedural and captures a government's payment and negotiation behaviour vis-à-vis foreign creditors *during* defaults. The second measure is the main outcome of debt renegotiations, namely the size of creditor losses or "haircuts" implied in the debt restructuring agreement *at the end* of a debt crisis.

We find that confrontational government behaviour during default is associated with a much steeper drop in output. On average, coercive or "hard" defaults see a significantly lower GDP growth of up to six percentage points annually compared to "soft" defaults in which the government opted for a consensual stance. This result is highly robust to a number of tests. Moreover, we find little evidence that it is driven by reverse causality. This suggests that not only the incidence of default matters, as implied by much of the previous literature, but also its severity. Surprisingly however, we do not find that the "type" of default also influences a country's post-default growth prospects, which appears to be remarkably independent from crisis characteristics.

Chapter 1

Monetary Policy Misperception and the Risk-Taking Channel

1.1 Introduction

As a result of the recent financial crisis, the relationship between monetary policy and financial sector risk-taking, which has long been ignored by economists and policy-makers alike, is now in the middle of an intense discussion. In search for the causes of the crisis, many economists today point at the monetary policy of the Federal Reserve as the main culprit. Its loose monetary policy stance, they say, has fuelled financial sector risk-taking and therefore substantially contributed to the dramatic build-up of financial imbalances over the pre-crisis years.

Following the terminology of Borio and Zhu (2012), the relationship between monetary policy and financial sector risk-taking is today known as the "risk-taking channel" of monetary policy. Simply put, the risk-taking channel posits that the interest rate set by the central banks and the risk appetite of financial institutions are inversely related, such that a drop in the central bank's policy rate induces financial institutions to increase their risk-taking while a rise in the policy rate causes them to downscale their risk exposure. Several empirical studies have verified the existence of such a structural relationship along multiple dimensions of financial sector risk-taking (credit risk, leverage risk, maturity transformation risk)[1]. While economists have identified several mechanisms through which the central bank's policy rate can affect the financial sector's risk aversion (see section 1.2 for a detailed discussion), monetary policy misperception has not been addressed by the literature so far.

The argumentation I am going to develop can be roughly summarised as follows: While it is commonly assumed that central banks set their policy rates taking into account the classic "Taylor rule" ingredients (inflation and the output gap), I presume that the central bank further includes financial stability concerns in its considerations when setting interest rates. Financial institutions are aware of this fact, but unsure *how much* importance the central bank places on financial stability. Since the extent of central bank remedy in case of a crisis crucially affects the financial sector's optimal loan allocation, the financial sector tries to infer the weight of financial stability concerns in the central bank's policy function from observing the central bank's policy rate setting over time. However, given that the central bank does not publish the economic forecasts on which its policy rate setting relies, diverging opinions on the future outlook of the economy can lead to a misinterpretation of monetary policy by the banking sector, which results in inefficiently high bank risk-taking.

As I am going to illustrate, the concept of monetary policy misperception can shed new light on the build-up of financial sector imbalances in the US over the pre-crisis years. Thus, a striking particularity of this period consists in the dramatic divergence of inflation expectations by the Fed and the private sector. From 2002 onward, the inflation forecasts of the private sector persistently turned out much

[1] See e.g. Altunbas et al. (2010); Jiménez et al. (2008); Ioannidou et al. (2009); López et al. (2010); Gambacorta (2009); Delis and Kouretas (2011).

higher than the inflation forecasts of the central bank. Based on the insights of the theoretical model, I argue that this divergence in economic outlooks can have led to a dangerous misinterpretation of monetary policy by the financial institutions which might have played an important role in the build-up of financial risk during the pre-crisis period.

The remainder of the chapter is organised as follows: In section 1.2, I will review the existing theoretical literature on the risk-taking channel. In section 1.3, I develop the theoretical model and show how the concept of monetary policy misperception can increase banks' risk-taking. Section 1.4 analyses the pre-crisis years in the US in the light of the theoretical model and shows that monetary policy misperception might have been a crucial factor for the build-up of financial risk over the pre-crisis period. Section 1.5 concludes.

1.2 Related literature

It has been only recently that economists became interested in the question how the central bank's interest rate setting affects the risk allocation of financial institutions. Until the middle of last decade, there has virtually been no research that explicitly studied the effects of monetary policy on risk-taking. On the one hand, the macroeconomic literature by and large abstracted from risk-taking choices and was much more concerned about the effects of monetary policy on the quantity rather than on the quality of loans. To the extent that "risk" was considered at all, it was rather the riskiness of borrowers than the risk attitude of lenders that constituted the focus of attention. On the other hand, the banking and finance literature has been studying financial sector risk-taking for a long time. However, this research typically focused on how to correct market failures stemming from limited liability and asymmetric information while basically ignoring the potential impact of monetary policy on a bank's risk choices.

Thus, it was only in the middle of the 2000s that economists started to become aware of the risk-taking side-effects of monetary policy. With the advent of the financial crisis, which revealed the extent of risk in the financial sector, this field of research quickly developed into one of the most vivid research branches of monetary policy. Since then, many papers have empirically verified that the stance of monetary policy indeed influences the banks' appetite for risk. This has been shown both at the international level (through cross-country analysis) and for single countries, for wide ranges of risk measures, and based on a variety of identification strategies (see e.g. Altunbas et al., 2010; Jiménez et al., 2008; Ioannidou et al., 2009; López et al., 2010; Gambacorta, 2009; Delis and Kouretas, 2011). But how does it come that loose monetary policy incentivises the financial sector to take on more risk?

The "search for yield" channel, described by Rajan (2005), is probably the most prominent explanation. Rajan derives the risk-taking channel from the fact that important financial institutions (such as pension funds or insurance companies) need to match the yield promised on their (long-term) liabilities with the return they obtain from their assets. While in "normal times" a conservative investment strategy is sufficient to generate the required returns, the low yields on save assets prevailing in low interest rate periods may compel these institutions to "search for yield" and to switch to riskier investments. Consequently, an environment of low policy rates exerts pressure on financial institutions to increase their risk exposure.

A second line of reasoning stresses the importance of the central bank's policy rate for valuations, incomes and cash flows in the economy (Borio and Zhu, 2012). In line with the "financial accelerator" of Bernanke et al. (1996), a monetary easing leads to revaluation effects on future incomes and cash flows that boost firms' collateral values. Given the risk management models employed by the financial sector, those revaluations *ceteris paribus* give rise to more benign assessments on the riskiness of borrowers. This decreased risk perception in turn induces the financial sector to increase its investment in ex-ante risky assets.

Another propagation mechanism for the risk-taking channel is the so-called "asset substitution channel" (see e.g. De Nicolò et al., 2010). Here, the risk-taking incentives emanating from monetary policy are attributed to technical portfolio adjustments following changes in the policy rate. Since a drop in the central bank's policy rate is equivalent to a drop in the interest rate on very safe, short-term assets or loans, it leads to an increase in the relative price of those assets. This price increase triggers substitution effects in the portfolio of financial institutions, which now increase their demand for risky assets. Under fairly general specifications of the financial sector's preferences (most importantly, under the standard assumption of a risk-neutral financial sector), it can be shown that the substitution effect dominates the opposing income effect and that therefore an interest rate drop should induce the financial sector to increase its investment in more risky and more long-term assets (Fishburn and Porter, 1976).

By focusing on the monetary policy *regime* rather than on the monetary policy *rate*, other authors tackle the monetary policy—risk-taking relationship from a completely different perspective. While the previous explanations described the risk-taking channel as a somewhat technical reaction of financial institutions to changes in policy rates, it is now assumed that the risk-taking incentives result from the financial sector's active attempt to exploit moral hazard effects that emanate from the central bank's anticipated reaction function. Focusing on the central bank's role as a lender of last resort, it is shown that if the central bank commits to provide unlimited liquidity support in crisis times, this gives rise to an "insurance effect" that boosts banks' investments in illiquid assets (Diamond and Rajan, 2012; Cao and Illing, 2012; Farhi and Tirole, 2012; Giavazzi and Giovannini, 2010).[2]

1.3 The model

This chapter proposes a new propagation mechanism for the risk-taking channel that combines the reasoning of both strands of the theoretical literature. In my model the monetary policy *rate* affects the banks' risk-taking choices by working as a *signal for* the monetary policy *regime*. In contrast to the existing literature on the monetary policy regime, I assume that banks are only imperfectly informed about the central bank's reaction function, which gives the monetary policy rate an important signalling function. Since the central bank's policy regime is highly relevant for the investment decision of banks, they try to infer the central bank's reaction function from its policy rate setting behaviour. Hence, by its interest rate decision the central bank not only affects the economy via the classical interest rate channel, but also affects the banks' expectations about its future policy.

If in times of financial distress the central bank reduces its interest rate below a level previously expected by banks, they will update their expectations and assume a similar central bank reaction pattern for comparable situations in the future. Since expectations about the monetary policy regime directly affect the banks' investment strategy, changes in expectations will automatically feed back on their investment behaviour. To the extent that a policy rate drop induces banks to expect a more accommodating monetary policy in the future, this gives the banks incentives to follow a more risky investment strategy — the risk-taking channel. Thus, I con-

[2]This strand of literature is, if anything, only very loosely related to the risk-taking channel. Even though the focus on bank's risk choices places this approach in close proximity to the risk-taking channel literature, Diamond and Rajan (2012), Farhi and Tirole (2012), as well as Giavazzi and Giovannini (2010) do not make explicit reference to the risk-taking channel with in their papers.

tribute to the literature on the risk-taking channel by stressing that it is not only the policy rate *per se* but also the interpretation of that policy rate which matters for the banks' attitude towards risk.

1.3.1 Basic model setup

The model builds on the basic framework of Cao and Illing (2010, 2011, 2012). The economy extends over an infinite time horizon, T={0,1,...,t,...}, and consists of four types of agents, (1) depositors, (2) entrepreneurs, (3) banks and (4) the central bank.

1. Depositors live for two periods in overlapping generations. In each period $t \in T$, a new generation of depositors, call them "young" depositors, is born with an endowment D_t. It is assumed that the endowment of young depositors depends negatively on the change in the current policy rate of the central bank, $D_t = D_t(\triangle r_t^{CB})$, which will be explained in more detail later on. To keep things simple, the number of "young" depositors is kept constant over time, so there is no change in population. Depositors do not care about consumption when they are young but derive their whole utility from consumption in period $t+1$ when they are old. In period t, they can either store their endowment for a nominal return of $\underline{d} = 1$, or deposit their funds in a bank at the deposit rate $d_t \geq 1$. Depositors are risk averse in the sense that their marginal utility of consumption is strictly decreasing in the amount of consumption. For simplicity, I assume a square root utility function for depositors: $U(C_t) = \sqrt{C_t}$.

2. Entrepreneurs live for three periods. In each period $t \in T$, a generation of "young" entrepreneurs is born. Entrepreneurs are born without any endowment but have the ability to run a business. However, before they can start a business they have to receive seed funding and therefore ask for a loan. There

are two different types of entrepreneurs, safe and risky ones, contingent on the type of business project that they want to start.[3] Safe projects yield a riskless return of $R_1 > 1$ in the following period. Risky projects generate a higher return $R_2 > R_1$, but finish only with probability p_t (which stochastically varies over time) in the next period. This means that with probability $(1 - p_t) \geq 0$ the project is delayed and does not yield returns in $t+1$ but only in $t+2$. Thus, the type of risk that risky projects exhibit is pure liquidity risk. Entrepreneurs always retain a share $(1 - \gamma) < 1$ of their projects' proceeds, which means that they can only commit to pay out a fraction $\gamma < 1$ of the project's return to their investor. In contrast to depositors, entrepreneurs are risk-neutral and indifferent about the timing of consumption, so consumption in $t + 1$ and $t + 2$ both provides them with the same level of utility.

3. Banks are infinitely lived and compete in each period for the funds of "young" depositors by setting their deposit rate d_t in a perfectly competitive market. Hence, in equilibrium banks make zero profit and all surplus is transferred to depositors in the form of deposit payments d_t. As experts in credit markets, banks possess superior monitoring skills compared to depositors, which means that the hold-up problem stemming from the retention of parts of the project's proceeds by the entrepreneurs is less severe for banks (higher γ for banks compared to depositors).[4] As financial intermediaries, banks maximise their depositors' expected return by investing their depositors' endowment D_t in the projects run by the entrepreneurs. A share $0 \leq \alpha_t \leq 1$ of the funds is

[3] It is assumed that the number of projects of each type always exceeds the endowment of depositors such that funding is scarce and not all projects are financed.

[4] The fact that the hold-up problem is more severe for depositors justifies the presence of banks as financial intermediaries. Assume that the depositors' monitoring skills are insufficient to make direct investments in entrepreneurs profitable, while the banks' γ is high enough for projects to be financed ($\gamma R_1 > 1$). This prevents the realisation of the frictionless market outcome in which each generation of depositors simply invests in the riskless project and consumes its proceeds in the subsequent period. Hence, from now on γ always denotes the γ of banks.

invested in safe and the remainder $(1-\alpha_t)$ in risky projects. It is assumed that the type of project (safe or risky) an entrepreneur intends to start is common information such that adverse selection effects are absent.

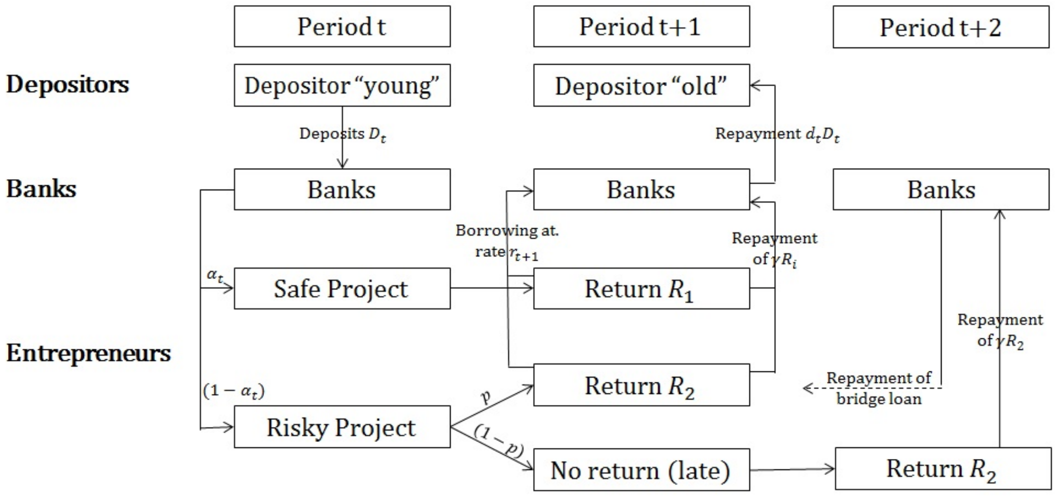
The three sectors interact in the model as following: In period t, a generation of young depositors is born with an endowment of D_t. Banks compete for the depositors' funds by promising a deposit rate d_t on endowment stored at their bank. The maximum deposit rate banks are willing to offer depends on their investment behaviour, i.e. the allocation of funds to safe and risky projects, which in turn depends on the banks' expectations on the share of risky projects that finishes early or gets delayed next period (i.e. the realisation of p_{t+1}). Consequently, banks conduct forecasts on p_{t+1} and condition their investment behaviour as well as their deposit rate offer on that forecast. Due to the assumption of perfect (Bertrand) competition among banks, in equilibrium all banks will offer an identical deposit rate d_t and exhibit an identical risk profile in their investments.

At the beginning of period $t+1$, the share of risky projects that finishes early (p_{t+1}) and the share that gets delayed $(1-p_{t+1})$ is revealed, i.e. p_{t+1} realises. The banks now have to make the promised payment of $d_{t-1}D_{t-1}$ to their "old" depositors, which now want to consume.[5] However, in period $t+1$ banks will only generate returns from their investments in safe projects and from the share p_{t+1} of risky projects that turns out early (i.e. is not delayed). In addition, entrepreneurs retain a share $(1-\gamma)$ of the projects' proceeds for themselves, such that banks receive the following payment stream on their period t investments: $\gamma \{\alpha_t R_1 + (1-\alpha_t)p_{t+1}R_2\}$.

In case that $\gamma \{\alpha_t R_1 + (1-\alpha_t)p_{t+1}R_2\} < d_t$, i.e. the return on "early" projects does not suffice to pay out "old" depositors, banks can turn to early entrepreneurs to bridge-finance the difference. Those retain $(1-\gamma)\{\alpha_t R_1 + (1-\alpha_t)p_{t+1}R_2\}$ and

[5] While the model is set in an overlapping generations framework, by the timing of a period I exclude the possibility that banks use the funds of "young" depositors to pay out "old" depositors.

Figure 1.1: **Basic structure and timing of the model**



Notes — The figure illustrates the basic structure and the timing of the model, as outlined in subsection 1.3.1, in the absence of a bank run.

— since they are indifferent between consuming in $t+1$ or $t+2$ — are willing to lend to banks at the market rate $r_t \geq 1$. The borrowing rate in $t+1$, r_{t+1}, is determined by the interaction of liquidity demand by banks and liquidity supply of early entrepreneurs: $r_{t+1} = \frac{\gamma\{\alpha_t R_1 + (1-\alpha_t)p_{t+1}R_2\} - d_t}{(1-\gamma)\{\alpha_t R_1 + (1-\alpha_t)p_{t+1}R_2\}}$. The numerator of the equation signifies liquidity demand and the denominator liquidity supply. The bigger the ratio of liquidity demand to liquidity supply, the higher will equilibrium borrowing rate. Figure 1.1 summarises the structure of the model as outlined so far.

Early entrepreneurs know that late projects will yield a return of R_2 in the next period (no credit risk). However, they will only lend to those banks that will be able to repay the bridge-loan (plus interest) in the next period $t+2$. But if the equilibrium borrowing rate r_t rises above a certain threshold level $\bar{r}$, the future income of the bank will not be enough to repay the entrepreneurs. The bank then becomes insolvent. Anticipating imminent insolvencies, depositors will run those banks with looming payment problems and force them to liquidate *all* their current investments (including safe projects) at an inferior return $R_3 < 1$.[6]

1.3.2 The market equilibrium

In equilibrium, a representative bank i allocates its investments to safe and risky projects in such a way that it maximises its expected returns. The optimal investment scheme crucially depends on the bank's belief about the share of risky projects that turns out early and late, i.e. on the realisation of p_{t+1}. As stated before, banks hence conduct forecasts on that parameter, p^e_{t+1}. With probability π, this forecast proves to be correct (i.e. $p^e_{t+1} = p_{t+1}$). But with probability $(1-\pi)$, p_{t+1} will deviate from the banks' forecasts by ξ_{t+1}, which is assumed to be about normally distributed in the range [-a, 0].[7] In the following I assume that the probability π for the banks' forecasts to be correct is sufficiently large to make it optimal for all banks to base their investment choice on that scenario.

Therefore, in each period $t \in T$ the market equilibrium results from each bank i choosing its share of safe investments α_i such as to maximise its expected profit for $p_{t+1} = p^e_{t+1}$:

$$\alpha_{i,t} = \arg \max_{\alpha_{i,t} \in [0;1]} \gamma \left\{ \alpha_{i,t} R_1 + (1-\alpha_{i,t}) \left[(p^e_{t+1} R_2 + \frac{(1-p^e_{t+1})R_2}{r^e_{t+1}}) \right] \right\}$$

Due to perfect (Bertrand) competition in the banking sector, bank i makes an expected profit of zero and has to pass its entire expected profit on to its depositors:

$$d_{i,t} = \max_{\alpha_{i,t} \in [0;1]} \gamma \left\{ \alpha_{i,t} R_1 + (1-\alpha_{i,t}) \left[(p^e_{t+1} R_2 + \frac{(1-p^e_{t+1})R_2}{r^e_{t+1}}) \right] \right\}$$

[6] It is assumed that a bank that gets run will be restructured and can restart its business in the same period such that, independent of the occurrence of bank runs, the number of banks stays constant over time.

[7] More precisely, I assume ξ_{t+1} to be equal to the sum of two *iid* parameters τ and υ, which are both uniformly distributed in the space $[\frac{-a}{2}; 0]$. According to the central limit theorem, the sum of two identically distributed *iid* variables is about normally distributed. Therefore ξ is about normally distributed in [-a; 0] with a mean of $\frac{a}{2}$. As p_t is a probability, I further assume that $a \leq 1$ and that $(p_t + \xi_t) \subseteq [0,1]$.

The market equilibrium thus features a unique optimal symmetric equilibrium of pure strategy with the following characteristics[8]:

1. All banks invest the share $\alpha_t^* = \frac{\gamma - p_{t+1}^e}{\gamma - p_{t+1}^e + (1-\gamma)\frac{R_1}{R_2}}$ of their funds in the safe asset and offer a deposit rate of $d_t^* = \gamma[\alpha_t^* R_1 + (1-\alpha_t^*)R_2]$ to their depositors.

2. If in period $t+1$ the share of delayed projects turns out as expected by the banks, i.e. $p_{t+1} = p_{t+1}^e$, then the borrowing rate $r_{t+1} = 1$ and all banks remain solvent.

3. If, however, the share of delayed projects exceeds the banks' expectations, i.e. $p_{t+1} < p_{t+1}^e$, liquidity demand increases and the borrowing rate r_{t+1} will rise above the threshold level $\bar{r} = 1$. There will be a bank run and those banks that are run have to liquidate all their assets at $R_3 < 1$.

1.3.3 The role of the central bank

In this model setup, the introduction of a central bank can help to increase the economy's general welfare in two ways:

(1) First, given risk averse depositors, the stochastic variation in the share of "early" risky projects p_t decreases the intertemporal welfare of depositor. Assume that p_t follows *iid* and is about normally distributed in $\Omega \subseteq [0,1]$. If p_{t+1} is correctly expected to turn out relatively high, banks will maximize profits by increasing their scale of risky investments in period t. This leads to an increase in "output" (the return generated by period t investments) in $t+1$, Y_{t+1}.[9] Due to perfect competition, banks pass this (anticipated) increase in returns to their depositors and promise them a relatively high deposit rate d_t in period t. In the absence of bank runs,

[8] The proof of these results is analogous to Cao (2010).

[9] It holds that production $Y_{t+1} = [\alpha_t^* R_1 + (1-\alpha_t^*)p_{t+1}R_2]D_t$ is increasing in p_{t+1}, as long as $p_{t+1}^e = p_{t+1}$.

this increases the consumption possibilities of depositors born in period t, which is equal to the endowment with which depositors have been born times the rate of return they receive on their deposits, $C_{t+1} = d_t D_t$. If, on the other hand, only a small share of the risky projects is (correctly) anticipated to finish early, then — in the same vein — banks have to downscale their high yield investments in period t, which results in a lower output Y_{t+1} and decreased consumption possibilities for period t depositors.

Hence, the parameter p_t can be interpreted as a (temporary) stochastic production shock that affects the depositors' consumption possibilities. Since depositors are risk averse, positive production shocks increase the depositors' utility to a lesser extent than negative shocks decrease it. By stabilising shocks to production, the central bank can thus increase the intergenerational welfare in the model economy.

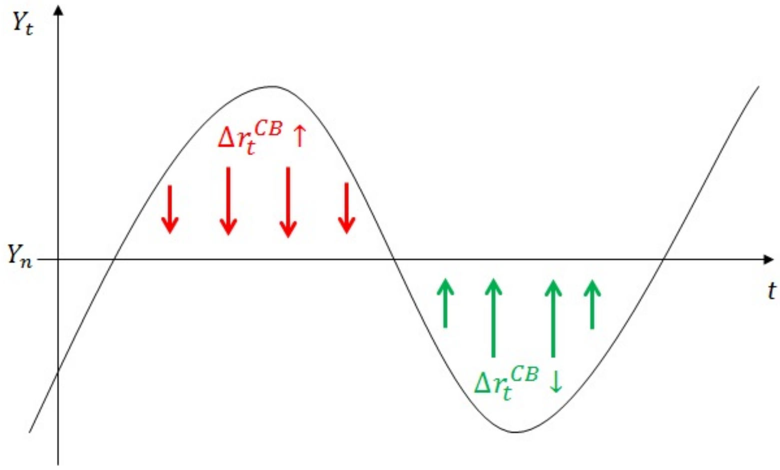
Since the level of endowment with which "young" depositors are born depends negatively on the change in the central bank's policy rate, $D_t = D_t(\triangle r_t^{CB})$, the central bank can influence future output and the depositors' consumption by adjusting its current policy rate r_t^{CB}. To make sure that its interest rate policy indeed stabilises and not amplifies production swings, the central bank conducts forecasts on the future production shock p_{t+1}. It is public information that $\forall t \in T$, p_t is about normally distributed in $\Omega \subseteq [0,1]$ around a mean of $\mu = 0.5$.[10] Therefore, the economy attains its "natural" level of output at $Y_n = [\alpha_n^* R_1 + (1 - \alpha_n^*)\mu R_2] D|_{\Delta r^{CB}=0}$ for $p_t = p_t^e = \mu$.[11]

To stabilise production (and thus depositors' consumption), the central bank reduces its policy rate ($\triangle r_t^{CB} < 0$) if its forecast signals a shock to future production that would decrease Y_{t+1} below Y_n, hence if its forecast signals a looming "output gap"

[10] More technically, assume that $p_t = 0.5 + \eta_t + \kappa_t$, where both γ_t and δ_t are *iid* and uniformly distributed in the interval [-0.25;0.25]. Since the sum of two *iid* and uniformly distributed random variables converges to a normal distribution for $t \to \infty$, p_t is about normally distributed in $\Omega \subseteq [0,1]$ with a mean of $\mu = 0.5$.

[11] α_n^* signifies the optimal investment allocation of banks if $p_t = p_t^e = \mu$.

Figure 1.2: **Central bank production stabilisation**



Notes — The figure illustrates the role of the central bank as a stabiliser of output volatility. If the central bank forecasts a production shock p_{t+1} that would increase production above its natural level next period, it increases its policy rate, thus lowering the endowment of "young" depositors and, hence, future production Y_{t+1}. If, however, it forecasts a p_{t+1} that would result in future production below potential, it decreases its policy rate, such that future production is stabilized by the resulting increase in the endowment of "young" depositors.

$X_{t+1} < 0$ (which is the case for any $p_{t+1} < \mu$). In turn it increases its policy rate ($\triangle r_t^{CB} > 0$) if it forecasts an output larger than potential in $t+1$ (which is the case for any $p_{t+1} > \mu$). Assume that, in contrast to private banks, the central bank receives a signal without any noise, such that $p^e_{t+1}(CB) = p_{t+1}, \forall t \in T$. In the absence of bank-runs the central bank can in this way completely stabilise production and depositors' consumption at its natural level. Figure 1.2 illustrates the central bank's stabilisation behaviour.

(2) Second, bank runs decrease the economy's overall welfare since the liquidation of projects at an inferior return of $R_3 < 1$ leads to a waste of resources. In the absence of a bank run, period t investments will yield a return of $(\alpha_t R_1 + (1-\alpha_t)R_2)$ over the next two periods (no credit risk). However, in case of a bank run, troubled banks have to liquidate all their investment projects and the return reduces to R_3. Hence, banks run induce a welfare loss of $\mathcal{L} = [\alpha_t R_1 + (1-\alpha_t)R_2 - R_3]D_t^{run}$, where

D_t^{run} denotes the amount of deposits at troubled banks. Therefore, the central bank possesses an incentive to avoid those costly bank runs.

In the model, a bank run happens only if the equilibrium borrowing rate r_t, determined by market forces (liquidity demand and supply), rises above the threshold level $\bar{r}$. At this threshold level, the borrowing rate depresses the collateral value of "late" projects so much, that a bank with payment problems is not able to raise sufficient funds to pay out all its current depositors. This occurs whenever the banks' forecast on the production shock p_{t+} turns out as too optimistic (i.e. $p_{t+1} < p_{t+1}^e$), which is the case with probability $(1-\pi)$.[12] What can the central bank do?

It is assumed that instead of borrowing from "early" entrepreneurs, banks can also turn to the central bank for a bridge-loan at the policy rate $r_t^{CB} \geq 1$. Since banks are going to borrow from the source that offers the more attractive conditions, the *effective* borrowing rate that banks face in each period t will therefore be equal to $r_t = min\left\{\frac{\gamma\{\alpha_{t-1}R_1+(1-\alpha_{t-1})p_tR_2\}-d_{t-1}}{(1-\gamma)\{\alpha_{t-1}R_1+(1-\alpha_{t-1})p_tR_2\}};\ r_t^{CB}\right\}$. By lowering its policy rate in crisis times to $\bar{r}$, the central bank can thus always prevent costly bank runs.

Hence, the central bank's policy rate plays a dual role. On the one hand, it is the central bank's instrument for stabilising future expected output fluctuations. On the other hand, it can also be used to avert financial turmoil in the current period. This dual role of the policy rate constitutes the core of the model.

Given these motives, the central bank behaves as follows: At the beginning of each period $t \in T$, it forecasts the future production shock p_{t+1}^{CB}. Based on that forecast, the central bank stabilises future production by following a Taylor-like interest rate rule that includes the (expected) future "output gap" $E_t^{CB}(X_{t+1})$ as an argument. At the same time, it observes the current conditions on the liquidity market and evaluates the financial stability of banks. Depending on its preferences, the expected

[12]In that case, the true p_{t+1} differs from the banks' forecast by ξ_t, which was assumed to be around normally distributed in the range [-a; 0].

loss of resources in case of a bank run will affect the central bank's interest rate decision with a weight of $\lambda \geq 0$. The larger the parameter λ in the central bank's policy rule, the stronger will the central bank react to financial stability concerns in the economy. If, however, the parameter λ is equal to zero, it will purely focus on the stabilisation of future output and be indifferent about the occurrence of a bank run. The central bank's policy rule then looks as follows:

$$r_t^{CB} = r_n + E_t^{CB}(X_{t+1}) - \lambda[\alpha_{t-1}R_1 + (1 - \alpha_{t-1})R_2 - R_3]D_{t-1}^{run}$$

It is important to note that for any $\lambda > 0$, financial stability concerns affect the central bank's policy rate setting asymmetrically, as they imply a reduced policy rate in times of financial turmoil but not an increase in interest rates as long as things work out smoothly. This notion of an asymmetrical reaction pattern is consistent with the strategy of "benign neglect" that has been developed by Bernanke and Gertler (1999, 2001) and which became the dominant view on financial markets among central bankers during the pre-crisis period. This strategy has been summarised by Bordo and Jeanne (2002) as follows: "The monetary authorities should deal with the financial instability that may result from a crash in asset prices if and when the latter occurs, but they should not adjust monetary policy pre-emptively in the boom phase". In other words, monetary policy should mitigate the consequences of financial busts, but not react to financial booms. Studies by Borio and Lowe (2004) as well as by Ravn (2012) provide empirical evidence for the presence of an asymmetric response pattern of central banks to financial imbalances, with central banks massively loosening policy in face the of financial crisis but not tightening it beyond normal during financial booms. Thus, alternatively one can interpret the factor λ in the model as what the Deputy General Manager of the BIS Hervé Hannoun describes as "financial dominance", i.e. the risk that "monetary policy becomes increasingly dominated by short-term concerns about adverse financial market developments [...] [which] arises when central banks factor in financial stability concerns in times of financial bust but fail to do it in times of financial boom when financial imbalances are building up" (Hannoun, 2012).

1.3.4 Monetary policy misperception and the risk taking channel

As the last ingredient of the model, I now assume that the banks are aware of the structure of the central bank's reaction function but unsure about (1) the central bank's economic outlook (i.e. its forecast on the future productions shock) and about (2) the exact weight it puts on financial stability considerations λ. Hence, if banks observe a change in the policy rate r_t^{CB}, they cannot exactly pin down the motive for the central bank to do so.

While the central bank's production stabilisation does not influence the banks' investment behaviour (as it does not systematically affect the expected profitability of its investments), the extent of central bank reaction to bank runs λ heavily impacts banks' investment allocation. Full central bank liquidity support in crisis times insures banks against the risk of illiquidity and therefore deprives them of any incentive to privately provide for that risk. Since the return of the risky project R_2 is higher than the return of the (liquidity) risk-free project R_1, the menace of a bank run, however, is the only thing that motivates banks to invest in safe projects in the first place. The higher the degree of insurance provided by the central bank, the more it pays for banks to free-ride on liquidity and to invest more heavily in risky projects (i.e. the lower α^* will turn out).[13]

In order to optimally invest the depositors' endowment, banks therefore try to infer the central bank's financial stability preferences λ from observing its policy rate setting r_t^{CB} in crisis times. Based on their knowledge of the structure of the central

[13]The technical proof of this result is analogous to Cao (2010).

bank's reaction function and their own forecasts on the future productivity shock p^e_{t+1}, they come up with a "counterfactual" interest rate, $E^B_t(r^{CB}_t)$, which they would expect the central bank to set in case of $\lambda = 0$.

$$E^B_t(r^{CB}_t) = r_n + E^B_t(X_{t+1})$$

If the central bank's policy rate turns out lower than the banks' counterfactual rate, they now assign this deviation to the central bank's financial stability motive λ.[14]

$$\widehat{\lambda}_t = \frac{\lambda \left\{[\alpha_{t-1}R_1 + (1-\alpha_{t-1})R_2 - R_3]D^{run}_{t-1}\right\} + E^B_t(X_{t+1}) - E^{CB}_t(X_{t+1})}{[\alpha_{t-1}R_1 + (1-\alpha_{t-1})R_2 - R_3]D^{run}_{t-1}}$$

As long as the banks' assessment of the future output shock corresponds with the central bank's (i.e. $E^B_t(X_{t+1}) = E^{CB}_t(X_{t+1})$), this procedure will give rise to an exact estimate of the central bank's financial stability preferences, ($\widehat{\lambda}_t = \lambda$). However, if the economic outlook of banks is more optimistic than the one of the central bank[15], banks will overestimate the central bank's aversion to bank runs. The greater the divergence in economic outlooks between the banks and the central bank (i.e. the greater the absolute value of ξ_t), the greater will also be the extent of monetary policy misperception by the banking sector. As a consequence, banks will adjust upwards their beliefs about $\widehat{\lambda}$ — which renders them less concerned about liquidity risk and more willing to take on additional risk. Hence, for any p^e_{t+1}, they will reduce their share of safe investments below previously optimal levels ($\alpha_t < \alpha^*_t$) and in turn increase their share of risky investments. As liquidity support by the central bank can only prevent bank runs but not create "real" resources, such an overinvestment in risky assets reduces *real* resources available in $t+1$ and thus adversely affects the welfare in the economy.[16]

[14] Since the central bank cannot reduce its policy rate below unity, the banks set $\widehat{\lambda}_t$ greater or equal to this expression in case of $r^{CB}_t = 1$.

[15] Which is the case with probability $(1-\pi)$.

[16] There is a decisive difference between the liquidity provision from early entrepreneurs and the central bank. While the liquidity from early entrepreneurs is backed by their (real) period t

1.4 The pre-crisis years revisited: Monetary policy misperception and the build-up of financial risk

In the last years, there has been an intense discussion about whether or to what extent the Federal Reserve's policy can be held responsible for the massive build-up of financial risk in the years preceding the financial crisis. John Taylor (2007, 2009, 2011) argues that from 2002 to 2006 the monetary policy of the Fed would have been way too loose compared to historic standards. Taylor (1993) found that the Federal Reserve's interest rate setting since the "Great Moderation" closely resembled the interest rate path prescribed by the following simple interest rate rule, today known as the "Taylor rule":

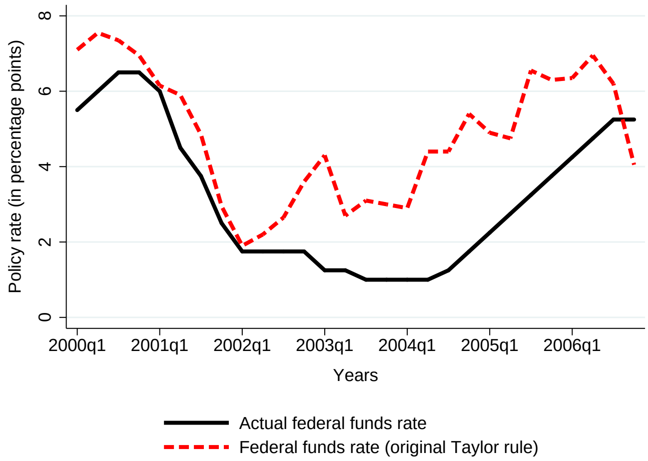
$$r_t = r_t^n + \pi_t + 0.5(\pi_t - \overline{\pi}) + 0.5(y_t - \overline{y_t}),$$

where r_t is the target policy rate set by the Fed, r_t^n the equilibrium real interest rate, π_t the inflation rate over the previous four quarters, $\overline{\pi}$ the inflation target of the Fed and $(y_t - \overline{y_t})$ the output gap measured as the deviation of real GDP from its target rate. It is commonly assumed that both the equilibrium real interest rate and the inflation target of the Fed is at 2%, $r_t^n = \overline{\pi} = 0.02$.

Figure 1.3 compares the target federal funds rate actually implemented by the Fed in the years from 2000 to 2006 with the policy rate prescribed by the original Taylor rule for the respective years. Indeed, from 2002 onward the Taylor rule stipulated higher policy rates than the Fed actually set. As Taylor regards the policy rates suggested by his rule as a counterfactual for what the interest rates should have

resources $(1-\gamma)\left\{\alpha_{t-1}^* R_1 + (1-\alpha_{t-1}^*)p_t R_2\right\}$, the central bank provides liquidity in the form of new (nominal) fiat money. So in contrast to loans from early entrepreneurs, central bank loans will be inflationary, since they increase the total money stock of the economy without real value creation in period t. Compare Cao and Illing (2010) and Cao and Illing (2011, 2012) for a further discussion of that issue.

Figure 1.3: **The Taylor critique**



Notes — The figure illustrates the critique of John Taylor. While the black line plots the actual federal funds rate set by the Fed, the red dotted line indicates the counterfactual policy rate that the Fed should have set according to Taylor's interest rate rule. As can be seen, the federal funds rate has been below the levels prescribed by the (original) Taylor rule for the whole period from 2002 to 2006.

been had the Fed held on to the successful rule-based monetary policy of the "Great Moderation", he interprets the deviation from his rule as "clear evidence of monetary excess during the period leading up to the housing boom" (Taylor, 2009). Based on this presumption of "monetary excess", he comes to the conclusion that "monetary policy was a key cause of the boom and hence the bust and the crisis" (Taylor, 2009).

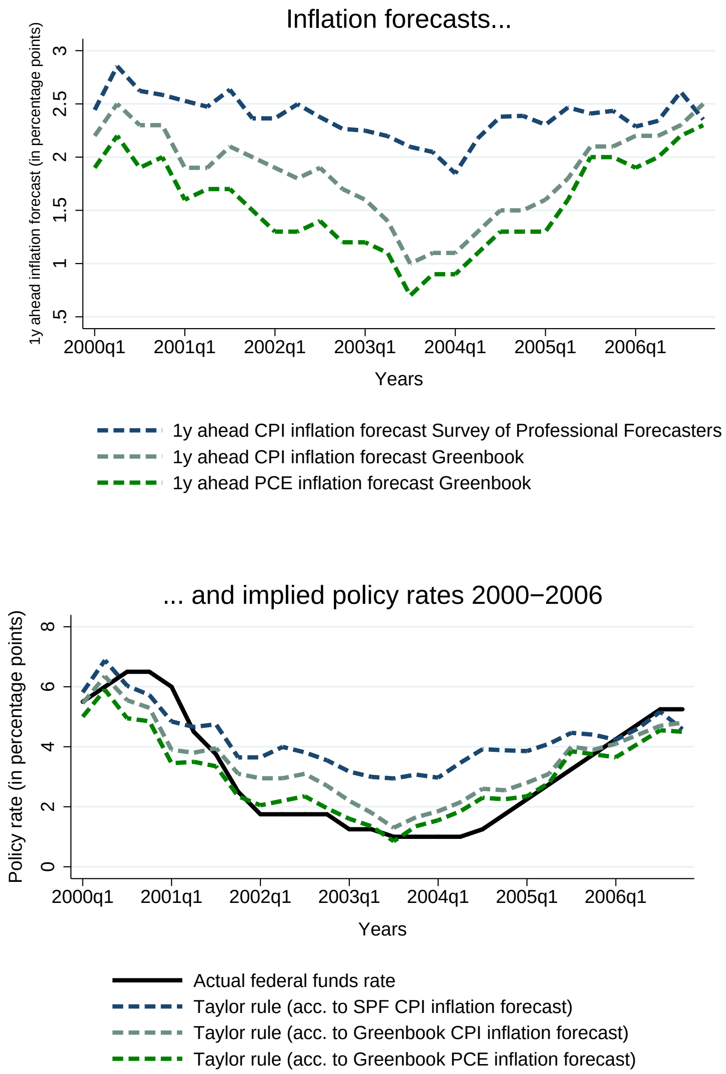
In January 2010, the chairman of the Fed Ben Bernanke answered this criticism by stressing that, contrary to the accusations of John Taylor, the Fed's monetary policy during pre-crisis years was in fact closely in line with the suggestions of the Taylor rule. However, since monetary policy affects inflation only with a significant lag, effective monetary policy must take into account the *forecast* values of inflation and the output gap rather than the *current* values as in the original Taylor rule. Given the economic background of the early 2000s, inflation forecasts by the Fed signalled only very low risk of inflation and even sowed fears that the United States might sink into deflationary territory. Hence, the policy rates prescribed by a forecast based forward-looking Taylor rule would have been lower than the rates advised by Taylor's original interest rate rule.

Furthermore, while Taylor's critique is based on the consumer price index (CPI) measure of inflation, the Fed typically focuses on inflation as measured by the price index for personal consumption expenditures (PCE), because it is less affected by the imputed rent of owner-occupied housing. Since the forecasts of PCE inflation did signal an even higher deflationary risk than CPI inflation forecasts, the choice of the inflation measure additionally impacted the policy rate setting by the Fed negatively. Hence, putting the Fed's monetary policy into perspective, the claim of an excessively easy monetary policy appears out of place (see Bernanke, 2010).

In the light of my theoretical model I claim that Bernanke's reply is only partially suited to clear the Fed from the accusation of complicity in the build-up of financial imbalances. Bernanke's argumentation just aims at the Fed's intentions while — as shown in the theoretical model — it is also monetary policy perception that influences the investment behaviour of banks and financial institutions. Thus, were market participants aware of the Fed's motives for setting low interest rates?

A huge problem for financial markets to put the Fed's interest rate setting into perspective is due to the fact that the forecasts prepared for each meeting of the FOMC (the so called Greenbook forecasts) and on which its policy rate decision crucially hinges are not immediately available to the public but only published with five years lag. Comparing the Greenbook inflation forecasts of the Fed with the mean inflation forecast of the Survey of Professional Forecasters (that can be interpreted as the "best guess" of market participants on the inflation outlook) shows that over the period from 2002 and 2006 the public was way more optimistic about inflation than the Fed.

Figure 1.4: **Central bank misperception in the US**



Notes — The upper figure plots the 1-year-ahead inflation forecasts by the Fed (in its Greenbook) and by the private sector in the US for the time period from 2000 to 2006. As the Survey of Professional forecasters only reports PCE inflation forecasts from January 2007 onwards, only Greenbook forecasts are shown for PCE inflation rates. The lower figure compares the policy rates prescribed by the (forward-looking) Taylor rule for the different inflation forecasts with the actual policy rates set by the Fed. All estimations of the Taylor rule are based on the realtime output gap estimates in the Greenbook.

This gap in inflation forecasts also translates into a gap in the policy rates deemed as adequate under current economic circumstances (according to a forward-looking Taylor rule). Figure 1.4 highlights that, based on the CPI inflation forecasts by the Survey of Professional Forecasters, policy rates should have been set much higher over the pre-crisis years. This gap amounts to more than two percentage points in 2002 and the subsequent years. Hence, by observing the policy rate setting of the Federal Reserve, the financial sector identified an unexplainable gap between the expected policy rate (based on public forecasts) and the actual federal funds rate, which they possibly attributed to financial stability considerations of the Fed. Expressed in the words of the theoretical model, the financial sector raised its estimate of the financial stability weight $\widehat{\lambda_t}$ — and consequently increased its exposure to risk. Had the public been aware of the CPI inflation forecasts in the Greenbook, the gap between the implied and the actual policy rate would have been much smaller. Indeed, had the Fed also communicated its reliance on PCE inflation for policy rate setting and its Greenbook PCE inflation forecasts, this gap would have almost reduced to zero. This might have also limited the degree of risk-taking by financial institutions and, hence, the extent of the financial crisis.

It is a distinct feature of the time period from 2002 to 2006 that inflation forecasts by the public (as expressed by the SPF) were continuously more optimistic than the ones by the Fed. The difference becomes even more extreme when public inflation expectations are not approximated by the SPF estimates but by the inflation expectations of private households as collected by the University of Michigan's Survey of Consumers (see Figure A.1 in the Appendix). Hence, I claim that monetary policy communication, or rather the lack of it, may help to explain parts of the increase in risk-taking observed before the start of the financial crisis. With a clear and open monetary policy communication, such as the immediate publication of its Greenbook forecasts, the Fed might have avoided a dangerous misinterpretation of its policy while stabilising the staggering economy at the same time. Thus, this study lends further support to the notion that a clear and transparent central bank communication policy has to be a central element of any successful monetary policy.

1.5 Conclusion

"Communication is what the receiver understands, not what the sender says"

In this chapter I have introduced monetary policy misperception as a new transmission mechanism for the risk-taking channel of monetary policy. Building on the idea that in a world of imperfect information the central bank's policy rate works as a signal for its policy preferences, it was shown that a lack of monetary policy communication can lead to a misperception of monetary policy by the financial sector. Specifically, if the banking sector is more optimistic about the future outlook of the economy than the central bank, it can misinterpret low policy rates as a signal for a monetary policy that effectively cuts off some of the banks' downside risks, which encourages bank risk-taking.

It has further been demonstrated that this view is consistent with the build-up of financial sector imbalances in the US. Indeed inflation forecasts by the Survey of Professional Forecasters persistently turned out much more optimistic than the corresponding (unpublished) inflation forecasts by the Fed over the pre-crisis years. Based on public forecasts, financial institutions could therefore perceive the Fed's policy rates as too low. To the extent that financial markets attributed this gap to financial stability concerns of the Fed, this incentivised banks to increase their risk-taking.

Over the course of the financial crisis central banks in almost all industrialised countries were forced to lower their policy rates to record lows. Even more than five years after the Lehman-shock, interest rates in most of these countries are still close to zero and expected to stay there for still some time. Given these circumstances, the development of a sound understanding of the mechanisms at play in the risk-taking channel is key for a lasting stabilisation of our economies. I hope that this chapter contributes to that.

Chapter 2

Credit Ratings and Cross-Border Bond Market Spillovers*

2.1 Introduction

Ever since tensions began to surface in the eurozone in late 2009, the announcements by credit rating agencies (CRAs) on the creditworthiness of member states have continuously made the headlines and rattled financial markets. In particular, while not specific to the ongoing crisis, the notion that rating actions pertaining to one country might have a major impact on the yields of other countries' sovereign bonds, too, has regained the attention of policymakers. In fact, concerns over so-called negative spillover effects have been running so deep that the European Commission was at one stage considering a temporary restriction on the issuance of ratings under exceptional circumstances (Financial Times, 2011). This provides the background for why the Commission has just recently set up stricter rules for the agencies. In particular, CRAs are now only allowed to issue three ratings for EU member states' sovereign debt at pre-defined dates every year (European Union, 2013).

*This chapter is joint work with Benjamin Böninghausen.

These considerations carry two major assumptions on the behaviour of sovereign bond markets in the wake of rating announcements. The first assumption is that, when a rating announcement is made for one country, there exist significant spillover effects on other countries' sovereign bond markets. Conditional on their existence, the second assumption posits that such spillovers must, in one way or another, be unwarranted to merit an intervention by the state. In more technical terms, it suggests that spillovers are unrelated to economic fundamentals. While both assumptions are highly policy relevant and therefore deserve close scrutiny, they are not straightforward to test.

This chapter sets out to cleanly identify the existence of cross-border spillover effects of sovereign rating announcements, and to establish the economic conditions under which those effects are strongest, or which countries are affected most. To this end, we collect an extensive dataset which comprises a complete history of both the sovereign rating actions by the "Big Three" (Standard & Poor's, Moody's, and Fitch) and daily sovereign bond market movements for up to 73 countries between 1994 and 2011. The dataset contains substantial variation as it covers both crisis and non-crisis periods as well as a broad set of developed and emerging countries across all continents.

Crucially, the variation allows us to pursue a novel empirical strategy to identify potential spillover effects. More precisely, we perform an explicit counterfactual analysis which pits bond market reactions to small revisions in an agency's assessment of a country's creditworthiness against bond market reactions to all other, more major changes. As explained below, this not only helps us get around the problems associated with a classic event-study approach in a spillover context. It also does not require the additional assumptions made by a number of papers.

A traditional event-study procedure, where bond market movements in an estimation window serve as the counterfactual for bond market reactions in the event window, is suitable in principle but, in a spillover context, places too high demands on the necessary non-contamination of the estimation window. This is because, if one entertains the possibility of cross-border spillovers after rating announcements, each country's bond yields are potentially affected by any sovereign rating change in the world. The estimation window can therefore only be considered uncontaminated if no such change has occurred anywhere. As the number of instances where this can be ensured is extremely low, the classic event-study approach appears ill-suited to thoroughly identify spillover effects. Hence, in this chapter, we focus on a pooled cross section of short event windows, in which small changes of the actual rating serve as the counterfactual for larger changes.

While some papers also investigate spillovers in a pooled cross section framework, their analyses do not postulate an explicit counterfactual, as we do.[1] Instead, they rely on a "comprehensive credit rating" which combines two different types of rating announcements — actual rating changes and watch, or review, changes — into a single scale. Their identification therefore depends on rather strong additional assumptions on the relative informational content of reviews and ratings. We, however, focus solely on the class of actual rating changes. In detail, we test whether a country's sovereign bonds react more heavily to upgrades or downgrades elsewhere when those are "large" — i.e., when the actual rating changes by two notches or more. The group of "small" one-notch changes serves as the counterfactual during that exercise. At the same time, we explicitly allow for differences in the informational content of sovereign rating changes by controlling for watch listings that may build anticipation in the market. Moreover, we are also able to account for the fact that an announcement is often followed by a similar one from a different agency soon after, which may further influence the reception of the later announcements.[2]

[1] See Gande and Parsley (2005), Ismailescu and Kazemi (2010), Afonso et al. (2012), and Alsakka and ap Gwilym (2012).

[2] To the best of our knowledge, we are the first to consider such interactions between the major CRAs in identifying spillover effects.

Our findings on the existence of cross-border spillover effects point to an important asymmetry in the sovereign debt market's treatment of ratings. On the one hand, we find significant spillovers in the wake of sovereign rating downgrades, which turn out to be robust to a number of tests. On the other hand, reactions to upgrades appear to be much more muted, if anything.

We then investigate to what extent spillovers are driven by country characteristics. Importantly, we find that spillovers from downgrades tend to be significantly more pronounced for countries within the same region. We proceed by testing whether this can be explained by bilateral trade linkages, financial integration, or fundamental similarities between countries but, even after controlling for these factors, we continue to find that belonging to a common region amplifies cross-border spillover effects. Note that a limit to the amount of fundamentals that can be measured implies that no study can by design "prove" that negative spillovers are unwarranted in some way. At the same time, however, our findings do not suggest that policymakers' concerns over countries being found "guilty by association" can be dismissed out of hand.

This chapter is related to a broad strand of literature that investigates the effects of sovereign rating announcements on different segments of the financial markets. The most common exercise is to conduct an event study gauging the *direct* impact of rating changes on the bonds issued by the country concerned. However, there is also a substantial body of research analysing the reaction of the country's stock and, more recently, of its CDS market. As a general result, this literature finds a strong and significant impact of sovereign rating downgrades, while upgrades have an insignificant or more limited impact (see e.g. Cantor and Packer, 1996; Larraín et al., 1997; Reisen and von Maltzan, 1999; Brooks et al., 2004; Hooper et al., 2008; Hill and Faff, 2010).

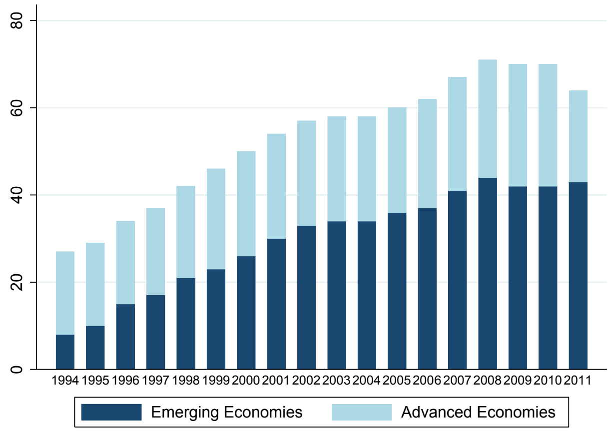
Moreover, in recent years a growing body of research has specifically studied whether sovereign rating changes also lead to *spillover* effects on other countries' sovereign bonds. Generally speaking, the literature affirms the existence of such spillovers, meaning that a rating action on one country is found to significantly affect the sovereign bond prices of other countries (e.g. Ismailescu and Kazemi, 2010; Arezki et al., 2011; De Santis, 2012). Some studies also point out that spillovers are not limited to sovereign debt markets but that rating changes also affect foreign stock and exchange markets (Kaminsky and Schmukler, 2002; Arezki et al., 2011; Alsakka and ap Gwilym, 2012). Regarding a potential asymmetry in the spillover effects of negative and positive rating events, the results of the literature so far remain inconclusive. Whereas Afonso et al. (2012) find spillovers to matter most for downgrades, with little or no effects of sovereign upgrades, Ismailescu and Kazemi (2010) find positive rating events to have a greater spillover effect on foreign CDS prices than negative ones.

With the exception of Gande and Parsley (2005), these studies focus either on spillover effects during specific regional crisis episodes[3] or on an otherwise homogeneous sample of countries only, such as emerging countries (Kaminsky and Schmukler, 2002; Ismailescu and Kazemi, 2010). In addition to some of the shortcomings already mentioned, this leaves open the question to what extent their findings are of more general relevance.

The chapter is organised as follows. In the next section, we describe the dataset and highlight some important characteristics of rating announcements. Section 2.3 discusses the estimation strategy for identifying cross-border spillovers. Section 2.4 presents our empirical results and discusses their interpretation. We end with a brief conclusion.

[3]See Arezki et al. (2011), Afonso et al. (2012), and De Santis (2012) for the Eurozone crisis, Kaminsky and Schmukler (1999) for the 1997/98 Asian crisis.

Figure 2.1: **Number of sovereign bonds in the dataset**



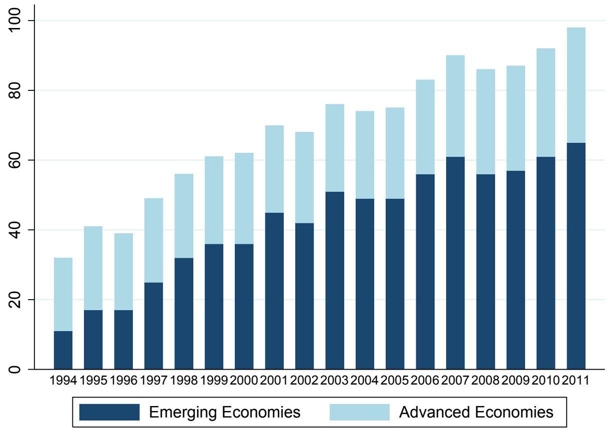
Notes — This figure shows the scope and composition, by economic development, of the sovereign bond sample between 1994 and 2011, highlighting a notable increase in the coverage of emerging economies over time. Countries are classified according to the IMF World Economic Outlook.

2.2 Data

2.2.1 The dataset

For our study, we compile a broad dataset of the yields of publicly traded sovereign bonds at daily frequency. The dataset starts in January 1994 and ends in December 2011. Since for many countries data are only available after 1994, we add those countries' sovereign bonds as soon as reliable information becomes available. Whereas our dataset only comprises sovereign bonds issued by 27 countries in 1994, this number increases to 74 countries towards the end of our sample period. This reflects both the increased financing needs of sovereigns and the growing prevalence of bond issuance, as opposed to bank financing, over the last 20 years. While for 1994 sovereign bond yields are mostly available for developed countries, the availability of emerging market bond yields picks up heavily over our sample period. Towards the end of the period, emerging markets even account for the bulk of sovereign bonds in the sample. Figure 2.1 illustrates the increasing scope of our dataset over time.

Figure 2.2: **Number of rated countries**

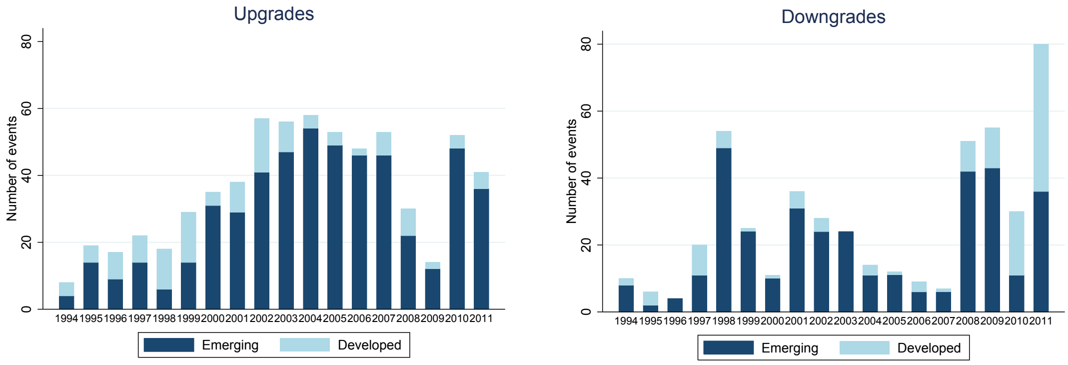


Notes — This figure shows the scope and composition, by economic development, of the sample of countries rated by at least one of the major rating agencies (S&P, Moody's, Fitch) between 1994 and 2011, with a notable increase in the coverage of emerging economies over time. Countries are classified according to the IMF World Economic Outlook.

In order to consider a broad spectrum of sovereign bonds, our sample draws on data from different sources. Our preferred data source is Bloomberg, from which we use generic 10-year yields for up to 33 countries. If data are not available on Bloomberg, we supplement them with yields from Datastream's 10-year Government Bond Benchmark Index, ensuring that this does not induce structural breaks in the series. Since sovereign bond availability for emerging markets is quite limited both on Bloomberg and on Datastream, we also use data from the JP Morgan Emerging Markets Bond Index Global (henceforth EMBI Global, see JP Morgan, 1999). While bonds included in the EMBI Global have to fulfil strict requirements regarding the availability of reliable daily prices, the average maturity of a country's bond index can vary remarkably from that of the other two sources. We therefore control for maturity in all regressions. Table B.1 in the Appendix gives a detailed overview of the sovereign bond market data included in our sample.

For the purpose of our later analysis, we compute sovereign bond *spreads*. The spread is the differential of the country's sovereign bond yield over that of a US

Figure 2.3: **Rating actions over time**



Notes — This figure shows upgrades and downgrades of developed and emerging economies made by S&P, Moody's, and Fitch between 1994 and 2011. Countries are classified according to the IMF World Economic Outlook.

Treasury bond of comparable maturity. We use 10-year maturities where possible, which is the case for the developed economies and some emerging markets. For the other emerging economies, we rely on the EMBI Global data. As those correspond to different maturities (depending on the average maturity of eligible instruments a country has issued), we obtain the relevant US Treasury yields by interpolating from the closest published yield curve rates.

Information on sovereign ratings comes from the rating agencies' websites and includes daily information both on rating changes and on sovereign watch listings by any of the "Big Three" (S&P, Moody's, Fitch) from 1994 to 2011. We choose the year 1994 as a natural starting point for our sample period since Fitch only started to assign sovereign ratings in that year. Like the number of publicly traded sovereign bonds, the scope and composition of countries rated by the "Big Three" changes quite substantially during our sample period. While in 1994 only 34 sovereigns were rated by at least one of the agencies, this number had increased to 98 countries by 2011 (see Figure 2.2).

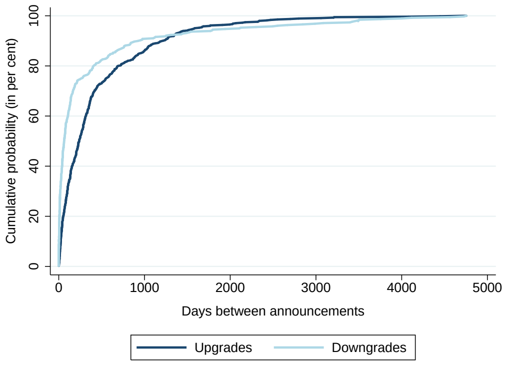
2.2.2 Characteristics of rating announcements

Over the sample period, we are able to consider a total of 1,097 rating changes, 635 of which were upgrades and 462 downgrades. Table B.2 in the Appendix provides a regional breakdown. In general, one can observe a significant increase in the number of sovereign credit ratings during our sample period, particularly in emerging market countries.

As Figure 2.3 illustrates, rating activity is not evenly distributed over time but, especially for downgrades, shows some hefty peaks during specific episodes of crisis. Whereas in "normal times", downgrades tend to be relatively scarce, a severe increase can be observed in the context of the 1997/98 Asian crisis (affecting mostly emerging countries plus South Korea and Hong Kong) and following the 2008–2011 financial and European debt crises (where for the first time advanced economies were exposed to downgrades at a large scale). This means that similar announcements tend to cluster around certain time periods.

In addition, it is an important stylised fact that the downgrading of a country is frequently followed by yet another downgrade announcement for that same country soon after. This is all the more probable because there is a strong overlap in country coverage by the "Big Three". Almost all countries in our sample are rated by more than one agency only and most are even rated by all three (70 out of 98 countries at the end of 2011). Hence, in what we term *within*-clustering, different agencies may make the same announcement for a *given country* in short succession or even on the same day. Figure 2.4 illustrates this issue by plotting the cumulative distribution function and summary statistics of the number of days between similar rating actions on the same country. As can be seen, clustering is particularly pronounced for downgrades. In around five per cent of all cases, a downgrade on a country is followed by another downgrade on that country within just one day. For example, in the

Figure 2.4: **Clustering of rating announcements**



	Upgrades	Downgrades
Mean	453	364
Median	238	63
5th pct	13	1
10th pct	23	3
25th pct	79	12

Notes — This figure shows the cumulative distribution functions and summary statistics of the number of calendar days between an upgrade (downgrade) announcement for a given country and a subsequent upgrade (downgrade) of the same country by any agency. Information is based on the sample of 1,097 rating announcements (635 upgrades, 462 downgrades) made by S&P, Moody's, and Fitch between 1994 and 2011.

course of the Asian crisis, S&P, Fitch and Moody's all downgraded South Korea's credit rating on successive days between 25 and 27 November 1997. Similarly, during the ongoing European debt crisis, Fitch issued a downgrade for Greece on 8 December 2009. One week later, S&P downgraded the country as well, as did Moody's yet another six days later.

The presence of clustering might be of crucial importance when examining the spillover effects from a rating announcement since its informational content is likely to vary depending on whether it has been announced in isolation or just a few days after a similar announcement by another agency. Not to control for these cases could seriously bias estimation results for the impact of rating announcements on sovereign bond markets.

Clustering *across* countries may matter, too. When CRAs change the rating of a number of *different countries* in the same direction simultaneously, one needs to control for the fact that some countries will then be both "non-event" and event countries. Otherwise, one might erroneously detect spillovers across sovereign bond markets when, in fact, one is looking at own effects of ratings. This is all the more important if the countries concerned share a common trait which leads CRAs to make simultaneous announcements in the first place, as appears to have happened on 3 October 2008 when Fitch downgraded Estonia, Latvia and Lithuania.[4] It is therefore a major advantage of our dataset that it enables us to explicitly take into account prior and parallel rating actions by other CRAs and on other countries.

Similarly, the informational content of a rating change might be conditional on whether it was preceded by the respective country being put on a watch list. As the literature on the effects of rating announcements on the refinancing conditions of the very same country shows (e.g. Ismailescu and Kazemi, 2010; Afonso et al., 2012), rating changes are often preceded by a similar change in the market's assessment of sovereign risk, especially when countries have been put "on watch", or "review", before.[5] Ignoring these anticipation effects risks underestimating bond market reactions to a sovereign rating action. Since our dataset includes all sovereign watch listings by the "Big Three", we can directly control for a country's watch list status and mitigate potential problems with anticipation.

2.3 Identifying sovereign spillovers

2.3.1 Counterfactual choice and estimation strategy

The existence of rating spillover effects in the sovereign debt market requires, by definition, that the announcement by a CRA on the creditworthiness of one country (*event country*) impact significantly on the bond yields of another (*non-event country*). Yet, the mere observation of a change in non-event country yields when an

[4]Other examples may be seen in S&P's downgrade announcements for South Korea and Taiwan during the Asian crisis on 24 October 1997, or in Fitch lowering the ratings of Estonia, Ireland, Latvia, and Lithuania on 8 April 2009.

[5]In the following, we use the two terms interchangeably. While S&P and Fitch issue watch listings, in the Moody's terminology those are called "reviews".

event-country announcement is made does not suffice to establish a causal relation because non-event country yields might have changed regardless. Hence, the key issue in identifying potential spillover effects is to find a suitable counterfactual.

We cannot apply the procedure traditionally used in event studies on *direct* announcement effects, however. This strand of literature focuses on, for instance, the bond yield response of a sovereign that has been downgraded. In this framework, effects are identified by the existence of abnormal returns, meaning that around the announcement (event window), returns are significantly different from normal, as estimated over a longer time frame before the announcement (estimation window). In order to be a reasonable guide to normal returns, the estimation window has to be chosen such that other events with a potentially significant impact on returns are excluded (see e.g. MacKinlay, 1997). In other words, the counterfactual for gauging the impact of rating announcements is “no rating change”. While this represents a challenge in direct announcement studies already, which focus on countries in isolation, the identification of *spillover* effects based on this counterfactual is essentially impossible.

The reason is that, in a spillover context, we would require that there be no announcements on *any* rated country within the estimation window.[6] There is obviously a trade-off between the length of that window and the number of announcements eligible for inclusion in the estimation. However, even at a 30-day length commonly used in sovereign event studies, which is towards the shorter end of the event-study literature more generally, only 23 upgrades would be eligible, and 36 downgrades.

[6] The universe of all rated countries is the relevant benchmark when analysing potential spillover effects in this framework. Of course, if we only required the estimation window to be free of announcements pertaining to the non-event country, the number of events eligible for inclusion would increase substantially. However, this would amount to assuming from the outset that only direct effects, as opposed to spillover effects, could possibly matter, which would defy the purpose of the investigation.

We therefore pursue an identification strategy that does not rely on "no rating change at all" as its counterfactual, but which discriminates between rating changes according to their severity. More precisely, rating changes of a single notch serve as the counterfactual for more severe changes of two notches or more.[7] This approach is implemented in the following estimation equation, which we run on upgrades and downgrades separately:

$$\Delta Spread_{n,t} = \alpha + \beta \cdot LARGE_{e,t} + RatEnv_{e,n,t} \cdot \gamma + Other_{e,n,t} \cdot \delta + \omega_{e,n,t} \,.$$

The dependent variable $\Delta Spread_{n,t}$ is the change in non-event country n's bond spread vis-à-vis the United States over the two-trading-day window $[-1,+1]$ around the announcement on day 0 of a change in the rating of event country e $(\neq n)$. The event window length accounts for the fact that by the time a CRA announces a rating change on day 0, markets in some parts of the world may have already closed (asynchronous trading). Hence, any impact on those would not materialise before day +1, and would go undetected using a shorter $[-1,0]$ window. The same argument applies to rating announcements made after the exchange has closed in the country concerned, which we cannot distinguish from those made during trading.[8]

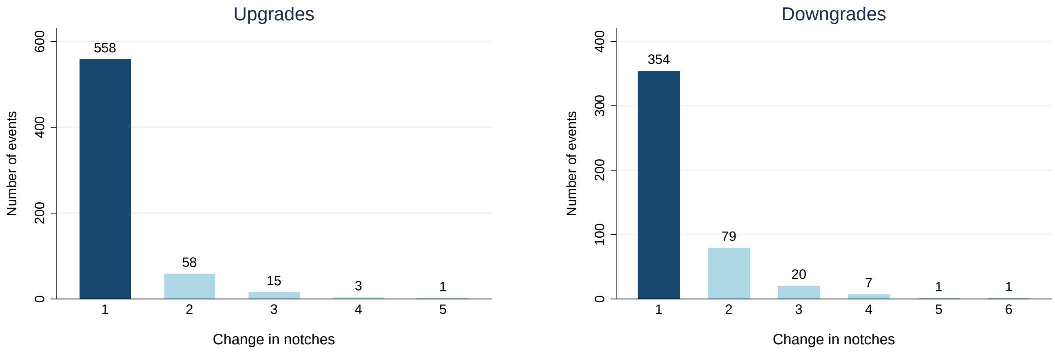
The key regressor in identifying possible spillover effects is $LARGE_{e,t}$, a dummy that takes on a value of one if e's rating is changed by two notches or more, and zero otherwise. We thereby treat rating changes of two notches or more as one single group. This is due to the distribution of the severity of upgrades and downgrades in our sample, which is shown in Figure 2.5.

The vast majority of rating announcements result in a one-notch change in a country's rating. Beyond that, we observe a significant amount of events only for changes

[7]See Table B.3 in the Appendix on the mapping of CRAs' letter ratings into a linear 17-notch scale.

[8]CRAs have made post-trading announcements during the Eurozone crisis, for instance (Financial Times, 2010; Wall Street Journal, 2012). In financial markets more generally, information which is deemed highly relevant is frequently released when exchanges are closed in order to limit or smooth the impact on prices.

Figure 2.5: **Distribution of rating changes**



Notes — This figure shows the distribution of the severity of rating changes, measured on a 17-notch scale (see Table B.3 in the Appendix). Numbers are based on the sample of 1,097 rating announcements (635 upgrades, 462 downgrades) made by S&P, Moody's, and Fitch between 1994 and 2011.

of two notches, while changes of three notches or more occur only very rarely. Therefore, we do not include separate dummy variables for the latter categories but group all rating changes of two notches or more into a single bin.

In this framework, positive (negative) spillover effects are equivalent to a drop (rise) in the spreads of country n which is significantly more pronounced in response to a two-or-more-notches upgrade (downgrade) of country e than to a single-notch one. We would then expect β to be significantly negative (positive) in the upgrade (downgrade) regressions.

This counterfactual choice also has implications for the estimation technique. Since we do not use "no change" as the counterfactual (due to the estimation window problem outlined above), we identify spillover effects in a cross-section of upgrades and downgrades rather than in a true panel setup.[9] We estimate the model by OLS.

At this point, it seems important to address some potential concerns about a possible endogeneity of the large-change dummy. The implicit assumption in the above design is that the rating announcement and its severity are not systematically related

[9]Thus, t denotes *generic* rather than actual time and can be thought of as indexing the different rating events.

to other spread-relevant information in the event window. Otherwise, $LARGE$ and the error term ω would be correlated, and β would be biased.

One concern might be, for instance, that CRAs downgrade a country instantaneously in reaction to "bad news" and do so by more notches for "particularly bad news". Note that an instantaneous response to other spread-relevant information *per se* would not induce any endogeneity in our framework whereas "fine-tuning" the severity of rating changes, *conditional* on an immediate response, clearly would. Hence, we demonstrate that there is very little to suggest instantaneous-response behaviour on the part of CRAs to begin with, and that endogeneity is therefore not a major issue in this regard. We would like to stress two points in particular.

Restricting the event window to two days already goes a long way towards alleviating the problem by limiting the amount of information that might potentially correlate with the large-change dummy. In other words, the scope for other relevant news to incite an immediate reaction from CRAs is rather small, even if such behaviour was characteristic of rating agencies and their announcements.

In addition, the proclaimed practice and a corresponding body of empirical literature suggest otherwise. The agencies state a preference for stable ratings (see e.g. Cantor, 2001; Cantor and Mann, 2003, 2007; Standard & Poor's, 2010), intending to announce a change only if it is unlikely to be reversed in the near future. This "through the cycle" approach contrasts with a "point in time" approach in that cyclical phenomena should not, in themselves, trigger rating changes. If CRAs actually pursued a stable rating policy, the fact that cyclical and permanent factors are difficult to disentangle (International Monetary Fund, 2010) should imply some delay between new information becoming available and an ensuing change in the credit rating. Empirical evidence for corporate bond rating indicates that this practice is indeed followed, thus reducing the timeliness of rating changes (Altman and Rijken, 2004; Liu et al., 2011), and that the CRAs are "slow" in processing new information (Löffler, 2005). This perception has also been expressed in investor surveys (Association for Financial Professionals, 2002; Baker and Mansi, 2002). Moreover, Sy (2004) notes for the sovereign sector that it may simply be concerns about rating changes precipitating significant increases in borrowing costs or outright crises which make CRAs opt for somewhat less timely announcements.

A second concern might be biases arising from differences across agencies in a pooled setup, as pointed out by Alsakka and ap Gwilym (2012).[10] Suppose, for example, that the large rating changes in our sample stemmed primarily from an agency in whose judgments the market placed more trust. Then, by pooling the announcements of S&P, Moody's, and Fitch, we would be picking up differences in the credibility of these CRAs rather than identifying spillover effects across sovereign bond markets. However, Figure B.1 in the Appendix shows that this is not very likely, in particular for downgrades where changes of two notches or more are distributed quite evenly across agencies: 32 for S&P, 46 for Moody's, and 30 for Fitch.[11] We are therefore confident that our approach provides a sound identification of spillover effects.

2.3.2 The rating environment

The rating environment may play an important role for the bond market reaction to an upgrade or downgrade announcement. Our regressions therefore control for a number of different rating variables, contained in $RatEnv_{e,n,t}$. For example, the spillover potential of a rating action might depend on the creditworthiness of the event country, which we proxy by the rating it held with the announcing CRA on the day before ($InitRat_{e,t}$). We also include the absolute difference between

[10] At the same time, the authors acknowledge that studies using pooled data (e.g. Kaminsky and Schmukler, 2002; Sy, 2004) constitute the norm in the literature as opposed to examining rating changes by CRAs separately.

[11] While the picture is not quite as unambiguous for upgrades, we have already stressed in the introduction that those results should be taken with more of a grain of salt (see next section).

the event country's initial rating and that of the non-event country ($\Delta InitRat_{e,n,t}$). This is because one might expect bilateral effects to differ depending on how similar countries are in terms of creditworthiness.

In addition, it is well established in the literature that the impact of rating announcements may vary according to whether they have been anticipated by the market (e.g. Reisen and von Maltzan, 1999; Gande and Parsley, 2005; Ismailescu and Kazemi, 2010). One potentially important and convenient measure of such anticipation is whether the actual rating action has been foreshadowed by a CRA putting the respective country on watch, or review (Kaminsky and Schmukler, 2002; Afonso et al., 2012). Hence, we add a dummy that takes on a value of one if a review in the indicated direction has been ongoing at the time of the upgrade or downgrade, and zero otherwise ($OnWatch_{e,t}$).

Introducing an explicit control variable differs from Gande and Parsley (2005), who amalgamate a country's watch status into a "comprehensive credit rating". More precisely, for any given day their measure is defined as the country's actual letter rating on a 17-notch scale, raised (lowered) if the country is on review for an upgrade (downgrade). Presumably due to the counterfactual issue discussed in 2.3.1, Gande and Parsley (2005) then focus on those days as events on which there is a non-zero change in the comprehensive credit rating. However, this identification crucially involves additional assumptions on how changes in review status and actual rating changes relate to one another quantitatively. Furthermore, one might argue that, despite the potential anticipation effects of watch listings, the latter are not qualitatively the same as actual rating changes. In any case, our much larger sample allows us to avoid those assumptions. We focus instead on the class of actual rating changes and their relative strengths only while controlling for anticipation through watch listings. This should provide for a cleaner identification of spillover effects.

Moreover, we have shown in 2.2.2 that similar announcements by different CRAs tend to cluster around certain dates, and that this is particularly true for rating downgrades. We account for potential clustering *within* countries by a variable which captures the number of similar announcements made for a particular country by other agencies over a 14-day window before the respective event ($SimActsWdwEvt_{e,t}$). For clustering *across* countries, i.e. one or more CRAs changing the rating of more than one country in the same direction simultaneously, we include the number of similar announcements made on the same day for the "non-event" country ($SimActsDayNonEvt_{e,t}$).

Finally, we add the volatility measure for the S&P 500 Index in the United States (VIX_t) to control for the "global market sentiment" in which the rating announcement is made. One might, for instance, imagine that in more turbulent times (i.e., in which volatility is high) borrowing conditions deteriorate across the board, so that spreads over the event window would be more likely to increase in any case. In that sense, VIX_t can be regarded as a technical control, which also adds a genuine time component to the pooled cross sections.

Definitions and sources of the above variables are provided in Table B.4 in the Appendix. In addition, all regressions include the vector $Other_{e,n,t}$ which contains a fixed set of controls, such as event and non-event country dummies. Importantly, we also account for common time effects in the pooled cross sections through the inclusion of year dummies. These capture global macroeconomic trends which might be reflected in the yields of US Treasuries and, hence, spread changes. For instance, there may be a stronger tendency for investments to flow into the US in some years due to a (perceived) "safe haven" status, or a "global savings glut" that has been discussed for the early 2000s. Moreover, each regression includes the following technical controls: the maturity of non-event country bonds in levels and squares to account for different positions on the yield curve, a dummy for EMBI Global bond yields, and a dummy for spread changes that need to be measured over weekends (as those correspond to longer intervals in terms of calendar days).

2.4 Results

2.4.1 Existence of cross-border spillover effects

Table 2.1 shows baseline estimation results on the existence of cross-border effects for upgrades and downgrades, respectively. We start with a parsimonious specification in column (1), which only contains our main variable of interest, the large-change dummy *LARGE* and initial ratings. We then control for potential anticipation effects from watch listings as well as clustering within and across countries in specification (2). Finally, specification (3) also accounts for global market turbulence, or risk aversion.

The key result is that the large-change dummy has the expected sign for both upgrades (i.e. negative) and downgrades (i.e. positive), and that it is highly significant in both cases. Moreover, this finding appears to be remarkably robust as the coefficient on *LARGE* is very stable and retains its significance across specifications. Comparison of the absolute coefficients, however, indicates an asymmetry in the spillover effects induced by upgrades and downgrades, respectively. Downgrades of two notches or more are associated with an average spread change over the event window which exceeds that of one-notch downgrades by about two basis points. In contrast, large upgrades are associated with spread changes that are roughly 1.2 basis points below those of one-notch upgrades. The asymmetry is also reflected in the lower significance levels for upgrades despite a larger number of rating events and observations. To further corroborate this, we confirm in a separate regression

Table 2.1: **Baseline regressions**

	Panel A: Upgrades			Panel B: Downgrades		
	(1)	(2)	(3)	(1)	(2)	(3)
LARGE	-0.0121**	-0.0124*	-0.0128*	0.0187***	0.0224***	0.0207***
	(0.0060)	(0.0064)	(0.0067)	(0.0061)	(0.0065)	(0.0066)
InitRat	0.0001	-0.0005	0.0000	-0.0013	-0.0013	-0.0008
	(0.0008)	(0.0009)	(0.0010)	(0.0014)	(0.0017)	(0.0017)
Δ*InitRat*	0.0010	0.0008	0.0009	0.0006	0.0008	0.0008
	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0009)
OnWatch		0.0057	0.0070		-0.0100*	-0.0046
		(0.0055)	(0.0058)		(0.0054)	(0.0054)
SimActsWdwEvt		-0.0020	-0.0013		0.0170***	0.0141**
		(0.0057)	(0.0057)		(0.0064)	(0.0065)
SimActsDayNonEvt		-0.0863*	-0.0877		0.1210**	0.1477**
		(0.0512)	(0.0546)		(0.0558)	(0.0635)
VIX			0.0017***			0.0006*
			(0.0004)			(0.0004)
N	31,986	30,564	29,950	23,734	22,413	21,931
Event countries	104	92	92	95	84	84
Non-event countries	73	73	73	73	73	73
Rating actions	635	606	595	462	436	427
R^2	0.0230	0.0216	0.0223	0.0397	0.0400	0.0423

Notes — This table shows baseline regressions explaining the percentage point change Δ*Spread* in non-event country spreads around the rating announcement for up to 635 upgrades and 462 downgrades made by S&P, Moody's, and Fitch between 1994 and 2011. For variable definitions, see Table B.4 in the Appendix. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and JP Morgan EMBI Global data, as well as levels and squares of non-event country bond maturities. Robust standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

that the absolute coefficients for upgrades and downgrades are statistically different from each other (see Table B.5 in the Appendix).[12]

Asymmetries in the reactions to positive and negative events have frequently been documented in the literature. For instance, Gande and Parsley (2005) find for a 1990s sample of developed and emerging countries that negative rating events in one country affect sovereign bond spreads in others whereas there is no discernible impact for positive events.[13] Recently, however, there has also been evidence of symmetric spillover reactions to sovereign rating announcements in the foreign exchange market (Alsakka and ap Gwilym, 2012), or even that positive announcements in emerging countries have both stronger direct and spillover effects in sovereign CDS markets (Ismailescu and Kazemi, 2010).

Turning to the rating-environment controls, neither the initial rating of the event country just before the rating announcement nor the difference in initial ratings between event and non-event country seem to play a role in terms of spillover effects. Both coefficients are far from significant across specifications. Previous evidence on this has been inconclusive. While Alsakka and ap Gwilym (2012) and Ferreira and Gama (2007) detect stronger spillover effects in the foreign exchange and stock markets, respectively, for event countries with lower initial ratings, Gande and Parsley (2005) find the opposite for bond market reactions (to sovereign downgrades).

We do find some evidence, though, that the impact of an actual rating change on spreads depends on whether it has been foreshadowed by a watch listing. The

[12] To this end, we pool *all* rating changes and replace the event-window spread changes for upgrades with their negative values for the sake of comparison. We then add a downgrade dummy (taking on a value of one for downgrades, and zero for upgrades) to all specifications both in levels and as interactions with the other explanatory variables. The interaction term of $LARGE$ with the downgrade dummy is positive and highly significant throughout, pointing to statistically significant differences in the absolute coefficients for upgrades and downgrades.

[13] Similar results have been obtained regarding the *direct* effects in sovereign bond and CDS markets (Larraín et al., 1997; Afonso et al., 2012), mirroring a well-established finding from event studies on bond, stock, and CDS returns in the corporate sector (e.g. Hand et al., 1992; Goh and Ederington, 1993; Steiner and Heinke, 2001; Norden and Weber, 2004).

corresponding dummy, *OnWatch*, is signed as expected for both upgrades and downgrades, yet there is again an asymmetry: the control variable turns out insignificant in all upgrade specifications but significant at almost the five per cent level for downgrades (specification (2) in Panel B). A possible explanation for this is given by Altman and Rijken (2006). They point out that watch listings partially ease the tension between the market's expectation of rating stability and the demand for rating timeliness. This suggests that watch listings contribute to the anticipation of actual rating changes. Given that investors tend to be more concerned about negative news, watch listings should be more important in building anticipation for downgrades than for upgrades. Figures from our dataset support this notion. While about a third of all downgrades are preceded by a watch listing, so are only 15 per cent of all upgrades. Finally, it has often been noted that there is an incentive to leak good news (e.g. Holthausen and Leftwich, 1986; Goh and Ederington, 1993; Gande and Parsley, 2005; Alsakka and ap Gwilym, 2012; Christopher et al., 2012), so the relevance of watch listings in building anticipation is conceivably much lower in the case of upgrades. We interpret the fact that our results are consistent with this literature as reassuring in terms of the validity of the regression specifications.

Our results also point to the importance of the clustering of rating announcements, especially for downgrades. While the controls for both clustering within (*SimActsWdwEvt*) and across countries (*SimActsDayNonEvt*) are highly significant in the downgrade regressions, the effect of across-clustering is only marginally significant once for upgrades. This appears plausible in light of the stylised facts presented in 2.2.2 because simultaneous announcements on several countries by one or more agencies occur much less frequently for upgrades than for downgrades. Moreover, the coefficients are correctly signed for both upgrades and downgrades, suggesting that the spread-decreasing (spread-increasing) spillover effects of an upgrade (downgrade) are all the more pronounced when one or more upgrades (downgrades) are announced for the "non-event" country at the same time.

A similar statement regarding the signs cannot be made with the same degree of confidence for *SimActsWdwEvt*, which measures the number of upgrades (downgrades) announced by other agencies over a 14-day window before the respective upgrade (downgrade).[14] While we again find strong differences in significance between upgrades and downgrades as well as opposing signs, one need not necessarily expect within-clustering to have an additional spread-increasing effect over the event window for downgrades. Instead, the variable might subsume two opposing effects. On the one hand, the clustering of downgrades over a short interval could imply that any announcement is less relevant individually. In that case, one would expect a negative coefficient. On the other hand, clustering is much more prevalent in crisis times (see 2.2.2). Thus, *SimActsWdwEvt* tends to be higher in times of market turbulence or global risk aversion when spreads against a "safe-haven" investment like US Treasuries are upward-trending, too (e.g. International Monetary Fund, 2004, 2006; García-Herrero and Ortíz, 2006; González-Rozada and Levy Yeyati, 2008). As this is consistent with a positive sign, the significantly positive coefficients for downgrades suggest that we may be picking up a substantial turbulence component.

Since the literature provides little guidance on whether this is what is driving our results, we include the S&P 500 Volatility Index (*VIX*), a commonly used proxy for global risk aversion (De Santis, 2012). As expected, its coefficient is positive and

[14] In choosing the window length, we follow Gande and Parsley (2005) who employ a two-week duration for a comparable control variable. However, using a one-week or three-week window instead does not alter the conclusions. Moreover, the reader may note that we do not report a variable capturing similar rating announcements made *on the same day* by other agencies in our baseline. This is due to the unattractive property that this variable drops out in the upgrade regressions since there is not a single event of multiple upgrades of a country on the same day in our sample. Therefore, in the interest of comparability, we choose not to report downgrade regressions with that control either. These regressions show, however, that the measure is always insignificant for downgrades, regardless of whether it is included in addition to, or as a stand-in for, *SimActsWdwEvt*. All results are shown in Table B.7 in the Appendix.

significant for both upgrades and downgrades, given the relation between market turbulence and yield spread drift. Interestingly, the coefficient on *SimActsWdwEvt* is still positive but slightly lower than before. This may be due to *VIX* picking up some of the turbulence effect previously captured by *SimActsWdwEvt*. Hence, there is indeed evidence that clustering may also reduce the spillover relevance of individual rating events that take place in a period of many similar announcements by other CRAs.

Finally, we subject our baseline regressions to a number of robustness checks. In doing so, we focus on downgrades because these are significantly more relevant from a policy perspective than upgrades and, as will be shown in 2.4.2, the findings on the latter should be taken with a grain of salt. The results of our robustness checks are reported in Table B.6 in the Appendix.

First, we address extreme rating events. One might be concerned, for instance, that grouping all downgrades of two notches or more into a single bin could obscure the impact of a few very severe rating changes that might be driving our results (see Figure 2.5). However, this is not the case as dropping downgrades of four notches or more and three notches or more, respectively, leaves the findings unchanged.

Second, we ensure that the results on negative spillovers are not merely the product of specific crisis episodes, namely the Eurozone crisis of 2010/11 and the Asian financial crisis of 1997/98. Again, our results appear to be more general as the key coefficient of interest remains robust to controlling for these two crises.

Third, in 2.3.1 we have already argued that an estimation bias due to different degrees of trust being placed in the three CRAs is unlikely by pointing to the distribution of the severity of rating changes across agencies in Figure B.1 (see the Appendix). However, the figure also shows that S&P stands out as the agency which is far less likely than the other two CRAs to issue a large downgrade conditional on announcing any downgrade at all (only 32 out of 210 negative announcements). By virtue of their relative rarity, S&P's large downgrades might hint at particularly strong deteriorations in a country's creditworthiness and thus incite especially strong reactions as well. It could therefore be a concern that those might account for our baseline result.[15] Yet, controlling for this does nothing to alter the conclusion of significant cross-border spillover effects of sovereign rating downgrades.

Finally, in 2.3.1 we have also dwelled quite extensively on literature which suggests that CRAs do not generally react instantaneously to other spread-relevant information. For lack of immediate-response behaviour in the first place, we then reasoned that it is even more unlikely that the agencies should "fine-tune" the severity of their rating changes to such information. However, concerns were pointed out to us that some large downgrades may have been motivated by particularly adverse spread developments in the run-up to the announcement.[16] Note that because we look at spillover effects on *other* countries, it is immaterial whether spreads in the event country also continue their particularly strong increase from prior to such announcements over the two-day event window. To interfere with our estimation results and bias the coefficient on $LARGE$ upwards, not only would negative spread developments in the event country need to be at least partly representative of those in non-event countries, but spreads in the latter would also need to widen particularly strongly during the event window. Moreover, as a global turbulence

[15] Moreover, some studies, such as Ismailescu and Kazemi (2010), continue to single out S&P and ignore other CRAs' announcements on the grounds that early research into sovereign credit rating announcements found S&P's to be less anticipated (e.g. Reisen and von Maltzan, 1999; Gande and Parsley, 2005). It is worth emphasising, though, that an agency such as Fitch, for example, only entered the business as late as 1994. Therefore, not only were there no corresponding rating actions to examine by earlier studies to begin with, but it is also quite conceivable that part of S&P's alleged special position was eroded over time. The summary of more recent research provided in Alsakka and ap Gwilym (2012) also suggests that there is no single agency whose announcements are generally more relevant than those of the other two CRAs.

[16] The ratings rationale provided by Moody's for its four-notch downgrade of Portugal on 5 July 2011 may be viewed as a case in point, which names as the "first driver informing [the] downgrade ... the increasing probability that Portugal will not be able to borrow at *sustainable rates* in the capital markets" (emphasis added). One could interpret this to refer to a widening of spreads prior to the rating change.

component, *VIX* should already capture some common component of spread developments across countries. We nonetheless also run a regression which includes as an additional control variable the change in the event country's spread over the 14-day window prior to the event. While data limitations on event country spreads allow us to do so for only about 60 per cent of the original downgrades, our key finding continues to hold.

2.4.2 Spillover channels

After providing evidence for the existence of spillover effects in the sovereign bond market, in particular for downgrades, we now turn to potential channels of those spillovers. While the regressions presented so far control for a multiplicity of factors pertaining to event and non-event countries *on their own*, they do not — with the exception of Δ*InitRat* — account for *bilateral* characteristics of event and non-event countries. However, bond market reactions in the wake of rating announcements in other countries might differ depending on similarities and bilateral linkages, which may be highly relevant from the perspective of policymakers.

We therefore augment our final baseline specification (column (3) in Table 2.1) by whether the event and non-event country belong to the same geographical region (*Region*), whether they are members of a common major trade bloc (*TradeBloc*), and the importance of the event country as an export destination for the non-event country (*ExpImpEvt*). We also account for the degree of financial integration by the event and non-event country's capital account openness (*CapOpenEvt* and *CapOpen-NonEvt*). Finally, we consider the size of the event country's GDP (*SizeEvt*) as well as differences between event and non-event countries in terms of GDP (Δ*Size*) and trend growth (Δ*TrendGrowth*). Definitions and sources for these variables are also reported in Table B.4 in the Appendix. The estimation results are shown in Tables 2.2 and 2.3.

Table 2.2: **Spillover channels, upgrades**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LARGE	-0.0128*	-0.0128*	-0.0111	-0.0094	-0.0117*	-0.0142**	-0.0115*
	(0.0067)	(0.0067)	(0.0071)	(0.0071)	(0.0068)	(0.0066)	(0.0069)
InitRat	0.0000	0.0001	-0.0005	0.0012	0.0027**	0.0031***	0.0032**
	(0.0010)	(0.0010)	(0.0010)	(0.0012)	(0.0013)	(0.0012)	(0.0014)
Δ*InitRat*	0.0009	0.0010	0.0006	0.0006	0.0012*	0.0011	0.0008
	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0008)
OnWatch	0.0070	0.0070	0.0066	0.0065	0.0080	0.0085	0.0072
	(0.0058)	(0.0058)	(0.0060)	(0.0061)	(0.0059)	(0.0061)	(0.0063)
SimActsWdwEvt	-0.0013	-0.0013	-0.0058	-0.0071	-0.0026	-0.0032	-0.0090
	(0.0057)	(0.0057)	(0.0059)	(0.0060)	(0.0058)	(0.0059)	(0.0062)
SimActsDayNonEvt	-0.0877	-0.0903	-0.1024	-0.1059*	-0.0883	-0.0950	-0.1128*
	(0.0546)	(0.0549)	(0.0625)	(0.0642)	(0.0546)	(0.0578)	(0.0681)
VIX	0.0017***	0.0017***	0.0019***	0.0018***	0.0017***	0.0018***	0.0019***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Region		0.0109	0.0146*	0.0144*	0.0128*	0.0125*	0.0169**
		(0.0071)	(0.0080)	(0.0081)	(0.0073)	(0.0075)	(0.0084)
TradeBloc			-0.0100	-0.0093			-0.0125*
			(0.0065)	(0.0065)			(0.0069)
ExpImpEvt			-0.1080	-0.1112			-0.0916
			(0.2149)	(0.2154)			(0.2148)

(continued on next page)

Spillover channels, upgrades (continued)

CapOpenEvt				-0.0082***			-0.0099***
				(0.0024)			(0.0024)
CapOpenNonEvt				0.0002			-0.0021
				(0.0048)			(0.0051)
SizeEvt					0.0279	0.0257	0.0427*
					(0.0190)	(0.0196)	(0.0219)
Δ *Size*					-0.0399**	-0.0404**	-0.0459**
					(0.0187)	(0.0194)	(0.0215)
Δ *TrendGrowth*						-0.0001	-0.0001
						(0.0001)	(0.0001)
N	29,950	29,950	27,962	27,627	29,329	28,904	27,050
Event countries	92	92	90	89	92	91	88
Non-event countries	73	73	71	70	72	72	70
Upgrades	595	595	582	577	592	584	566
R^2	0.0223	0.0223	0.0221	0.0221	0.0235	0.0271	0.0269

Notes — This table shows regressions investigating potential spillover channels for up to 595 upgrade announcements made by S&P, Moody's, and Fitch between 1994 and 2011. The dependent variable is the percentage point change Δ*Spread* in non-event country spreads around the rating announcement. For this and other variable definitions, see Table B.4 in the Appendix. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and JP Morgan EMBI Global data, as well as levels and squares of non-event country bond maturities. Robust standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

There is again a notable asymmetry between the findings on upgrades and those on downgrades. This applies to both the results on the potential channels themselves and to the impact that the inclusion of additional controls has on the robustness of our baseline findings. Whereas the results for downgrades are highly stable and intuitive, they paint a more nuanced picture for upgrades.

In more detail, we find consistently that spillover effects in the case of downgrade announcements are significantly stronger within the same region than to countries outside it (see Table 2.3). The coefficient on *Region* has the correct sign, indicating that borrowing costs increase by up to almost four basis points more for non-event countries in the same region as the event country than for those outside it. Our findings appear plausible since countries in the same geographical region are more likely to share institutional or cultural characteristics and to have important real and financial links to one another. Apart from fundamental factors, a more mundane explanation might posit that financial markets simply find non-event countries from the same region "guilty by association". The results are also in line with a number of studies which focus on one or more particular regions from the start (e.g. Arezki et al., 2011; Alsakka and ap Gwilym, 2012; De Santis, 2012). Surprisingly, we obtain positive coefficients for upgrades in Table 2.2 as well, which would suggest that those are less likely to induce spillovers within than across regions. While one could imagine that belonging to a particular region does not matter for upgrade announcements due to an asymmetric perception by investors, the fact that the coefficients are often significant is not easily rationalised. On a positive note, though, the magnitude for upgrades is only about a third of that for downgrades — and statistical significance is also lower. Therefore, in the interest of comparability and as an important economic control, we retain *Region* in all specifications.

The two trade controls, i.e. common membership in a major trade bloc (*Trade-Bloc*) and the non-event country's ratio of exports to the event country to domestic

Table 2.3: **Spillover channels, downgrades**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LARGE	0.0207***	0.0206***	0.0217***	0.0231***	0.0222***	0.0224***	0.0244***
	(0.0066)	(0.0066)	(0.0069)	(0.0069)	(0.0070)	(0.0070)	(0.0073)
InitRat	-0.0008	-0.0006	-0.0010	-0.0014	-0.0017	-0.0017	-0.0031
	(0.0017)	(0.0017)	(0.0018)	(0.0018)	(0.0019)	(0.0019)	(0.0021)
Δ*InitRat*	0.0008	0.0012	0.0017*	0.0015	0.0008	0.0008	0.0013
	(0.0009)	(0.0009)	(0.0010)	(0.0011)	(0.0010)	(0.0010)	(0.0011)
OnWatch	-0.0046	-0.0046	-0.0031	-0.0042	-0.0009	-0.0008	-0.0003
	(0.0054)	(0.0054)	(0.0058)	(0.0058)	(0.0056)	(0.0057)	(0.0059)
SimActsWdwEvt	0.0141**	0.0141**	0.0135**	0.0137**	0.0146**	0.0146**	0.0141**
	(0.0065)	(0.0065)	(0.0066)	(0.0067)	(0.0067)	(0.0067)	(0.0069)
SimActsDayNonEvt	0.1477**	0.1451**	0.1426**	0.1170*	0.1160*	0.1161*	0.1136*
	(0.0648)	(0.0643)	(0.0653)	(0.0610)	(0.0623)	(0.0623)	(0.0619)
VIX	0.0006*	0.0006*	0.0006	0.0006	0.0006*	0.0006*	0.0005
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Region		0.0376**	0.0329**	0.0350**	0.0379**	0.0380**	0.0348**
		(0.0153)	(0.0164)	(0.0166)	(0.0157)	(0.0157)	(0.0168)
TradeBloc			0.0159	0.0120			0.0120
			(0.0111)	(0.0116)			(0.0121)
ExpImpEvt			0.0687	0.0746			0.0580
			(0.2200)	(0.2237)			(0.2268)

(continued on next page)

Spillover channels, downgrades (continued)

CapOpenEvt				0.0102*			0.0126**
				(0.0060)			(0.0063)
CapOpenNonEvt				0.0090			0.0081
				(0.0083)			(0.0088)
SizeEvt					0.0222	0.0221	0.0247
					(0.0290)	(0.0294)	(0.0330)
Δ*Size*					-0.0169	-0.0170	-0.0146
					(0.0218)	(0.0223)	(0.0253)
Δ*TrendGrowth*						0.0000	0.0000
						(0.0000)	(0.0000)
N	21,931	21,931	20,633	20,352	21,031	20,885	19,724
Event countries	84	84	81	80	82	82	79
Non-event countries	73	73	71	70	72	72	70
Downgrades	427	427	416	414	416	416	405
R^2	0.0423	0.0428	0.0423	0.0416	0.0441	0.0442	0.0434

Notes — This table shows regressions investigating potential spillover channels for up to 427 downgrade announcements made by S&P, Moody's, and Fitch between 1994 and 2011. The dependent variable is the percentage point change Δ*Spread* in non-event country spreads around the rating announcement. For this and other variable definitions, see Table B.4 in the Appendix. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and JP Morgan EMBI Global data, as well as levels and squares of non-event country bond maturities. Robust standard errors in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

GDP (*ExpImpEvt*), are signed as expected throughout, pointing to more pronounced spillover effects for both upgrades and downgrades when such linkages exist, or when they are stronger. However, they are only mildly significant once for upgrades (see specification (7) in Table 2.2). Moreover, the stability in magnitude and significance of *Region* upon inclusion of the trade variables, in particular for downgrades, seems to indicate that stronger spillover effects within regions cannot easily be explained by real linkages.[17]

Apart from real linkages, we would ideally also like to control directly for bilateral financial linkages, e.g. the exposure of non-event country investors to event country sovereign bonds. Unfortunately, even use of the most comprehensive data from the IMF's Coordinated Portfolio Investment Survey leads to a massive reduction in the number of observations and major selection effects along the time series and country dimensions. This renders virtually impossible any comparison with the baseline results.

However, to the extent that trade also captures a notable portion of variation in bilateral asset holdings, our findings for real linkages also hold for financial linkages. As shown by Aviat and Coeurdacier (2007), there is indeed strong evidence that trade is a powerful determinant of bilateral (bank) asset holdings.[18] The disadvantage of using trade as a proxy for financial linkages, though, is that we cannot discriminate between the effects of real and financial linkages.

To get an idea of the distinct impact of financial linkages, we therefore approximate financial integration by the degree of the event and non-event country's capital

[17]The fact that the correlation of the two trade variables with the region control is low does not support multicollinearity as a technical explanation for this result. Moreover, replacing *ExpImpEvt* by other proxies for bilateral trade does not change the picture either (see Table B.8 in the Appendix).

[18]In addition, through its correlation with FDI, trade may proxy for cross-country bank exposure since bank lending may follow domestic companies when those set up operations abroad (see e.g. Goldberg and Saunders, 1980, 1981; Brealey and Kaplanis, 1996; Yamori, 1998).

account openness as measured by the Chinn-Ito index (Chinn and Ito, 2006).[19] While this index cannot be used to gauge the effects of *bilateral* financial linkages, it is still interesting in its own right to look at and control for level effects. The results show that the event country's capital account openness tends to significantly amplify cross-border spillover effects. Since bonds of financially open countries should be more likely to be held by foreign investors, this result is highly intuitive.

The evidence on the remaining potential channels is succinctly summarised for downgrades. In no specification do the size of the event country's GDP (*SizeEvt*), its increment over that of the non-event country ($\Delta Size$), or differences in trend growth between event and non-event countries ($\Delta TrendGrowth$) turn out to be significant determinants of the strength of bond market spillovers. At the same time, all results from the baseline and augmented baseline regressions (columns (1) and (2) in Table 2.3) prove remarkably stable in terms of both magnitude and significance.

This contrasts with the corresponding findings for upgrades. On the one hand, we obtain a number of interesting results for the size and growth controls. On the other hand, the augmented regressions raise some doubts on our main variable of interest, *LARGE*, in terms of statistical significance. The latter alternates between specifications and vanishes in some, yet in view of the considerably stronger baseline results for downgrades, this is not entirely surprising. It merely serves to underscore the asymmetry that exists between positive and negative rating changes. However, this also means that the evidence on the potential channels for upgrades should be taken with a grain of salt.

In this regard, the most interesting result is probably the observation that, given the event country's size and initial rating, positive spillovers are larger the smaller the non-event country relative to the event country ($\Delta Size$). The magnitude of the co-

[19] We choose this index due to its broad coverage over time, which allows us to maintain comparability with the baseline results. The index has also been used extensively in recent literature (e.g. Fratzscher, 2012; Hale and Spiegel, 2012; Frankel et al., 2013).

efficient suggests that non-event countries which are half (two-thirds) the size of the event country experience an additional positive spillover effect of about four (two) basis points, as compared to non-event countries as large as the event country.[20] While the effect appears to be relatively small, its direction is still interesting, in particular when viewed in conjunction with the fact that, across the whole sample, larger and more highly rated countries induce smaller spillovers (columns (5) to (7) in Table 2.2). This would be consistent with a world in which positive spillover effects matter primarily within a group of small developed and emerging countries but less so within a group of large, developed countries, and in which the latter have little impact on the former. The insignificance of the absolute difference in trend GDP growth rates between event and non-event countries ($\Delta\, TrendGrowth$) as a further measure of differences in economic development does nothing to contradict this interpretation. In view of the generally more ambiguous results for upgrades, however, we do not wish to overemphasise this point.

2.4.3 Discussion

Our results can be condensed into the following stylised facts. First, there is strong evidence of statistically significant, negative spillover effects of downgrade announcements. This result proves highly robust to controlling for anticipation through watch listings and the clustering of rating announcements. Second, negative spillover effects are more pronounced among countries in a common region, which cannot be explained by measurable fundamental links and similarities between countries. Third, reactions to upgrades are, if anything, much more muted than for downgrades, suggesting important asymmetries in the sovereign bond market's treatment of the two

[20] $\Delta Size$ is defined as the difference between the event and non-event country's log GDPs or, equivalently, the log of the ratio of the two GDP levels. Therefore, a decrease in relative non-event country size by half (two-thirds) amounts to an increase in $\Delta Size$ of about one hundred (fifty) per cent. With an absolute coefficient of roughly 0.04, the (semi-elasticity) marginal effects therefore obtain as four and two basis points, respectively.

types of announcements. Fourth, evidence on the channels behind positive spillover effects, if any, offers a more complex picture and appears relatively ambiguous.

Which conclusion to draw from this? To begin with, there is a strong case for the notion that negative sovereign rating announcements, i.e. those of most concern to policymakers, do matter in inducing spillovers across markets. Such is the outcome of the explicit identification strategy used in this chapter, which demonstrates that, all other things equal, large downgrades of two notches or more cause larger hikes in spreads than small one-notch downgrades. This suggests a role for CRAs and their actions in sovereign bond markets, be it through the revelation of new information on creditworthiness which acts as a "wake-up call" for investors to reassess fundamentals in other countries (Goldstein, 1998), or simply by providing a coordinating signal that shifts expectations from a good to a bad equilibrium (Masson, 1998; Boot et al., 2006).

However, a major regulatory focus on the activities of CRAs would also require negative spillover effects of substantial *economic* magnitude. In this chapter, we find the incremental impact of large downgrades to be a little over two basis points, which may appear limited at first glance. Yet, it is important to note that this does not represent the total effect that policymakers would be concerned about. This total effect can be thought of as consisting of a "base effect" that small downgrades have, compared to a benchmark scenario of no downgrades anywhere, plus an additional impact for large downgrades — which is what we measure. Of course, the reason we focus on the latter lies in the impossibility of cleanly identifying the "base effect" of rating changes unless one rules out the existence of rating-induced spillovers from the beginning (see the discussion in 2.3.1). Nonetheless, the total effect is conceivably a multiple of the one we estimate. Suppose the "base effect" were only twice as large as the incremental one we measure. Then, the implied total effect would already amount to approximately six basis points. To put this into perspective, the average sovereign bond spread vis-à-vis US Treasuries at the time of the downgrade announcements in our sample is 3.25 per cent, or 325 basis points. While the total effect of downgrades is relatively small in comparison, one has to bear in mind that governments often need to refinance large amounts of debt, which magnifies the impact of even small spread differences. Moreover, there is still a regional effect of up to four basis points on top of that, suggesting that concerns about negative spillovers in the sovereign debt market should not be lightly dismissed.

Finally, from a policymaker's point of view, the finding that the increased strength of negative spillovers within regions cannot be explained away by measurable linkages and similarities between countries might also be a cause for concern. Even though limited data availability precludes an all-encompassing analysis of potential channels, there is little to suggest that one can comfortably rule out that some countries are found "guilty by association" with the event country. Moreover, such behaviour on the part of investors would likely extend to their reactions to news other than rating announcements. While it is hard to see an obvious remedy, the potential problem would seem to be much more general and, above all, rooted in investor behaviour. Hence, it is not clear that putting the primary emphasis on CRAs will prove effective in this regard.

2.5 Conclusion

Concerns about negative spillovers across sovereign debt markets in the wake of sovereign rating changes have recently resurfaced on the agenda of policymakers. In this chapter, we study the existence and potential channels of such spillover effects. More specifically, we avail of an extensive dataset which covers all sovereign rating announcements made by the three major agencies and daily sovereign bond market movements of up to 73 developed and emerging countries between 1994 and 2011.

Based on this, we propose an explicit counterfactual identification strategy which compares the bond market reactions to small changes in an agency's assessment of a country's creditworthiness to those induced by all other, more major revisions. In doing so, we account for a number of factors that might impact on the reception of individual announcements.

We find strong evidence in favour of negative cross-border spillovers in the wake of sovereign downgrades. At the same time, there is no similarly robust indication as to positive spillovers since reactions to upgrades are much more muted at best, which points to an important asymmetry in the sovereign debt market's treatment of positive and negative information. Regarding the channels of negative spillover effects, our results suggest that those are more pronounced for countries within the same region. Strikingly, however, this cannot be explained by fundamental linkages and similarities, such as trade, which turn out to be insignificant.

Therefore, there is reason to believe that policymakers' concerns about negative spillover effects are not unfounded. In fact, the lack of power of a set of fundamentals in explaining the added regional component may reinforce, or give rise to, concerns about the ability of investors to discriminate accurately between sovereigns. This could also be of more general interest because such behaviour is likely to carry over to reactions to various kinds of non-CRA news in other markets and sectors, too. Hence, important though they are, a sole focus on CRAs and their actions might be missing a bigger picture.

Chapter 3

The Output Costs of Soft and Hard Sovereign Defaults*

3.1 Introduction

It is widely recognised that sovereign debt crises have adverse economic effects. But how costly is a sovereign default? Answering this question is of crucial importance both for the theory of sovereign debt[1] and for policymakers in crisis situations. Past empirical research on the costs of default has commonly relied on a binary debt crisis measure of default versus non-default. In this chapter, we propose the use of more continuous crisis measures to study the output costs of default. Specifically, we distinguish between cases of "hard" and "soft" default based on a new procedural index that tracks a government's payment and negotiation behaviour vis-à-vis foreign creditors during a default spell. We also differentiate between defaults using an outcome measure of debt crises, namely the size of creditor losses or "haircuts" captured at the end of a debt crisis. Our results show that the output loss during

*This chapter is joint work with Christoph Trebesch.

[1]Since Eaton and Gersovitz (1981), assumptions on the cost of default have shaped the setup and results of sovereign debt models in a fundamental way (see the surveys by Eaton and Fernandez, 1995; Panizza et al., 2009).

a debt crisis is much deeper for episodes of "hard" defaults. This suggests that not only the incidence of default matters, as implied by much of the previous literature, but also its severity.

Our research design is motivated by the existence of striking differences between debt crisis events, as documented in case studies by Roubini and Setser (2004) or Sturzenegger and Zettelmeyer (2007). On the one hand, there are cases such as Russia during the 1990s, Ecuador 2008/2009 or Argentina 2002-2005, in which governments opted for a unilateral payment moratorium, engaged in anti-creditor rhetoric, and at times even refused to negotiate with their foreign banks and bondholders. These confrontational defaults also involved high creditor losses (haircuts) of up to 70%. On the other hand, there are debt crises that got resolved in a consensual manner, with close creditor consultations, little (or no) missed payments, and low haircuts of around 10-20%. Recent examples include the Ukraine in 1999/2000 and Uruguay in 2003.

The aim of this chapter is to take the heterogeneity in sovereign debt crises seriously and to empirically assess whether the output costs of default differ depending on the type of default. We proceed in two steps: In the first step, we analyse the link between what we call government "coerciveness" towards creditors and GDP growth during the default episode. We measure "coerciveness" based on a new database on debt crisis resolution processes, which categorises a government's debtor policies on a scale from 1 (very creditor-friendly) to 10 (very confrontational). This dataset, compiled by Enderlein et al. (2012), tracks government actions towards private external creditors throughout a debt crisis along nine dimensions of payment and negotiation behaviour. The indicator of coerciveness is coded on an annual basis and shows a strong variation not only within but also across debt crises and defaulting countries. This is advantageous compared to a simple default dummy, since it allows us to exploit both the time variation and the cross-sectional variation in debtor behaviour. In a second step, we build on the database of investor losses by Cruces and Trebesch (2013), and investigate how the haircut size is related to post-crisis GDP growth. With this two-step approach we are able to trace out the output effects of hard and soft defaults over the whole default episode, starting from the first missed payments to the conclusion of the debt restructuring and the subsequent post-default period.

We find that coercive government behaviour during default is associated with a much steeper drop in output. On average, coercive or "hard" defaults see a significantly lower GDP growth of up to six percentage points annually compared to "soft" defaults in which the government opted for a consensual stance. Renegotiation patterns are thus an important predictor for growth during debt crises, which can take up to 15 years. However, we do not find a robust relationship between the size of haircuts at the end of a debt crisis and the subsequent growth performance.

The main challenges for interpreting these results are (i) omitted variable bias, as common shocks could affect both output and coerciveness/haircuts, and (ii) reverse causality, since changes in output could explain the type of default and not vice versa. In the main body of the chapter, we do our best to address these challenges: We account for a battery of control variables, including the set of macro controls commonly used in the growth literature, but also crisis duration, banking and currency crises, and country ratings. We further include lagged growth as well as country and time fixed effects in our regression and control for the presence of country-specific time trends. Besides, we test the influence of lagged growth on government behaviour and find that it is not a good predictor for current coerciveness. Moreover, we attempt to tease out the surprise component in debtor coerciveness by using start-of-year country credit ratings as well as lagged coerciveness as predictors and conclude that it is unexpected debtor coerciveness which explains the significance of our main coefficient of interest. All in all, we find little evidence for reverse causality and have a hard time identifying a confounder that can explain away our main result. The correlation between the "type" of default and output performance is quantitatively large and proves to be highly robust during defaults.

Our findings have important implications for theory. Specifically, we shed doubt on a widely used assumption of modern dynamic general equilibrium papers with defaultable debt, namely that sovereign defaults trigger a lump-sum output cost which is fixed and does not depend on the share of debt repudiated (see e.g. Aguiar and Gopinath, 2006; Arellano, 2008; Yue, 2010; Arellano and Ramanarayanan, 2012; Hatchondo and Martinez, 2012; Hatchondo et al., 2013; Aguiar et al., 2013, to name just a few). For calibration purposes, this literature has often assumed a fixed output loss of two per cent in default years.[2] Our results indicate that the output costs of default may in fact be much higher or lower than that, depending on the severity of default. We thus provide empirical backing for recent contributions in which the costs of a default increase in the scope of default or in the size of (expected) haircuts (see in particular Bolton and Jeanne, 2007; Adam and Grill, 2013; Arellano et al., 2013).[3] This notion of proportional default costs shapes modelling in a fundamental way and also has "far-reaching implications for policy analysis", as emphasised by Corsetti and Dedola (2012).

Regarding the empirical literature, we are among the first to account for the magnitude and the severity of sovereign defaults. Several earlier studies have emphasised the important differences across debt crises events. Obstfeld and Taylor (2003), for example, distinguish between "partial" and "full" defaulters, while Eichengreen (1991) refers to "light" versus "heavy" defaults. However, no contribution has yet

[2] This figure has been used with reference to Sturzenegger (2004), who run cross-country and panel growth regressions for the period between 1974 and 2000.

[3] Earlier seminal papers with proportional output costs are Calvo (1988) and Bulow and Rogoff (1989). In the corporate context, proportional default costs are more established, see e.g. Zame (1993) and Dubey et al. (2005).

quantitatively analysed how different crisis characteristics affect a country's GDP growth in a large sample of countries and crises.

Previous papers on the output costs of debt crises by Sturzenegger (2004), Borensztein and Panizza (2009), and Furceri and Zdzienicka (2012) all use a binary default measure by Standard & Poor's and conclude that defaults are associated with a steep drop in output, with estimates ranging from two to six percentage points lower growth, depending on the sample and estimation method. De Paoli et al. (2009) show, that the fall in output is particularly large when defaults are accompanied by banking and/or currency crises. Panizza et al. (2009) use quarterly data to show that, on average, output contractions *precede* defaults and that growth picks up after the quarter in which default occurs. To our knowledge, there is barely any work on the real effects of debt renegotiation patterns during and after default. Thus, we add to this literature by conducting the first in-depth study on debtor country behaviour (the "type" of default) and the associated output dynamics during and after a debt crisis. In line with Cruces and Trebesch (2013), we conclude that it is crucial to account for the scope of default when studying its consequences. A dichotomous categorisation may be overly simplistic and can introduce measurement error.

The remainder of this chapter is organised as follows: Section 3.2 frames our analysis, describes our empirical strategy and discusses the construction of our coerciveness and haircut measure. In section 3.3, we analyse the link between a government's payment and negotiation behaviour and GDP growth during the default episode, while section 3.4 investigates how the haircut size is related to post-crisis growth. Section 3.5 concludes.

3.2 Theory and Data

3.2.1 Theoretical considerations

Why do sovereign defaults result in output losses? And why should output losses be higher in debt crises with high haircuts and confrontational government behaviour? Theory points to several potential channels. Eaton and Gersovitz (1981) famously propose that a default will result in exclusion from international capital markets, which undermines a country's ability to smooth out consumption and should hence result in lower output during crises. Recent contributions show that sovereign defaults also negatively affect the access of private firms to foreign credit, which should further intensify this effect (Arteta and Hale, 2008; Mendoza and Yue, 2012). Bulow and Rogoff (1989) stress the role of sanctions, such as trade sanctions or legal sanctions, which increase in the share of debt that is repudiated, resulting in a disruption of goods and asset trade.

Another branch of the literature emphasises the role of reputational spillovers and signalling. Grossman and van Huyck (1988) suggest that lenders differentiate between excusable defaults and cases of inexcusable debt repudiation. High creditor losses and coercive debt policies that are not justified by a bad state of the economy could thus lead to a deterioration of country reputation and, thereby, to "collateral damage" on the domestic economy and lower output. Relatedly, Cole and Kehoe (1997, 1998) develop a model of generalised reputation. Governments who are deemed untrustworthy in one area will also be seen as untrustworthy in other fields. Confrontational behaviour in the sovereign debt arena could therefore curb foreign investments, capital flows or the country's standing in international negotiations, with adverse growth effects. More recently, Sandleris (2008) argues that the repayment behaviour of sovereigns acts as a signal on the country's fundamentals and on the government's willingness (or ability) to undertake reforms and to protect property rights. Expropriative debt policies could thus affect agents' beliefs both at home and abroad, leading to less investments and lower growth.

Based on these theoretical considerations, the chapter's central hypothesis is that high (expected) haircuts and confrontational debt policies vis-à-vis foreign creditors create "collateral" damage on the domestic economy, resulting in lower growth. To test this hypothesis, a key challenge is to control for potential confounding factors that influence both output and the type of default, as well as to account for potential reverse causality. In the main body of this chapter, we spend a lot of effort to approach these challenges. As in most of the earlier literature, we will however not analyse the underlying channels at work, meaning that we do not test whether the observed link between default and growth can be explained by sanctions, reputational damage and/or signalling. The simple reason is that it is difficult, if not impossible, to tease out the role of reputation or signalling from country-level data.

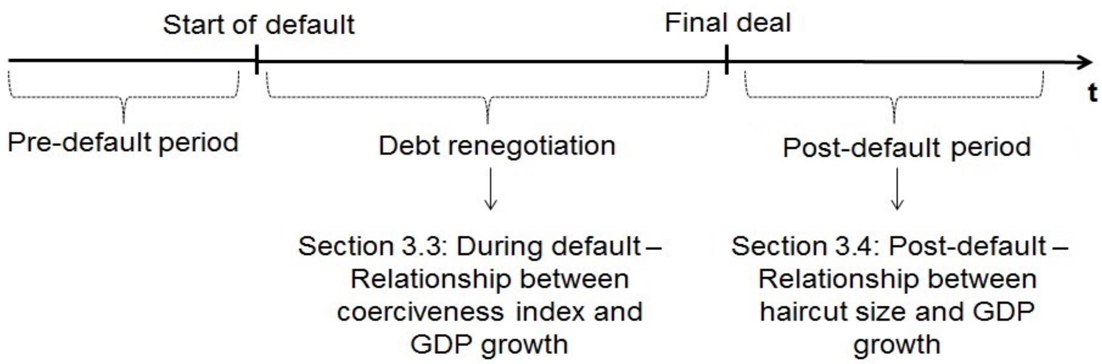
3.2.2 Empirical approach

Existing work on the link between default and growth, such as Sturzenegger (2004) and Borensztein and Panizza (2009), has regressed the annual growth rate of real GDP per capita on a dummy for the start of default, lagged values of this dummy, and a set of standard control variables as used in the cross-country growth literature. We argue that a binary categorisation of sovereign defaults is too simplistic as it hides the substantial variation in crisis characteristics. We therefore propose the usage of more continuous measures and run a horse-race between those and the binary default dummy. In addition, we seek to trace the relationship between debtor default behaviour and GDP over the entire default episode — from the start of default, over the whole debt renegotiation period (which lasts more than five years on average) and up to five years after the crisis ends with a final restructuring.

We distinguish between "hard" and "soft" defaults by building on two distinct empirical measures on the heterogeneity of debt crisis events. The first measure is the index of debtor coerciveness, which is procedural and captures differences in crisis characteristics during default (see subsection 3.2.3 for details). The second measure is the main outcome of debt renegotiations, namely the size of creditor losses or "haircuts" implied in the debt restructuring agreement. We focus on "final" restructurings as defined by Cruces and Trebesch (2013), meaning those restructurings that cured the debt crisis events, with no new default in the following four years (see subsection 3.2.4 for details).

An advantage of the index of debtor coerciveness is that it varies on an annual basis and is observable throughout the debt crisis. In contrast, haircuts are only observable once, namely at the end of debt renegotiations, which can take many years. An illustrative example is the default of Peru, which lasted from the mid-1980s until the late 1990s, when the crisis got resolved with the Brady deal of 1997. During these 15 years, Peru's debt policy varied substantially. The government's stance vis-à-vis its foreign banks was very confrontational after President Garcia imposed a unilateral debt moratorium following his inauguration in 1985, but the debt policy became more cooperative after President Fujimori took over in 1990. This variation in debtor policy is captured accurately by the coerciveness index, while a haircut is only available for the end of default in 1997. In principle, one can make the argument that creditors quickly form expectations on the scope of losses which they are likely to suffer and that this expected haircut will be roughly in line with the actual final haircut. However, for longer crises, such the one in Peru, it is far-fetched to use the 1997 haircut as a proxy for loss expectations in the mid- or late 1980s. We therefore use both the procedural index and the haircut estimates for our analysis. Specifically, we rely on the coerciveness index during the default

Figure 3.1: **Stylised timeline of a debt crisis and structure of this chapter**



Notes — This figure illustrates a stylised timeline of a debt crisis and shows in which section of this chapter we plan to address each stage.

period, but use haircuts as our preferred measure for the analysis of post-default growth.

The main body of this chapter proceeds as follows: In section 3.3, we analyse the relationship between "hard" and "soft" defaults and GDP growth *during* the default and debt renegotiation period, using the index of our coerciveness index as our preferred measure. In section 3.4 we then analyse the *post*-default period, now relying on haircuts as our measure to classify defaults. Our research agenda is illustrated in Figure 3.1, based on a stylised debt crisis timeline.

3.2.3 The coerciveness index

In order to classify the payment and negotiation behaviour of governments, we rely on an index constructed by Enderlein et al. (2012). This "index of debtor coerciveness" (or "coerciveness index" hereafter) was coded from quantitative as well as qualitative sources, including 20,000 pages of articles form the financial press. The idea of categorising different types of debtor behaviour towards creditors is not new. Authors like Aggarwal (1996), Andritzky (2006), Cline (2004) or Roubini (2004) all suggested that debt policies and restructuring processes vary on a spectrum from "soft" to "hard" or from "voluntary" to more "involuntary". However, Enderlein et al. (2012) provide the first comprehensive and systematic dataset suitable for econometric analysis.

The coerciveness index captures coercive measures which governments take against their private external creditors during the default episode. The index is coded for debt distress episodes only and consists of nine sub-indicators, each of which gauges observable government actions vis-à-vis foreign banks and bondholders. Each sub-indicator is a dummy variable, which is coded as one if the respective action by the government can be observed in a given year, and zero otherwise. The sub-indicators can be grouped into two broad categories: (1) "Indicators of Payment Behaviour", capturing steps by the government that directly impact on financial flows towards international banks or bondholders, and (2) "Indicators of Negotiation Behaviour", measuring negotiation patterns and aggressive rhetoric of governments.

Enderlein et al. (2012) give the exact definitions of and the theoretical rationales for each sub-indicator and provide the detailed coding procedures, descriptive statistics and stylised facts on the index. Here, we summarise each sub-indicator briefly.

The indicators of government *payment behaviour* during debt crises are the following:

1. **Payments missed?** (yes/no): Are there any payments missed by the sovereign (principal and/or interest)? Although arrears occur in most debt crisis episodes, there have been many cases in which countries restructure their debt pre-emptively, without missing payments. Examples include Chile, Algeria and Uruguay in the 1980s and, more recently, Ukraine 1998-2000 or the Dominican Republic 2005.

2. **Unilateral payment suspension?** (yes/no) The next sub-indicator asks whether the sovereign did *unilaterally* suspend payments to its creditors, i.e.

without a previous agreement with and/or notification of creditors. This indicator enables us to differentiate between outright defaults on the one hand and "negotiated defaults" on the other. Most defaults have been unilateral, but roughly one third of all debt suspensions were negotiated, e.g. in the form of a 3-month debt roll-over or a temporary suspension of principal payments.

3. **Full moratorium, incl. interest payments?** (yes/no): Is there a full moratorium of debt payments that extends also to the sovereign's payments of interest on government debt? The Institute of International Finance highlights in its "Principles for Stable Capital Flows and Fair Debt Restructuring" the importance of partial debt service and full continuation of interest payments as a sign of good faith (cp. Annex 1 of Institute of International Finance, 2013). A complete suspension of interest payments therefore constitutes a strong signal of the government's unwillingness to pay. As such, this only happens pretty rarely and is the case in only around a quarter of all annual crisis observations.

4. **Freeze on foreign assets?** (yes/no): Does the government issue emergency decrees that effectively lead to a freeze of creditor assets in the country? As this is a particularly tough and aggressive measure by the government, this is observed only on rare occasions. Examples include Argentina in 1982 and 2002, which set up capital controls that prohibited private Argentine firms to make any debt repayments to foreign creditors, and the Ukraine, which enacted harsh exchange controls during its debt crisis at the end of the 1990's.

The indicators of government *negotiation behaviour* during debt crises are:

5. **Breakdown or refusal of negotiations?** (yes/no): Does the government refuse to engage in negotiations with its creditors and/or do government ac-

tions lead to a breakdown of debt negotiations for a period of three months or more in a given year? Regular and continuous dialogue between the sovereign and its creditors are usually considered to be a key ingredient for the consensual solution of a debt crisis. Nonetheless, negotiation delays or negotiation stalemates are quite common and take place in almost half of all crisis years.

6. **Explicit moratorium or default declaration?** (yes/no): Does a key government actor (president, prime minister, minister of finance or economy, the country's chief negotiator or the president of the central bank) officially proclaim the decision to default? Such official default declarations occur quite rarely. However, once such a public proclamation is made, this is clear sign of an escalation of the crisis as it comes close to being a "declaration of war" against the country's creditors.

7. **Explicit threats to repudiate on debt?** (yes/no): Does a key government actor publicly threaten to repudiate from debt? While this is very uncommon, examples include Chile in 1986, where president Pinochet used the threat of debt repudiation as a reaction to US pressure on human rights and, most recently, Ecuador in 2008, where president Correa threatened to repudiate from debt, branding substantial parts of Ecuadorian government debt as "odious" and "illegitimate".

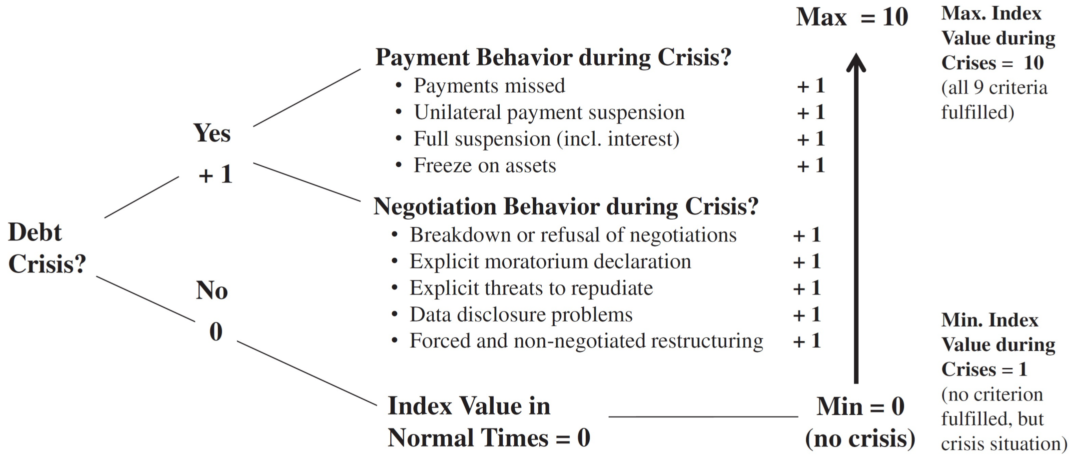
8. **Data disclosure problems?** (yes/no): Are there any data disclosure problems, i.e. does the government refuse to provide information on crucial negotiation issues or is there an open dispute with creditors due to inaccurate data? The provision of accurate and reliable data by the government constitutes a basic requirement for negotiations. Lacking accurate information, private creditors cannot reasonably evaluate restructuring proposals by the government or the country's capacity to repay. As such, information provision

is a key element for the consensual resolution of a crisis. While data disclosure problems are not very frequent, they have been of high importance in some cases, as for example in Brazil 1987, in Peru 1996 or in Ecuador 2008/09.

9. **Forced and non-negotiated restructuring?** (yes/no): Was the restructuring negotiated with creditors or unilaterally imposed by the government? This sub-indicator differentiates between restructurings that result from negotiations and those restructurings that are enforced unilaterally or launched without prior consultations on the terms and conditions. Forced and non-negotiated restructurings are rare and constitute a strong sign of coercive debt policies. The restructuring of Argentina in 2005 as well as a forced debt roll-over in Peru of 1986 are among the few examples.

The score of the final coerciveness index is additive, summing up the individual sub-indicators. The index takes the value of 1 if a country announced or started debt renegotiations but did not fulfil any other coerciveness criterion, not even missed payments. During debt crisis periods the index therefore ranges from a minimum of 1, indicating very cooperative government behaviour, to 10, for particularly aggressive debt policies. In the absence of default or debt renegotiations, the index is simply coded as 0. Figure 3.2 illustrates the construction of the coerciveness index graphically. Moreover, Figure C.2 in the Appendix shows the distribution of the coerciveness index and reports summary statistics for the coerciveness index and its sub-indicators.

Figure 3.2: **Construction of the coerciveness index**



Notes — The figure illustrates the construction of the coerciveness index. It is taken from Enderlein et al. (2012).

3.2.4 The size of haircuts

We capture the central outcome of the debt restructuring process by the size of the creditor haircuts implied by the "final" restructurings between the government and its creditors. For this purpose, we build on the database of investor losses by Cruces and Trebesch (2013) that measures creditor haircuts based on the methodology proposed by Sturzenegger and Zettelmeyer (2008) as:

$$H^i_{SZ_t} = 1 - \frac{Present\,Value\,of\,New\,Debt\,(r^i_t)}{Present\,Value\,of\,Old\,Debt\,(r^i_t)},$$

where r^i_t is the discount factor employed to calculate the present value of old and new debt instruments.

The number of final deals in our sample totals to 30 cases. Figure C.3 shows how the haircuts in our sample are distributed over time and reports some basic summary statistics for haircuts.

3.2.5 Default coding and sample composition

Our analysis covers the years between 1980 and 2009 and is based on a sample of 61 developing and emerging market economies. Starting with a full universe of economies, we arrive at our final country sample as follows: Given our focus on debt crises involving commercial creditors, we first exclude those countries for which we can reasonably assume that they had only very limited access to private credit over our sample period. Specifically, we drop all those countries that have been classified by the International Monetary Fund and the World Bank as highly indebted poor countries (HIPCs) and are therefore eligible for special support within the IMF's and the World Bank's HIPC debt relief initiative. For the same reason, we also drop small countries with a population of less than 1 million (as measured at the end of our sample period). As no advanced economy defaulted during our sample period, we moreover exclude all advanced economies in order to make our sample as homogeneous as possible. Furthermore, we leave out those countries whose debt restructurings took place under exceptional circumstances (namely Iraq and the successor states of the Socialist Republic of Yugoslavia). Finally, we drop a few defaulters for which no sufficient qualitative information on the debt restructuring process has been available (Côte d'Ivoire, Cuba, Gabon, Iran, Jamaica, Kenya, Paraguay, Trinidad and Tobago and Vietnam). Our final sample thus includes 61 developing economies, of which 25 countries experienced at least one debt crisis during our sample period while 36 countries did not. Table C.1 in the Appendix lists all countries and years included.

As it is common in the literature on sovereign defaults, we follow the default definition of Standard & Poor's and rely on their annual default list as a starting point. S&P codes a government as being in default if the government misses payments on either interest or principal of bonds or bank loans on the due date or, alternatively, if it announces a debt exchange offer that leads to less favourable conditions for creditors than those in the original contracts (cp. Appendix 1 of Standard & Poor's, 2011). However, in a few cases we extend this data, since S&P does not account for pre-default renegotiations, i.e. debt renegotiations that take place without missed payments by the sovereign. Consequently, we also consider a country to be in default in case the government publicly announced to restructure its debt. All in all, our sample covers 1,638 annual country observations, of which 217 observations are debt crisis years.

3.3 Government coerciveness and GDP growth during default

3.3.1 Graphical analysis and stylised facts

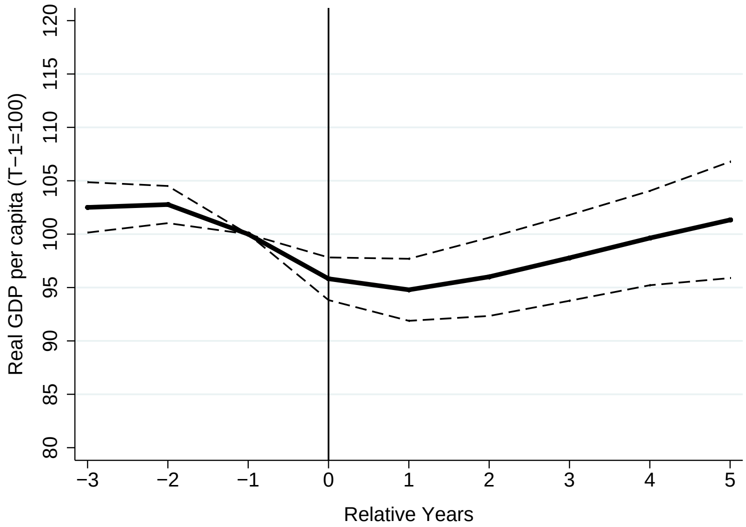
We start our analysis of growth *during* debt crises with a graphical view at the data. Figure 3.3 illustrates the development of real GDP per capita from three years before until five years after start of default. The starting year itself is labelled as year zero (black vertical line) and GDP is normalised to 100 in the year prior to default.

Panel A depicts the *average* evolution of GDP for the debt crisis episodes in our sample.[4] In line with Levy-Yeyati and Panizza (2011), we find that the onset of a debt crisis roughly marks the beginning of a recovery, at least in the full sample of crises. On average, GDP already starts to decline prior to a debt crisis and shrinks

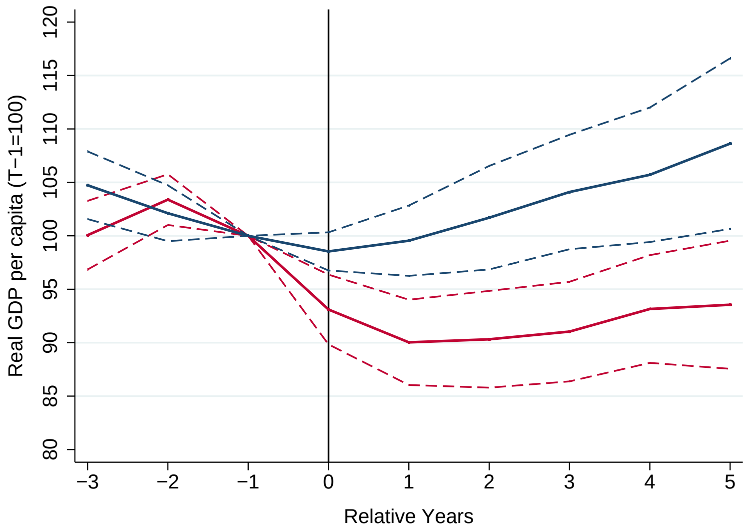
[4] In total, our sample includes 38 debt crises. This number is bigger than the number of defaulting countries (25) due to the fact that some of the countries defaulted multiple times (cp. Table C.1 in the Appendix). Figure 3.3 is based on 33 crisis episodes. For the case of Poland, our data only starts in the year 1991 and Poland is coded as being in default that year. However, since Poland has been in default since already 1981, it would be a mistake to interpret the year 1991 as the start of the Polish debt crisis and we therefore dropped this case for our graphical analysis. Furthermore, we left out the debt crises of Uruguay in 1987, Romania in 1986, Morocco in 1986 and South Africa in 1989 due to the fact that these debt crises followed within five years after the start of a prior debt crisis and therefore cannot be interpreted as independent events.

Figure 3.3: **Evolution of GDP around start of default**

(a) Panel A: All defaults



(b) Panel B: Hard and soft defaults



Notes — The solid lines plot the average development of real GDP per capita from three years before until five years after the start of default, and the dashed lines are 90% confidence intervals. Real GDP per capita has been normalised to 100 in the year before the start of default. Whereas Panel A pictures the evolution of GDP over all 33 default episodes, Panel B splits the sample into cases of “soft” (blue) and “hard” (red) defaults (at the median value of the observed average coerciveness during the default episodes) and plots the evolution of GDP for both groups separately.

in the year of default by around four per cent. One year afterwards, however, output starts to recover and reaches its pre-crisis level four years after.

In Panel B of Figure 3.3, we divide our sample into cases of hard and soft defaults. For this purpose, we compute the average value of the coerciveness index over each debt crisis and cut the sample at the median value, which is 3.4. This results in 16 cases categorised as soft defaults (government coerciveness of less than 3.4) and 17 cases of hard default (coerciveness index larger than 3.4). As can be seen, output behaves very differently for both groups. In soft default spells, output drops only marginally in the first crisis year and quickly picks up afterwards. However, the picture looks drastically different for hard defaults, as output collapses by around seven per cent in the first crisis year and continues to tumble during the subsequent year. Thereafter, the economy recovers only sluggishly, such that five years after the outbreak of the crisis, GDP still remains more than five percentage points below its pre-crisis level. Thus in stark contrast to the stylised fact of Levy-Yeyati and Panizza (2011), the default clearly does not mark the beginning of recovery for hard defaulters. As the confidence bands of the two sub-groups do not overlap, the differences in real GDP performance are statistically significant at the 10% level.

Figure 3.3 gives a first impression of the link between government coerciveness and growth. However, it should be interpreted cautiously, since it only compares the development of the unconditional GDP averages of both sub-groups. In the following subsection, we therefore analyse the relationship between debtor coerciveness and GDP in a more systematic way.

3.3.2 Regression analysis

We start with a bare bones specification in Model 1, in which we regress the annual growth rate of real GDP per capita ($Growth_{i,t}$) on a dummy variable capturing whether a country is in default ($Default_{i,t}$) and on a set of year dummies to control for global (i.e. not country-specific) time trends in GDP growth in a pooled OLS setting. In line with previous research, the default dummy turns out highly significant and negative (cp. Table 3.1, column 1). Its coefficient value of around -1.1 indicates that being in a debt crisis reduces a country's GDP growth by around 1.1% per year.

In Model 2, we now run a horse race between the binary default dummy and the coerciveness index ($Coerc_{i,t}$). As can be seen, the coerciveness index turns out highly significant with a large negative coefficient. A one notch increase in the coerciveness index is associated with a 0.6 percentage point decline in a country's real GDP growth for each crisis year. We also find that the default dummy becomes insignificant, suggesting that the coerciveness index captures relevant additional information over and above the crisis dummy.

In Models 3-7, we add the set of macro controls ($X_{i,t}$) most commonly used in the cost of default literature (Sturzenegger, 2004; Borensztein and Panizza, 2009; Levy-Yeyati and Panizza, 2011). Specifically, we include investment to GDP (*InvGDP*), rate of population growth (ΔPop), log of total population (*Log(Pop)*), percentage of the population that completed secondary education (*SecEdu*), lagged annual growth of government consumption ($GovtCons_{t-1}$), an index of civil liberties (*CivLib*), annual change in terms-of-trade (ΔToT), openness, as proxied by the ratio of imports plus exports to GDP (*Openness*), and a dummy variable for banking crises (*BankingCrisis*). Table C.3 in the Appendix provides a detailed description of each control variable and its source.

Column (3) shows that controlling for the set of macro controls barely changes the coefficient and significance of the default dummy compared to the parsimonious Model 1. It is worth highlighting that the results in Model 3 are almost identical to those of the previous literature on growth and default. Indeed, the default coefficient of -1.1 is very similar to what Panizza et al. (2009) estimate in a com-

Table 3.1: **Government coerciveness and GDP growth**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Default	-1.1404**	1.1102	-1.0994**	0.3695	0.4465	1.8683	1.1285
(Dummy)	(0.4684)	(0.7128)	(0.4418)	(0.7508)	(0.9553)	(1.2881)	(1.0057)
Coerc		-0.6181***		-0.4223**		-0.4132*	-0.5658**
		(0.1908)		(0.2021)		(0.2365)	(0.2208)
AvgCoerc					-0.4378*	-0.3742	
					(0.2636)	(0.3189)	
InvGDP			17.6827***	17.5880***	17.5669***	17.6881***	17.6366***
			(2.0774)	(2.0573)	(2.0726)	(3.4559)	(3.4972)
ΔPop			-0.3389**	-0.3194**	-0.3212**	-0.0820	-0.0869
			(0.1436)	(0.1422)	(0.1433)	(0.3446)	(0.3448)
Log(Pop)			-0.0930	-0.0843	-0.0833	-3.4221	-3.4912
			(0.0898)	(0.0891)	(0.0894)	(3.7410)	(3.7828)
SecEdu			0.0078	0.0087	0.0088	-0.0049	-0.0040
			(0.0098)	(0.0097)	(0.0097)	(0.0347)	(0.0347)
$GovtCons_{t-1}$			0.1123***	0.1109***	0.1123***	0.0886***	0.0883***
			(0.0198)	(0.0195)	(0.0197)	(0.0181)	(0.0178)
CivLib			-0.0922	-0.1043	-0.1103	-0.0529	-0.0207
			(0.0907)	(0.0904)	(0.0910)	(0.2088)	(0.2179)
ΔToT			10.3639***	10.2400***	10.3603***	9.8919***	9.8378***
			(1.2204)	(1.2030)	(1.2092)	(1.3586)	(1.3513)

(continued on next page)

Government coerciveness and GDP growth (cont.)

Openness			-0.0025	-0.0024	-0.0026	-0.0074	-0.0072
			(0.0024)	(0.0024)	(0.0024)	(0.0140)	(0.0141)
BankingCrisis			-2.5331***	-2.4218***	-2.4778***	-2.1796**	-2.1846**
(Dummy)			(0.8196)	(0.8147)	(0.8102)	(0.8442)	(0.8504)
N	1,638	1,638	1,113	1,113	1,113	1,113	1,113
Countries	61	61	45	45	45	45	45
Time fixed effects	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	NO	NO	NO	NO	NO	YES	YES
R^2	0.1291	0.1352	0.3542	0.3591	0.3571	0.3224	0.3213

Notes — The dependent variable is the annual growth rate of real GDP per capita, measured in per cent. Key explanatory variables are the coerciveness index, *Coerc*, and the average coerciveness index over a debt crisis, *AvgCoerc*. The results of the baseline specification in column 7 indicate that a one standard deviation increase in *Coerc* (equivalent to an increase of 1.97), is associated with a reduction in GDP growth of 1.15% annually. All specifications include a (non-reported) constant. Robust standard errors (for column 6 and 7 clustered by country) are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

parable estimation setup.[5] However, the default dummy again turns insignificant once we include the coerciveness index in the regression (Model 4). While the coefficient of the coerciveness index drops to (still substantial) -0.4 upon controlling for macroeconomic conditions, it continues to be highly significant.

So far, our estimations took place in a pooled OLS framework. This leaves open the question where the explanatory power of the coerciveness index stems from. Given its annual coding, it could result (1) from variation in the coerciveness index *across* different default episodes, (2) from variation of the coerciveness index *within* the default episodes, or (3) from both.

Figure 3.3 already sheds some light on this question. The significant differences in growth performance we found between episodes of hard and soft defaults suggest that the index variation *across* crises should play an important role. The results in column 5 of Table 3.1 provide further support in this regard. In this specification, we replace our (annually coded) coerciveness index with a variable capturing the average coerciveness over the entire debt crisis episode (*AvgCoerc*). While the default dummy remains insignificant, average crisis coerciveness turns out as significant and negative, albeit only at the 10% significance level. As average coerciveness varies only across but not within debt crises, this underlines our previous descriptive insight that debt crises in which governments adapt a tougher payment and negotiation stance towards their creditors are indeed associated with weaker GDP growth.

The fixed effect panel regression of Models 6 is a way to test the role of within-crisis variation in the coerciveness index. By adding country fixed effects, the coerciveness index will no longer pick up cross-country differences in coerciveness, but only

[5]Panizza et al. (2009) estimate a default dummy coefficient of -1.3. We also get a result very similar to Sturzenegger (2004), once we replace the default dummy (for each year during default) with a dummy variable that only captures the first and the second year of a debt crisis. Debt crises can then be associated with a decline in GDP growth of around 2% during the first two years of default.

the variation within countries over time. In addition, we control for the average coerciveness of each debt crisis episode. This accounts for the fact that some countries in our sample defaulted multiple times, such that the estimated coerciveness coefficient indeed only picks up the variation *within* the same debt crisis. Column 6 of Table 3.1 shows that the coerciveness index again turns out to be significant and negative in this framework, supporting the view that within-crisis variation matters for our main result. Overall, we conclude that the coerciveness index helps to explain output growth both within and across debt crisis events.

An important advantage of the country fixed effects model is that it accounts for time-invariant confounders on the country level. The coefficient of the coerciveness index in the pooled OLS Models 2 and 4 could be spurious if weak institutions or some other unobserved country characteristic drive both the level of government coerciveness and output performance during crises. The inclusion of fixed effects thus avoids that our estimation results are biased due to time-invariant country-specific characteristics.

Our baseline specification (7) therefore looks as follows:

$$Growth_{i,t} = \alpha_i + \alpha_t + \beta Default_{i,t} + \gamma Coerc_{i,t} + \delta X_{i,t} + \epsilon_{i,t} \ ,$$

where α_i and α_t stand for country and time fixed effects, respectively, $X_{i,t}$ is the vector for our set of macroeconomic controls, and $\epsilon_{i,t}$ are heteroskedasticity robust standard errors, clustered by country.

This specification only differs from Model 6 in that we leave out average coerciveness as a control. The coerciveness index therefore captures the time variation within debt crises as well as the variation between crises in the same country. Unsurprisingly, the estimated coefficient and the significance of the coerciveness index increase (to -0.57) when dropping average coerciveness.

We next check the validity of our main results in a series of robustness checks.

3.3.3 Robustness checks

Autocorrelation of standard errors

First, it is well known that the past growth performance of a country importantly predicts its contemporaneous and future growth. It is therefore possible that the regression residuals are serially correlated. Autocorrelation in the error terms would bias the estimated standard errors downwards and thus overestimate the t-statistics (Cameron and Tivedi, 2005). One way to address this problem, is to add a lagged value of our dependent variable ($Growth_{i,t-1}$), which we do in column 1 of Table 3.2. As can be seen, the results remain largely unaltered and the coerciveness index continues to be strongly significant and negative.

While including lagged GDP growth as an explanatory variable indeed solves the problem of autocorrelated error terms in our model, this step can also bias the estimation results, as famously pointed out by Hurwicz (1950) and Nickell (1981). The fixed effects centre all variables by country, which induces a correlation between the centred lagged dependent variable on the one hand and the centred error term on the other. This "Nickell bias" is of order $1/T$, such that it decreases with rising T but is very serious for panels with a short time horizon. A sample of T=30, as it is the case here, may still result in a bias of up to 20% of the true coefficient value, as Monte Carlo simulations have shown (Judson and Owen, 1999).

In order to correct for this bias, we move back to a simple OLS framework.[6] Column 2 shows that our results continue to hold, although the coerciveness index decreases in size and remains significant only at the 10% level. We therefore conclude that our baseline estimation results are robust even after accounting for the possibility of serially correlated errors.

[6]As has been shown by Beck and Katz (2011) and Judson and Owen (1999), simple OLS performs about as good as other, more complicated, techniques to correct for the bias.

Table 3.2: **Robustness checks**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Default	1.4331	0.9156	0.4132	4.2991*		-0.2283	0.4374
(Dummy)	(0.9879)	(0.7678)	(0.8686)	(2.3260)		(0.7224)	(0.8297)
Coerc	-0.4767**	-0.3603*	-0.6365***	-0.6377***	-0.8621***	-0.3825**	-0.4641**
	(0.2064)	(0.2005)	(0.2349)	(0.2214)	(0.2555)	(0.1897)	(0.2134)
$Growth_{t-1}$	0.2641***	0.3287***					
	(0.0670)	(0.0471)					
InvGDP	11.2869***	10.5757***	-12.3342**	17.2970***		-6.8872	-17.9315**
	(3.7646)	(2.2677)	(4.8568)	(3.3573)		(5.1702)	(8.1867)
ΔPop	-0.1925	-0.3184**	-0.2219	-0.1255		-0.3280	-0.4930*
	(0.3058)	(0.1370)	(0.2663)	(0.3527)		(0.2666)	(0.2919)
Log(Pop)	-1.1865	-0.0493	-0.6246	-3.2752		0.2617	-14.4855
	(2.5132)	(0.0829)	(1.5614)	(3.5767)		(2.5451)	(11.3265)
SecEdu	-0.0076	0.0013	-0.0105	-0.0033		-0.0358	0.0298
	(0.0275)	(0.0090)	(0.0222)	(0.0336)		(0.0272)	(0.0521)
$GovtCons_{t-1}$	0.0505***	0.0575***	0.1144***	0.0921***		0.0723***	0.0664***
	(0.0178)	(0.0221)	(0.0227)	(0.0178)		(0.0199)	(0.0209)
CivLib	-0.0710	-0.0518	-0.2322	-0.0277		-0.1871	-0.4380*
	(0.1664)	(0.0873)	(0.1866)	(0.2032)		(0.2163)	(0.2660)
ΔToT	9.0809***	9.1221***	9.8846***	9.6839***		7.7912***	5.7821***
	(1.1478)	(1.1134)	(1.3009)	(1.3530)		(1.1178)	(1.1677)

(continued on next page)

Robustness checks (cont.)

Openness	-0.0061	-0.0011	0.0349***	-0.0080		0.0272***	0.0953***
	(0.0107)	(0.0023)	(0.0114)	(0.0138)		(0.0094)	(0.0205)
BankingCrisis	-2.4143***	-2.6297***	-3.2914***	-1.8588**		-2.6278***	-2.3845**
(Dummy)	(0.8049)	(0.7529)	(0.9845)	(0.7821)		(0.9968)	(0.9989)
CurrencyCrisis						-4.9754***	-5.1594***
(Dummy)						(0.9458)	(0.9869)
DebtGDP						0.0038	0.0010
						(0.0085)	(0.0139)
Inflation						-0.0016***	-0.0015***
						(0.0004)	(0.0004)
N	1,113	1,113	1,073	1113	217	965	965
Countries	45	45	45	45	25	45	45
Time fixed effects	YES	YES	YES	YES	NO	YES	YES
Country fixed effects	YES	NO	YES	YES	YES	YES	YES
Crisis duration controls	NO	NO	NO	YES	NO	NO	NO
Ctr-spec. time trend	NO	NO	NO	NO	NO	NO	YES
R^2	0.3699	0.4328	0.1400	0.3447	0.0517	0.3420	0.3546

Notes — The dependent variable is the annual growth rate of real GDP per capita, measured in per cent. The key explanatory variable is the coerciveness index, *Coerc*. All specifications include a (non-reported) constant. Robust standard errors (for columns 1 and 3-7 clustered by country) are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

Endogeneity of control variables

A further problem consists in the possibility that the "true" effect of debtor coerciveness might, at least partially, be captured by other variables now included as controls, such as investment to GDP (*InvGDP*) and/or the level of openness of a country (*Openness*).[7] Consequently, we want to make sure that those variables are not endogenous to GDP growth, which in turn is affected by a country's coerciveness. To address this concern, we repeat our baseline regression in column 3, but now instrument investment to GDP as well as openness by their first two lags. Compared to the baseline estimation, our main results remain unchanged, while (*Openness*) now becomes significant and positive.

Controlling for crisis duration

One important fact about debt crises is that they vary greatly in their length. The debt crises of South Africa 1993 and Uruguay 2003, for example, only took a few months to be resolved, while the crises of Panama and Peru started in the 1980s and persisted for 14 and 15 years, respectively. If the duration of a debt crisis is correlated with debtor coerciveness, this could bias our estimation results. Descriptive statistics do not suggest a close correlation of these two variables (cp. Figure C.4 in the Appendix).[8] We nonetheless extend our regression to explicitly control for crisis duration by adding dummy variables that indicate the duration of

[7]See Sturzenegger (2004) for a similar argumentation with respect to the measurement of the output costs of a default.

[8]The pairwise correlation between crisis duration and the coerciveness index is just 0.14. Most importantly, changes in coerciveness do not exhibit any significant trend patterns over the course of a crisis. Furthermore, the coerciveness index is more or less uniformly distributed across the length of a debt crisis.

the crisis.[9] The results (reported in column 4 of Table 3.2) remain stable, suggesting that crisis duration does not bias our estimation results in a significant way.

Sub-sample of debt crisis years

Our regressions so far covered a broad sample including both debt crisis and non-crisis years as well as defaulters and non-defaulters. As a robustness check, we now test the explanatory power of the coerciveness index on growth, by restricting the sample to the sub-sample of debt crisis years only (217 annual observations). The small number of observations, however, makes it difficult to use the set of controls from our baseline regression. In column 5 of Table 3.2, we therefore regress annual GDP growth on the coerciveness index, using country fixed effects as the only control. Again, the results confirm our prior findings, which is also true if we additionally control for crisis duration.

Additional control variables

Our baseline model with time and country fixed effects avoids any bias due to unobserved time-invariant country idiosyncrasies and also accounts for the influence of a common time trend (such as the influence of the world business cycle) on country growth. However, our estimation results could still be biased due to the omission of time-varying country-specific variables correlated with both the government payment and negotiation behaviour and real GDP growth. The inclusion of the vector of macro controls $X_{i,t}$ should ease this concern to some extent. However, there could be additional variables that affect both growth and coerciveness of a country and for which we do not yet control.

[9] More technically, we add dummy variables that take on the value of one during each year in which the respective country has been *at least* i years in default (for $i \in \{1; 15\}$). This approach should provide a clean identification of the effects of crisis duration and avoids *ad hoc* assumptions on the functional form of how crisis duration affects GDP growth.

In column 6 of Table 3.2, we hence expand the set of macroeconomic controls and additionally control for the occurrence of currency crises (*CurrencyCrisis*), the debt to GDP ratio (*DebtGDP*), and a country's level of inflation (*Inflation*). Since *DebtGDP*, similar to *InvGDP* and *Openness*, might expose our estimation to endogeneity, we instrument that variable by its first two lags. The results leave our baseline estimation results by and large unchanged with coerciveness retaining its highly significant and negative coefficient.

Finally, we also add country-specific time trends as control variables to account for the presence of any country-specific (linear) time trend in GDP growth. Again, the results confirm the significant correlation of the coerciveness index with growth (see column 7 of Table 3.2). We thus find the negative correlation between coerciveness and growth to be highly robust.

3.3.4 Can we interpret our results causally?

It is possible that the observed negative correlation between coerciveness and GDP growth is due to reverse causality. Thus, output growth could well influence a government's payment and negotiation behaviour vis-à-vis its external creditors. Steep declines in GDP can erode a country's tax base and foreign exchange revenues, thus damaging the country's ability to repay. Therefore, the steeper a country's GDP decline in the context of a debt crisis, the more willing it might actually become to engage in coercive creditor policies.

To address this possibility, we test whether lagged values of real GDP per capita growth can predict current debtor coerciveness. Columns 1-3 of Table 3.3 show that the coefficients of lagged GDP growth are clearly insignificant at different lag lengths, suggesting that past growth performance does not significantly affect the government's subsequent debt policies. Of course, this does not preclude the possibility of a contemporaneous causal effect of real growth on debtor coerciveness. But

Table 3.3: **Enquiry of causality**

	Dependent variable				
	(1) *Coerc*	(2) *Coerc*	(3) *Coerc*	(4) *Coerc*	(5) *Growth*
$Growth_{t-1}$	-0.0136	-0.0128	-0.0148		
	(0.0090)	(0.0091)	(0.0091)		
$Growth_{t-2}$		-0.0107	-0.0058		
		(0.0091)	(0.0099)		
$Growth_{t-3}$			-0.0106		
			(0.0087)		
IICCR				-0.0035**	
				(0.0015)	
$Coerc_{t-1}$				0.7782***	
				(0.0298)	
SurpCoerc					-0.4677**
					(0.2264)
ExpCoerc					0.0656
					(0.2847)
N	965	964	937	1,451	965
Countries	45	45	45	45	45
Time fixed effects	YES	YES	YES	YES	YES
Country fixed effects	YES	YES	YES	YES	YES
Standard macro ctrl's	YES	YES	YES	NO	YES
R^2	0.6460	0.6461	0.6467	0.5507	0.4291

Notes — In columns (1) to (4), the dependent variable is the coerciveness index, which measures a country's negotiation and payment behaviour during each year of default. In column (5), the dependent variable is the annual growth rate of real GDP per capita, measured in per cent. Robust standard errors (clustered by country) are given in parentheses. In column (5), the standard errors have been adjusted to account for the presence of an imputed regressor bias due to the fact that *SurpCoerc* and *ExpCoerc* are not actually observed but estimated with sampling error in regression (4) (Murphy-Topel standard errors). ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

the results provide some assurance that reverse causality is not the main channel behind our findings.

To shed further light on the issue, we try to isolate the direct effects of coerciveness from potential expectation effects. This is in line with Borensztein and Panizza (2009) and Panizza et al. (2009), who argue that the drop in output at the start of debt crises could (to some extent) be driven by investor expectations about a country's default rather than by the default event *per se*. We therefore explore whether the observed output contraction can mostly be explained by imminent coercive actions of the government ("surprise coerciveness") or rather by the market's expectations about coerciveness. To the extent that investors take into account the future growth prospects of economies when forming their coerciveness expectations, the expected part of coerciveness should pick up the part of the contemporaneous correlation between coerciveness and growth that is potentially affected by reverse causality. Surprise coerciveness, on the other hand, should be predominantly free from this concern and, hence, approximate the imminent causal effect of coerciveness on growth.

To disentangle expected and unexpected coerciveness, we resort to a strategy similar to Barro (1977). It consists in dividing the coerciveness index into an anticipated and an unanticipated component and then to test the marginal influence of both components on GDP growth. To this end, we first regress a country's coerciveness on the country's credit rating ($IICCR$) at the start of each year (in January) and on lagged coerciveness ($Coerc_{t-1}$).

$$Coerc_{i,t} = \alpha_i + \alpha_t + \beta_1 Coerc_{i,t-1} + \beta_2 IICCR_{i,t} + u_{i,t} .$$

The rationale behind the explanatory variables in the regression is the following. Whereas it is reasonable to assume that the country's negotiation and payment behaviour of the past year influences the market's expectations on coerciveness for the current year, a country's start-of-year credit rating should also pick up expectations about its future payment and negotiation behaviour. If this is true, one can interpret the fitted values of this regression as the "expected" part of coerciveness, whereas the residual of the equation should proxy the "unexpected" or "surprise" part of coerciveness.

As our rating measure, we use the *Institutional Investor's* country credit ratings ($IICCR$), which have been widely used in the debt crisis literature (see Reinhart et al., 2003). The $IICCR$ is based on information provided by senior economists and sovereign risk analysts at leading global banks and money management firms. Survey participants grade each country's credit risk on a scale from 0 (maximum credit risk) to 100 (minimum credit risk). In the final index, the survey responses are weighted according to the global credit exposure of each participating institution, such that the measure is a reasonable proxy of the average market assessment of a country's willingness and ability to repay.

An important advantage of the $IICCR$ is that it has a much broader coverage than ratings by the three major rating agencies (S&P, Moody's and Fitch), going back as far as 1978 and covering more than 100 countries. For our purposes, a further crucial advantage of the $IICCR$ is that it is available on a continuous basis even *within* debt crises. This differs from most other credit ratings, which simply rate countries as "in default" without further differentiation. During defaults, the $IICCR$ credit score can thus be interpreted as indicating the (perceived and expected) severity of a debt crisis at each point in time. More specifically, the $IICCR$ survey is conducted semi-annually, in January and July of each year. Since we are working with annual data, we use the January country credit rating to capture the market's country credit risk assessment at the start of that year.

The results of the first step regression are shown in column 4 of Table 3.3. Both regressors turn out highly significant. Even though the regression includes only two explanatory variables, its R^2 of around 0.55 indicates a good fit with coerciveness data.

In a second step, the residual and the fitted value of the first step regression (interpreted as "surprise" and "expected" coerciveness, respectively) are now included as regressors in our standard growth regression, replacing the original coerciveness index. In order to avoid the problem of biased estimators — due to the fact that the imputed variables are not actually observed, but estimated with sampling error in the first step regression (cp. Murphy and Topel, 1985) —, we correct the standard errors according to the procedure proposed by Hardin (2002) and Hole (2006).

The results of the second stage regression are shown in column 5 of Table 3.3. Surprise coerciveness (i.e. the unexpected component) is highly significant and negative, while expected coerciveness does not seem to impact a country's growth. We interpret this result as a further sign that our main findings are not driven by reverse causality. This gives us confidence that our finding can be interpreted causally, i.e. that confrontational debtor policies indeed *lead to* a decline in GDP growth.

The "gold standard" to deal with the issue of reverse causality in empirical studies certainly consists in finding a strong and exogenous instrument for the dubious regressor. This, however, turns out to be a very difficult task since we need to find an instrument that is closely correlated with the coerciveness index while being exogenous to GDP growth. The exogeneity assumption is doubtful for any macroeconomic variable. We therefore turned to institutional and political variables, such as the timing of democratic elections (using only regular elections, i.e. those foreseen by the electoral cycle) and measures of democratisation. However, these political variables do not exhibit enough variation to qualify as strong instruments for the time-varying coerciveness index. Hence, even though we do find results that support our main findings, we prefer not to show the instrumental variable regressions as they are not sufficiently credible.

3.4 Haircut size and post-default growth

3.4.1 Graphical analysis and stylised facts

We now turn to the question to what degree haircuts, the central outcome of an often protracted restructuring process, help to predict a country's post-crisis growth.

As in section 3.3, we start with a graphical analysis of the data. Figure 3.4 depicts the development of real GDP per capita around the end of default, from three years before until five years after the conclusion of a final deal. The end of the debt crisis is labelled as year zero (black vertical line) and, as in Figure 3.3, GDP is normalised to 100 in year -1.

Panel A of Figure 3.4 shows the average development of real GDP per capita for all end-of-default episodes in our sample.[10] One can see that output increases notably around the end of a default and continues to grow at a rapid pace for the subsequent five years.

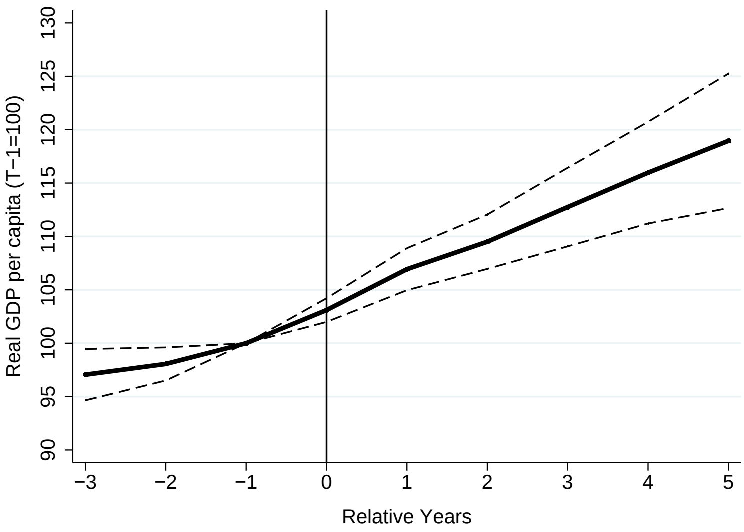
As in Figure 3.3, we divide our sample into cases of hard and soft default (Panel B), now using the median haircut size (which is 35.8%) as the separation criterion. The 15 debt crises that ended with a haircut larger than 35.8% are coded as hard defaults, while the 15 crises involving lower haircuts are coded as soft defaults.

Surprisingly, the graphical analysis shows almost no difference in the post-default growth performance of soft and hard defaulters. Both average GDP growth and the confidence bands look nearly identical, suggesting that also the dispersion in post-default growth rates is similarly distributed in both sub-groups. Hence, in contrast

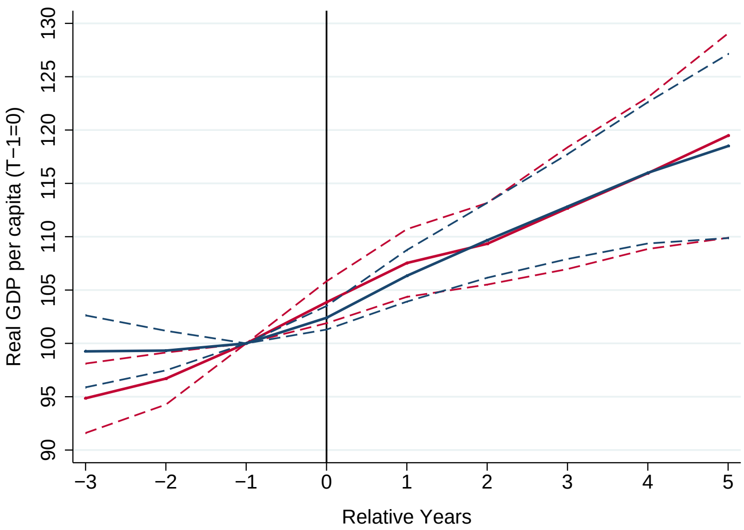
[10] In total there are 38 debt crises in our sample (cp. Table C.1 in the Appendix). In line with Cruces and Trebesch (2013), we identify post-crisis episodes as those episodes that were not followed by another restructuring vis-à-vis private creditors within the subsequent four years. This is not the case for Morocco 1983, Romania 1983, South Africa 1987 and 1989, Uruguay 1985 and 1987 as well as for Venezuela 1988, such that we do not include them in this part of the analysis. Furthermore, we leave out the case of the Venezuela 2004/2005 since this default has been very peculiar and it is very controversial to what extent this even constituted a case of sovereign default. The default ended in 2005 without any restructuring negotiations and/or haircut with Venezuela making all due payments on the oil-indexed bond that caused the default.

Figure 3.4: **Evolution of GDP around end of default**

(a) Panel A: All defaults



(b) Panel B: Hard and soft defaults



Notes — The solid lines plot the average development of real GDP per capita from three years before until five years after the conclusion of a final deal, and the dashed lines are 90% confidence intervals. Real GDP per capita has been normalised to 100 in the year before the conclusion of a final deal. Whereas Panel A pictures the evolution of GDP over all 30 default episodes in the sample, Panel B splits the sample into cases of "soft" (blue) and "hard" (red) defaults (at the median value of the observed final deal haircut size) and plots the evolution of GDP for both groups separately.

to our graphical result for growth *during* crises, we do not find much evidence for the idea that the severity of a crisis matters for *post-default* growth. In the next section, we test the relationship between growth and haircuts in a more systematic way.

3.4.2 Regression analysis

We begin with a naïve OLS estimation, regressing per capita GDP growth on haircuts (*Haircuts5*). In order to avoid that our estimation results get biased by the low growth rates experienced during the default episodes, we restrict our sample to include only non-default observations. Since it is our purpose to measure the explanatory power of haircuts on post-default growth, our variable of interest, *Haircuts5*, takes on the size of the final deal haircut in the year of the end of default and the subsequent five years. In this first regression, we include year dummies to account for the presence of a global time trend in GDP growth as the only controls. Column 1 of Table 3.4 shows that in this estimation framework haircuts show up highly significant and positive, indicating that countries with high haircuts tend to grow faster in the post-default period.

A crucial problem with this naïve estimation is the fact that there is a *general* tendency of GDP growth to strongly increase after the settlement of a debt crisis.[11] To account for this effect, in Model 2 we add a dummy control variable (*FinalDeal5*) that takes on the value one in the final year of the debt crisis and the subsequent five years. With the inclusion of this control, our haircut measure virtually ceases to be significant (p-value of 0.099, see column 2 of Table 3.4).

In Model 3, we add the (previously used) vector of macroeconomic controls, $X_{i,t}$. This causes the coefficient of *Haircuts5* to switch signs, but it remains insignificant.

[11] Average growth of real GDP per capita during the five past-default years is close to 3%, while during "normal" non-default periods it is only around 2.5% in our sample. This may be due to a "catch-up" effect following the low GDP growth during default.

Table 3.4: **Haircuts and post-default GDP growth**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Haircuts5	2.5483***	2.9136*	-2.1184	-1.8378	-2.0218	-1.0506	
	(0.8891)	(1.7694)	(1.3779)	(1.7856)	(1.5652)	(1.3605)	
FinalDeal5		-0.1885	1.5070**	1.5247*	1.2193*	0.8655	
(Dummy)		(0.7265)	(0.6329)	(0.8731)	(0.7143)	(0.6425)	
HaircutsDef							-3.4780*
							(1.9788)
InvGDP			18.6608***	20.1809***	-2.0127	-1.6139	18.0441***
			(2.0701)	(3.4450)	(4.6312)	(5.2230)	(2.0775)
ΔPop			-0.4207**	-0.0485	-0.3670	-0.4885	-0.3280**
			(0.1701)	(0.4174)	(0.3066)	(0.3127)	(0.1439)
Log(Pop)			-0.0648	1.2606	0.9994	0.4483	-0.1115
			(0.0858)	(3.2114)	(2.6402)	(2.6550)	(0.0905)
SecEdu			0.0290***	0.0324	-0.0005	-0.0133	0.0089
			(0.0086)	(0.0502)	(0.0265)	(0.0287)	(0.0097)
$GovtCons_{t-1}$			0.0975***	0.0871***	0.0882***	0.0706***	0.1104***
			(0.0215)	(0.0212)	(0.0217)	(0.0223)	(0.0197)
CivLib			0.0501	0.1219	0.1630	0.2008	-0.1076
			(0.1018)	(0.2645)	(0.2145)	(0.2272)	(0.0913)
ΔToT			9.0917***	9.4594***	7.6331***	7.4170***	10.4752***
			(1.4475)	(1.7449)	(1.3428)	(1.2450)	(1.2069)

(continued on next page)

Haircuts and post-default GDP growth (cont.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Openness			-0.0029	-0.0090	0.0244**	0.0246**	-0.0030
			(0.0025)	(0.0161)	(0.0097)	(0.0095)	(0.0024)
BankingCrisis			-2.7208***	-2.6138***	-2.9338***	-2.0403**	-2.4708***
(Dummy)			(0.8003)	(0.6857)	(0.8627)	(0.8123)	(0.8197)
CurrencyCrisis						-5.3903***	
(Dummy)						(1.2605)	
DebtGDP						-0.0067	
						(0.0090)	
Inflation						-0.0014	
						(0.0014)	
Default							0.3252
(Dummy)							(0.8292)
N	1,354	1,354	894	894	873	797	1,113
Countries	61	61	45	45	45	45	45
Time fixed effects	YES	YES	YES	YES	YES	YES	YES
Country fixed effects	NO	NO	NO	YES	YES	YES	NO
R^2	0.1277	0.1277	0.3879	0.3408	0.2949	0.3734	0.3572

Notes — The dependent variable is the annual growth rate of real GDP per capita, measured in per cent. The key explanatory variable is haircut size as negotiated in the final deal, *Haircuts5*, which is carried forward for the subsequent five years. All specifications include a (non-reported) constant. Robust standard errors (for columns 4-6 clustered by country) are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

So far we have tested the relationship between haircuts and growth in a (pooled) OLS framework. To avoid that our estimation results are biased due to the existence of unobserved (time-invariant) country-specific variables, we now re-estimate Model 3 with country fixed effects. Again, this leaves our estimation results by and large unaltered (see column 4 of Table 3.4). Most importantly, the haircut variable now clearly turns out insignificant.

As before, we also instrument the investment to GDP ratio (*InvGDP*) and the level of openness (*Openness*) with their first two lags in Model 5. Again, the insignificance of haircuts remains unchanged, which is also true when adding (*CurrencyCrisis*), (*DebtGDP*) and (*Inflation*) as further controls (column 6 of Table 3.4).

How can we interpret the insignificance of the haircut coefficient, especially given the fact that debtor coerciveness proved to be highly significant in explaining GDP growth *during* default? One way of interpretation would be that only coerciveness affects growth while haircuts do not. In order to test this hypothesis, we check whether, in contrast to haircuts, the average coerciveness of a country across the whole debt crisis episode (*AvgCoerc*) helps to explain post-default growth. Table C.2 in the Appendix shows that this is also not the case.

A different interpretation is that, at the time of the final deal conclusion, the final haircut size does not come as a surprise to capital markets but has already been anticipated in the time of restructuring negotiations. To the extent that the size of haircuts reduces the growth rates of countries, it should therefore have already done so during the restructuring period and no additional growth effect should be expected from the revelation of the final deal haircut. In this sense, the significance of our coerciveness index, which captures the negotiation and payment behaviour of governments *during* the debt crises, could be interpreted as a measure of market expectations on future creditor losses/haircuts.

The fact that a country's average coerciveness over the debt crisis and the final haircut are closely correlated (pairwise correlation of about 60%) lends support to this interpretation. Furthermore, we test this hypothesis in column 7 of Table 3.4 by using haircuts instead of (average) coerciveness to explain GDP growth *during* default in the setup of specification 5 of subsection 3.3.2.[12] Indeed, haircuts turn out to be significant at the 10% level. The coefficient of around -3.5 indicates that an increase in the haircut by 10 percentage points goes along with an annual reduction in GDP growth of 0.35% during defaults. We interpret this as clear support for the "expectation hypothesis" of haircuts. Taken together, our results therefore suggest that the "type" of default only affects GDP growth *during* a debt crisis, but not after it has been resolved.

3.5 Conclusion

In this chapter, we move beyond the classical dichotomous treatment of sovereign defaults and analyse how the severity of a debt crisis affects output growth during and after default. We find strong evidence that the "type" of a debt crisis is of crucial importance for a country's growth performance during default. In particular, we find that coercive government behaviour towards external creditors is associated with a much steeper drop in output when compared to cases with consensual crisis resolution.

On average, "hard" defaults go along with up to six percentage points lower growth *during* a crisis than "soft" defaults. This result is robust in a cross-section of defaulters and non-defaulters, in a sub-sample of crisis years, in a panel framework with country and year fixed effects, and when accounting for autocorrelated standard errors, endogenous control variables and crisis duration. Moreover, we tackled

[12]To do so, we define *HaircutsDef* to take on the value of the final deal haircut during each year of default.

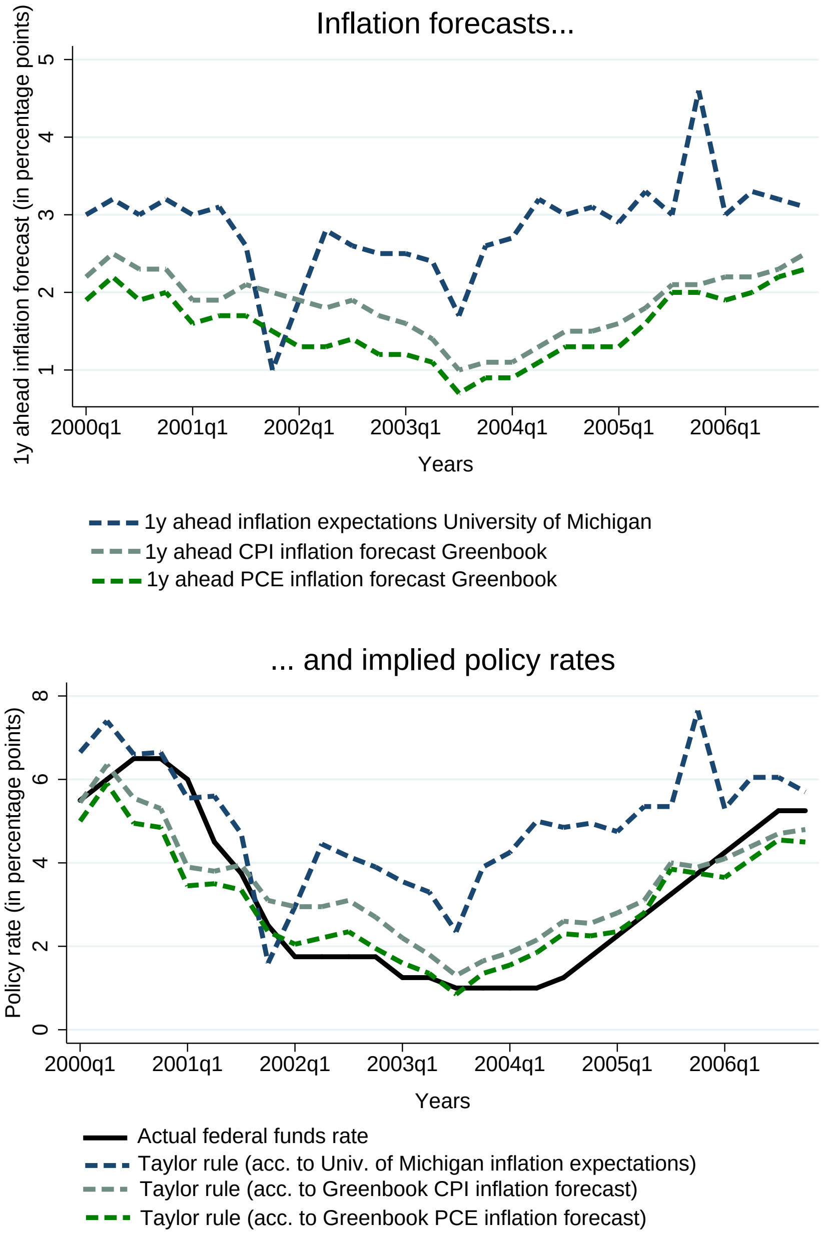
the issue of reverse causality and found evidence that our results can indeed be interpreted causally. Surprisingly however, our findings suggest that the growth effects of the type of default are limited to the years during a debt crisis and do not to extend to the post-default period.

We conclude that any analysis on the cost of a sovereign debt crisis needs to account for the magnitude of default and not only for its occurrence. This is in line with the recent survey piece by Aguiar and Amador (2013) who recommend to consider "richer notions of default". We therefore hope that our empirical insights may motivate and discipline future theoretical work on the issue.

Appendix A

Appendix to Chapter 1

Figure A.1: **Central bank misperception in the US based on University of Michigan 1y ahead inflation expectations**



Notes — The upper figure plots the 1-year-ahead inflation forecasts (CPI and PCE) by the Fed (in its Greenbook) and the one year inflation expectations as collected by the University of Michigan in its monthly Survey of Consumers from 2000 to 2006. The lower figure compares the policy rates prescribed by the (forward-looking) Taylor rule for the different inflation forecasts/expectations with the actual policy rates set by the Fed. All estimations of the Taylor rule are based on the realtime output gap estimates in the Greenbook.

Appendix B

Appendix to Chapter 2

Table B.1: **Sovereign bond yield data sources and availability**

Bloomberg (33 countries)	
1994	Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Spain, Sweden, United Kingdom, United States (January), Switzerland (February)
1997	Portugal (February), Greece (July)
1998	Hong Kong (March), Singapore (June), India (November)
1999	Taiwan (April)
2000	Thailand (January), Czech Republic (April), South Korea (December)
2002	Slovakia (June), Romania (August)
2006	Israel (February)
2007	Slovenia (March)
2008	Iceland (April)
JP Morgan EMBI Global (41 countries)	
1994	Argentina, Mexico, Nigeria, Venezuela (January), China (March), Brazil (April), Bulgaria (July), Poland (October), South Africa (December)
1995	Ecuador (February)
1996	Turkey (June), Panama (July), Croatia (August), Malaysia (October)
1997	Colombia (February), Peru (March), Philippines, Russia (December)
1998	Lebanon (April)
1999	Hungary (January), Chile (May)
2000	Ukraine (May)
2001	Pakistan (January), Uruguay (May), Egypt (July), Dominican Republic (November)
2002	El Salvador (April)
2004	Indonesia (May)
2005	Serbia (July), Vietnam (November)
2007	Belize (March), Kazakhstan (June), Ghana, Jamaica (October), Sri Lanka (November), Gabon (December)
2008	Georgia (June)
2011	Jordan (January), Senegal (May), Lithuania, Namibia (November)

Notes — This table lists the sources of the sovereign bond yield data in the sample and the years in which the respective time series are first observed (months in parentheses). If there are gaps in the Bloomberg 10-year generic yield series, we add observations of 10-year generic yields from Datastream, ensuring that this does not induce structural breaks. Moreover, for some emerging countries we include 10-year generic yields until the EMBI Global series become available.

Table B.2: **Rating changes, by region**

Region	Upgrades	Downgrades
Caribbean	26	29
Central & Southwestern Asia	24	9
Central America	12	18
Central Europe	53	19
Eastern Asia	46	26
Eastern Europe	41	38
Middle East	61	24
North America	17	9
Northern Africa	5	14
Northern Asia	23	12
Northern Europe	23	14
Oceania	17	12
South America	108	77
Southeastern Asia	50	34
Southeastern Europe	55	32
Southern Asia	14	13
Southern Europe	28	54
Sub-Saharan Africa	23	10
Western Europe	9	18
	635	462

Notes — This table shows the regional distribution of the sample of 1,097 upgrade and downgrade announcements made by S&P, Moody's, and Fitch between 1994 and 2011. Regions are defined based on the CIA World Factbook.

Table B.3: **Rating scales and transformation**

Characterisation of debt and issuer		Letter rating			Linear transformation
		S&P	Moody's	Fitch	
Highest quality	Investment grade	AAA	Aaa	AAA	17
High quality		AA+ AAA AA–	Aa1 Aa2 Aa3	AA+ AA AA–	16 15 14
Strong payment capacity		A+ A A–	A1 A2 A3	A+ A A–	13 12 11
Adequate payment capacity		BBB+ BBB BBB–	Baa1 Baa2 Baa3	BBB+ BBB BBB–	10 9 8
Likely to fulfil obligations, ongoing uncertainty	Speculative grade	BB+ BB BB–	Ba1 Ba2 Ba3	BB+ BB BB–	7 6 5
High credit risk		B+ B B–	B1 B2 B3	B+ B B–	4 3 2
Very high credit risk		CCC+ CCC CCC–	Caa1 Caa2 Caa3	CCC+ CCC CCC–	1
Near default with possibility of recovery		CC	Ca	CC C	
Default		SD D	C	DDD DD D	

Notes — This table shows how the letter ratings used by S&P, Moody's, and Fitch correspond to one another and to different degrees of credit risk, and how they are mapped into the linear 17-notch scale used in the investigation. The transformation is the same as in Afonso et al. (2012), from which this table is adapted.

Table B.4: **Variable definitions**

Variable	Definition	Sources
Δ*Spread*	Change in the non-event country spread vis-à-vis US Treasuries of comparable maturity over the two-trading-day window $[-1, +1]$ around the rating announcement (day 0), measured in percentage points.	Bloomberg, Datastream, JP Morgan, US Department of the Treasury
LARGE	Dummy variable taking on a value of one for "large" rating changes of two notches or more; zero otherwise. Notches are measured according to the linear transformation in Table B.3.	S&P, Moody's, Fitch
InitRat	Credit rating held by the event country with the announcing CRA prior to the event, measured on the 17-notch scale from Table B.3.	S&P, Moody's, Fitch
Δ*InitRat*	Absolute difference between *InitRat* and the average of all credit ratings held by the non-event country with the three CRAs, measured on the 17-notch scale from Table B.3.	S&P, Moody's, Fitch
OnWatch	Dummy variable taking on a value of one if the event country was on watch, or review, by the announcing CRA at the time of the event; zero otherwise.	S&P, Moody's, Fitch
SimActsWdwEvt	Number of upgrade (downgrade) announcements made on the event country by respective other CRAs over the two-week interval $[-14, -1]$ (calendar days) before the upgrade (downgrade) event.	S&P, Moody's, Fitch
SimActsDayNonEvt	Number of upgrade (downgrade) announcements made on the non-event country by any CRA on the same day as the upgrade (downgrade) of the event country.	S&P, Moody's, Fitch

VIX	Volatility measure for the S&P 500 stock market index in the United States.	Bloomberg
Region	Dummy variable taking on a value of one if the event and non-event country belong to the same geographical region (also see Table B.2); zero otherwise.	CIA World Factbook
TradeBloc	Dummy variable taking on a value of one if the event and non-event country are members of a common major trade bloc; zero otherwise. The trade blocs are: EU, NAFTA, ASEAN, Mercosur, CARICOM, Andean Community, Gulf Cooperation Council, Southern African Customs Union, Economic Community of Central African States, Economic Community of West African States, Organisation of Eastern Caribbean States.	Authors' definition
ExpImpEvt	Importance of the event to the non-event country in terms of exports, measured as the non-event country's ratio of exports to the event country to domestic GDP.	World Bank
CapOpen(Non)Evt	*De jure* measure of the event (non-event) country's degree of capital account openness. Based on dummy variables, it codifies the restrictions on cross-border financial transactions reported in the IMF's Annual Report on Exchange Rate Arrangements and Exchange Restrictions.	Chinn and Ito (2006)
SizeEvt	Size of the event country, measured in logs of US dollar GDP.	World Bank
Δ*Size*	Size differential of the event over the non-event country, measured in logs of US dollar GDP.	World Bank
Δ*TrendGrowth*	Absolute difference between the event and non-event country's GDP trend growth, calculated for the sample period 1994–2011 on the basis of annual data using a Hodrick-Prescott filter with smoothing parameter 6.25.	World Bank

Table B.5: **Baseline regressions — Pooling all rating changes**

LARGE	0.0102
	(0.0064)
LARGE × *Down*	0.0178**
	(0.0087)
InitRat	-0.0012
	(0.0008)
InitRat × *Down*	-0.0005
	(0.0009)
Δ*InitRat*	-0.0005
	(0.0006)
Δ*InitRat* × *Down*	0.0012
	(0.0008)
OnWatch	-0.0023
	(0.0056)
OnWatch × *Down*	-0.0153*
	(0.0078)
SimActsWdwEvt	-0.0036
	(0.0053)
SimActsWdwEvt × *Down*	0.0206**
	(0.0082)
SimActsDayNonEvt	0.0935*
	(0.0541)
SimActsDayNonEvt × *Down*	0.0598
	(0.0849)
VIX	-0.0001
	(0.0004)
VIX × *Down*	0.0008**
	(0.0004)
Down	-0.0217
	(0.0141)
N	51,881
Event countries	104
Non-event countries	73
Rating actions	1,022
R^2	0.0183

Notes — This table shows regressions based on the full baseline specification (see column (3) in Table 2.1) after pooling 635 upgrades and 462 downgrades made by S&P, Moody's, and Fitch between 1994 and 2011. For reasons of comparability, the dependent variable equals Δ*Spread* for downgrades, and -Δ*Spread* for upgrades. *Down* is a dummy variable taking on a value of one for downgrades, and zero otherwise. The interaction term *LARGE* × *Down* indicates that there is a statistically significant difference between the absolute coefficients for upgrades and downgrades.

Table B.6: **Baseline regressions, downgrades — Robustness checks I**

	Baseline	Ex notches ≥ 4	Ex notches ≥ 3	Crises	S&P effect?	Endogenous downgrades?
LARGE	0.0207***	0.0206***	0.0263***	0.0184***	0.0273***	0.0179**
	(0.0066)	(0.0068)	(0.0077)	(0.0063)	(0.0065)	(0.0078)
InitRat	-0.0008	-0.0020	-0.0019	-0.0006	-0.0010	-0.0061***
	(0.0017)	(0.0018)	(0.0019)	(0.0017)	(0.0017)	(0.0023)
Δ*InitRat*	0.0008	0.0007	-0.0001	0.0008	0.0008	-0.0014
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0011)
OnWatch	-0.0046	-0.0026	0.0023	-0.0048	-0.0052	0.0291***
	(0.0054)	(0.0056)	(0.0059)	(0.0055)	(0.0054)	(0.0071)
SimActsWdwEvt	0.0141**	0.0173***	0.0192***	0.0138**	0.0140**	-0.0080
	(0.0065)	(0.0066)	(0.0074)	(0.0065)	(0.0065)	(0.0055)
SimActsDayNonEvt	0.1477**	0.1540**	0.1538**	0.1472**	0.1480**	0.2223***
	(0.0648)	(0.0658)	(0.0674)	(0.0649)	(0.0649)	(0.0712)
VIX	0.0006*	0.0008**	0.0008**	0.0006*	0.0006*	0.0013***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Euro $\times$ *LARGE*				0.0107		
				(0.0118)		
Asian $\times$ *LARGE*				0.0261		
				(0.0395)		

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Baseline regressions, downgrades — Robustness checks I (continued)

S&P × *LARGE*					-0.0234*	
					(0.0128)	
Δ*Spread* $[-15,-1]$						0.0131***
						(0.0026)
N	21,931	21,519	20,510	21,931	21,931	13,953
Event countries	84	84	84	84	84	47
Non-event countries	73	73	73	73	73	73
Downgrades	427	418	399	427	427	268
R^2	0.0423	0.0434	0.0437	0.0423	0.0425	0.0551

Notes — This table shows the robustness of our baseline results on the main variable of interest, *LARGE*. For purposes of comparison, the first column reports the results from the full baseline specification for downgrades (see Panel B, column (3) in Table 2.1). Since we group all rating downgrades of two notches or more into a single bin, we ensure that our findings are not driven by downgrades of four and three notches or more, respectively, by dropping those rating events from the sample (Ex notches ≥ 4, Ex notches ≥ 3). Moreover, to check that the results are not solely due to the main crisis episodes over the sample period, namely the eurozone and Asian crises, we add two dummy variables, *Euro* and *Asian*, and interact them with the large-change dummy (Crises). *Euro* takes on a value of one if the downgrade was announced in 2010 or 2011 and if both the event and non-event country were members of the eurozone at that time, and zero otherwise. Similarly, *Asian* takes on a value of one for all downgrades between July 1997 and December 1998 in which both the event and the non-event country are from either of the following countries: Indonesia, Malaysia, Philippines, Singapore, South Korea, Thailand. We also interact the large-change dummy with *S&P*, which takes on a value of one if the downgrade was announced by Standard & Poor's, to test whether this agency's relatively infrequent large downgrades (see Figure B.1) account for our results (S&P effect?). Finally, we add Δ*Spread* $[-15,-1]$, the change in the event country's spread over the 14-day period before the announcement, to control for downgrades that may have come about as timely reactions to adverse spread developments (Endogenous downgrades?).

Table B.7: **Baseline regressions, downgrades — Robustness checks II**

		Window length		Same day actions	
	Baseline	Seven days	21 days		
LARGE	0.0207***	0.0207***	0.0200***	0.0166**	0.0208***
	(0.0066)	(0.0066)	(0.0067)	(0.0065)	(0.0066)
InitRat	-0.0008	-0.0011	-0.0009	-0.0007	-0.0008
	(0.0017)	(0.0017)	(0.0017)	(0.0014)	(0.0017)
Δ*InitRat*	0.0008	0.0008	0.0008	0.0005	0.0008
	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
OnWatch	-0.0046	-0.0029	-0.0044	-0.0040	-0.0047
	(0.0054)	(0.0055)	(0.0055)	(0.0054)	(0.0054)
SimActsWdwEvt	0.0141**	0.0244**	0.0175***		0.0143**
	(0.0065)	(0.0109)	(0.0063)		(0.0067)
SimActsDayNonEvt	0.1477**	0.1489**	0.1481**	0.1654***	0.1477**
	(0.0648)	(0.0646)	(0.0649)	(0.0634)	(0.0648)
VIX	0.0006*	0.0007*	0.0006*	0.0007**	0.0006*
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0004)

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Baseline regressions, downgrades — Robustness checks II (continued)

SimActsDayEvt				0.0173	-0.0024
				(0.0146)	(0.0151)
N	21,931	21,931	21,895	23,252	21,931
Event countries	84	84	84	95	84
Non-event countries	73	73	73	73	73
Downgrades	427	427	426	453	427
R^2	0.0423	0.0425	0.0426	0.0430	0.0423

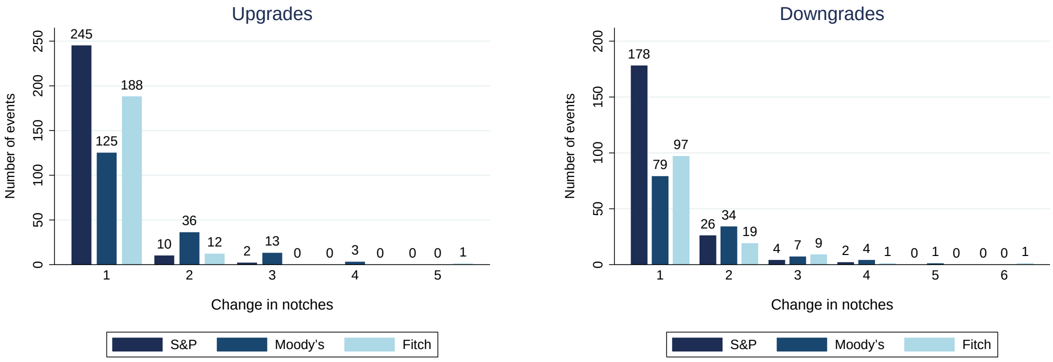
Notes — This table shows the robustness of our baseline results with regard to variables on clustering and anticipation. For purposes of comparison, the first column reports the results from the full baseline specification for downgrades (see Panel B, column (3) in Table 2.1). The second and third columns report regression results when the within-clustering control *SimActsWdwEvt* takes on the number of downgrades announced by other agencies before the respective downgrade over a seven and 21-day period, respectively, as opposed to a 14-day period in the baseline. The fourth and fifth columns add as replacement and additional control, respectively, *SimActsDayEvt*. The latter indicates the number of downgrades announced by other agencies on the day of the respective downgrade.

Table B.8: **Spillover channels, downgrades — Different trade measures**

	Trade measure			
	ExpImpEvt	*TradeImpEvt*	*ExpShEvt*	*TradeShEvt*
LARGE	0.0244***	0.0246***	0.0244***	0.0246***
	(0.0073)	(0.0073)	(0.0073)	(0.0073)
InitRat	-0.0031	-0.0030	-0.0031	-0.0030
	(0.0021)	(0.0021)	(0.0021)	(0.0021)
Δ*InitRat*	0.0013	0.0013	0.0013	0.0013
	(0.0011)	(0.0011)	(0.0011)	(0.0011)
OnWatch	-0.0003	-0.0005	-0.0003	-0.0004
	(0.0059)	(0.0060)	(0.0059)	(0.0060)
SimActsWdwEvt	0.0141**	0.0145**	0.0141**	0.0145**
	(0.0069)	(0.0069)	(0.0069)	(0.0069)
SimActsDayNonEvt	0.1136*	0.1129*	0.1137*	0.1129*
	(0.0619)	(0.0619)	(0.0619)	(0.0619)
VIX	0.0005	0.0005	0.0005	0.0005
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Region	0.0348**	0.0324*	0.0345**	0.0326*
	(0.0168)	(0.0167)	(0.0168)	(0.0167)
TradeBloc	0.0120	0.0139	0.0118	0.0139
	(0.0121)	(0.0122)	(0.0120)	(0.0121)
Trade measure	**0.0580**	**0.0517**	**0.0298**	**0.0247**
	(0.2268)	**(0.1143)**	**(0.0659)**	**(0.0538)**
CapOpenEvt	0.0126**	0.0131**	0.0127**	0.0131**
	(0.0063)	(0.0063)	(0.0063)	(0.0063)
CapOpenNonEvt	0.0081	0.0088	0.0081	0.0088
	(0.0088)	(0.0088)	(0.0088)	(0.0089)
SizeEvt	0.0247	0.0259	0.0244	0.0258
	(0.0330)	(0.0333)	(0.0330)	(0.0332)
Δ*Size*	-0.0146	-0.0187	-0.0144	-0.0186
	(0.0253)	(0.0255)	(0.0253)	(0.0255)
Δ*TrendGrowth*	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
N	19,724	19,511	19,715	19,502
Event countries	79	79	79	79
Non-event countries	70	70	70	70
Downgrades	405	405	405	405
R^2	0.0434	0.0435	0.0434	0.0435

Notes — This table shows the robustness of our results on the spillover channels of downgrade announcements to different measures of bilateral trade linkages. For purposes of comparison, we first report the results from the most comprehensive specification using *ExpImpEvt*, the non-event country's exports to the event country relative to non-event country GDP (see column (7) in Table 2.3). Alternatively, we use *TradeImpEvt*, which is bilateral trade (imports + exports) with the event country relative to non-event country GDP. Finally, *ExpShEvt* and *TradeShEvt* measure the event country's share in the non-event country's total exports and total bilateral trade, respectively.

Figure B.1: **Distribution of rating changes, by agency**



Notes — This figure shows the distribution of the severity of rating changes by agency, measured on a 17-notch scale (see Table B.3). Numbers are based on the sample of 1,097 rating announcements (635 upgrades, 462 downgrades) made by S&P, Moody's, and Fitch between 1994 and 2011.

Appendix C

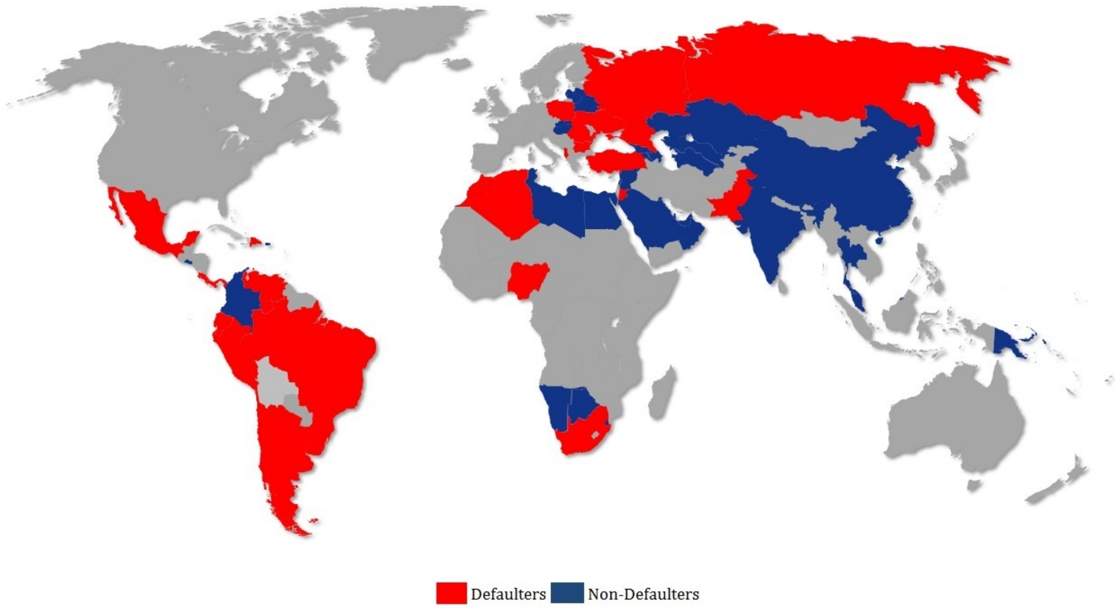
Appendix to Chapter 3

Table C.1: **Country sample composition**

Defaulting countries (25 countries)	
Albania: 1980-2009 (1991-1995)	Pakistan: 1980-2009 (1998-1999)
Algeria: 1980-2009 (1991-1996)	Panama: 1980-2009 (1983-1996)
Argentina: 1980-2009 (1982-1993; 2001-2005)	Peru: 1981-2009 (1983-1997)
Brazil: 1980-2009 (1983-1994)	Philippines: 1980-2009 (1983-1992)
Bulgaria: 1980-2009 (1990-1994)	Poland: 1991-2009 (1991-1994)
Chile: 1980-2009 (1983-1990)	Romania: 1980-2009 (1981-1983; 1986)
Costa Rica: 1980-2009 (1981-1990)	Russia: 1989-2009 (1991-2000)
Dominican Republic: 1980-2009 (1982-1994; 2004-2005)	South Africa: 1980-2009 (1985-1987; 1989;1993)
Ecuador: 1980-2009 (1982-1995; 1999-2000; 2008-2009)	Turkey: 1980-2009 (1982)
Jordan: 1980-2009 (1989-1993)	Ukraine: 1987-2009 (1998-2000)
Mexico: 1980-2009 (1982-1990)	Uruguay: 1980-2009 (1983-1985; 1987; 1990-1991; 2003)
Morocco: 1980-2009 (1983; 1986-1990)	Venezuela: 1980-2009 (1983-1988; 1990; 2004-2005)
Nigeria: 1980-2000 (1982-1992)	
Non-defaulting countries (36 countries)	
Armenia: 1995-2009	Lithuania: 1990-2009
Azerbaijan: 1995-2009	Malaysia: 1980-2009
Bahrain: 1980-2009	Mauritius: 1980-2009
Belarus: 1990-2009	Namibia: 1980-2009
Botswana: 1980-2009	Oman: 1980-2009
China: 1980-2009	Papua New Guinea: 1980-2009
Colombia: 1980-2009	Puerto Rico: 1980-2009
Egypt: 1980-2009	Qatar: 1995-2009
El Salvador: 1980-2009	Saudi Arabia: 1980-2009
Georgia: 1980-2009	Singapore: 1980-2009
Hong Kong: 1980-2009	Slovak Republic: 1984-2009
Hungary: 1980-2009	Swaziland: 1980-2009
India: 1980-2009	Syria: 1980-2009
Kazakhstan: 1990-2009	Thailand: 1980-2009
Kuwait: 1995-2009	Tunisia: 1980-2009
Latvia: 1980-2009	Turkmenistan: 1987-2009
Lebanon: 1992-2009	United Arab Emirates: 1980-2009
Libya: 1999-2009	Uzbekistan: 1987-2009

Notes — This table lists all countries included in the sample and reports the time period for which each country is included (years of default in brackets).

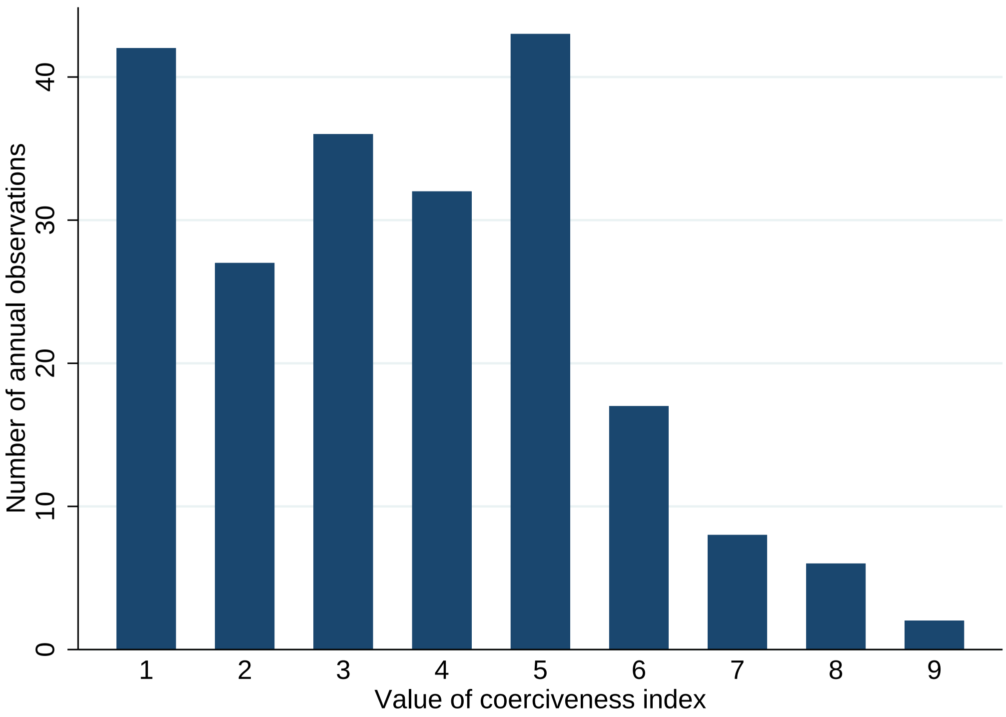
Figure C.1: **Map of sample composition**



Notes — This figure illustrates the sample composition. Countries that defaulted during the sample period are coloured red, countries that did not default are coloured blue.

Figure C.2: **Coerciveness**

(a) Distribution of coerciveness index



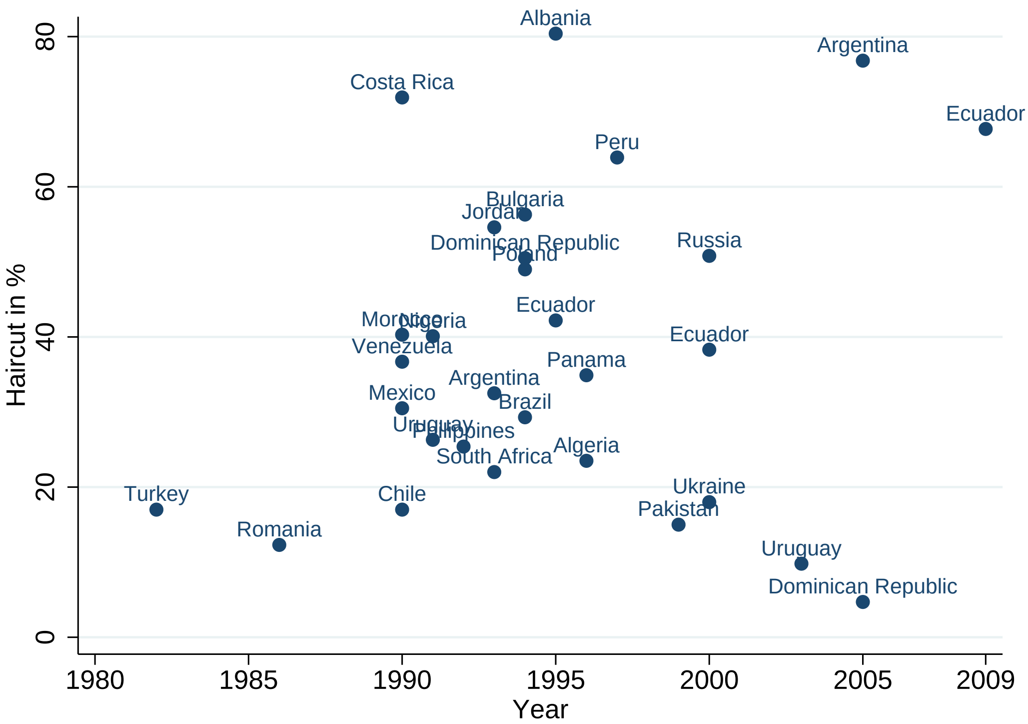
(b) Descriptive statistics

Variable	Frequency	Mean	Std. Dev.
Coerciveness index	217	3.61	1.98
Payments missed	155	0.71	0.45
Unilateral suspension	126	0.58	0.49
Full suspension	54	0.25	0.43
Freeze on assets	27	0.12	0.33
Negotiations breakdown	98	0.45	0.50
Moratorium declaration	28	0.13	0.34
Threats to repudiate	37	0.17	0.38
Data disputes	20	0.09	0.29
Forced restructuring	13	0.06	0.24

Notes — Figure (a) plots the distribution of the annual coerciveness index (*Coerc*), while Figure (b) provides basic summary statistics on the coerciveness index and its sub-indicators. See section 3.2.3 for a detailed description of the index and its sub-indicators.

Figure C.3: **Haircuts**

(a) Size of haircuts across countries and time



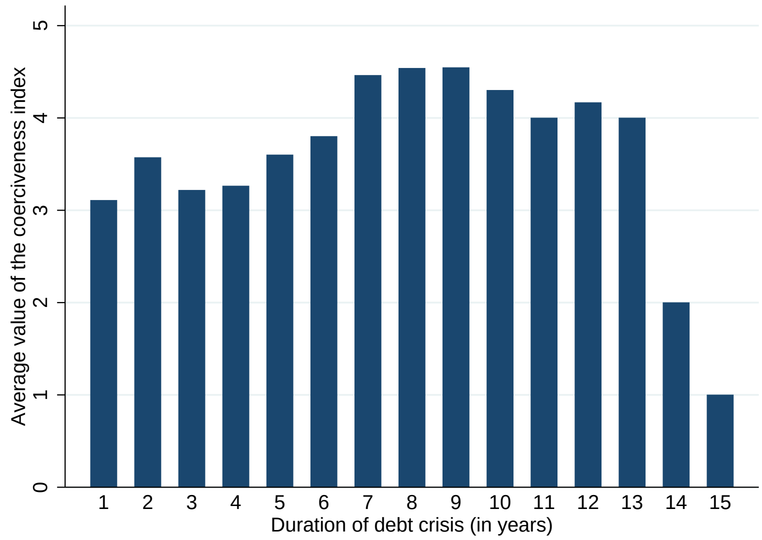
(b) Descriptive statistics

	# of obs.	Min.	Max.	Mean	Median	Std. Dev.
Haircuts	30	5%	81%	38%	36%	21%

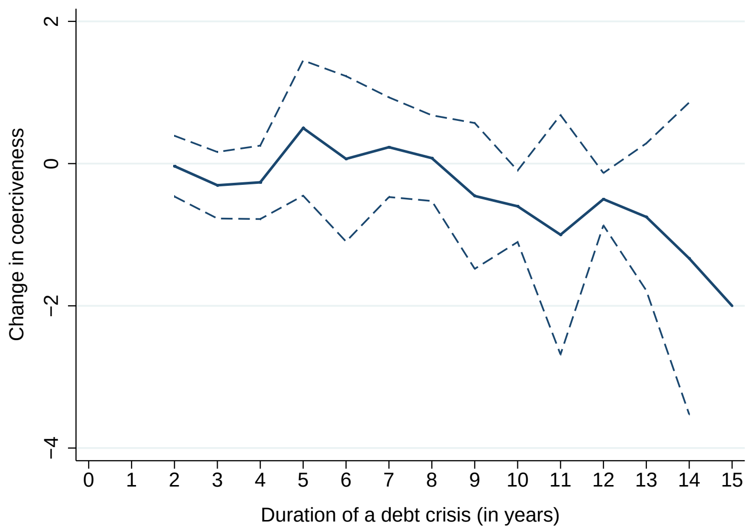
Notes — Figure (a) plots the size of haircuts in percentage points (H_{SZ}) across countries and time, while Figure (b) provides basic summary statistics. While haircuts range from 5% (Dominican Republic, 2005) to more than 80% (Albania, 1995), on average final deals schedule haircuts of around 40%. See section 3.2.4 for a detailed description of haircuts.

Figure C.4: **Government coerciveness and crisis duration**

(a) Average coerciveness and duration of sovereign default



(b) Average change in coerciveness and duration of sovereign default



Notes — Figure (a) shows the average value of the coerciveness value depending on the length of a debt crisis. During the first year of a debt crisis the coerciveness index on average takes on a value of 3.1, in the second year a value of around 3.6, and so on. Figure (b) plots the average change in coerciveness during each year of a debt crisis (beginning from the second year of the debt crisis), with the dashed lines indicating 90% confidence intervals. Note that the number of observations decreases with increasing crisis length. For example the 15th year of a debt crisis is observed just once (Peru), and the 14th year only thrice (Peru, Panama and Ecuador).

Table C.2: **Average coerciveness and post-default GDP growth**

	(1)	(2)	(3)	(4)	(5)	(6)
AvgCoerc5	0.1483	-0.0967	0.0027	0.0269	-0.0195	0.0835
	(0.0905)	(0.1256)	(0.1302)	(0.1659)	(0.1447)	(0.1376)
FinalDeal5		1.1923**	0.6592	0.7297	0.4838	0.2411
(Dummy)		(0.5124)	(0.5611)	(0.6809)	(0.6087)	(0.6036)
InvGDP			18.5388***	20.1409***	-2.0953	-1.6752
			(2.0469)	(3.4590)	(4.6403)	(5.2127)
ΔPop			-0.4165**	-0.0319	-0.3518	-0.4694
			(0.1709)	(0.4151)	(0.3067)	(0.3126)
Log(Pop)			-0.0532	1.1184	0.8151	0.3286
			(0.0859)	(3.2089)	(2.6393)	(2.6535)
SecEdu			0.0283***	0.0314	-0.0015	-0.0145
			(0.0086)	(0.0505)	(0.0265)	(0.0288)
$GovtCons_{t-1}$			0.0945***	0.0845***	0.0855***	0.0686***
			(0.0217)	(0.0207)	(0.0214)	(0.0218)
CivLib			0.0438	0.1269	0.1731	0.1900
			(0.1021)	(0.2714)	(0.2174)	(0.2301)
ΔToT			8.9927***	9.4376***	7.6166***	7.3998***
			(1.4488)	(1.7416)	(1.3514)	(1.2482)
Openness			-0.0028	-0.0096	0.0239**	0.0239**
			(0.0025)	(0.0162)	(0.0097)	(0.0094)

(continued on next page)

Average coerciveness and post-default GDP growth (cont.)

BankingCrisis			-2.7339***	-2.6278***	-2.9424***	-2.0544**
(Dummy)			(0.8020)	(0.6895)	(0.8682)	(0.8132)
CurrencyCrisis						-5.4415***
(Dummy)						(1.2626)
DebtGDP						-0.0069
						(0.0091)
Inflation						-0.0014
						(0.0014)
N	1,354	1,354	894	894	873	797
Countries	61	61	45	45	45	45
Time fixed effects	YES	YES	YES	YES	YES	YES
Country fixed effects	NO	NO	NO	YES	YES	YES
R^2	0.1245	0.1264	0.3864	0.3399	0.2934	0.3733

Notes — This table replicates the estimations from columns 1-6 of Table 3.4 with *AvgCoerc5* instead of *Haircuts5* as a regressor. The dependent variable is the annual growth rate of real GDP per capita, measured in per cent. The key explanatory variables is average coerciveness over the whole default episode, *AvgCoerc5*, which is carried forward for the subsequent five years. All specifications include a (non-reported) constant. Robust standard errors (for column 4-6 clustered by country) are given in parentheses. ***, **, and * denote significance at the 1, 5, and 10 per cent levels, respectively.

Table C.3: **Variable definitions**

Variable	Definition	Sources
Growth	Annual growth rate of real GDP per capita (in per cent)	World Development Indicators
Default	Dummy variable taking on a value of one for each year a country has been listed as being in default by Standard & Poor's or during debt renegotiation periods without missed payments	Standard & Poor's; Enderlein et al. (2011)
InvGDP	Investment to GDP ratio	World Development Indicators
ΔPop	Annual growth rate of population (in per cent)	World Development Indicators
Log(Pop)	Natural logarithm of total population (in million)	World Development Indicators
SecEdu	Share of population that attained secondary schooling (in per cent)	Barro and Lee (2010)
$GovtCons_{t-1}$	Lagged annual growth rate of government consumption expenditure (in per cent)	World Development Indicators
CivLib	Annual index value of the civil liberties index published by Freedom House in its annual Freedom in the World report. The index ranges from 1 (highest degree of freedom) to 7 (lowest degree of freedom)	Freedom House Freedom in the World
$\Delta\, ToT$	Annual rate of change in current price value of exports of goods and services deflated by the import price index (exports as capacity to import)	World Development Indicators

Openness	Ratio of average exports plus imports ($=\frac{Exports+Imports}{2}$) to GDP (in per cent)	World Development Indicators
BankingCrisis	Dummy variable taking on a value of one at the beginning of a banking crisis	Laeven and Valencia (2012)
CurrencyCrisis	Dummy variable taking on a value of one at the beginning of a currency crisis	Laeven and Valencia (2012)
DebtGDP	Ratio of government debt to GDP (in per cent)	World Development Indicators
Inflation	Annual percentage change in the consumer price index (all items)	International Financial Statistics
IICCR	*Institutional Investor's* Country Credit Rating issued at the start of each year. The credit rating scale ranges from zero (extremely high credit risk) to 100 (extremely low credit risk)	Institutional Investor Journal

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(Michael Zabel)