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## Conference Paper

# Credit Ratings and Cross-Border Bond Market Spillovers

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# Credit Ratings and Cross-Border Bond Market Spillovers\*

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February 12, 2013

PRELIMINARY VERSION. PLEASE DO NOT CITE.

## Abstract

This paper studies spillovers across sovereign debt markets in the wake of sovereign rating changes. To this end, we use an extensive dataset covering all announcements by the three major agencies (Standard & Poor's, Moody's, Fitch) and daily sovereign bond market movements of up to 74 developed and emerging countries between 1994 and 2011. On the basis of an explicit counterfactual and controlling for important dimensions of the announcement environment, we find asymmetric reactions to upgrades and downgrades. While there is strong evidence of negative spillover effects in response to sovereign downgrades, positive spillovers from upgrades are much more limited. Our results also suggest that negative spillover effects are more pronounced for countries within the same region. This does not appear to be due to (measurable) fundamental linkages and similarities, such as trade, which turn out to be strikingly insignificant.

**JEL classification:** G15, F36

**Keywords:** Sovereign debt market, credit rating agencies, cross-border spillover effects, international financial integration

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# 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 seem to be running deep, which is probably best exemplified by reports that the European Commission was at one stage considering a temporary restriction on the issuance of ratings under exceptional circumstances (Financial Times 2011). This begs two interesting, wider research questions. First, when a rating is made for a given country, do we generally observe significant spillover effects on other countries' bond markets? Second, if so, under which conditions are those effects strongest, and which countries are affected most?

In this paper, we address these questions based on an extensive dataset comprising both a complete history of sovereign rating actions by the “Big Three” (Standard & Poor's, Moody's and Fitch) and daily sovereign bond market movements for up to 74 countries between 1994 and 2011. Not only do we cover both crisis and non-crisis periods and a broad set of developed and emerging countries across all continents. Our dataset also contains sufficient variation to run an explicit counterfactual analysis which pits small revisions in an agency's assessment of a country's creditworthiness against all other changes. Moreover, it characterizes along several important dimensions the environment in which a rating change is made. For instance, we are crucially able to account for the fact that an announcement is often followed by a similar one from a different agency soon after, which may influence the reception of the later announcements.<sup>1</sup>

Our main findings can be summarized as follows. Whereas we find strong evidence for the existence of significant cross-border spillover effects of sovereign rating downgrades, reactions to upgrades appear to be, if anything, much more muted. This points to an important asymmetry in the sovereign debt market's treatment of ratings. Regarding the influence of country characteristics, we find that negative spillover effects tend to be more pronounced for countries within the same region, a finding that persists even after controlling for measurable fundamental links and similarities between countries, which are strikingly insignificant.

Our paper 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

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<sup>1</sup>To the best of our knowledge, ours is the first paper to consider such interactions between the major CRAs in identifying spillover effects.

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 analyzing 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 sovereign rating action on one country is found to significantly affect the sovereign bond price of other countries (e.g., Arezki et al. 2011, De Santis 2012, Ismailescu and Kazemi 2010). Some studies also point out that spillovers are not limited to sovereign debt markets but that sovereign 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. (2011) 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.

Most of these studies focus either on spillover effects during specific regional crisis episodes (see Afonso et al. 2011, Arezki et al. 2011, and De Santis 2012 for the eurozone crisis, Kaminsky and Schmukler 1999, for the 1997–1998 Asian crisis) or on an otherwise homogeneous sample of countries only, such as emerging economies (Ismailescu and Kazemi 2010, Kaminsky and Schmukler 2002). This leaves the question open to what extent their findings are specific to the episode analyzed or of more general relevance.

The study most closely related to our work is Gande and Parsley (2005). Based on a sample of 34 developed and emerging economies from 1991 to 2000, they find evidence for the existence of spillover effects. In line with Afonso et al. (2011), they report asymmetries in the effects of upgrades and downgrades, with the latter triggering large and significant spillovers but no discernable impact of upgrades. Analyzing potential transmission mechanisms of these spillovers, they find trade and financial linkages to be the most relevant and physical proximity and cultural or institutional linkages to be of little importance. Our study differs from theirs in several ways. Firstly, our study is based on a bigger and more up-to-date sample of up to 74 countries for the 1994–2011 period. Secondly, their dataset only includes information of ratings by Standard & Poor's (S&P), which risks to bias estimation results since (as we

will show later on) a rating action by one rating agency is often foreshadowed or closely followed by a similar action from another rating agency. Ignoring the informational content of rating actions by other agencies may therefore prove to be problematic. Thirdly, their identification of spillovers uses a comprehensive credit rating scale, which combines actual rating changes and credit watch, or review, changes into a single 17-notch scale. Hence, they make additional assumptions on the relative informational content of reviews and ratings. In this study, we exploit the variation in our sample to focus solely on the class of actual rating changes and their relative strengths as the only observable difference between them. At the same time, we do allow for differences in the informational content of sovereign upgrades and downgrades by controlling for watchlistings that may build anticipation by foreshadowing actual future rating changes. Finally, due to a lack of data, Gande and Parsley (2005) only use an indirect measure of trade integration. Relying on direct bilateral data, we find trade to be remarkably unrelated to the strength of spillovers.

The remainder of the paper is organized as follows. In the next section we describe our dataset. Section 3 discusses the estimation strategy for identifying cross-border spillovers and introduces the main control variables used in the regression analysis. Section 4 presents our empirical results and discusses their interpretation. We end with a brief conclusion.

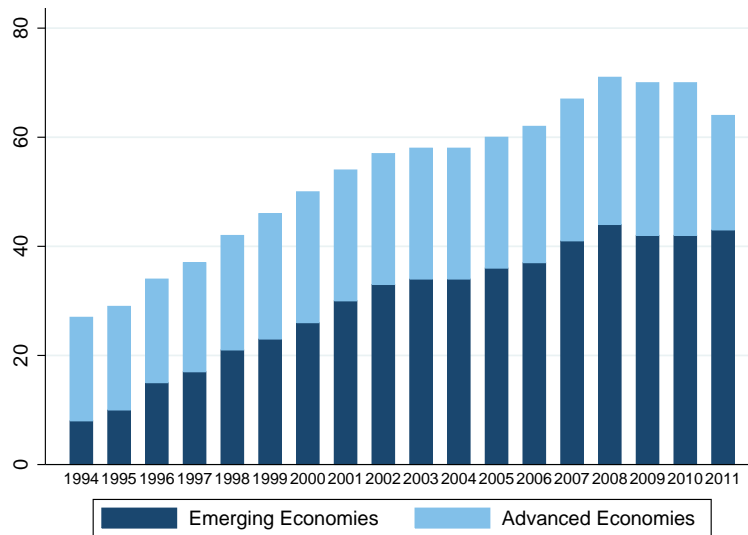
## 2 Data

### 2.1 The dataset

For our study, we compile a broad dataset of the bond 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 only start to become available after 1994, we add those countries' sovereign bonds as soon as reliable information is available. Whereas our dataset only comprises sovereign bonds issued by 27 countries in 1994, this number increases to 76 countries toward 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, during the last 20 years. While for 1994 sovereign bond yields are mostly available for developed countries, availability of emerging market bond yields picks up heavily over our sample period. Towards the end of our sample period, emerging markets even account for the bulk of sovereign bonds in the sample. Figure 1 illustrates the increasing scope of our dataset during the sample period.

In order to be able 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

Figure 1: Number of sovereign bonds in dataset, by development



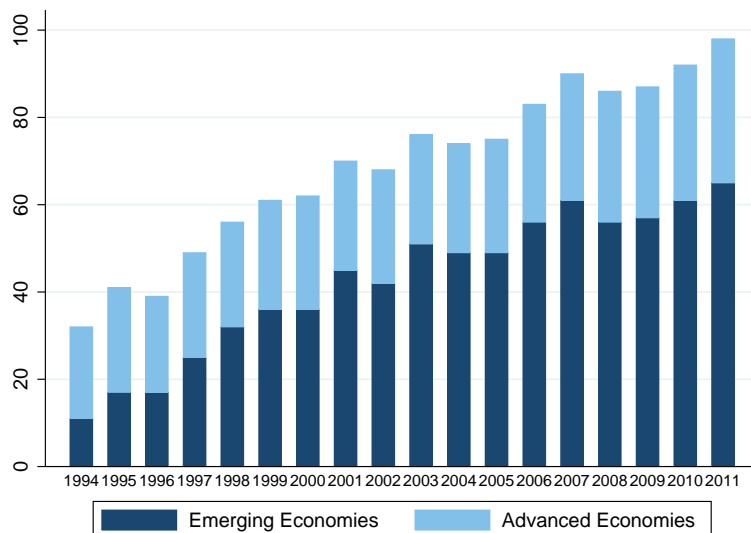
*Notes* — This figure shows the evolution of the total number of sovereign bonds in the dataset between 1994 and 2011. We follow the IMF World Economic Outlook in classifying countries as advanced or emerging economies.

10-year yields for up to 33 countries. If data is not available on Bloomberg, we supplement this with 10-year 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 J.P. Morgan Emerging Markets Bond Index Global (henceforth EMBI Global, see J.P. Morgan 1999). While sovereign bonds included in the EMBI Global have to fulfill 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 two other sources. To avoid that this leads to perturbations of our later results, we control for maturity in all regressions. Table A.1 gives a detailed overview of the sovereign bond market data included in our sample.

For the purpose of our later analysis, we use the sovereign bond yields to compute sovereign bond *spreads*. The spread is the differential of the country’s sovereign bond yield over that of a U.S. Treasury bond of comparable maturity.<sup>2</sup> As explained before, we use 10-year maturities where generic 10-year yields are available, 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

<sup>2</sup>Given the number of available yields, this means that we can consider the spread reactions of up to 75 countries.

Figure 2: Number of rated countries, by development



*Notes* — This figure shows the evolution of the total number of countries rated by at least one of the rating agencies (S&P, Moody’s, Fitch) in the dataset between 1994 and 2011. We follow the IMF World Economic Outlook in classifying countries as advanced or emerging economies.

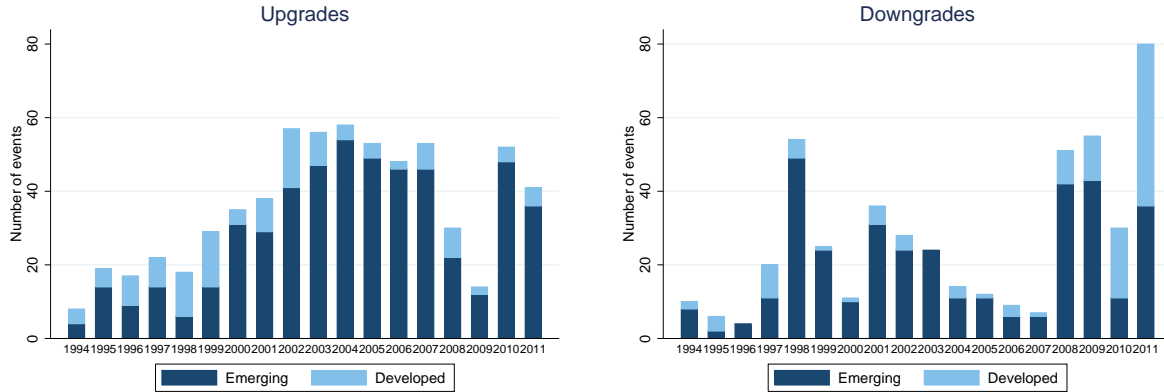
eligible instruments a country has issued), we obtain the relevant U.S. Treasury yields by interpolating from the closest published yield curve rates.

Information on sovereign ratings comes from the rating agencies’ websites and includes both daily information on rating changes and sovereign watch listings by any of the “Big Three” (S&P, Moody’s and Fitch) from 1994 to 2011. We choose the year 1994 as a natural starting point for our sample period since Fitch Ratings 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” also changes quite substantially during our sample period. While in 1994 only 34 sovereigns were rated by at least one of the agencies, that number increased to 98 countries in 2011 (see Figure 2).

## 2.2 Characteristics of rating announcements

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

Figure 3: Upgrades and downgrades over time, by development



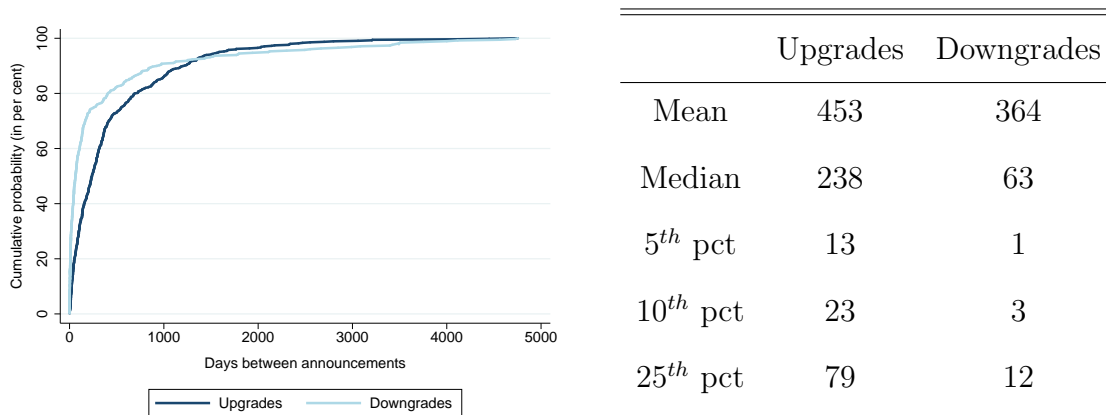
*Notes* — This figure shows the evolution of upgrade and downgrade announcements made by S&P, Moody’s and Fitch over the 1994–2011 sample period. We follow the IMF World Economic Outlook in classifying countries as advanced or emerging economies.

As Figure 3 illustrates, rating activity is not evenly distributed over time but, particularly for downgrades, shows some hefty peaks during specific episodes of crisis. Whereas in “normal times” downgrades by rating agencies 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 crisis (where for the first time advanced economies have been 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 stylized 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 at the same time (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 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 percent of all cases, a downgrade on a country is followed by another downgrade on that country within just one day. For example, in the course of the 1997 Asian crisis, S&P, Fitch and Moody’s all downgraded South Korea’s credit rating on successive days between November 25<sup>th</sup> and 27<sup>th</sup>. Similarly, during the ongoing European debt crisis Fitch issued a downgrade for Greece on December 8<sup>th</sup>, 2009. One week later, S&P downgraded the country as well, as did Moody’s yet another six days later.



Figure 4: Clustering of rating announcements, by direction



*Notes* — This figure shows the cumulative distribution functions, and summary statistics, of the number of calendar days that pass between an upgrade (downgrade) announcement for a given country and a subsequent upgrade (downgrade) of the same country by any agency. The numbers are based on the sample of 1,097 rating announcements (635 upgrades, 462 downgrades) made by Standard & Poor’s, Moody’s and Fitch between 1994 and 2011.

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 (or even on the same day of) 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 a spillover in ratings. This is all the more important if the countries concerned share a common trait of some form which leads CRAs to make simultaneous announcements for the countries concerned in the first place, as appears to have happened on October 3<sup>rd</sup>, 2008 when Fitch downgraded Estonia, Latvia and Lithuania.<sup>3</sup> 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. This should provide for a particularly clean identification of relevant effects, which compares favorably with Gande and Parsley (2005) and other work in the sovereign spillovers literature (e.g., Christopher et al. 2012, Ismailescu and Kazemi 2010).

<sup>3</sup>Other examples in our dataset may be seen in S&P’s downgrade announcements for South Korea and Taiwan during the Asian financial crisis on October 24<sup>th</sup>, 1997, or Fitch lowering the ratings of Estonia, Ireland, Lithuania, and Latvia on April 8<sup>th</sup>, 2009.

Similarly, the informational content of a rating change might be conditional on whether it has been preceded by the respective country being put on a watchlist. As the body of literature on the effects of rating announcements on the refinancing condition of the very same country shows (see, e.g., Afonso et al. 2011, Ismailescu and Kazemi 2010), 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.<sup>4</sup> Ignoring these anticipation effects therefore bears the risk of underestimating bond market reactions to a sovereign rating action. Since our dataset includes all data on sovereign watchlistings by the “Big Three”, we are able to directly control for a country’s watchlist status and to mitigate potential problems with anticipation.

### 3 Identifying sovereign spillovers

#### 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 (hereafter referred to as *event country*) impacts significantly on the bond yields of another (*non-event country*). Yet, the mere observation of a change in non-event country yields when an even-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 an announcement is “no rating change”. While this represents a challenge in *direct* announcement studies already, it makes it essentially impossible to identify *spillover* effects in our data.

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<sup>4</sup>In our paper, we use the two terms interchangeably. While S&P and Fitch issue watchlistings, in the Moody’s terminology those are called “reviews”.

The reason is that there are too few rating announcements in our dataset that are preceded by sufficiently long windows with no announcements on *any* rated country.<sup>5</sup> There is obviously a trade-off between the number of eligible announcements and the length of the estimation window. However, even at a 30-day estimation window commonly used in sovereign event studies, which is toward the shorter end of the event-study literature more generally, only 23 upgrades would be eligible, and 36 downgrades.

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 only one notch serve as the counterfactual for more severe changes of two notches or more. This approach is implemented in the following estimation equation, which we run on upgrades and downgrades separately:<sup>6</sup>

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

The dependent variable  $\Delta Spread_{n,t}$  is the change in non-event country  $n$ 's sovereign bond spread vis-à-vis the United States over the two-trading-day window  $[-1, +1]$  surrounding the announcement on day 0 of a change in the rating of event country  $e$  ( $\neq n$ ). The event window 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. Hence, any potential impact on those would not materialize 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.<sup>7</sup>

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 notch distribution of upgrades and downgrades in our sample, which is shown in Figure 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 two-notch changes, while

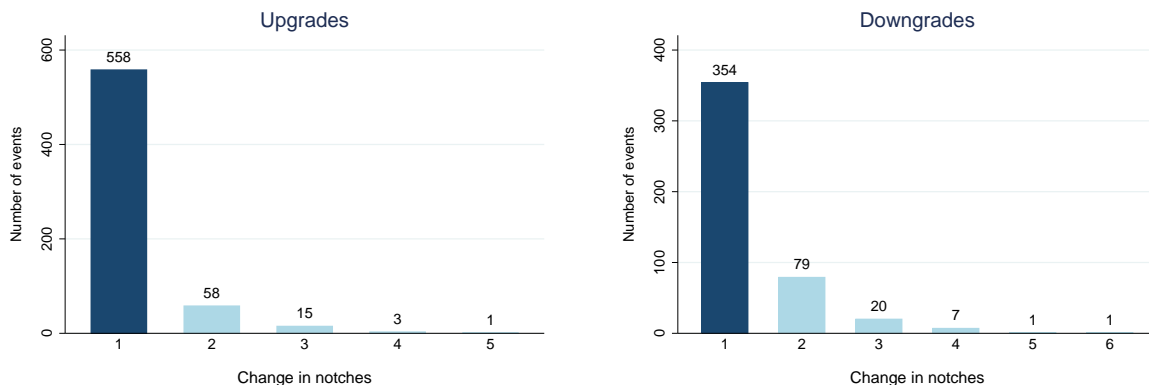
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<sup>5</sup>The universe of all rated countries is the relevant benchmark when analyzing potential spillover effects in this framework. Of course, if we only required the estimation window to be free of announcements pertaining to the non-event country, the number 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.

<sup>6</sup>This is in keeping with the methodology employed by the bulk of the spillover and event-study literature. The asymmetries in reactions to positive and negative announcements consistently found in those studies further reinforce the notion of treating upgrades and downgrades distinctly.

<sup>7</sup>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 5: Distribution of rating changes



*Notes* — This figure shows the distribution of the intensity of rating changes. Numbers are based on the sample of 1,097 rating announcements (635 upgrades, 462 downgrades) made by Standard & Poor’s, Moody’s and Fitch between 1994 and 2011.

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.<sup>8</sup>

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  $\beta$  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 counterfactual (due to the estimation window problem outlined above), we identify spillover effects in pooled cross sections of upgrades and downgrades rather than in a true panel setup.<sup>9</sup> We therefore estimate the model by OLS.

At this point, it seems important to address a concern about potential endogeneity of the large-change dummy. The implicit assumption in the above design is that the rating announcement constitutes the dominant event during the event window, and that its severity is not systematically related to other spread-relevant information being released simultaneously. Otherwise,  $LARGE_{e,t}$  and the error term  $\omega_{e,n,t}$  would be correlated, so that  $\beta$  would be biased. Restricting the event window to two days already goes a long way toward alleviating the problem by limiting the amount of information that might potentially correlate with the large-change dummy. However, there might still remain a concern if, for instance,

<sup>8</sup>For the same reason, Alsakka and ap Gwilym (2012) also apply this grouping in their analysis of the direct effects of sovereign rating changes on exchange rate movements.

<sup>9</sup>Thus,  $t$  denotes *generic* rather than actual time and can be thought of as indexing the different rating events.

CRAAs downgraded a country instantaneously in reaction to “bad news” and did so by more notches for “particularly bad news”.

While one cannot rule this out for every single announcement in the sample — and because soft information is notoriously hard to measure —, the proclaimed practice of CRAAs and a corresponding body of empirical literature suggest that such a bias might not be a major issue. 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 CRAAs 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 ratings indicates that this practice might indeed be followed, thus reducing the timeliness of rating changes (Altman and Rijken 2004, Liu et al. 2011), and that the CRAAs may be “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). Finally, 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 CRAAs opt for somewhat less timely announcements.

## 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 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 those have been anticipated by the market (see, e.g., Ismailescu and Kazemi 2010, Gande and Parsley 2005, Reisen and von Maltzan 1999). 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 (Afonso et al. 2011, Kaminsky and Schmukler 2002, Hand et al. 1992). 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 contrasts with Gande and Parsley (2005), who amalgamate a country’s watch status into the 17-notch scale in order to construct a “comprehensive credit rating” which is supposed to reflect ratings and reviews both at the same time. We thereby avoid having to make an additional assumption as to how reviews and actual changes relate to one another quantitatively. One might also argue that, despite the potential anticipation effects of watchlistings, the latter are not qualitatively the same as actual rating changes. In any case, our much larger sample allows us to focus on the class of actual rating changes only and their relative strengths within that class as the only measurable difference. This should provide for a cleaner identification of spillover effects.

Moreover, we have shown in 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 ( $SimActsDayNonEvt_{n,t}$ ).

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 would 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 more technical control, which also adds a genuine time component to our pooled cross sections.

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. Those capture global macroeconomic trends which might be reflected in the yields of U.S. Treasuries and, hence, spread changes. For instance, there may be a stronger tendency for investments to flow into the U.S. 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 positions on the yield curve, a dummy for bond yields from EMBIG, and a

dummy for spread changes that need to be measured over weekends as those correspond to longer intervals in terms of calendar days.

## 4 Results

### 4.1 Existence of cross-border spillover effects

Tables 1 and 2 show baseline estimation results on the existence of cross-border effects for upgrades and downgrades, respectively. We start with a parsimonious specification in Model 1, which only contains our main variable of interest, the large-change dummy *LARGE*. Subsequently, we add different variables that characterize the rating environment to test how our findings hold up in the presence of different initial ratings, potential anticipation effects from watchlistings as well as clustering within and across countries (Models 2 to 5). Finally, we also account for global market turbulence or risk aversion (Model 6).

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 all specifications.<sup>10</sup> 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 that 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 (unreported) regression that the absolute coefficients for upgrades and downgrades are statistically different from each other.<sup>11</sup>

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

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<sup>10</sup>This is also robust to the exclusion of rating changes of four notches or more. Due to the very few observations in this bin (see Figure 5), it might be those events which are driving our results. However, this is not the case as the findings remain virtually unchanged.

<sup>11</sup>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 respective explanatory variables. The interaction term of *LARGE* with the new dummy is positive and highly significant throughout, pointing to statistically significant differences in the absolute coefficients for upgrades and downgrades. Results are available upon request.

Table 1: **Baseline regressions, upgrades**

	(1)	(2)	(3)	(4)	(5)	(6)
<i>LARGE</i>	-0.0131** (0.0061)	-0.0121** (0.0060)	-0.0139** (0.0064)	-0.0125* (0.0065)	-0.0124* (0.0064)	-0.0128* (0.0067)
<i>InitRat</i>		0.0001 (0.0008)	0.0001 (0.0008)	-0.0005 (0.0009)	-0.0005 (0.0009)	0.0000 (0.0010)
$\Delta$ <i>InitRat</i>		0.0010 (0.0006)	0.0010 (0.0006)	0.0009 (0.0007)	0.0008 (0.0006)	0.0009 (0.0007)
<i>OnWatch</i>			0.0076 (0.0053)	0.0059 (0.0055)	0.0057 (0.0055)	0.0070 (0.0058)
<i>SimActsWdwEvt</i>				-0.0019 (0.0057)	-0.0020 (0.0057)	-0.0013 (0.0057)
<i>SimActsDayNonEvt</i>					-0.0863* (0.0512)	-0.0877 (0.0546)
<i>VIX</i>						0.0017*** (0.0004)
<i>N</i>	33,303	31,986	31,986	30,564	30,564	29,950
Event countries	104	104	104	92	92	92
Non-event countries	75	73	73	73	73	73
Upgrades	635	635	635	606	606	595
$R^2$	0.0237	0.0230	0.0230	0.0212	0.0216	0.0223

*Notes* — This table shows baseline regressions for up to 635 upgrade announcements made by S&P, Moody’s and Fitch between 1994 and 2011. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and J.P. Morgan EMBIG data, as well as levels and squares of non-event country bond maturities. For variable definitions, see Table A.3. Robust standard errors in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

developed and emerging countries that negative rating events in one country affect sovereign bond spreads in others whereas there is no discernable impact for positive events. Similar results have been obtained regarding the direct effects in sovereign bond and CDS markets (Afonso et al. 2011, Larraín et al. 1997), mirroring a well-established finding from event studies on bond, stock, and CDS returns in the corporate sector (e.g., Norden and Weber 2004, Steiner and Heinke 2001, Goh and Ederington 1993, Hand et al. 1992). 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 similarity in initial ratings between event and non-event country seem to play a role in terms of spillover effects. Both coefficients are far



Table 2: **Baseline regressions, downgrades**

	(1)	(2)	(3)	(4)	(5)	(6)
<i>LARGE</i>	0.0213*** (0.0063)	0.0187*** (0.0061)	0.0206*** (0.0063)	0.0224*** (0.0065)	0.0224*** (0.0065)	0.0207*** (0.0066)
<i>InitRat</i>		-0.0013 (0.0014)	-0.0013 (0.0014)	-0.0013 (0.0017)	-0.0013 (0.0017)	-0.0008 (0.0017)
$\Delta$ <i>InitRat</i>		0.0006 (0.0008)	0.0006 (0.0008)	0.0008 (0.0009)	0.0008 (0.0009)	0.0008 (0.0009)
<i>OnWatch</i>			-0.0103* (0.0053)	-0.0102* (0.0054)	-0.0100* (0.0054)	-0.0046 (0.0054)
<i>SimActsWdwEvt</i>				0.0170*** (0.0064)	0.0170*** (0.0064)	0.0141** (0.0065)
<i>SimActsDayNonEvt</i>					0.1210** (0.0558)	0.1477** (0.0635)
<i>VIX</i>						0.0006* (0.0004)
<i>N</i>	24,637	23,734	23,734	22,413	22,413	21,931
Event countries	95	95	95	84	84	84
Non-event countries	75	73	73	73	73	73
Downgrades	462	462	462	436	436	427
$R^2$	0.0374	0.0397	0.0399	0.0396	0.0400	0.0423

*Notes* — This table shows baseline regressions for up to 462 downgrade announcements made by S&P, Moody’s and Fitch between 1994 and 2011. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and J.P. Morgan EMBIG data, as well as levels and squares of non-event country bond maturities. For variable definitions, see Table A.3. Robust standard errors in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

from significant across specifications (Models 2 to 6). 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 watchlisting. The 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 percent level for downgrades (Models 3 to 5). A possible explanation for this is given by Altman and Rijken (2006). They point out that watchlistings partially ease the tension between the market’s expectation of rating stability and the market’s demand for rating timeliness. This suggests that watchlistings contribute to the anticipation of

actual rating changes. Given that investors tend to be more concerned about negative news, watchlistings 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 watchlisting, so are only 15 percent of all upgrades. Finally, it has often been noted that there is an incentive to leak good news (see, e.g., Alsakka and ap Gwilym 2012, Rachel et al. 2012, Gande and Parsley 2005, Goh and Ederington 1993, Holthausen and Leftwich 1986), so the relevance of watchlistings 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 all downgrade regressions, only once do we detect a marginally significant effect of across-clustering for upgrades (Model 5). This appears plausible in light of the stylized facts presented in 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).<sup>12</sup> 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 (Models 4 to 6). 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). Thus, *SimActsWdwEvt* tends to be higher in times of market turbulence or global risk aversion when spreads against a “safe-haven” investment

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<sup>12</sup>In choosing the window length, we follow Gande and Parsley (2005) who employ a two-week duration for a comparable control variable. Extending the window 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. 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 available on request.

like U.S. Treasuries are upward-trending, too (e.g., González-Rozada and Levy Yeyati 2008, García-Herrero and Ortíz 2006, International Monetary Fund 2004, 2006). 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 significant for both upgrades and downgrades, given the relation between market turbulence and yield spread drift (Model 6). 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.

## 4.2 Channels of cross-border spillover effects

We now turn to potential channels of spillover effects in the sovereign bond market. 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  $\Delta InitRat$  — 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 (Model 6 in Tables 1 and 2) 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 consider the size of the event country’s GDP ( $SizeEvt$ ) as well as differences between event and non-event countries in terms of GDP ( $\Delta Size$ ) and trend growth ( $\Delta TrendGrowth$ ). Definitions and sources for all control variables are reported in Table A.3. The results are shown in Tables 3 and 4.

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.

Table 3: Spillover channels, upgrades

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>LARGE</i>	-0.0128*	-0.0128*	-0.0111	-0.0117*	-0.0142**	-0.0112	-0.0137**
	(0.0067)	(0.0067)	(0.0071)	(0.0068)	(0.0066)	(0.0070)	(0.0069)
<i>InitRat</i>	0.0000	0.0001	-0.0005	0.0027**	0.0031***	0.0008	0.0013
	(0.0010)	(0.0010)	(0.0010)	(0.0013)	(0.0012)	(0.0014)	(0.0013)
$\Delta$ <i>InitRat</i>	0.0009	0.0010	0.0006	0.0012*	0.0011	0.0009	0.0007
	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0008)
<i>OnWatch</i>	0.0070	0.0070	0.0066	0.0080	0.0085	0.0068	0.0075
	(0.0058)	(0.0058)	(0.0060)	(0.0059)	(0.0061)	(0.0061)	(0.0062)
<i>SimActsWdwEvt</i>	-0.0013	-0.0013	-0.0058	-0.0026	-0.0032	-0.0067	-0.0073
	(0.0057)	(0.0057)	(0.0059)	(0.0058)	(0.0059)	(0.0060)	(0.0061)
<i>SimActsDayNonEvt</i>	-0.0877	-0.0903	-0.1024	-0.0883	-0.0950	-0.1002	-0.1091*
	(0.0546)	(0.0549)	(0.0625)	(0.0546)	(0.0578)	(0.0621)	(0.0663)
<i>VIX</i>	0.0017***	0.0017***	0.0019***	0.0017***	0.0018***	0.0019***	0.0020***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
<i>Region</i>		0.0109	0.0146*	0.0128*	0.0125*	0.0162**	0.0169**
		(0.0071)	(0.0080)	(0.0073)	(0.0075)	(0.0080)	(0.0083)
<i>TradeBloc</i>			-0.0100			-0.0107	-0.0133*
			(0.0065)			(0.0066)	(0.0069)
<i>ExpImpEvt</i>			-0.1080			-0.0767	-0.0875
			(0.2149)			(0.2134)	(0.2145)
<i>SizeEvt</i>				0.0279	0.0257	0.0425**	0.0409**
				(0.0190)	(0.0196)	(0.0202)	(0.0208)
$\Delta$ <i>Size</i>				-0.0399**	-0.0404**	-0.0430**	-0.0442**
				(0.0187)	(0.0194)	(0.0197)	(0.0205)
$\Delta$ <i>TrendGrowth</i>					-0.0001		-0.0001
					(0.0001)		(0.0001)
<i>N</i>	29,950	29,950	27,962	29,329	28,904	27,798	27,380
Event countries	92	92	90	92	91	90	89
Non-event countries	73	73	71	72	72	71	71
Upgrades	595	595	582	592	584	579	571
$R^2$	0.0223	0.0223	0.0221	0.0235	0.0271	0.0231	0.0267

*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. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and J.P. Morgan EMBIG data, as well as levels and squares of non-event country bond maturities. For variable definitions, see Table A.3. Robust standard errors in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

Table 4: Spillover channels, downgrades

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>LARGE</i>	0.0207*** (0.0066)	0.0206*** (0.0066)	0.0217*** (0.0069)	0.0222*** (0.0070)	0.0224*** (0.0070)	0.0227*** (0.0072)	0.0229*** (0.0072)
<i>InitRat</i>	-0.0008 (0.0017)	-0.0006 (0.0017)	-0.0010 (0.0018)	-0.0017 (0.0019)	-0.0017 (0.0019)	-0.0023 (0.0020)	-0.0024 (0.0020)
$\Delta$ <i>InitRat</i>	0.0008 (0.0009)	0.0012 (0.0009)	0.0017* (0.0010)	0.0008 (0.0010)	0.0008 (0.0010)	0.0013 (0.0011)	0.0013 (0.0011)
<i>OnWatch</i>	-0.0046 (0.0054)	-0.0046 (0.0054)	-0.0031 (0.0058)	-0.0009 (0.0056)	-0.0008 (0.0057)	0.0009 (0.0058)	0.0011 (0.0059)
<i>SimActsWdwEvt</i>	0.0141** (0.0065)	0.0141** (0.0065)	0.0135** (0.0066)	0.0146** (0.0067)	0.0146** (0.0067)	0.0139** (0.0068)	0.0139** (0.0068)
<i>SimActsDayNonEvt</i>	0.1477** (0.0648)	0.1451** (0.0643)	0.1426** (0.0653)	0.1160* (0.0623)	0.1161* (0.0623)	0.1136* (0.0622)	0.1137* (0.0622)
<i>VIX</i>	0.0006* (0.0004)	0.0006* (0.0004)	0.0006 (0.0004)	0.0006* (0.0004)	0.0006* (0.0004)	0.0006 (0.0004)	0.0006 (0.0004)
<i>Region</i>		0.0376** (0.0153)	0.0329** (0.0164)	0.0379** (0.0157)	0.0380** (0.0157)	0.0331** (0.0168)	0.0332** (0.0168)
<i>TradeBloc</i>			0.0159 (0.0111)			0.0151 (0.0115)	0.0153 (0.0116)
<i>ExpImpEvt</i>			0.0687 (0.2200)			0.0635 (0.2265)	0.0594 (0.2259)
<i>SizeEvt</i>				0.0222 (0.0290)	0.0221 (0.0294)	0.0263 (0.0309)	0.0260 (0.0313)
$\Delta$ <i>Size</i>				-0.0169 (0.0218)	-0.0170 (0.0223)	-0.0190 (0.0234)	-0.0189 (0.0240)
$\Delta$ <i>TrendGrowth</i>					0.0000 (0.0000)		0.0000 (0.0000)
<i>N</i>	21,931	21,931	20,633	21,031	20,885	20,035	19,896
Event countries	84	84	81	82	82	79	79
Non-event countries	73	73	71	72	72	71	71
Downgrades	427	427	416	416	416	405	405
<i>R</i> <sup>2</sup>	0.0423	0.0428	0.0423	0.0441	0.0442	0.0435	0.0436

*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. All specifications include a constant, dummies for event and non-event countries, years, spread reactions over weekends and J.P. Morgan EMBIG data, as well as levels and squares of non-event country bond maturities. For variable definitions, see Table A.3. Robust standard errors in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

In more detail, we find consistently that spillover effects are significantly stronger within the same region in the case of downgrade announcements. 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 these 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 (Alsakka and ap Gwilym 2012, Arezki et al. 2011, De Santis 2012). Surprisingly, we obtain positive coefficients for upgrades 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 rationalized. On a positive note, though, the magnitude for upgrades is only about a third of that for downgrades. 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 (*TradeBloc*) and the non-event country’s ratio of exports to the event country to domestic 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 Model 7 in Table 3). Moreover, the stability of 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.<sup>13</sup> To the extent that trade also captures a notable portion of variation in bilateral asset holdings, the same applies to financial linkages.<sup>14</sup> Hence, it appears more likely that our findings on belonging to the same region are driven by shared institutional and cultural features or, less sophisticated, that this has an effect by itself.

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<sup>13</sup>Note that the correlation of the two trade variables with the region control does not support multicollinearity as a technical explanation for the results.

<sup>14</sup>We would like to control directly for financial linkages of event and non-event countries, e.g. the exposure of non-event country banks 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, which renders virtually impossible any comparison with the baseline results. This also applies to data on foreign direct investment (FDI). However, bilateral trade is likely to pick up some of the financial linkages we intend to capture as there is evidence that trade is a powerful determinant of bilateral (bank) asset holdings (Aviat and Coeurdacier 2007). 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 (e.g., Goldberg and Saunders 1980, 1981, Brealey and Kaplanis 1996, Yamori 1998). Aiming to maintain broad coverage and composition through time, we rely on trade data.

The evidence on the remaining potential channels is succinctly summarized 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 (Models 1 and 2 in Table 4) 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 further the asymmetry that exists between positive and negative rating changes. However, this also means that for upgrades the evidence on potential channels 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 coefficient 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.<sup>15</sup> 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 (see Models 4 to 7 in Table 3).<sup>16</sup> 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 also does nothing to contradict this interpretation. In view of the generally more ambiguous results for upgrades, however, we do not wish to overemphasize this point.

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<sup>15</sup> $\Delta Size$  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 GDPs in 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) percent. With an absolute coefficient of roughly 0.04, the (semi-)elasticity marginal effects therefore obtain as four and two basis points, respectively.

<sup>16</sup>Larger countries tend to hold more favorable credit ratings. This correlation may explain why, in Models 4 and 5, we find  $InitRat$  significant and  $SizeEvt$  insignificant, and vice versa for Models 6 and 7.

### 4.3 Discussion

Our results can be condensed into the following stylized 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 watchlistings 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 types of announcements. Fourth, evidence on the channels behind positive spillover effects, if any, offers a more complex picture and appears relatively inconclusive.

So, 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 paper, 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 (Boot et al. 2006, Masson 1998).

However, a major regulatory focus on the activities of CRAs would also require negative spillover effects of substantial *economic* magnitude. In this paper, 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 can be thought of as consisting of a “base effect” that “small” downgrades have, compared to the benchmark scenario of no downgrade 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 effects” of rating changes unless one rules out the existence of rating-induced spillovers from the beginning (see our discussion in 3.1). Nonetheless, the total effect is conceivably a multiple of the one we estimate. At factors of 2 and 5, for instance, the implied total effects amount to approximately 4 and 10 basis points, respectively. To put this into perspective, the average sovereign bond spread vis-à-vis U.S. Treasuries at the time of the downgrade announcements in our sample is 3.25 percent, 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 4 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 countries are found “guilty by association” with the event country. Moreover, such behavior 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 behavior. Hence, it is not clear that putting the primary emphasis on CRAs would prove effective in this regard.

## 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 paper, 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 74 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 favor 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, at best, much more muted, 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 (measurable) 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, failure to reject the irrelevance 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 this kind of investor behavior 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.

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# Appendix

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

<b>Bloomberg (33 countries)</b>	
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)
<b>JP Morgan EMBI Global (43 countries)</b>	
1994	Argentina, Mexico, Nigeria, Venezuela (all 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	Côte d'Ivoire, 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)
2006	Iraq (April)
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 from which sources the sovereign bond yield data in the sample were obtained and the years in which the respective time series were first observed (months in parentheses). If there were gaps in the Bloomberg 10-year generic yield series, we added observations of 10-year generic yields from Datastream, ensuring that this did not induce structural breaks. Moreover, for some emerging countries we included 10-year generic yields until the EMBIG series became available.

Table A.2: **Rating scales and transformation**

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

*Notes* — This table shows how the letter ratings employed by Standard & Poor's, Moody's, and Fitch correspond to one another and to different degrees of credit risk, and how those are mapped into the linear 17-notch scale used in the investigation. The transformation is the same as in Afonso et al. (2011), from which the table is adapted.



Table A.3: Variable definitions

Variable name	Definition	Source
$\Delta Spread$	Change in the non-event country spread vis-à-vis U.S. Treasuries of comparable maturity over the two-trading-day window $[-1, +1]$ around the rating announcement (on day 0), measured in percentage points.	Bloomberg, Datastream, J.P. Morgan, U.S. Department of the Treasury
$LARGE$	Dummy variable taking on a value of one for “large” rating upgrades or downgrades of two notches or more, zero otherwise.	S&P, Moody’s, Fitch
$InitRat$	Credit rating held by the event country with the announcing credit rating agency prior to the event, measured on a 17-notch scale (see Table A.2).	S&P, Moody’s, Fitch
$\Delta InitRat$	Absolute difference between $InitRat$ and the average of all active credit ratings held by the non-event country with the three agencies, both measured on a 17-notch scale (see Table A.2).	S&P, Moody’s, Fitch
$OnWatch$	Dummy variable taking one a value of one if the event country was on watch/review by the announcing agency 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 other agencies over the two-week interval $[-14, -1]$ (calendar days) before the respective upgrade (downgrade) event.	S&P, Moody’s, Fitch
$SimActsDayNonEvt$	Number of upgrade (downgrade) announcements made on the non-event country by any agency 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, 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: the European Union, NAFTA, ASEAN, Mercosur, CARICOM, the Andean Community, the Gulf Cooperation Council, Mercosur, the Southern African Customs Union, the Economic Community of Central African States, the Economic Community of West African States, and the Organization of Eastern Caribbean States.	Authors’ definition
$ExpImpEvt$	Importance of the event country 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.	OECD, UNCTAD, World Bank
$SizeEvt$	Size of the event country, measured in logs of U.S. dollar GDP.	World Bank
$\Delta Size$	Size differential of the event over the non-event country, measured in logs of U.S. dollar GDP.	World Bank
$\Delta TrendGrowth$	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