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Credit ratings and cross-border bond market spillovers

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Abstract

This paper studies spillovers across sovereign debt markets in the wake of sovereign rating changes. We compile 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 73 developed and emerging countries between 1994 and 2011. To cleanly identify the existence of spillover effects, we perform an explicit counterfactual analysis which pits bond market reactions to small revisions in ratings against reactions to all other, more major changes. We also control for the environment in which an announcement is made, such as the anticipation through watchlistings and the interaction of similar rating actions by different agencies. While there is strong evidence of negative spillover effects in response to downgrades, positive spillovers from upgrades are much more limited at best. Furthermore, negative spillover effects are more pronounced for countries within the same region. Strikingly, this cannot be explained by fundamental linkages and similarities between countries.

JEL classification: G15, F36

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

Non-Technical Summary

The announcements of sovereign rating changes by credit rating agencies (CRAs) have recently resurfaced on the agenda of policy makers. In particular, concerns over so-called negative spillover effects on other countries' government bond yields have loomed large in the wake of sovereign stress in the euro area. Moreover, transcending mere debate, these concerns provide one rationale for recent legislative changes that have limited the ability of CRAs to issue ratings on the debt of European Union member states. Even though spillovers are thus highly relevant from a policy perspective, their existence is not straightforward to identify in financial markets, where confounding events are ubiquitous and hamper the establishment of clear counterfactuals.

We therefore propose a novel empirical strategy to cleanly identify the existence of cross-border spillover effects from sovereign rating announcements. In an explicit counterfactual analysis, we test whether sovereign bond yields react more strongly to rating upgrades or downgrades in another country when those are "large" (i.e., when the rating changes by two notches or more) than when those are "small" (i.e., when the rating changes by a single notch). This allows us to not only avoid problems associated with a classic event-study approach in a spillover context, but also does not require the additional assumptions made by a number of papers in the literature.

To this end, we compile an extensive dataset on the complete history of sovereign rating announcements by the "Big Three" (Standard & Poor's, Moody's, and Fitch) and daily sovereign bond market movements for up to 73 developed and emerging countries between 1994 and 2011. In addition to major variation along the time and country dimensions, the substantial heterogeneity in announcements allows us to establish the explicit counterfactual based on the strength of rating changes. At the same time, we are able to control for anticipation effects, be they due to a country being placed on a "watchlist" first or due to a prior rating change by another CRA.

Our findings suggest a major asymmetry in the sovereign debt market's reaction to rating changes. On the one hand, there is evidence for significant negative spillovers following sovereign rating downgrades — the main concern of policy makers. This result is robust to a number of tests, such as the exclusion of extreme rating events and specific crisis episodes, potential differences in the market's perception of the different agencies, or CRAs reacting to adverse yield developments by further downgrade announcements. On the other hand, cross-border market reactions to upgrades appear to be much more muted, if anything.

As a further piece of analysis, we also investigate potential channels of cross-border spillover effects. Importantly, we find that negative spillovers are more pronounced for countries within the same region. However, “fundamental” factors like bilateral trade linkages, financial integration or other similarities between countries strikingly fail to account for this finding.

This is particularly interesting against the background of the notion inherent in some policy discussions that negative spillovers are in some sense unwarranted, so as to merit an intervention by the state, such as constraining CRAs’ scope of action. Hence, our results do not suggest that concerns over financial markets finding countries “guilty by association” can be dismissed out of hand.

1 Introduction

Ever since tensions began to surface in the euro area 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 this period, 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 policy makers. 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 may provide one explanation for why the Commission has just recently set up stricter rules for the agencies. In particular, CRAs are now only allowed to issue ratings for European Union member states' sovereign debt at three pre-defined dates every year (European Union, 2013).

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. In this paper, we therefore propose a novel empirical strategy to cleanly identify the existence of cross-border spillover effects of sovereign rating announcements. To this end, we collect an extensive dataset that comprises a complete history of both the sovereign rating actions by the "Big Three" (Standard & Poor's [S&P], 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, this variation allows us to perform an explicit counterfactual analysis that 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. 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 an event window, is suitable in principle but, in a spillover context, places too high demands on the necessary non-contamination of the estimation window. Hence, in this paper, we use 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, they rely on a so-called "comprehensive credit rating" (see Afonso et al., 2012; Alsakka and ap Gwilym, 2012; Gande and Parsley, 2005; Ismailescu and Kazemi, 2010). This combines two different types of rating announcements — actual rating changes and watchlistings — into a single scale. Their identification therefore depends on additional assumptions on the relative informational content of watchlistings and actual rating changes.

In contrast, we 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 watchlistings 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.

Our findings on the existence of cross-border spillover effects point to an important asymmetry in the sovereign debt market's reaction to rating changes. 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. However, even after controlling for these factors, we still find that belonging to a common region amplifies cross-border 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. Hence, our findings suggest that concerns over countries being found "guilty by association" in financial markets cannot be easily dismissed.

The paper is organised as follows. In the next section, we review the existing literature, before describing the dataset and highlighting some important characteristics of rating announcements in Section 3. Section 4 discusses the estimation strategy for identifying cross-border spillovers. Section 5 presents our empirical results and discusses their interpretation. We end with a brief conclusion.

2 Literature review

This paper is related to the literature that investigates the market impact of sovereign rating announcements. One strand of the literature focuses on the *direct* effect of rating changes on the sovereign bonds of the country concerned. Starting with Cantor and Packer (1996), there is a host of papers that use an event-study type approach and show that rating announcements significantly affect the pricing of sovereign bonds. Moreover, most of these studies tend to find that bond market reactions to negative announcements are more pronounced than for positive announcements (see, e.g., Larraín et al., 1997; Reisen and von Maltzan, 1999; Hill and Faff, 2010).[1]

Some papers also look at the effect of rating announcements on other financial markets of the rated country. Brooks et al. (2004), for example, report that sovereign rating downgrades negatively affect both the returns of local stock markets and the exchange rate of the country vis-à-vis the US dollar. Hooper et al. (2008) show that rating upgrades may also have an impact on the stock market, as prices rise and volatility decreases on announcement.

In addition, a growing body of research has studied so-called cross-border *spillover* effects in the wake of sovereign rating announcements. This strand of the literature examines whether the market impact of announcements also extends to economies beyond the respective country.

In particular, episodes of regional financial crises have been the subject of a number of papers. For instance, Arezki et al. (2011) focus on the European debt crisis and find that rating downgrades are associated with significant spillover effects across euro area sovereign stock and credit default swap (CDS) markets. De Santis (2012) detects sizeable spillover effects among Greece, Italy, Ireland, Portugal, and Spain, with Greek downgrades having an especially strong impact on the other crisis countries' bond and CDS

[1]This mirrors 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).

spreads. Baum et al. (2013) also show important foreign exchange market effects as they find downgrade announcements to negatively affect the euro's exchange rate against the US dollar. These results mirror findings by Kaminsky and Schmukler (1999) that, during the 1997/98 Asian financial crisis, CRA announcements contributed substantially to negative spillovers in regional stock markets.

In contrast to the above, studies that also consider non-crisis episodes are able to investigate differential spillover effects between positive and negative announcements.[2] Compared to the literature on the direct effects of sovereign rating actions, the evidence on a potential asymmetry in spillovers to other countries' financial markets is more mixed. While most papers find more pronounced spillovers for sovereign downgrades and other negative rating announcements (e.g., Gande and Parsley, 2005; Ferreira and Gama, 2007; Li et al., 2008; Afonso et al., 2012), a recent contribution by Alsakka and ap Gwilym (2012) documents symmetric spillover effects in the foreign exchange market. Ismailescu and Kazemi (2010) even detect a stronger direct and spillover impact for positive announcements than for negative ones based on a sample of CDS market reactions in emerging economies. It is therefore important to note that results cannot be easily compared as even studies that do not focus on specific regional crisis episodes frequently use an otherwise homogeneous sample of countries, such as emerging markets (e.g., Kaminsky and Schmukler, 2002; Ismailescu and Kazemi, 2010).

A notable exception is the paper by Gande and Parsley (2005) in that it uses a sample which covers 34 economies from the developed and the emerging world, and which spans both crisis and non-crisis periods between 1991 and 2000.[3] They find that while downgrades trigger large and significant negative cross-border spillovers, there is no discernible market impact for upgrades. Gande and Parsley (2005) is also notable methodologically in that their identification of potential spillover effects has been used as the starting point by other contributions in the field (e.g., Ferreira and Gama, 2007; Ismailescu and Kazemi, 2010; Alsakka and ap Gwilym, 2012). In particular, their idea of comparing rating events of different strengths and the market impact that those have within a short window around the announcement has been very influential. As we will discuss in more detail, we also

[2]Of course, this is due to the fact that, by definition, there are virtually no positive announcements to examine during crises. However, since negative announcements occur at basically all times, one can compare the impact of those events during crises to that in normal times. Kaminsky and Schmukler (2002), for example, find for a sample of 16 emerging markets that rating changes contribute more strongly to cross-country contagion in debt and equity markets during crisis times.

[3]Ferreira and Gama (2007) employ a sample that is very similar and, in part, based on Gande and Parsley (2005). Also see Alsakka and ap Gwilym (2012), who consider up to 101 countries between 1994 and 2010, but conduct their analysis using regional subsamples.

borrow from their work whilst exploiting our comprehensive sample to address some of the caveats.

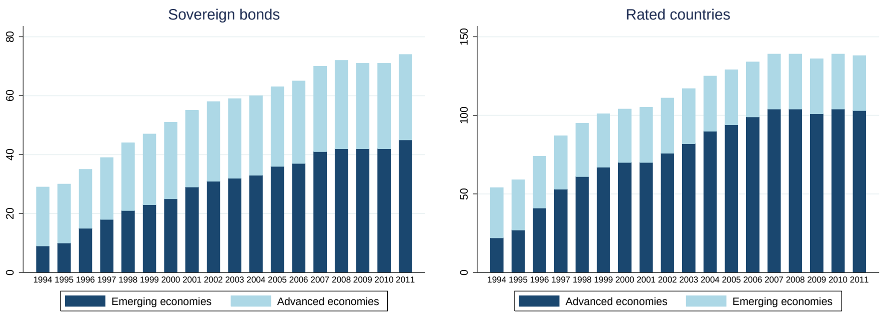
3 Data

3.1 The dataset

We compile a broad dataset of the yields of publicly traded sovereign bonds at daily frequency between January 1994 and December 2011. 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. In particular, the availability of emerging market bond yields picks up heavily, to the extent that those markets account for the majority of sovereign bonds by the end of the sample period. The increasing scope of our dataset is illustrated in Figure 1.[4]

Information on sovereign ratings comes from the rating agencies' websites and includes daily information on both rating changes and sovereign watchlistings by the "Big Three" (S&P, Moody's, Fitch) from 1994 to 2011. 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 54 sovereigns were rated by at least one of the agencies, this number had increased to 138 countries by 2011 (also see Figure 1).

Figure 1: **The dataset**



Notes — This figure shows the scope and composition, by economic development, of the sovereign bond sample and of the sample of countries rated by at least one of the major agencies (S&P, Moody's, Fitch) between 1994 and 2011. Countries are classified according to the IMF World Economic Outlook.

[4]For a detailed overview of the sovereign bond market data included in the sample, see Table A.1 in the Appendix.

3.2 Characteristics of rating announcements

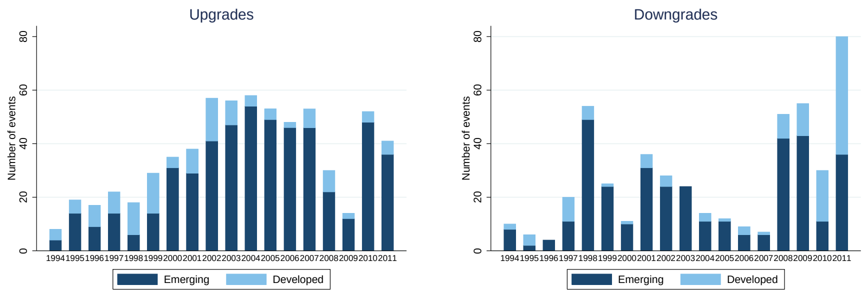
Over the whole sample period, we are able to consider a total of 1,097 rating changes, 635 of which were upgrades, while 462 of those were downgrades. In general, one can observe a significant increase in the number of sovereign credit ratings during our sample period, particularly in emerging market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

As Figure 2 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 (83 out of 138 countries at the end of 2011). Hence, in what we term *within*-clustering, different CRAs may make the same announcement for a *given country* in short succession or even on the same day.

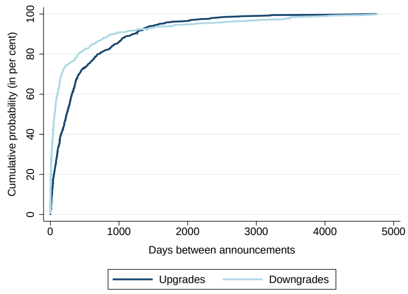
Figure 3 illustrates this issue by plotting the cumulative distribution function and summary statistics of the number of days between similar rating actions on the same country. As can be seen, clustering is particularly pronounced for downgrades. In around five per cent of all cases, a downgrade of a country is followed by another downgrade of that country within just one day. For example, in the course of the Asian crisis, S&P, Fitch

Figure 2: **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.

Figure 3: **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.

and Moody's all downgraded South Korea's credit rating on successive days between 25 and 27 November 1997.

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 as) 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 in the first place. That appears to have happened on 3 October 2008, for instance, when Fitch downgraded Estonia, Latvia and Lithuania. 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 has been preceded by the respective country being put on a watchlist. As the literature on the effects of rating announcements on the refinancing conditions of the very same country shows (e.g., Afonso et al., 2012; 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" before.[5] Ignoring these anticipation effects risks underestimating bond market reactions to a sovereign rating action. Since our dataset includes all sovereign watchlistings by the "Big Three", we can directly control for a country's watchlist status and mitigate potential problems with anticipation.

4 Identifying sovereign spillovers

4.1 Counterfactuals in a spillover context

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*) 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 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

[5] While S&P and Fitch issue watchlistings, in the Moody's terminology those are called "reviews". In the following, we will only use the terms "watch" or "watchlisting".

[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.

between the length of that window and the number of announcements eligible for inclusion in the estimation. For example, 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, in our sample only 23 upgrades would be eligible, and 36 downgrades. Hence, comparing returns in an event window to those in an uncontaminated estimation window is not a viable strategy for the sound identification of spillover effects.

An alternative approach is to focus on the event windows only and establish a counterfactual by exploiting the fact that rating announcements differ along several dimensions. In more detail, the counterfactual for event-window returns in a country is no longer the return development over a prior estimation window in which no rating changes occur, but the returns observed in a short window around events *other* than the one in question. This works when two conditions are met. First, one needs to make different events comparable as they take place at different points in time, in different countries, and under different circumstances. While this can be handled in standard fashion by controlling for various aspects of the rating environment (see 4.3), the second condition is more critical. It requires that rating announcements show some kind of variation. Because all events are associated with a rating announcement by definition, it would be impossible to establish a counterfactual if there were no differences in the strength and/or type of those announcements. But indeed, CRAs may decide to adjust a country's actual rating by more than one notch, or they may issue so-called watchlistings or outlooks to indicate possible future up- or downgrades.

4.2 Identification strategy

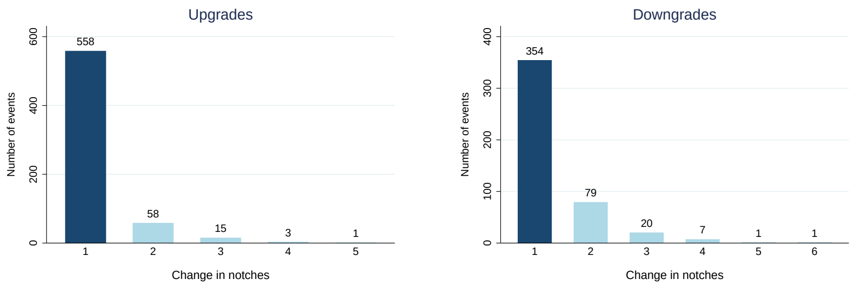
In this paper, we identify potential spillover effects by discriminating between actual rating changes according to their severity whilst controlling for the rating environment, including prior watchlistings.[7] More precisely, rating changes of a single notch serve as the counterfactual for more severe changes of two notches or more. This approach is implemented in the following estimation equation, which we run on upgrades and downgrades separately:

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

However, this would amount to assuming from the outset that only direct effects, as opposed to spillover effects, could possibly matter, which would defeat the purpose of the investigation.

[7] We map CRAs' letter ratings into a linear 17-notch scale following Afonso et al. (2012). Also see Table A.2 in the Appendix.

Figure 4: **Distribution of rating changes**



Notes — This figure shows the distribution of the severity of rating changes, measured on a 17-notch scale. 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.

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 is chosen to account for asynchronous trading. We estimate the model by OLS.

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.[8] This is due to the distribution of the severity of upgrades and downgrades in our sample, which is shown in Figure 4.

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 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 strategy differs from other papers that have also exploited heterogeneity in rating announcements to identify spillovers.[9] Those studies rely on a so-called "comprehensive credit rating" (CCR) that pools not only actual rating changes of different strengths, but

[8]Note that t denotes *generic* rather than actual time and can be thought of as indexing the different rating events.

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

also watchlistings and outlooks. Events are then defined on the basis of changes in the CCR. In a nutshell, the CCR is equal to the numerical value of a country's actual rating on a predefined scale, adjusted upwards (downwards) if the country is on watch for a future upgrade (downgrade).[10] Importantly, defining events based on changes in the CCR as opposed to changes in a country's actual rating increases the variation that can be exploited for identification (see the discussion in 4.1). However, there are also some caveats associated with that strategy.

First, by design actual rating changes and watchlistings are being mixed despite constituting qualitatively different kinds of events. Second, even if one does not consider this to be a major issue, use of a CCR requires an additional assumption about the relative informational content of actual rating changes and watchlistings. For example, one may choose to treat the announcement of watchlistings as having a lesser impact than actual rating changes by a single notch. This is the road most contributions in the literature have gone down, but there is quite some heterogeneity in exactly how the CCR is adjusted for the announcement of watchlistings, which can substantially impact on the significance of potential spillover effects.[11] Third, if alternatively one were to treat watchlistings like single-notch changes in the actual rating, a considerable number of actual downgrades would be lost as observations. To see this, note the following common case. When a country previously on negative watch is both being downgraded and has its watchlisting retracted, the CCR will remain unchanged as the two rating actions exactly offset each other. Hence, that type of downgrade will no longer count as an event.

In contrast, the size of our sample contains sufficient variation in the severity of upgrades and downgrades alone to stay within the class of actual rating changes only. While this allows us to identify potential spillover effects without additional assumptions on the relative informational content of ratings and watchlistings, we do account for possible anticipation effects by controlling for prior watchlistings using the dummy $OnWatch_{e,t}$.[12]

[10] Similar adjustments are made if the country carries a positive or negative outlook.

[11] For example, in Gande and Parsley (2005), the CCR is adjusted upwards (downwards) by half a notch whenever a country is put on watch for a potential upgrade (downgrade). On the contrary, the study by Ismailescu and Kazemi (2010), which is otherwise closely modelled on the aforementioned, equates the impact of watchlistings to merely a quarter of that of an actual rating change. Alsakka and ap Gwilym (2012) treat watchlistings differently yet again, namely as amounting to two thirds of a notch. Finally, De Santis (2012) codes watchlistings depending on their timing, but their impact can never exceed one half of a notch.

[12] However, we also check whether the aforementioned sample selection and scaling effects are present, which turns out to be the case. To this end, we run several regressions in which events are defined based on changes in CCRs that entail different assumptions on the coding of watchlistings. In these regressions, we then replace the dummy $LARGE_{e,t}$ with a variable capturing the strength of change in the event country's

Finally, it seems important to address a potential concern regarding 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 to other spread-relevant information in the event window. Otherwise, *LARGE* and the error term ω would be correlated, and β would be biased.

More precisely, one might be concerned 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 being 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

CCR. Accordingly, we also drop $OnWatch_{e,t}$ as a control since watchlistings are already built into the CCR. All results are available upon request.

rating changes precipitating significant increases in borrowing costs or outright crises which make CRAs opt for somewhat less timely announcements.

4.3 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 $X_{e,n,t}$. For example, the spillover potential of a rating action might depend on the country's initial rating ($InitRat_{e,t}$). We also include the absolute difference between the event country's initial rating and that of the non-event country ($\Delta InitRat_{e,n,t}$) as one might expect bilateral effects to differ depending on the similarity of countries in this regard.

Moreover, similar announcements by different CRAs tend to cluster around certain dates, in particular for rating downgrades (see 3.2). We account for clustering *within* countries by a variable that captures the number of similar announcements made for a particular country by other agencies over a 14-day window before the 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.

All regressions further include a vector that contains a fixed set of controls, such as event and non-event country dummies.[13] We also account for common time effects across different event windows 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. 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 EMBI Global bond yields, and a dummy for spread changes

[13]Event country dummies capture, for example, that rating changes in relatively remote, isolated countries may have only limited spillover effects.

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

5 Results

We first employ our identification strategy to test for the existence of spillover effects on other countries' sovereign bond markets in the wake of credit rating announcements in 5.1, including a number of robustness checks. Then, conditional on our findings, we investigate potential channels of cross-border spillovers in 5.2.

5.1 Existence of cross-border spillover effects

5.1.1 Baseline results

Table 1 shows baseline estimation results on the existence of cross-border spillover effects for upgrades and downgrades, respectively. We start with a parsimonious specification in Model 1, which only contains the large-change dummy *LARGE* as our main variable of interest and initial ratings. We then control for potential anticipation effects from watch-listings as well as clustering within and across countries in Model 2. Finally, Model 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 2 basis points. In contrast, large upgrades are associated with spread changes that are roughly 1.2 basis points below those of one-notch upgrades. Significance levels for upgrades are also lower despite a larger number of rating events and observations. This asymmetry is in line with the literature discussed in Section 2.

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

Table 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 A.3 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.

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 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 per cent level for downgrades (Model 2 in Panel B). A possible explanation for this is given by Altman and Rijken (2007). They point out that watchlistings partially ease the tension between the market's expectation of rating stability and the 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 per cent of all upgrades. Finally, it has often been noted that there is an incentive to leak good news (e.g., Alsakka and ap Gwilym, 2012; Christopher 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 a 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 3.2 because simultaneous announcements on several countries by one or more CRAs 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 more pronounced when at least one upgrade (downgrade) is announced for the "non-event" country at the same time.

With regard to *SimActsWdwEvt*, which measures the number of upgrades (downgrades) announced by other agencies over a 14-day window before the respective upgrade (downgrade), we again find strong differences in significance between upgrades and downgrades as well as opposing signs.[14] However, one need not necessarily expect within-clustering to have an additional spread-increasing effect over the event window for downgrades. In fact, 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 3.2). Thus, *SimActsWdwEvt* tends

[14]In choosing the window length for *SimActsWdwEvt*, 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. 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 A.6 in the Appendix.

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., 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.

Therefore, we also 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. 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, clustering may reduce the spillover relevance of individual rating events that take place in a period of many similar announcements by other CRAs.

5.1.2 Robustness checks

We also subject our baseline regressions to a number of robustness checks (see Table A.5 in the Appendix). In doing so, we focus on downgrades because these are significantly more relevant from a policy perspective and, as will be shown in 5.2, the findings on upgrades should be taken with a grain of salt.

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 very few severe rating changes (see Figure 4). 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 euro area 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, there might be biases arising from the fact that we pool the announcements of different CRAs, as pointed out by Alsakka and ap Gwilym (2012). 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, we would be picking up differences in the credibility of S&P, Moody's, and Fitch rather than identifying spillover effects across sovereign bond markets. However, Figure A.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. On the other hand, 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 scarcity, S&P's large downgrades might hint at particularly strong deteriorations in a country's creditworthiness and thus incite especially strong reactions as well. Those might therefore 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 4.2 we argued that CRAs do not generally react instantaneously to other spread-relevant information, and that it is even more unlikely that they should "fine-tune" the severity of their rating changes to such information. However, 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 event window. Hence, to 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 non-event countries would also need to widen particularly strongly during the event window. Moreover, *VIX* should already capture some common component of spread developments across countries. We nonetheless 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, but our key finding continues to hold.

[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., Gande and Parsley, 2005; Reisen and von Maltzan, 1999). It is worth emphasising, though, that 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 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. The authors further 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.

[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.

5.2 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 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 policy makers.

We therefore augment our final baseline specification (Model 3 in Table 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 *CapOpenNonEvt*). Finally, we consider the size of the event country's GDP (*SizeEvt*) as well as differences between event and non-event country size (Δ*Size*) and trend growth (Δ*TrendGrowth*). Definitions and sources for all control variables are reported in Table A.3 in the Appendix. The results are shown in Tables 2 and 3.

There is again a notable asymmetry between upgrades and 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 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

Table 2: **Spillover channels, upgrades**

	(1)	(2)	(3)	(4)	(5)	(6)
LARGE	-0.0128*	-0.0111	-0.0094	-0.0117*	-0.0142**	-0.0115*
	(0.0067)	(0.0071)	(0.0071)	(0.0068)	(0.0066)	(0.0069
InitRat	0.0001	-0.0005	0.0012	0.0027**	0.0031***	0.0032**
	(0.0010)	(0.0010)	(0.0012)	(0.0013)	(0.0012)	(0.0014)
Δ*InitRat*	0.0010	0.0006	0.0006	0.0012*	0.0011	0.0008
	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0008)
OnWatch	0.0070	0.0066	0.0065	0.0080	0.0085	0.0072
	(0.0058)	(0.0060)	(0.0061)	(0.0059)	(0.0061)	(0.0063)
SimActsWdwEvt	-0.0013	-0.0058	-0.0071	-0.0026	-0.0032	-0.0090
	(0.0057)	(0.0059)	(0.0060)	(0.0058)	(0.0059)	(0.0062)
SimActsDayNonEvt	-0.0903	-0.1024	-0.1059*	-0.0883	-0.0950	-0.1128*
	(0.0549)	(0.0625)	(0.0642)	(0.0546)	(0.0578)	(0.0681)
VIX	0.0017***	0.0019***	0.0018***	0.0017***	0.0018***	0.0019***
	(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)
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	27,962	27,627	29,329	28,904	27,050
Event countries	92	90	89	92	91	88
Non-event countries	73	71	70	72	72	70
Upgrades	595	582	577	592	584	566
R^2	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. For variable definitions, see Table A.3 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.

from the start (e.g., 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

Table 3: **Spillover channels, downgrades**

	(1)	(2)	(3)	(4)	(5)	(6)
LARGE	0.0206***	0.0217***	0.0231***	0.0222***	0.0224***	0.0244***
	(0.0066)	(0.0069)	(0.0069)	(0.0070)	(0.0070)	(0.0073)
InitRat	-0.0006	-0.0010	-0.0014	-0.0017	-0.0017	-0.0031
	(0.0017)	(0.0018)	(0.0018)	(0.0019)	(0.0019)	(0.0021)
Δ*InitRat*	0.0012	0.0017*	0.0015	0.0008	0.0008	0.0013
	(0.0009)	(0.0010)	(0.0011)	(0.0010)	(0.0010)	(0.0011)
OnWatch	-0.0046	-0.0031	-0.0042	-0.0009	-0.0008	-0.0003
	(0.0054)	(0.0058)	(0.0058)	(0.0056)	(0.0057)	(0.0059)
SimActsWdwEvt	0.0141**	0.0135**	0.0137**	0.0146**	0.0146**	0.0141**
	(0.0065)	(0.0066)	(0.0067)	(0.0067)	(0.0067)	(0.0069)
SimActsDayNonEvt	0.1451**	0.1426**	0.1170*	0.1160*	0.1161*	0.1136*
	(0.0643)	(0.0653)	(0.0610)	(0.0623)	(0.0623)	(0.0619)
VIX	0.0006*	0.0006	0.0006	0.0006*	0.0006*	0.0005
	(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)
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	20,633	20,352	21,031	20,885	19,724
Event countries	84	81	80	82	82	79
Non-event countries	73	71	70	72	72	70
Downgrades	427	416	414	416	416	405
R^2	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. For variable definitions, see Table A.3 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.

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. Therefore, in the interest of comparability and as an important control, we retain *Region* in all specifications.

The two trade controls, 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. They point 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 in one case (see Model 6 in Table 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]

Besides 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 loss of observations and major selection effects along the time series and country dimensions, which 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, however, 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 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 the 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 are more likely to be held by foreign investors, this result is highly intuitive.

[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 A.7 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 (e.g., Goldberg and Saunders, 1980, 1981; Brealey and Kaplanis, 1996; Yamori, 1998).

[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., Frankel et al., 2013; Fratzscher, 2012; Hale and Spiegel, 2012).

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 augmented baseline regression (Model 1 in Table 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. 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 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.[20] While the effect appears to be relatively small, its direction is still interesting, in particular considering the fact that, across the whole sample, larger and more highly rated countries induce smaller spillovers (Models 4 to 6 in Table 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.

[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.

5.3 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 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, appears relatively ambiguous.

We therefore conclude that there is a strong case for the notion that negative sovereign rating announcements do matter in inducing spillovers across markets. 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 policy makers would be concerned about. This 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 difficulty of cleanly identifying the "base effects" (see the discussion in 4.1). 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 at the time of the downgrade announcements in our sample is 325 basis points. While the total effect of downgrades is relatively small in comparison, 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 policy maker'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. Crucially, such behaviour on the part of investors would likely extend to their reactions to news other than rating announcements. Thus, the potential problem would seem to be much more general and rooted in investor behaviour above all. Hence, it is not clear that putting the primary emphasis on CRAs will prove effective in this regard.

6 Conclusion

Concerns about negative spillovers across sovereign debt markets in the wake of sovereign rating changes have recently resurfaced on the agenda of policy makers. 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 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 policy makers' 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.

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Appendix

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

Characterisation of debt and issuer		Letter rating: S&P	Letter rating: Moody's	Letter rating: Fitch	Linear transformation
Highest quality	Investment grade	AAA	Aaa	AAA	17
High quality		AA+	Aa1	AA+	16
		AAA	Aa2	AA	15
		AA–	Aa3	AA–	14
Strong payment capacity		A+	A1	A+	13
		A	A2	A	12
		A–	A3	A–	11
Adequate payment capacity		BBB+	Baa1	BBB+	10
		BBB	Baa2	BBB	9
		BBB–	Baa3	BBB–	8
Likely to fulfil obligations, ongoing uncertainty	Speculative grade	BB+	Ba1	BB+	7
		BB	Ba2	BB	6
		BB–	Ba3	BB–	5
High credit risk		B+	B1	B+	4
		B	B2	B	3
		B–	B3	B–	2
Very high credit risk		CCC+	Caa1	CCC+	
		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 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 A.3: **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 Treasury Department
LARGE	Dummy variable taking on a value of one for "large" rating changes of two notches or more; zero otherwise. Notches are measured on a linear 17-notch scale.	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.	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.	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; zero otherwise. Also see Table A.4.	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 A.4: **Regions**

Region	Countries
Caribbean	Bahamas, Barbados, Dominican Republic, Grenada, Jamaica, Trinidad and Tobago
Central & Southwestern Asia	Armenia, Azerbaijan, Georgia, Kazakhstan, Turkmenistan
Central America	Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, Panama
Central Europe	Czech Republic, Hungary, Poland, Slovak Republic, Slovenia
Eastern Asia	China, Hong Kong, Japan, Macao, South Korea, Taiwan
Eastern Europe	Belarus, Estonia, Latvia, Lithuania, Moldova, Ukraine
Middle East	Bahrain, Cyprus, Iran, Iraq, Israel, Jordan, Kuwait, Lebanon, Oman, Qatar, Saudi Arabia
North America	Bermuda, Canada, Mexico, United States
Northern Africa	Egypt, Libya, Morocco, Tunisia
Northern Asia	Mongolia, Russia
Northern Europe	Denmark, Finland, Iceland, Norway, Sweden
Oceania	Australia, Cook Islands, Fiji, Micronesia, New Zealand, Papua New Guinea
South America	Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, Suriname, Uruguay, Venezuela
Southeastern Asia	Cambodia, Indonesia, Malaysia, Philippines, Singapore, Thailand, Vietnam
Southeastern Europe	Bosnia and Herzegovina, Bulgaria, Croatia, Macedonia, Montenegro, Romania, Serbia, Turkey
Southern Asia	India, Pakistan, Sri Lanka
Southern Europe	Andorra, Greece, Italy, Malta, Portugal, San Marino, Spain
Sub-Saharan Africa	Angola, Benin, Botswana, Cameroon, Côte d’Ivoire, Gabon, Gambia, Ghana, Kenya, Lesotho, Madagascar, Malawi, Mozambique, Namibia, Nigeria, Rwanda, Senegal, Seychelles, South Africa
Western Europe	Austria, Belgium, France, Germany, Guernsey, Ireland, Isle of Man, Netherlands, Switzerland, United Kingdom

Table A.5: **Baseline regressions, downgrades — Robustness checks I**

	Baseline	Ntchs < 4	Ntchs < 3	Crises	S&P effect	Endg. dgs
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)		
S&P $\times$ *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 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 (Ntchs < 4, Ntchs < 3). Moreover, to check that the results are not solely due to the main crisis episodes over the sample period, namely the euro area 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 S&P, to test whether this agency's relatively infrequent large downgrades (see Figure A.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 (Endg. dgs).

Table A.6: **Baseline regressions, downgrades — Robustness checks II**

	Baseline	Window length: Seven days	Window length: 21 days	Same day actions	
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)
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

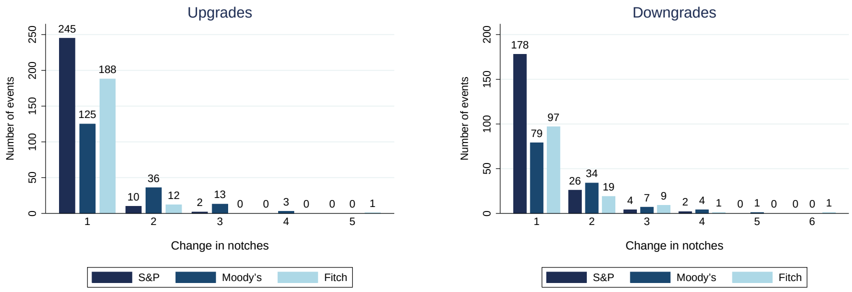
Notes — This table shows the robustness of our baseline results with regard to variables on clustering and anticipation. For purposes of comparison, the first column reports the results from the full baseline specification for downgrades (see Panel B, column (3) in Table 1). The second and third columns 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 A.7: **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 (6) in Table 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 A.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. 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.

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