Unsolicited Sovereign Ratings and The Sovereign-Bank Ceiling: An Unintended Consequence of Regulatory Disclosure

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Abstract

This paper integrates three themes on regulation, unsolicited credit ratings, and the sovereign-bank rating ceiling. We reveal an unintended consequence of the EU rating agency disclosure rules upon rating changes, using data for S&P-rated banks in 44 countries between 2006 and 2013. The disclosure of sovereign solicitation status for 13 countries in February 2011 has an adverse effect on the ratings of intermediaries operating in these countries. The unsolicited sovereign rating status transmits risk to banks via the rating channel. The results suggest that banks bear a penalty for the solicitation status of their host sovereign's ratings, thus revealing an unintended and adverse impact of EU regulation.

Keywords: rating agency regulation, unsolicited ratings, sovereign-bank rating channel.

Credit Rating Agencies (CRAs) play a prominent role in modern financial markets. Globalization and the increasing complexity of investment products have triggered a growing demand for widely recognised risk assessment. Sovereign ratings serve as a basis for evaluating the creditworthiness of a country, and thereby influence long-term investment and lending decisions across borders. Rating downgrades have major implications for financial markets and institutions, including rising costs of credit and hindered market access (e.g. BIS 2011).

The global financial crisis brought CRAs renewed publicity and ongoing scrutiny by regulators. CRAs were blamed for worsening economic conditions by downgrading some sovereigns too quickly and too severely. The overreliance on ratings by market participants led to cliff effects where downgrades had a disproportionate effect. The situation in Europe has further emphasised the hazardous effects of negative spillovers while highlighting interconnectedness between international financial institutions (e.g. Arezki et al. 2011). The influence of ratings on global financial stability has become a major concern.

In 2009, the European Commission (EC) implemented a new set of regulations aimed at CRAs including registration procedures, governance requirements, internal controls, disclosure rules and improvements in rating methodologies (CRA I Regulation).[1] This regulation was amended in May 2011 (CRA II Regulation) and in November 2011 (CRA III Regulation). The responsibility for supervising and certifying CRAs was handed to the European Securities and Markets Authority (ESMA) in July 2011. This paper draws attention to the disclosure rules with particular focus on Article 10 (5) of the EU Regulation 1060/2009, which requires that when a CRA issues an unsolicited rating,[2] it needs to be identified as such. As a result of implementing the Article in February 2011, Standard and Poor's Corporation (S&P) disclosed

1. Regulation (EC) No 1060/2009 of the European Parliament and of the Council of 16 September 2009 on credit rating agencies.

2. A solicited rating is a credit rating requested by the issuer who incurs the cost of the appraisal. An unanticipated (by the issuer) assessment by the CRA using public information about the issuer is known as an unsolicited rating.

the conversion to unsolicited status of 14 of its rated sovereign governments (S&P 2011). Unsolicited ratings are one of the most controversial features of the CRA business. Prior literature (e.g. Poon et al. 2009; Bannier et al. 2010; Van Roy 2013) finds that banks and corporations rated on an unsolicited basis have significantly lower ratings. Concerns exist that unsolicited ratings are biased downward because CRAs are not compensated for their service. Additionally, policymakers have focused on this feature, because both solicited and unsolicited ratings are permitted for regulatory purposes.

The broad aim of this paper is to examine whether the disclosure rule on solicitation status achieves its objective of more transparent and credible rating services and whether it has unintended consequences. Specifically, we investigate whether conversion to sovereign unsolicited rating status (induced by regulation) results in lower bank ratings (in the re-designated unsolicited sovereign states). Previous literature on the controversies related to unsolicited (non-sovereign) ratings provides a theoretical framework (see Section 1.2). Additionally, it is well known that sovereign risk spills over to financial institutions through many channels (BIS 2011; De Bruyckere et al. 2013; Alsakka et al. 2014; Correa et al. 2014). Studying the means through which the mandatory disclosed unsolicited status of sovereign ratings transmits risk to banks is a key motivation for this research.

The novelty of this study derives from building on three streams of research, which meaningfully overlap and result in a synergy which has not been previously explored. The first theme relates to the unique opportunity to investigate the dynamics of rating solicitation for sovereigns. To our knowledge, no prior study has investigated the rationale and impact of rating solicitation status for sovereigns. The existing literature concentrates on the solicitation of corporate and bank ratings (Poon 2003; Poon et al. 2009; Bannier et al. 2010; Van Roy 2013) yet it does not include any study of solicitation changes.[3] The second theme relates to the

3. S&P (2011) refers to a set of sovereign ratings being "converted to unsolicited ratings" in February 2011.

impact of sovereign ratings on bank ratings through the rating channel. This aspect of the paper builds on recent work (Alsakka et al. 2014; Huang and Shen 2015) while adding a new dimension to the type of constraints imposed by sovereigns on banks via rating ceilings.

Thirdly, this paper considers the influence of the recent EU CRA regulation. To the best of our knowledge, there is no published empirical work on the effects of enhanced disclosure by CRAs introduced since 2009. Jorion et al. (2005) study the effect of U.S. Regulation Fair Disclosure (Reg FD), introduced in 2000, and find that both positive and negative rating changes have a stronger informational effect on stock prices after the Reg FD took effect. Poon and Evans (2013) find that the impact of rating downgrades on bond yield premia (after Reg FD) depends on the size of the firm. Studies on other forms of regulation relate to periods prior to the EU CRA regulation (e.g. Becker and Milbourn (2011) utilize a U.S. sample from 1995 to 2006). Additional uniqueness arises from the methodology which uses a quasi experimental design to capture the effects of EU regulation.

The paper uses a large sample of 152 banks rated by S&P incorporated in 44 countries in Europe, Asia-Pacific and Latin America for the period between 2006 and 2013. We apply an ordered probit model and difference-in-difference (DID) estimation, along with many robustness checks including placebo tests and matching exercises. We strongly endeavour to rule out the possibility of sample selection bias or that the observed phenomenon arises from events other than the adoption of EU disclosure rules for CRAs.

The results strongly suggest that disclosure of unsolicited sovereign status adversely influences bank ratings through the rating channel. Banks in countries converted to unsolicited status are more likely to be downgraded and less likely to be upgraded compared with banks in sovereigns which retained solicited ratings at all times. The marginal effects (MEs) analysis suggests that the former banks are 2.21%, 0.99% and 0.65% more likely to be downgraded by

1, 2 and ≥ 3 Comprehensive Credit Rating (CCR) points respectively.[4] The significance of the MEs should be considered in relation to the total number of bank (sovereign) rating downgrades which represent 3.31% (2.66%) of all observations. Additionally, the analysis confirms a strong ceiling effect between sovereigns and banks.

These findings have clear policy implications for regulators and banks, since there are potential costs to the institutions and the wider economies through this rating ceiling effect. The phenomenon arises directly from the introduction of new EU CRA Regulation, therefore represents an unintended consequence of regulation, and suggests a need for greater awareness of CRA rating policies in designing future regulation. Policymakers should take a closer look at unsolicited sovereign ratings and their implications. The findings of this study reveal an undesirable impact of recent regulatory developments on the European economy and will be informative in shaping future proposals.

The paper is structured as follows. Section 1 draws from prior theoretical and empirical literature to frame the research questions and the testable hypothesis. The data and descriptive statistics are discussed in Section 2. Section 3 discusses research methodologies, Sections 4 and 5 present the empirical results and Section 6 concludes the study.

1. LITERATURE REVIEW AND HYPOTHESIS DEVELOPMENT

1.1 Spillover Channels

In studying the close relationship between sovereign and bank ratings, we are interested in whether the rating solicitation status of the government has an effect on bank ratings. The literature suggests a possible contagion which feeds from the sovereign sector to other asset classes (e.g. Arezki et al. 2011; Ehrmann et al. 2011). Although the sovereign rating ceiling technically no longer exists, there is evidence that sovereign ratings strongly affect the ratings

4. These figures relate to outlook action, watch event, and downgrade by one notch or more.

of non-sovereigns (Borensztein et al. 2013; Huang and Shen 2015), and banks very rarely pierce the sovereign ceiling.

The spillover between sovereigns and banks, affecting the latter's costs and funding opportunities, is known to transmit through four main channels: (i) asset holdings, (ii) collateral, (iii) government guarantees and (iv) ratings. Firstly, when banks hold sovereign debt they are faced with a loss in balance sheet value and overall profitability while funding becomes more expensive if the sovereign risk increases (BIS 2011; Arezki et al. 2011; De Bruyckere et al. 2013). Secondly, higher sovereign risk results in lower value of collateral available to banks when negotiating costs of funds e.g. with the central bank (Sy 2009; BIS 2011; De Bruyckere et al. 2013; Correa et al. 2014). Thirdly, any reduced creditworthiness of the sovereign lessens the funding opportunities for banks arising from implicit and explicit government guarantees. A weakened government position undermines the credibility of any support for banks (BIS 2011; De Bruyckere et al. 2013).

The last channel relates to the fact that lower ratings of sovereigns are found to translate directly into lower bank ratings in that country (Alsakka et al. 2014; Huang and Shen 2015). The spillover is known to occur for two reasons. Firstly, the lower sovereign ratings affect the cost of debt and equity funding. Secondly, the ceiling effect arises because sovereigns have greater resources and policies at their disposal which mean that a higher non-sovereign rating is rarely justifiable. Borensztein et al. (2013) suggest that sovereign risk transmits onto non-sovereign issuers via the capital and other administrative controls and restrictive measures available to the government. Prohibitions against inflow and outflow of investment into the country (transfer and convertibility risk) restrain companies from repaying their external debt when the government reaches default or near default. In such a relationship, the non-sovereign debt always defaults when the state defaults, as it cannot access currency or transfer its funds outside the borders. Duggar et al. (2009) identify that 71% of corporate defaults occur at a time of sovereign default. Fitch (2012) suggest that sovereign actions such as altered regulated tariffs, deposit freezes, penalty taxation or expropriation are other reasons which justify the sovereign ceiling.

Additional channels are highlighted in the literature. For instance, European banks tend to participate in cross-holding claims of other intermediaries across countries, and thereby become exposed to one another (Arezki et al. 2011; Ehrmann et al. 2011). Other channels include banking regulation, CDS contracts, and investment mandates (Sy 2009).

1.2 Theories of Deflated Unsolicited Ratings

Theoretical insights on deflated unsolicited ratings arise under three main concepts: (i) self-selection bias, (ii) strategic conservatism, and (iii) blackmail theory.[5,6]

Self-selection bias indicates that entities with unsolicited ratings who wish to convey a message that their creditworthiness is in fact better than stated will request solicited ratings. Once the rating improves, such entities benefit from a lower cost of capital. The overall reduction in cost explains the willingness of firms to incur fees for solicitation. Conversely, issuers which are aware of their weak creditworthiness, do not request and pay for a solicited rating (Fulghieri et al. 2014). Consequently, low quality issuers remain with their (relatively low) unsolicited ratings. Self-selection is thereby predicted to assist in reaching the most adequate credit appraisal for issuers regardless of the solicitation status. In our context, one could argue that a sovereign expecting a future rating downgrade would be more relaxed about an impending conversion to unsolicited status (e.g. not wishing to pay fees to a CRA if they consider a lower rating to be inevitable).

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5. A fourth concept is the geographical discrimination theory. Li et al. (2006) conclude that raters outside Japan (Moody's and S&P) do not reflect the keiretsu affiliation status. However, this is not applicable in this paper.

6. In contrast, Byoun et al. (2014) find no evidence of deflated unsolicited ratings. Based on the long-run stock market performance of Japanese firms, they find that the release of new unsolicited ratings leads to a negative long-run stock performance whereas announcements of new solicited ratings have an insignificant effect. Their argument suggests that there is no bias between the two types of ratings since the information conveyed is different.

The exact opposite of this premise arises under the strategic conservatism theory. Bannier et al. (2010) suggest that unsolicited ratings might be driven by "*by agencies' strategic considerations in the rating process" (p.264).* When CRAs lack inside information about the issuer, they might prefer to rate the issuer "too low" rather than "too favourably". These authors argue that issuers who share the same creditworthiness can be assigned different ratings, based on different solicitation status. Those who do not mandate for ratings receive a (lower) unsolicited rating whereas those who purchase a rating obtain a (higher) solicited rating. Likewise, the same rating level assigned to both solicited and unsolicited borrowers conveys a message that the unsolicited issuer is in fact less risky than implied by its rating. However, this does not assist a non-sovereign with solicited ratings in our context. In a scenario where an issuer switches to unsolicited status, the CRA loses access to private information and might therefore decide to rate lower after the switch in order to ensure conservatism. In addition, prior literature has not considered how this effect may proceed under the sovereign-bank ceiling e.g. a bank paying for its solicited rating might be downgraded due to the sovereign's decision-making in opting for an unsolicited rating.

The blackmail theory assumes that CRAs might persuade issuers to purchase ratings, otherwise threatening them (indirectly) by releasing disproportionately low unsolicited ratings. The rationale suggests that when the issuer is not transparent and does not disclose information, the risk assessment is difficult to perform and therefore downward biased ratings are not prone to being questioned by market participants (Van Roy 2013). Ramakrishnan and Thakor (1984) stipulate that blackmail is not a tenable position for the CRAs, since their reputational capital plays a more important role than any short-term financial gains. However, Bar-Isaac and Shapiro (2013) and Opp et al. (2013) argue that the reputational concerns of CRAs change over the business cycle. CRAs increase their ratings quality in low points of the economic cycle and relax them during booms.

1.3 Hypothesis

Two streams of literature discussed above provide a potential explanation for differentials between ratings of banks incorporated in solicited versus unsolicited sovereigns. Firstly, the theories of downward biased ratings for unsolicited non-sovereigns provide reasons to believe that the solicitation status of sovereigns will impact their credit ratings. Secondly, the evidence that bank ratings are influenced by sovereigns through the rating channel (i.e. BIS 2011; Alsakka et al. 2014) might lead the sovereign's solicitation status to become a concern for banks. By interacting two observable phenomena we propose:

Hypothesis: *Bank ratings are more likely to be downgraded in countries whose sovereign rating status is converted to 'unsolicited'.*

Such an effect has a negative impact on the funding costs of banks in that country. Investigation of the interplay between sovereigns and banks in this setting poses challenges in interpretation of the competing theories of unsolicited ratings (in Section 1.2). There is no literature (theoretical or empirical) which examines the issue of solicitation of sovereigns, not to mention the dynamics of any conversion in status. Individual governments do not reveal their rating subscription details in a public arena and it is difficult to deduce whether self-selection plays a dominant role in determining sovereign ratings. On the other hand, sovereigns are relatively transparent in terms of their liability structures, unlike banks. Despite this, when a sovereign converts its solicitation status, the CRA might perceive a deficiency of soft information and start rating the sovereign more conservatively. The blackmail theory could offer a plausible explanation in a case where the issuer is less transparent (Van Roy 2013). However, it is unlikely that a CRA providing services not only to a sovereign but also to a number of non-sovereigns in that constituency would threaten its current or potential clientele without genuine concern about harming its reputation (Ramakrishnan and Thakor 1984).

2. SAMPLE AND DATA SOURCES

2.1 Dataset

The sample comprises monthly long term foreign-currency ratings for banks and sovereigns rated by S&P between January 2006 and January 2013. The rating solicitation status for all sovereigns is obtained from S&P Global Credit Portal publications. In February 2011, S&P released reports on conversions of solicitation status (disclosing the 'unsolicited' rating status) for: 1) seven European countries (Belgium, France, Germany, Italy, Netherlands, Switzerland, United Kingdom); 2) six Asia-Pacific countries (Australia, Cambodia, India, Japan, Singapore, Taiwan); and 3) U.S.A. (see S&P 2011). Argentina's solicitation status was converted on the 4^{th}April 2011 and was the final case arising from the regulatory requirements on disclosure. U.S.A. is excluded from the reported results due to it having a high proportion (approx. 20%) of all S&P-rated financial institutions, which would distort the sample and dominate any evidence on the research question.[7] Cambodia has only one S&P-rated bank, and is therefore excluded from the sample.[8]

Bank rating data is obtained from the Interactive Data Credit Ratings International (CRI) database. The sample only comprises financial institutions because there is a far stronger link between sovereigns and banks than between sovereigns and corporations (see Borensztein et al. 2013; Huang and Shen 2015). For example, corporates do not use sovereign bonds as collateral and for this reason are not equally affected by sovereign rating fluctuations. In addition, for most countries, banks are typically much more likely than corporations to be rated

7. Initially, the pre-matched data presented 489 S&P-rated US banks with 2284 banks distributed among the remaining 44 countries. Since U.S.A. is in the group of sovereigns whose ratings were converted to unsolicited by S&P, the ratio between sovereign/bank ratings (in our sample 13/79) would be unreasonably inflated in the denominator. With one extra sovereign but a significantly higher number of banks for that one country, any results would be driven by the US case. This is especially problematic because most US banks are rated several notches below the sovereign rating, and hence the sovereign-bank ceiling effect is far more muted for US banks than in other countries.

8. Also, S&P's rating on Cambodia was withdrawn in September 2014.

at the sovereign ceiling. The sample is narrowed by matching the credit rating data with the (annual frequency) financial and accounting statistics of publicly listed intermediaries available from Bankscope. Data on the macroeconomic environment (at annual frequency), such as the Consumer Price Index (CPI) is obtained from DataStream while countries' income level classifications are sourced from the World Bank.

The sample contains 152 S&P-rated listed banks in 44 countries. This includes 13 sovereigns which were converted from solicited to unsolicited (hereafter referred to as 'unsolicited') and 31 sovereigns whose S&P ratings remain solicited throughout the sample period. The sovereign and bank coverage is available in Table 1.

2.2 Rating Events

Rating events are identified using a comprehensive credit rating scale (CCR-58 point scale) which includes rating, watch and outlook status. Much recent literature identifies the importance of watch and outlook (e.g. Kim and Wu 2011; Alsakka and ap Gwilym 2013). Rating classes are assigned values from 1 to 58 such that: AAA = 58, AA+ = 55, …, CCC = 7, CCC- = 4, C/SD/CC/D = 1. For positive watch we add +2, for positive outlook +1, whereas for negative outlook and negative watch we subtract 1 and 2, respectively. Types of events include rating change events (positive or negative) with no change in outlook or watch status. 'Solo' outlook or watch signals (positive or negative) are when no rating events occur. Additionally, combined events (positive or negative) involve a rating change accompanied by either a watch signal or outlook signal. A change from negative watch to negative outlook or from positive watch to positive outlook without simultaneous rating change is also an event on the 58 point scale.

Panel A of Table 2 reports the descriptive statistics for the monthly bank credit events. The total number of monthly observations is 11101, where 7708 (3393) are observed pre (post) the regulation-induced disclosure of solicitation status (March 2011).[9] In summary, there are 659 events with 292 positive and 367 negative actions. The positive (negative) events in the pre-event period amount to 2.06% (1.9%) of monthly observations. After solicitation disclosure rules were implemented by S&P, positive (negative) events for banks summed to 0.57% (1.4%) of the sample observations. Data from the entire sample period suggests that 478 (72%) of events relate to investment-grade ratings, whereas 181 (28%) are for speculative-grade ratings.

Panel B of Table 2 presents the sovereign events. In total, the qualifying sovereigns faced 173 rating events; 81 positive and 92 negative. The pre-regulatory phase yields positive and negative credit events in similar proportions (59 positive versus 53 negative). They are distributed relatively evenly among the two groups of countries in the sample. Namely, sovereigns with ratings disclosed as unsolicited after February 2011 yield 11 positive versus 10 negative events whereas countries which remain solicited after that date have 48 positive versus 43 negative events. In contrast, the post-regulatory phase suggests deteriorating ratings for the former group with 3 upgrades and 15 downgrades.

The descriptive statistics identify a strong ceiling effect which is observed for 97.3% of sample observations, 79% with bank ratings lower than sovereign ratings (B<S) and 18% with bank ratings equal to sovereign ratings (B=S), while bank ratings pierce the ceiling only in 2.7% of cases (B>S). Sovereigns are assigned an average numerical rating of 44.5 (approx. A+) with banks at 36.7 (approx. BBB+). Both bank and sovereign ratings are lower after March 2011, on average. The banks' mean rating drops from 37.5 (approx. BBB+) to 34.5 (approx. BBB), whereas sovereigns' mean ratings decline from 45.4 (approx. A+) to 42.5 (approx. A).

Fig.1 plots annual sovereign ratings against bank ratings per country at the aggregated level. Fig.1 illustrates trends between sovereign and bank ratings for a sample of four countries which

9. In February 2011, S&P disclosed the unsolicited status on 14 of its sovereign issuers. Because of the monthly data frequency, March 2011 onwards is the post-treatment period.

switch to unsolicited ratings. Bank ratings in this group rarely exceed those of the sovereign issuer. In the cases of Belgium, France, Germany and Italy, both sovereign and bank ratings show a substantial decline in the first quarter of 2011 (the time of solicitation status disclosure).[10]

3. METHODOLOGIES

The studied phenomenon of a regulatory change provides grounds for applying a quasi-experimental research design such as difference-in-difference (DID) estimation. The methodology allows capturing the exogenous shock in the sample (effect of disclosure rules) and its effect on the variable of interest (bank ratings). However, the DID method relies on the linearity assumption and since the dependent variable is of a discrete nature, one could fall into censoring issues. To avoid this problem, the ordered probit model is applied in the main regressions and the linear probability model is studied in robustness tests. In the DID framework, the assumption that the shock induced by the European regulation is exogenous is fundamental for proper estimation of its effect on the dependent variable (Roberts and Whited 2011). Additionally, the parallel trend assumption dictates that, in the absence of the treatment in the form of disclosure rules, the pre-treatment difference between treatment and control group is constant over time. The treatment group comprises 13 sovereigns whose solicitation status was converted to 'unsolicited', whereas the control group uses the 31 sovereigns whose S&P ratings remained solicited throughout the sample period (see Table 1).

3.1 Univariate Tests

To examine whether the data complies with the parallel trend assumption, required for proper estimation in the multivariate analysis, we test for differences in the financial profiles

10. The timing of this effect is not coincidental with the downgrades of countries such as Portugal, Ireland, Greece or Spain, associated with events in the European debt crisis. See Section 5 for formal robustness tests relating to this point.

of both groups. To verify this, we generate t-statistics of means for a number of covariates. These control variables are selected in line with prior research highlighting determinants of bank and sovereign ratings and are also used in the multivariate analysis (Poon 2003; Hau et al. 2013). The summary statistics, abbreviations and definitions of variables used appear in Table 3.

Following Poon (2003), when the covariate is an ordinal variable, we apply the Mann-Whitney U test. To reduce the frequency of “0” in the sample, the lower frequency (annual) data is used in this exercise. The examples of covariates include change in bank ratings, banks’ mean ratings and financial variables and ratios. The null hypothesis is rejected for 5 out of 10 (6 out of 10) covariates in the pre-regulatory (entire) sample period. This suggests banks incorporated in the two groups of sovereigns have distinctive characteristics and are not balanced groups (see Table 4). In the pre-treatment period, banks in the treatment group are characterised by lower net interest margins, returns on equity and lower loan loss reserves. Their mean bank ratings are higher than those of the control group on average. Assets of these banks feature less impaired loans and they hold higher percentages of capital. According to the mean population comparison, banks in both groups are not different in terms of net interest revenues, the interbank ratio, equity to total assets and changes in bank ratings. To correct for the differences in profiles across banks, we conduct a more detailed paired subsample test later in the paper (see multivariate analysis in Section 5.2).

3.2 Ordered Probit Model

To test the hypothesis from Section 1.3, the ordered probit framework is employed. The methodology is widely used in the credit ratings literature because it accounts for the ordinal nature of the dependent variable. Equation (1) captures the effect of an external and exogenous event (disclosure rules) which feeds through sovereign ratings and to the banks in the considered countries, as follows:

$$\Delta y^*_{i,j,t} = \beta_1(Post * Treatment)_{j,t} + \beta_2 \Delta SovR_{j,s} + \beta_3 BankR_{j,t} + \beta_4 X_{j,t} + \lambda CF * \gamma YF + \varepsilon_{i,j,t}$$

$$\varepsilon_{i,j,t} \sim N(0,1) \tag{1}$$

$\Delta y^*_{i,j,t}$ is an unobserved latent variable connected to the ordinal responses of $\Delta y_{i,j,t}$; change in rating of bank i in country j at month t based on the 58-point CCR scale and taking values of -3, -2, -1, 0, 1, 2, 3, by the measurement model:

$$\Delta y_{i,j,t} = \begin{bmatrix} -3\ (i.e.\ bank\ rating\ downgrade\ by\ 3\ or\ more\ CCR\ points) if\ \Delta y^*_{i,j,t} \leqslant \alpha 1 \\ -2\ (i.e.\ bank\ rating\ downgrade\ by\ 2\ CCR\ points) if\ \alpha 1 < \Delta y^*_{i,j,t} \leqslant \alpha 2 \\ -1\ (i.e.\ bank\ rating\ downgrade\ by\ 1\ CCR\ point) if\ \alpha 2 < \Delta y^*_{i,j,t} \leqslant \alpha 3 \\ 0\ (i.e.\ no\ bank\ rating\ change) if \alpha 3 < \Delta y^*_{i,j,t} \leqslant \alpha 4 \\ 1\ (i.e.\ bank\ rating\ upgrade\ by\ 1\ CCR\ point) if\ \alpha 4 < \Delta y^*_{i,j,t} \leqslant \alpha 5 \\ 2\ (i.e.\ bank\ rating\ upgrade\ by\ 2\ CCR\ points) if\ \alpha 5 < \Delta y^*_{i,j,t} \leqslant \alpha 6 \\ 3\ (i.e.\ bank\ rating\ upgrade\ by\ 3\ or\ more\ CCR\ points) if\ \alpha 6 < \Delta y^*_{i,j,t} \end{bmatrix}$$

The (β, λ, γ) parameters of the regression as well as thresholds (α) are estimated using maximum likelihood (ML) estimation and are subject to the constraint $\alpha_1 < \alpha_2 < \ldots < \alpha_J$.

Treatment is a dummy variable taking the value of 1 if the country belongs to the treatment group; 0 otherwise.

Post is a dummy variable taking the value of 1 if the observation is from the post-treatment period (March 2011 onwards); 0 otherwise.

This main interaction dummy (Post*Treatment) captures the impact of disclosure rules in the regression model. In line with the theoretical explanations of deflated unsolicited ratings (see Section 1.2) and economic intuition, the expected sign of this variable is negative. This is consistent with the notion that bank ratings in countries with unsolicited ratings might face more downgrades (and fewer upgrades) than the banks in the other group due to the rating ceiling effect.

$\Delta SovR_{j,s}$ represents the change in sovereign CCR by S&P based on the 3-month window prior to month t (i.e. s = the t-3 to t window). It takes the value of -3, -2, -1, 0, 1, 2 or 3. The predicted sign of the coefficient is positive since bank ratings have the tendency to move in the same direction as ratings of their home sovereign issuers (e.g. Huang and Shen 2015).

$BankR_{j,t}$ represents banks' CCR taking values 1-58. This controls for the banking environment in the regression. The variable is expected to have a positive sign given that higher bank ratings result in higher probability of bank upgrades and lower probability of bank downgrades and vice versa.

$X_{j,t}$ is the set of control variables relating to bank characteristics (see points 1-6 in Table 3).

CF is the country fixed effect; a full set of country dummy variables.

YF is the year fixed effect; a full set of year dummy variables.

Further, we calculate the marginal effects (MEs) to estimate the economic significance of each independent variable on the probability of bank rating changes.

4. EMPIRICAL RESULTS

4.1 Baseline Model

Table 5 presents the results of Eq. (1). We discuss Model I initially. To account for unobserved differences in the economic development, industrialisation level or geographical bias concerning sovereigns, Model I includes year-country dummies. Interacting fixed effects became more common practice in the recent empirical literature (e.g. Jiménez et al. 2012). This approach enables us to control for possible omitted variable bias which could result in endogeneity issues (Lemmon and Roberts 2010). The interaction term accounts for any variation across the time and country spectrum, and controls for the differences in development level of sample countries. The identification of macroeconomic conditions derives entirely from the interactions, in line with Thompson (2011) and Jiménez et al. (2012) who suggest that when the fixed effects are used, one needs to drop the macroeconomic covariates from the regression because they become collinear with the dummy variables.

The coefficient of Post*Treatment is significant with negative sign implying that the conversion of the sovereign status to unsolicited leads to higher probability of bank downgrades and lower probability of upgrades. Hence, *ceteris paribus*, banks which belong to the treatment group are more likely to be downgraded and less likely to be upgraded compared with banks not in the treatment group. The marginal effects analysis suggests that such banks are 1.5%, 0.76% and 0.64% more likely to be downgraded by 1, 2 and ≥ 3 CCR points respectively (see Table 6). The effect of the treatment dummy standalone represents a strong marginal effect in comparison with the 3.31% (2.66%) of negative bank rating (sovereign) events recorded in the entire data sample (see Table 2).

The estimated coefficients of $\Delta\mathrm{SovR}_{\mathrm{j,t}}$ and $BankR_{j,t}$ are significant and economically relevant. The sign on both coefficients is in line with the predictions and remains robust to inclusion of bank and/or monthly fixed effects. The positive sign on the sovereign rating changes indicates the presence of the ceiling effect. Banks incorporated in countries which received a rating downgrade are more likely to be downgraded and less likely to be upgraded. The MEs suggest a sovereign rating upgrade by 1 CCR point increases the probability by 0.77%, 0.4% and 0.2% of a bank rating upgrade by 1, 2 and ≥ 3 CCR points and leads to reduced probability of downgrade by 1, 2 and ≥ 3 CCR points by 0.88%, 0.41% and 0.3% (see Table 6, Panel A).

The positive sign on the bank ratings coefficient reflects that banks with higher credit ratings are more likely to be upgraded and less likely to be downgraded. The marginal effects imply that a one CCR point higher rating increases the probabilities of an upgrade by 1, 2 and ≥ 3 CCR points by 0.08%, 0.04% and 0.02% and decreases the probabilities of a downgrade by 1, 2 and ≥ 3 CCR points by 0.09%, 0.04% and 0.03%.

Apart from ln(Assets), the coefficients of the remaining explanatory variables are marginally or not significant although their signs are consistent with economic rationale. The negative coefficient on ln(Assets) indicates that larger banks are more likely to be downgraded and less prone to become upgraded in this sample. This could link with such banks taking on more risky investments and participating in less traditional forms of banking.

4.2 Various Fixed Effect Models

Model I of Eq. (1) accounts for observed time-varying bank specifics (e.g. profitability measures, size, credit rating) and identifies the impact of observed and unobserved changes in macro-economic conditions through the year-country interaction dummy. However, the model could still be considered incomplete because it does not control for unobservable time-variant bank heterogeneity (e.g. bank's risks, quality and investment prospects, and access to finance) (see Petersen and Rajan 1994). To address this issue, Models II, III and IV in Table 5 present the estimation of Eq. (1) using various fixed effect models and clustering options applied to the baseline Model I (following an approach similar to Jiménez and et al. 2012).

Model II adds bank dummy variables to the baseline specification testing for firm effects. The inclusion of bank controls in the model addresses observable differences in banks' profiles affecting rating changes but does not handle the unobserved effects which could be driving these differences and the dependent variable itself. Incorporating the bank dummy variables in the regression helps to correct for these omitted and possibly unobserved effects and confirms the randomisation of the treatment effect (Roberts and Whited 2011). Absolute values of all significant coefficients are larger than in Model I with the same predicted sign, which supports the validity of the method. Model II has a slightly larger coefficient for the treatment dummy and remains robust.[11]

11. Further, we extend this model by estimating both the bank fixed effect together with the clustering option on the bank level following Petersen (2009), Cameron et al. (2011) and Jiménez et al. (2012) (results available upon request). After including the bank dummies along with the bank clustering, the standard errors on treatment dummy and bank rating increase significantly suggesting that the firm effect in our data sample is non-constant (see Petersen 2009). For instance, this could be observed if an effect for one bank in 2006 was more highly correlated with its residual in 2007 than with the residual in a different time period.

Model III tests whether any time effect is present, using month fixed effects applied to the baseline Model I. Monthly fixed effects apprehend variations in macro-economic conditions such as shocks to the economy, inflation or interest rates. The coefficient on the treatment dummy increases by 23 per cent, and remains strongly statistically significant with the expected negative sign. A notable increase in the marginal effects is also recorded (Table 6, Panel C). The results suggest that the overall magnitude of the effect triggered by the solicitation conversion is stronger when monthly fixed effects are included. Similarly to Model II, we applied clustering on the same level as the fixed effects (monthly) and find that the time effect is constant.[12] This means that the given effect influences each bank by the same amount at the same point in time, and can be ultimately absorbed with the use of time dummy variables (Petersen 2009). This assumption does not hold when the effect is not constant.

In Model IV, we control for correlation between time and cross-section dimensions simultaneously (Faulkender and Petersen 2006; Thompson 2011). In addition to monthly dummies, from Model III, we cluster on the bank level. In this setting, monthly dummies eliminate the correlations between observations occurring at the same time intervals (time effects). This results in a ‘pure’ firm effect with unbiased standard errors (Petersen 2009). We also include bank fixed effects discussed along with Model II where a non-constant firm effect is observed. The coefficient on the treatment dummy in Model IV increases further (-0.83). The negative sign of the coefficient once more confirms the robustness of our results and supports the underlying hypothesis. MEs suggest that banks in the treatment group are 2.21%, 0.99% and 0.65% more likely to be downgraded by 1, 2 and ≥ 3 CCR points than banks in the other group (Table 6, Panel D). In terms of goodness of fit, the model improves compared to earlier versions (pseudo R^2 value is 0.227). The explanatory power also remains the highest

12. The tentative test following Petersen (2009) (not reported here) added month clustering to the month fixed effects. No substantial differences were observed among the standard errors, which indicates the time effect is constant.

(log likelihood of -2103). The AIC and BIC values are the smallest amongst all variations, which suggest that this is the preferred model.

In brief, the results show that the effect of sovereign solicitation conversion is negative and statistically significant for those banks belonging to the group of sovereigns whose ratings are disclosed as unsolicited. Results remain robust across various specifications.

5. ROBUSTNESS TESTS

5.1. Falsification Tests

To link the impact of solicitation disclosure rules to the bank rating changes, we focus on the differences arising between the treatment and control group. In this experimental setting, one needs to rule out the possibility that any other events coincide with the adoption of the disclosure rules. This relates to the notion that changes (due to disclosure rules) should only be observed across banks incorporated in the treated countries and not for the opposite group or in a different time than the first quarter of 2011. To confirm that no undetected issues interfere with the results, we run a set of placebo regressions focusing on: (a) time spectrum variations and (b) cross-sectional variations.

(a) To perform the first test, we run each of the Models I-IV, estimated in Section 4, with the treatment assigned to earlier dates than its true occurrence. Using this identification strategy, we find as expected that leads before the intervention yield insignificant results (see Table 7 for the case of Model IV).[13] This confirms that there are no potential unobserved events, which could be driving our results around the time at which the treatment is measured.

(b) The second falsification test examines whether any unobserved effect, which could be driving the results, is due to a selection bias. We investigate whether the treatment will yield

13. For instance, in Model II when the leads are assumed 6, 9, 12, 18 months earlier, the estimates of the treatment dummy become insignificant. In model IV, the leads are insignificant at 3, 4 and 5 months prior to the authentic event date (see Table 7). The remaining results are available on request.

significant results if the group which received it was switched. We examine whether the treatment, henceforth the placebo effect, received by the control group rather than treatment group is statistically different from zero (since the control group did not receive the treatment). We base this on randomisation on (i) bank[14] and (ii) sovereign levels (see Table 8).[15] The vast majority of placebo estimates are insignificant, thus demonstrating that the initial findings remain robust and hold for the group in question only.

5.2 Matching Methods

The univariate analysis (Section 3.1) indicated some significant differences between the treatment and control groups prior to solicitation conversions. This could be a violation of the parallel trend assumption hence further investigation would be prudent. Using covariate and propensity score matching, we construct a sample which shares similar characteristics for both groups prior to the treatment.

The covariate matching uses a nearest-neighbour matching estimator with replacement (one-to-one match and multiple neighbours match) and finds negative and statistically significant estimates for the treatment dummy (not tabulated, but available on request). The applied covariates include: sovereign and bank ratings, bank accounting data and macroeconomic indicators.[16] Although the average effect on the treated group is higher than for the rest of the

14. The random number generator in STATA assigns a placebo to a subset of banks from the control group. The placebo equals one when the (randomly assigned) bank in question belongs to the control group and if the observation is from the post-treatment period. The results of three consecutive trials are presented in Panel A of Table 8.

15. The generator randomly selects a fraction of sovereigns belonging to the control group and assigns the placebo effect to all banks which operate in those sovereigns. Subsequently, replicated regressions with the placebo effect follow. As in the first instance, the treatment group is excluded from the sample. The results of three consecutive trials are presented in Panel B of Table 8.

16. Covariates include: BankR, Leverage, ln(Assets), ROAE , LLR/GL, ETOTAY (previously described in Table 3); Sov58scale: sovereign credit rating expressed in 58-point scale, Sov20scale (Bank20scale): sovereign (bank) credit rating expressed in 20-notch scale; CPI: Consumer Price Index, mean(CPI): mean CPI value per sovereign; Incomeclass is the World Bank's income level classification of sovereigns coded as 1-low & lower middle , 2-upper middle and 3–high income countries. Note: Inflation is measured as average annual consumer price inflation growth on a year-over-year basis for the previous three

sample, its magnitude is significantly lower than in the non-matched sample (Table 5). Therefore, we perform matching with the use of propensity score.

According to Rosenbaum and Rubin (1985, p.41) the "propensity score is the conditional probability of assignment to a particular treatment given a vector of observed covariates". For the conditional mean independence assumption to hold, the outcome variable must be independent of treatment bounded by the propensity score (Smith and Todd 2005). Additionally, all variables which affect both the fact that the treatment is observed and the outcome of that treatment need to be included in the propensity score. To the best of our knowledge, there is no literature examining the economics (determinants) of the sovereign solicitation status. The observable characteristics which could potentially persuade a government to seek a solicited rating (or remain unsolicited) might be triggered by the state of the national economy represented by inflation and income levels, among others. Nonetheless, there is a possibility that the treatment has impact on the factors which are selected to explain its phenomenon and therefore we fix them over time. Firstly, we incorporate the mean value of inflation throughout the sample years, which ensures that its levels are not pre-determined by disclosure rules. Secondly, we apply the World Bank 2012 Analytical Classification of income by sovereigns as a measure of countries' prosperity. Matching reports the Average Treatment of the Treated (ATT) for the treatment (control) group of -0.064 (0.285) respectively. The t-statistic suggests that the differences between both groups are no longer statistically significant. Then, we test the balancing assumption that the means of covariates in the opposing groups do not differ from each other after the matching (Rubin and Thomas 1996). The results confirm that all the covariates are insignificant after the matching is performed and there is significant reduction in bias (see Table 9). On average, covariates bias decreases by 85 per cent. The

years in per cent terms to correct for procyclicality. This method helps in eliminating the business cycle effect. The second specification of inflation (mean(CPI)) is fixed over time. A similar approach is taken in propensity matching.

matched sample is characterised by mean (median) bias of 2.3 (1.9) whereas the pre-matched sample figures are 20.0 (22.8) respectively.

Subsequently, we re-estimate the ordered probit model where the bank rating change is a function of the propensity score, treatment dummy and previously used fixed effects (i.e. Models I-IV, Table 5). All tested models generate negative coefficients for the treatment dummy. The magnitude of the effect in Model I and II is akin to that in the unmatched sample. The regression approach confirms that the estimates used prior to the matching exercise robustly represented the economic significance of the effect of the solicitation disclosure rules on the studied sample.

5.3 DID Approach Using OLS

The final robustness test estimates Eq. (1) using Ordinary Least Squares which takes the form of a difference-in-difference (DID) estimation. Although the discrete nature of the dependent variable is best estimated with the ordered probit model, it became recent practice to use the OLS as an alternative method (e.g. Becker and Milbourn 2011; Van Roy 2013).

The economic inference of estimated Eq. (1) does not change and the treatment effect remains negative and significant throughout all specifications (see Table 10). These results imply that the increase in the number of bank rating downgrades is due to conversion of the solicitation status of sovereigns by S&P.

5.4 Endogeneity Issues

For the estimation of unbiased and consistent parameters, which allow a reliable inference, the possible sources of endogeneity need to be considered and minimised. We are interested to know whether the regulatory events were exogeneous with respect to bank rating changes. A potential concern could be that the negative rating events among banks (and sovereigns) in the pre-disclosure period were the main reason why regulators pressed for transparency when issuing unsolicited ratings. The concerns which led to regulatory changes in our sample period could be justified if there were signs of the anticipated decline in the creditworthiness of banks and sovereigns. Descriptive statistics in Table 2 invalidate this explanation given that bank upgrades outweighed downgrades in the period prior to introduction of the disclosure rules.

Economic rationale further disqualifies the possibility that S&P changed the solicitation status on several sovereigns due to operations of banks incorporated in these countries. It is implausible that the bank rating changes would in any way affect the decision of the CRA since the ceiling effect is observed in 98 per cent of cases. It is very rare to find bank rating actions which would precede their home sovereign actions within the two months window. Similarly to Alsakka et al. (2014), we find no evidence of a bank-to-sovereign rating channel. The motivation of the new EU Disclosure rules regarding solicitation status is a far-reaching issue, however it can be hypothesised that the measures taken were linked to better transparency, disclosure and presentation of credit ratings rather than anticipated declines in economic activity. To empirically verify this reasoning and rule out the possibility of the reverse causality between the dependent variable and other covariates, we run the reverse probit model regression. The dependent variable is replaced with the treatment dummy while the other covariates are used as independent variables.[17] As expected, all variables yield insignificant coefficients, thus providing reassurance about the lack of relationship with the treatment dummy.

It is not possible to completely eliminate the endogeneity problems and to guarantee the

17. The reversed baseline model includes interacting country-year fixed effects and the set of covariates used in the original model. The reverse models do not include bank or month fixed effects or clustering options because we no longer need to control for the unobserved bank related factors driving the dependent variable. On the same note, the dual dimension correlation between the clusters is also not a concern in this model setting. Moreover, unlike the original model, the variables on the right hand side are measured on the sovereign rather than bank individual level. This is due to the fact that the dependent variable (solicitation switch of sovereigns i.e., the treatment effect) is now measuring the phenomenon which arose on the country and not bank level. For this reason, we fix bank related covariates and rating variables at the country level. The results are not reported in the interests of brevity but are available on request.

exact inferences. However, this paper proposes possible explanations and a rationale for the view that the treatment effect is an exogenous shock.

6. CONCLUSIONS

Recently, the regulatory oversight of CRAs in Europe underwent significant reform with the introduction of the CRA I Regulation in September 2009 and assigning to ESMA the function of supervising and certifying CRAs across the EU from July 2011. In February 2011, as a result of Article 10 (5) of EU Regulation 1060/2009, S&P converted the solicitation status on 14 sovereigns to unsolicited. This paper aims to answer whether the regulatory change on CRAs negatively affected bank ratings in countries whose sovereign ratings were converted to unsolicited. The dataset comprises S&P ratings of 152 listed banks from 44 sovereigns in Europe, Asia and Pacific and Latin America for the period January 2006-January 2013. We examine the direct impact of disclosure rules on banks using an ordered probit model and DID framework.

The results suggest that banks incorporated in states whose ratings converted to unsolicited demonstrate higher probabilities of rating downgrades in comparison to other banks. Among the existing theories of deflated unsolicited ratings, the concept of strategic conservatism is most plausible in explaining these findings. Specifically, a lack of inside information about the issuer following the solicitation switch justifies lower ratings issued by S&P. The results are statistically robust and economically relevant. Several specifications with a number of fixed effects and clustering options are applied. The sign and significance of the effect remains unchanged. We apply several falsification tests to rule out the possibility that any other events coincide with the adoption of the regulation or that selection bias is present in our sample. To minimise the risk of endogeneity, we estimate the reversed model where the treatment dummy replaces the dependent variable. As expected, the estimates yield insignificant results.

The ceiling effect pronounced in the previous empirical literature does not differentiate between the solicitation statuses of sovereigns. Following from literature on lower unsolicited ratings (Poon, et al. 2009; Bannier et al. 2010; Van Roy 2013), we assume that bank ratings changes in the treatment group are expected to trend towards downgrades even more following sovereign rating actions. We find that banks which belong to the treatment group are more likely to be downgraded and less likely to be upgraded compared with banks from the control group. The results also suggest that sovereign rating downgrades adversely influence bank ratings through the rating channel.

These findings fill a clear void in the literature by examining the dynamics of the effect of sovereign solicitation status on the banking sector. The synergy of three overlapping streams of research reveals a phenomenon which has not been tackled by earlier theoretical nor empirical papers. The study contributes to research on unsolicited credit ratings by uncovering the significance of the solicitation status of sovereigns and its role in the domestic markets. In addition, the paper supplements scarce empirical efforts examining the rating channel between sovereigns and banks. We find that the sovereign solicitation status matters for market participants in each country due to the rating ceiling effect. Last but not least, the paper incorporates the new EU regulatory changes imposed on CRAs and is one of the first to report its outcomes on relevant markets.

The findings are of importance to regulators, politicians, CRAs and market participants alike. There are obvious implications of how the sovereign rating methods influence the functioning of financial markets. Governments need to consider their decision-making with regard to solicitations. The current disclosure rules regarding sovereign ratings and their solicitation status need closer attention from regulators and policymakers. The previously debated harmful effect of unsolicited ratings (i.e. for banks and corporates in prior literature) should return to the policy agenda. In general, the future regulatory reforms need to be taken with caution as they might aggravate the conditions for debt issuers and lead to unintended consequences.

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TABLE 1
LIST OF BANKS AND SOVEREIGNS USED IN THE SAMPLE

	CONTROL GROUP Sovereigns	No. of banks	TREATMENT GROUP Sovereigns	No. of banks
1	Austria	2	Argentina	3
2	Bolivia	1	Australia	9
3	Chile	5	Belgium	1
4	China	5	France	4
5	Colombia	1	Germany	3
6	Croatia	1	India	8
7	Czech Republic	1	Italy	8
8	Denmark	1	Japan	28
9	El Salvador	1	Netherlands	1
10	Finland	2	Singapore	1
11	Georgia	1	Switzerland	2
12	Greece	3	Taiwan	6
13	Hong Kong	2	United Kingdom	5
14	Hungary	1		
15	Indonesia	3		
16	Ireland	1		
17	Kazakhstan	6		
18	Korea (Republic of)	2		
19	Malaysia	1		
20	Mexico	1		
21	Norway	1		
22	Papua New Guinea	1		
23	Peru	4		
24	Philippines	2		
25	Poland	2		
26	Portugal	3		
27	Russian Federation	4		
28	Spain	7		
29	Thailand	5		
30	Ukraine	2		
31	Vietnam	1		

NOTES: This table lists sovereigns and S&P-rated banks incorporated in these countries used for our analysis. The treatment group comprises sovereigns whose solicitation status was converted to unsolicited whereas the control group comprises sovereigns whose S&P ratings remained solicited throughout the sample period.

TABLE 2

DESCRIPTIVE STATISTICS OF THE DATA SAMPLE

Countries	44	No. of "unsolicited sovereigns"		13	
No. of listed S&P-rated banks	152	No. of "solicited sovereigns"		31	
Panel A- BANKS	PRE-REGULATORY		POST-REGULATORY		TOTAL

Panel A- BANKS	PRE-REGULATORY		POST-REGULATORY		TOTAL	
Observations	7708		3393		11101	
Average numerical rating	37.53		34.53		36.67	
Upgrade by 1 CCR point	127	1.14%	21	0.19%	148	1.33%
Upgrade by >1 CCR point	102	0.92%	42	0.38%	144	1.30%
Downgrade by 1 CCR point	113	1.02%	56	0.50%	169	1.52%
Downgrade by >1 CCR point	96	0.86%	102	0.92%	198	1.78%
Positive events	229	2.06%	63	0.57%	292	2.63%
Negative events	209	1.88%	158	1.42%	367	3.31%
B>S	228	2.05%	73	0.66%	301	2.71%
B=S	1290	11.62%	714	6.43%	2004	18.05%
B<S	6190	55.76%	2606	23.48%	8796	79.24%
Events of speculative grade ratings	104	15.78%	77	11.68%	181	27.47%
Events of investment grade ratings	334	50.68%	144	21.85%	478	72.53%

Panel B- SOVEREIGNS	PRE-REGULATORY		POST-REGULATORY		TOTAL	
Observations	2463		992		3455	
"Unsolicited" sovereigns						
Average numerical rating	49.88		47.33		49.13	
Upgrade by 1 CCR point	6	0.17%	2	0.06%	8	0.23%
Upgrade by >1 CCR point	5	0.14%	1	0.03%	6	0.17%
Downgrade by 1 CCR point	6	0.17%	7	0.20%	13	0.38%
Downgrade by >1 CCR point	4	0.12%	8	0.23%	12	0.35%
Positive events	11	0.32%	3	0.09%	14	0.41%
Negative events	10	0.29%	15	0.43%	25	0.72%
Events of speculative grade ratings	8	20.51%	3	7.69%	11	28.21%
Events of investment grade ratings	13	33.33%	15	38.46%	28	71.79%
"Solicited" sovereigns						
Average numerical rating	39.82		37.25		39.00	
Upgrade by 1 CCR point	22	0.64%	9	0.26%	31	0.90%
Upgrade by >1 CCR point	26	0.75%	10	0.29%	36	1.04%
Downgrade by 1 CCR point	20	0.58%	7	0.20%	27	0.78%
Downgrade by >1 CCR point	23	0.67%	17	0.49%	40	1.16%
Positive events	48	1.39%	19	0.55%	67	1.94%
Negative events	43	1.24%	24	0.69%	67	1.94%
Events of speculative grade ratings	35	26.12%	26	19.40%	61	45.52%
Events of investment grade ratings	56	41.79%	17	12.69%	73	54.48%

NOTES: This table presents summary statistics for the credit rating dataset, which includes monthly bank (Panel A) and sovereign ratings (Panel B) by S&P including outlook and watch for 152 banks from 44 countries for pre- regulatory (January 2006 to February 2011) and post-regulatory (March 2011 to January 2013) periods. B=S, B < S, and B > S identify: banks rated the same as the sovereign, banks rated worse than the sovereign, and banks rated better than the sovereign, respectively. Investment (speculative) grade consists of ratings including BBB- (BBB) and above (below).“Unsolicited” refers to sovereigns whose S&P rating status changed from solicited to unsolicited in 2011.

TABLE 3

SUMMARY STATISTICS MONTHLY FREQUENCY

Variable	Units	Definition	n	Mean	S.D.	Min	Max
Dependent variable							
ΔBANK,j,t-1	+-{0,1,2,3}	Change in bank ratings using CCR scale; coded as ordinal values: -3,-2,-1,0, 1,2,3.	11101	-0.02	0.48	-3	3
Independent variables							
Post*Treatment	0/1	Post dummy= 1 if the observation is from the post-treatment period; =0 otherwise.	11101	0.17	0.38	0	1
		Treatment dummy= 1 if the country belongs in the treatment group; =0 otherwise.					
ΔSovR	+-{0,1,2,3}	Change in sovereign ratings using CCR scale; coded as ordinal values: -3,-2, -1,0,1,2,3.	11101	-0.02	0.45	-3	3
BankR	1-58	Banks credit ratings expressed in CCR scale, taking values 1-58.	11101	36.68	10.3	1	55
Bank characteristics							
1) Profitability							
ROAE	%	Return on average equity: Net income over average equity	10744	9	47.68	-992.29	817.24
2) Liquidity							
LTA	%	Net loans to total assets	10224	56.09	18.26	-0.42	89.42
3) Asset quality							
LLR/GL	%	Loan loss reserves to gross loans	9231	2.77	2.85	0.08	32.5
4) Capital adequacy							
ETOTAY	%	Equity to total assets	9922	9.4	10	-20.37	97.35
5) Size							
Ln(Assets)	-	Logarithm of book value of total assets	10272	18.07	1.82	13.3	22.06
6) Leverage	%	Total assets over equity all divided by 100	10272	0.14	0.17	-4.22	0.76

NOTES: This table presents summary statistics, abbreviations and definitions of variables used in the univariate and multivariate analysis for monthly observations of the sample of 152 banks originating from 44 countries for the period January 2006- January 2013. “n” stands for number of observations, “S.D.” is standard deviation. The sample represents a balanced panel data with regards to the dependent variable and main explanatory variables. The missing observations are encountered only in the control variables listed.1-6.

TABLE 4

MANN-WHITNEY U TEST RESULTS

Variable	No. of observations	Sample period	Mean (control)	Mean (treatment)	Difference	Wilcoxon *p*-value
ΔBANK	1040	whole	0.438	0.314	0.123	0.133
ΔBANK	600	pre-treatment	0.727	0.725	0.002	0.751
[1]Bank mean ratings	1040	whole	32.645	39.932	-7.286	0.000***
Bank mean ratings	600	pre-treatment	34.10	40.816	-6.715	0.000***
Net interest margin	1007	whole	4.192	1.578	2.613	0.000***
Net interest margin	574	pre-treatment	3.689	1.521	2.168	0.000***
Net interest revenue	1007	whole	3,844,072	4,331,178	-487105.9	0.660
Net interest revenue	574	pre-treatment	3,521,393	4,276,582	-755188.8	0.697
ROAE	1011	whole	10.615	7.117	3.497	0.000***
ROAE	578	pre-treatment	16.806	7.438	9.367	0.000***
Interbank ratio	887	whole	140.747	119.230	21.516	0.786
Interbank ratio	501	pre-treatment	149.273	137.317	11.956	0.825
Loan Loss Reserve / Gross Loans	876	whole	3.349	2.386	0.963	0.000***
Loan Loss Reserve / Gross Loans	476	pre-treatment	2.928	2.289	.638	0.011**
(NPLs)/ Gross Loans	857	whole	3.651	3.343	0.307	0.012**
(NPLs)/ Gross Loans	465	pre-treatment	3.311	3.010	.301	0.083*
Total capital ratio	797	whole	14.709	19.025	-4.316	0.002***
Total capital ratio	435	pre-treatment	14.071	18.516	-4.445	0.115
Equity/total assets	940	whole	8.763	9.834	-1.070	0.106
Equity/total assets	514	pre-treatment	8.162	10.290	-2.128	0.704

NOTES: The table presents results of the Mann-Whitney U test where differences between financial profiles of sovereigns which converted (treatment) and did not convert (control) the solicitation status are tested with use of ten covariates for the pre-treatment (Jan 2006 to Feb 2011) and the entire period (Jan 2006 to Jan 2013) using yearly data (see section 3.1).

[1] The ratings are presented as an average mean of all bank ratings occurring at each particular time period with distinction between treatment and control group. The ratings are expressed in the 58 point CCR scale. Net interest margin: ration of net interest revenue to average total earning assets[18]; Net interest revenue: net interest revenue to average total assets; Interbank ratio: money lent to other financial intermediaries/money borrowed from other institutions; (NPL)/Gross Loans: non-performing loans to gross loans; Total capital ratio: capital adequacy ratio used to determines Tier 1 and 2 capital required by Basel (minimum 8%).

The remaining variable definitions are presented in Table 3.Significance level such that: *** p<1%, ** p<5%, * p<10%.

18. Net interest revenue=interest received minus interest paid; total earning assets= loans plus other earning assets excluding fixed assets.

TABLE 5

ORDERED PROBIT MODEL RESULTS

MODEL VARIABLES	(I)	(II)	(III)	(IV)
Post*Treatment	-0.5313***	-0.5319***	-0.7759***	-0.8297**
	(-4.30)	(-3.91)	(-3.56)	(-2.48)
ΔSovR	0.4260***	0.4264***	0.4419***	0.4407***
	(13.29)	(13.44)	(12.79)	(13.69)
BankR	0.0419***	0.1309***	0.0449***	0.1557***
	(5.94)	(7.23)	(6.05)	(4.49)
ln(Assets)	-0.0997***	-0.6570***	-0.1057***	-0.1046
	(-3.95)	(-5.28)	(-4.17)	(-0.64)
Leverage	-0.3032	-0.8562	-0.2617	-0.8963
	(-0.80)	(-1.13)	(-0.68)	(-1.18)
ROAE	-0.0003	-0.0005	-0.0003	-0.0006
	(-0.57)	(-0.76)	(-0.64)	(-1.08)
LLR/GL	0.0051	-0.0217	0.0052	-0.0232
	(0.52)	(-1.34)	(0.50)	(-1.53)
ETOTAY	-0.0127*	-0.0260	-0.0138*	-0.0188
	(-1.73)	(-1.58)	(-1.83)	(-0.88)
Observations	8478	8478	8478	8478
Log likelihood	-2295	-2242	-2166	-2103
Pseudo R^2	0.157	0.176	0.204	0.227
Number of clusters	.	.	.	135[a]
AIC	5031.327	5107.256	4994.975	4474.193
BIC	6588.323	7305.367	7326.946	5418.254
Year-country dummies	yes	yes	yes	yes
Bank dummy	no	yes	no	yes
Cluster by bank ID	-	no	-	yes
Month dummy	-	-	yes	yes

NOTES: This table reports the estimated coefficients and robust z-statistics in parentheses from various specifications of the ordered probit model of Equation 1 (see section 3.3). The credit rating dataset consists of monthly sovereign and bank ratings for 152 banks originating from 44 countries for the period January 2006-January 2013. The dependent variable is ΔBANK. The variables definitions and summary statistics are presented in Table 3. Fixed effects are included ("yes"), not included ("no") or not applicable in given specification ("-"). The year-country fixed effect is the interaction term between full set of country and year dummies. The month fixed effect comprises a fixed effect for every (but one) year: month during the sample period. Significance level such that: *** p<1%, ** p<5%, * p<10%.

Note a: The number of clusters is lower than number of banks (152). The estimation discards 10 (7) banks from the control (treatment) group respectively due to collinearity.

TABLE 6

MARGINAL EFFECTS FOR TABLE 5 ESTIMATIONS

			Marginal effect %						
Variables	Coefficient	t-value	-3	-2	-1	0	1	2	3
Panel A- MODEL I									
Post*Treatment	-0.5313***	-3.95	0.64	0.76	1.50	-1.68	-0.71	-0.35	-0.20
ΔSovR	0.4260***	-13.29	-0.30	-0.41	-0.88	0.22	0.77	0.40	0.20
BankR	0.0419***	-5.94	-0.03	-0.04	-0.09	0.02	0.08	0.04	0.02
Panel B- MODEL II									
Post*Treatment	-0.5319***	-3.91	0.54	0.70	1.42	-1.55	-0.66	-0.32	-0.10
ΔSovR	0.4264***	-13.44	-0.25	-0.37	-0.82	0.19	0.72	0.36	0.16
BankR	0.1309***	-7.23	-0.08	-0.11	-0.25	0.06	0.22	0.11	0.05
Panel C- MODEL III									
Post*Treatment	-0.7759***	-3.28	0.73	1.02	2.18	-2.72	-0.74	-0.34	-0.10
ΔSovR	0.4419***	-13.70	-0.17	-0.30	-0.74	0.11	0.65	0.31	0.14
BankR	0.0449***	-6.29	-0.02	-0.03	-0.08	0.01	0.07	0.03	0.01
Panel D- MODEL IV									
Post*Treatment	-0.8297**	-2.48	0.65	0.99	2.21	-2.75	-0.69	-0.30	-0.10
ΔSovR	0.4407***	-13.69	-0.13	-0.25	-0.66	0.07	0.59	0.27	0.11
BankR	0.1557***	-4.49	-0.05	-0.09	-0.23	0.03	0.21	0.09	0.04

NOTES: This table presents the impact of three main control variables on the probability of bank rating change (MEs: marginal effects) resulting from Equation 1 (see Table 5). Panels A, B, C, D present the MEs results from Models I-IV, respectively. The same MEs estimates are obtained when Models II and III (Panel B and C) are supplemented with one way clustering on bank level. The economic magnitude of the results remains the same (only the statistical inference is changed). Significance levels such that: *** p<1%, ** p<5%, * p<10%.

TABLE 7

PLACEBO EFFECTS-TIME VARIATIONS (Model IV)

MODEL	(IV)	(IV)	(IV)
VARIABLES	f3.	f4.	f5.
Placebo	-0.3401	-0.2061	-0.1375
	(-1.35)	(-0.84)	(-0.66)
ΔSovR	0.4232***	0.4166***	0.4184***
	(13.09)	(12.81)	(12.58)
BankR	0.1613***	0.1647***	0.1632***
	(4.32)	(4.28)	(4.23)
ln(Assets)	-0.9493	-0.9731	-0.9555
	(-1.19)	(-1.20)	(-1.19)
Leverage	-0.1028	-0.1011	-0.0669
	(-0.61)	(-0.59)	(-0.40)
ROAE	-0.0006	-0.0006	-0.0006
	(-1.02)	(-0.99)	(-0.99)
LLR/GL	-0.0244	-0.0248	-0.0250
	(-1.57)	(-1.58)	(-1.56)
ETOTAY	-0.0174	-0.0169	-0.0140
	(-0.78)	(-0.75)	(-0.61)
Observations	8,082	7,950	7,818
Log likelihood	-2083	-2074	-2055
Pseudo R^2	0.228	0.229	0.228
Number of clusters	135	135	135
Year -country dummies	yes	yes	yes
Bank dummy	yes	yes	yes
Cluster by bank ID	yes	yes	yes
Month dummy	yes	yes	yes

NOTES: The table presents the estimated coefficients and robust z-statistics in parentheses from results of falsification test performed on Model IV of Equation 1 (seen in Table 5) using the ordered probit model estimation on the sample of 152 banks from 44 countries for the period January 2006- January 2013 (see Section 5.1a). The dependent variable is ΔBANK. f.3; f.4; f.5 are leads in which the treatment was assigned (3, 4 and 5 months earlier than the regulatory action was announced respectively). Therefore, the Placebo serves a purpose of a "placebo effect". The variables definitions and summary statistics are presented in Table 3. The year-country fixed effect is the interaction term between full set of country and year dummies. The month fixed effect comprises a fixed effect for every (but one) year: month during the sample period. Significance level such that: *** p<1%, ** p<5%, * p<10%.

TABLE 8

PLACEBO EFFECTS- CROSS SECTION VARIATION (Model IV)

	Panel A			Panel B		
	Subset of banks			All banks		
VARIABLES	1	2	3	1	2	3
Placebo	0.0431	0.1580	0.0380	0.7139	0.5183	0.0447
	(0.20)	(0.77)	(0.20)	(1.32)	(1.02)	(0.08)
ΔSovR	0.4485***	0.4484***	0.4481***	0.4438***	0.4494***	0.4485***
	(9.23)	(9.26)	(9.17)	(9.04)	(9.26)	(9.23)
BankR	0.0886***	0.0877***	0.0882***	0.0886***	0.0896***	0.0881***
	(3.24)	(3.22)	(3.20)	(3.10)	(3.36)	(3.19)
ln(Assets)	-0.2456	-0.2275	-0.2527	-0.2149	-0.2181	-0.2146
	(-0.42)	(-0.43)	(-0.47)	(-0.41)	(-0.42)	(-0.42)
Leverage	0.1816	0.2502	0.2156	0.1867	0.1812	0.2004
	(0.63)	(0.93)	(0.78)	(0.70)	(0.69)	(0.77)
ROAE	-0.0004	-0.0004	-0.0004	-0.0004	-0.0004	-0.0004
	(-1.15)	(-1.17)	(-1.23)	(-1.18)	(-1.20)	(-1.21)
LLR/GL	-0.0144	-0.0127	-0.0143	-0.0145	-0.0144	-0.0143
	(-0.75)	(-0.66)	(-0.74)	(-0.74)	(-0.74)	(-0.74)
ETOTAY	-0.0118	-0.0112	-0.0122	-0.0123	-0.0124	-0.0124
	(-0.29)	(-0.28)	(-0.30)	(-0.30)	(-0.30)	(-0.30)
Observations	3,645	3,645	3,645	3,645	3,645	3,645
Log likelihood	-972.3	-972.1	-972.3	-970.9	-971.5	-972.3
Pseudo R^2	0.237	0.237	0.237	0.238	0.237	0.237
Number of clusters	63	63	63	63	63	63
Year-country dummies	yes	yes	yes	yes	yes	yes
Bank dummy	yes	yes	yes	yes	yes	yes
Cluster by bank ID	yes	yes	yes	yes	yes	yes
Month dummy	yes	yes	yes	yes	yes	yes

NOTES: The table presents the estimated coefficients and robust z-statistics in parentheses from results of falsification test performed on Model IV of Equation 1 (seen in Table 5) using the ordered probit model estimation on the sample of 152 banks from 44 countries for the period January 2006- January 2013 (see section 5.1b). The dependent variable is ΔBANK. The variables definitions and summary statistics are presented in Table 3. Test in Panel A randomly assigns the placebo to subset of banks which belong to the control group sovereigns. Test in Panel B randomly selects sovereigns which belong to the control group and assigns placebo to all banks belonging in that subset. The results of three consecutive trials are presented in columns 1, 2 and 3 of Panel A and B. The number of entities selected by the random number generator in each trial is available on request. The sample is restricted to control group only and for this reason the number of observations is constant in all trials. The number of institution clusters amounts to 73, however the package drops 10 banks due to collinearity issues. Fixed effects are included ("yes"), not included ("no") or not applicable in given specification ("-"). The year-country fixed effect is the interaction term between full set of country and year dummies. The month fixed effect comprises a fixed effect for every (but one) year: month during the sample period. Significance level such that: *** p<1%, ** p<5%, * p<10%.

TABLE 9

BALANCING TEST- PROPENSITY SCORE MATCHING

Variable		Mean			%reduct	t-test	
		Treated	Control	%bias	|bias|	t	p>t
Sov20scale	Unmatched	16.357	15.197	29.8		10.06	0.00
	Matched	16.357	16.294	1.6	94.5	0.5	0.61
Bank20scale	Unmatched	13.581	12.764	26.8		8.68	0.00
	Matched	13.581	13.52	2	92.6	0.65	0.51
Leverage	Unmatched	0.15546	0.14422	14.8		4.83	0.00
	Matched	0.15546	0.15485	0.8	94.6	0.27	0.78
ln(Assets)	Unmatched	18.515	17.939	33.2		11.37	0.00
	Matched	18.515	18.439	4.4	86.7	1.29	0.19
LLR/GL	Unmatched	2.5332	2.8416	-11.5		-3.83	0.00
	Matched	2.5332	2.4768	2.1	81.7	0.73	0.46
ETOTAY	Unmatched	8.0232	8.2887	-6.3		-2.13	0.033
	Matched	8.0232	7.9413	1.9	69.1	0.56	0.57
Mean(CPI)	Unmatched	2.3841	3.2458	-28.4		-10.44	0.00
	Matched	2.3841	2.2267	5.2	81.7	1.49	0.13
Incomeclass	Unmatched	2.6629	2.5034	22.8		7.96	0.00
	Matched	2.6629	2.6711	-1.2	94.8	-0.36	0.72

NOTES: The table presents results of a balancing exercise performed directly after the propensity score matching (see Section 5.2). The null hypothesis states that difference in means of covariates is equal to zero.

TABLE 10

MONTHLY DATA RESULTS: ORDINARY LEAST SQUARES (Difference-in-Difference)

MODEL	(I)	(II)	(III)	(IV)
VARIABLES				
Constant	0.2465*	5.8912***	0.3346**	0.7027
	(1.92)	(3.74)	(2.48)	(0.47)
Post*Treatment	-0.1285***	-0.1044***	-0.1815***	-0.1378**
	(-4.28)	(-3.76)	(-2.94)	(-2.36)
ΔSovR	0.1947***	0.1920***	0.1962***	0.1919***
	(9.68)	(9.64)	(9.59)	(9.83)
BankR	0.0166***	0.0746***	0.0168***	0.0823***
	(3.16)	(3.75)	(3.22)	(5.70)
ln(Assets)	-0.0345**	-0.3981***	-0.0341**	-0.1423
	(-2.47)	(-3.73)	(-2.49)	(-1.63)
Leverage	-0.0272	-0.6385	-0.0213	-0.6348
	(-0.12)	(-0.99)	(-0.10)	(-1.17)
ROAE	-0.0003	-0.0005	-0.0003	-0.0006*
	(-0.54)	(-0.96)	(-0.55)	(-1.71)
LLR/GL	0.0013	-0.0122*	0.0012	-0.0111
	(0.43)	(-1.81)	(0.40)	(-1.11)
ETOTAY	-0.0046*	-0.0131	-0.0045*	-0.0091
	(-1.85)	(-1.57)	(-1.85)	(-1.44)
Observations	8,478	8,478	8,478	8,478
R^2	0.1148	0.1351	0.1386	0.1609
Root MSE	0.639	0.636	0.634	0.629
Number of clusters	.	.	.	135
Year and country dummies	no	no	no	no
Year-country dummies	yes	yes	yes	yes
Bank dummy	no	yes	no	yes
Cluster by bank ID	no	no	no	yes
Month dummy	no	no	yes	yes

NOTES: The table reports the estimated coefficients and robust z-statistics in parentheses from specifications of Eq. (1) using Ordinary Least Squares. The credit rating dataset consists of monthly sovereign and bank ratings for 152 banks originating from 44 countries for the period January 2006-January 2013. The dependent variable is ΔBANK58ALL and stands for change in bank ratings using 58-point scale (continuous variable). The remaining variables definitions and summary statistics are presented in Table 3. Fixed effects are included ("yes"), not included ("no") or not applicable in given specification ("-"). The year-country fixed effect is the interaction term between full set of country and year dummies. The month fixed effect comprises a fixed effect for every (but one) year: month during the sample period. Significance level such that: *** p<1%, ** p<5%, * p<10%.

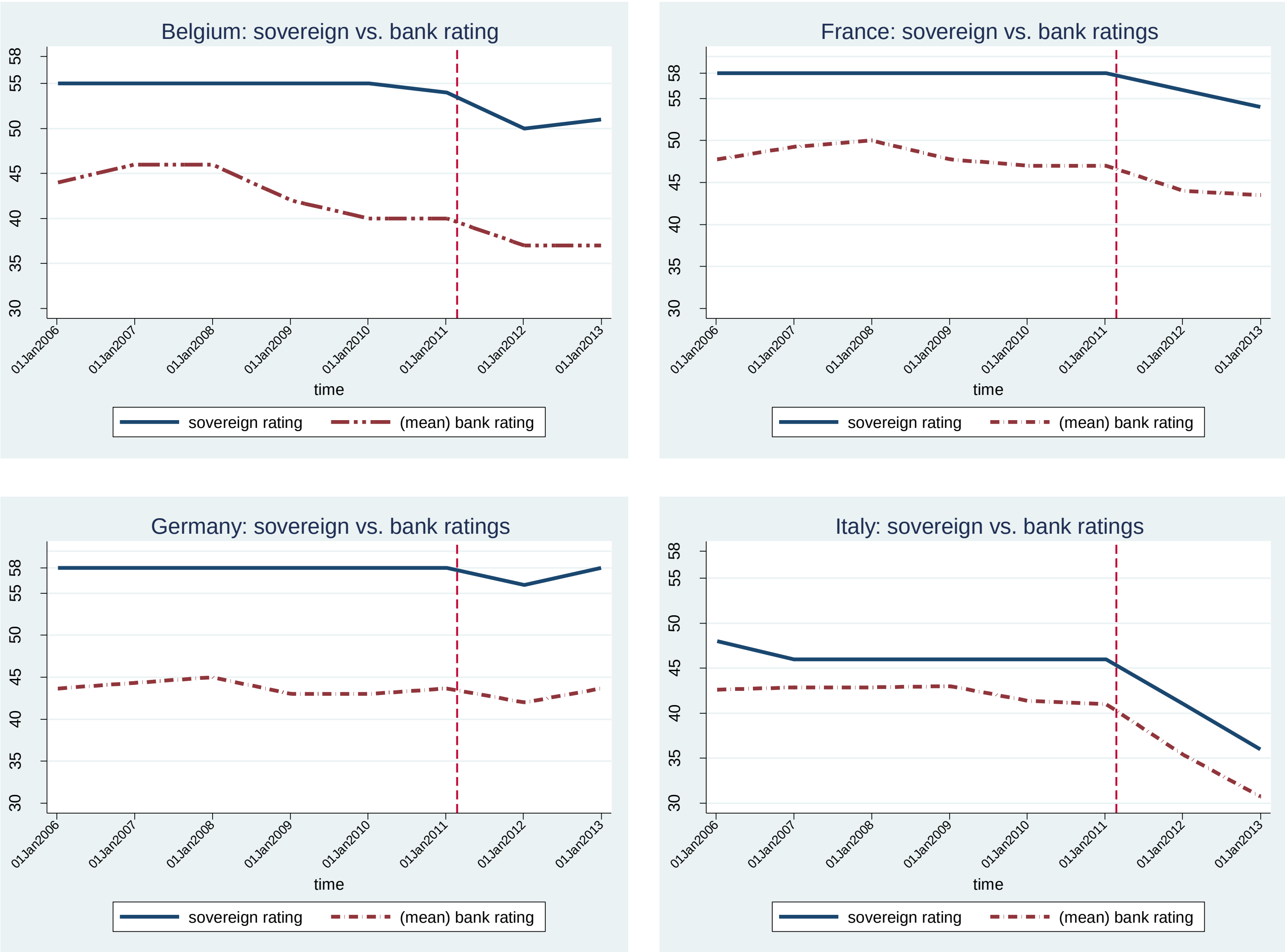

FIG.1. Trends Between Sovereign and Bank Ratings

NOTES: This figure represents the trend between sovereign and bank ratings for a sample of sovereigns from the group under intervention during the sample period (January 2006 to January 2013). The sovereign (thick) line represents a rating of a country whereas the bank rating (dash dot) line corresponds to an average rating of the listed financial institutions incorporated in that country. The dashed (vertical) line represents the timing of the regulatory change to disclose the solicitation status of the sovereign rating. The credit ratings scale is transformed into a 58-point rating scale.