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Rating opaque borrowers:
why are unsolicited ratings lower?

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Rating opaque borrowers: why are unsolicited ratings lower?[*]

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Abstract

This paper examines why unsolicited ratings tend to be lower than solicited ratings. Both self-selection among issuers and strategic conservatism of rating agencies may be reasonable explanations. Analyses of default incidences of non-U.S. borrowers between January 1996 and December 2006 show that rating conservatism may play a role for industrial firms, but self-selection cannot be fully rejected. Neither can it for insurance companies, though data restrictions impede further conclusions. For unsolicited bank ratings, however, we find strong evidence that rating conservatism is an important cause. The downward bias also appears to increase along with banks' opaqueness.

JEL Classification: G15, G24

Keywords: Unsolicited Ratings, Self-Selection, Conservatism, Opaqueness

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1 Introduction

Among the most controversial aspects of the credit rating business is the practice of assigning unsolicited ratings. Unsolicited ratings are assessments of credit quality "that credit rating agencies conduct without being formally engaged to do so by the issuer" (IOSCO, 2003). As a consequence, unsolicited ratings do not entail the payment of a rating fee and they are usually not based on the sharing of private (i.e., soft) information or other forms of cooperation between rated entity and rating agency.

Recent critique with respect to unsolicited ratings has centered on the concern that unsolicited ratings "do not appear to be empirically as favorable as solicited ratings" (SEC, 2005), i.e., credit rating agencies tend to assign lower unsolicited ratings than when hired and paid to do so. This critique is particularly strongly voiced in Europe and Asia and has led to a vehement debate among market participants and regulators about the use of unsolicited ratings (JCIF, 1999-2001; Fitch, 2006).[1]

In this paper, we examine the reasons for the alleged downward bias in unsolicited ratings. Lower unsolicited ratings could simply be caused by a self-selection of high-quality companies into the solicited rating status and of low-quality firms into the unsolicited (or no) rating status. Alternatively, a downward bias in uncommissioned ratings could be caused by agencies' strategic considerations in the rating process. Given that unsolicited ratings lack the soft information component, conservative agencies may have a strong interest in erring on the side of caution, i.e., rating an issuer "too bad" rather than "too good" - particularly in the case of opaque borrowers.

Several recent papers have analyzed whether unsolicited ratings are, indeed, lower than solicited ratings. Relying on a sample of 595 non-U.S. companies with Standard & Poor's (S&P) ratings between 1998 and 2000, Poon (2003) finds that particularly Japanese firms are dealt lower unsolicited ratings. Poon and Firth (2005) and Van Roy (2006) observe that Fitch's so-called shadow

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[1]Generally, the new capital adequacy rules issued by the Basel Committee on Banking Supervision allow the consideration of commissioned as well as of unsolicited ratings. Paragraph 108 of the Basel II Accord states: "As a general rule, banks should use solicited ratings from eligible ECAIs (external credit assessment institutions). National supervisory authorities may, however, allow banks to use unsolicited ratings in the same way as solicited ratings."

ratings are downward biased for Asian and international banks, respectively. Most recently, using an endogenous regime-switching model for a sample of non-U.S. banks with S&P ratings between 1998 and 2003, Poon et al. (2009) show that unsolicited bank ratings would be higher if they were solicited and, vice versa, that solicited bank ratings would be lower if they were unsolicited. By decomposing the observed rating level differences into two components - the clientele effect, caused by a bank's financial profile, and the treatment effect, due to the solicitation status - they address a potential self-selection bias, but conclude that, at times, the solicitation-status effect may outweigh it.

Common to all these studies is their employment of an *ex-ante* viewpoint: they examine the effect of the solicitation status and other control variables on the rating level, i.e. the default risk prediction. As a consequence, they are unable to account for those characteristics of the rated firms that may be hidden ex-ante, but materialize in the default realization, i.e. *ex-post*. The study by Gan (2004) has been the only one so far that applies an *outcome test* on rating data from Moody's and S&P between 1994 and 1998. Regressing actual default observations of U.S. industrial companies on a vector of control variables including the rating level and a dummy variable indicating the rating status, she finds no significant results for the unsolicited dummy. This leads her to conclude that self-selection seems to have driven the rating level differences between solicited and unsolicited ratings of U.S. industrial firms.

Our paper makes several contributions to the existing literature. First, our dataset allows us to differentiate between firms from various industries. Examining several sub-samples of particular interest, we are, thus, able to draw clearer conclusions regarding the reasons for the observed rating level differences than, for example, Gan (2004). Second, to the best of our knowledge, our study is the first to provide evidence from an *ex-post* point of view that unsolicited bank ratings in our sample overstate default risk, i.e. are downward biased. Third, we explore whether this downward bias varies with bank opaqueness. Our results provide tentative, but not conclusive, evidence that the downward bias is, indeed, more pronounced when bank opaqueness is high. Both results are novel and nicely complement the results by Poon et al. (2009). Finally, in contrast to

most of the earlier studies, our dataset is very clear and uncontaminated because we employ S&P rating data for non-U.S. firms, where the rating status is directly observable and verifiable.[2] Fitch's shadow-ratings, for example, do not always qualify as "unsolicited" due to mergers with other rating institutions, which used diverging rating policies, and Moody's did not consistently label unsolicited ratings as such. These data impediments clearly weaken the results of several of the earlier studies. Given the regional exposition of our data, our analyses furthermore focus exactly on those countries where concerns about unsolicited ratings are most strongly voiced.

We use an extensive set of S&P rating data on non-U.S. companies between January 1996 and December 2006 and study the effect of the solicitation status on default outcomes. Univariate analyses show significant rating level differences with respect to solicitation status for non-defaulting issuers. Differences are particularly pronounced for financial firms, i.e., allegedly rather opaque types of businesses (Morgan, 2002; Iannotta, 2006; Hirtle, 2006; Haggard and Howe, 2007).

Contrary to the analysis by Gan (2004) on U.S. industrial firms, our multivariate results for non-U.S. *industrial companies* are mixed. We find that the probability of observing defaults differs significantly with respect to solicitation status, controlling for rating level, time, and regional effects. However, this no longer holds when we substitute the rating level with a non-linear measure of credit quality to control for firm-specific risk assessments. Hence, for the sub-sample of industrial firms, rating agencies' conservatism seems to play a role, but we cannot fully refute that self-selection among borrowers also contributes to lower unsolicited ratings.

Interestingly, we do not find signs of excessive conservatism affecting the unsolicited ratings of *insurance companies* - despite the alleged opaqueness of this industry (Morgan, 2002; Park, 2008). This may have to do with the fact that particularly European insurance firms do not appear to be extremely opaque (Iannotta, 2006). Our conclusions are, however, hampered by severe data restrictions in this industry.

For the sub-sample of *banks*, in contrast, we obtain strong and robust evidence that ex-post riskiness differs between banks with unsolicited and those with solicited ratings. This clearly rejects

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[2]Prior to September 2007, rating agencies operating in the U.S. have not been required to report whether a credit rating is solicited or not (SEC, 2007).

the self-selection hypothesis and suggests that, to some extent at least, rating level differences can be explained by deliberate actions on the part of the rating agencies. Our initial findings are supported by the employment of alternative outcome measures such as a market-based measure for the probability of default and a purely accounting-based risk measure. We proceed by testing whether, indeed, banks' opaqueness induces agencies to announce lower unsolicited ratings. Accounting for bank opaqueness explicitly, we find that global, country-specific (in-)transparency does not explain the gap between solicited and unsolicited ratings, nor does the existence of splits in individual bank ratings. However, employing the rating level deviation between S&P and Fitch as a measure of opaqueness, we find a significant influence: differences between solicited and unsolicited ratings are more pronounced the more opaque the individual bank is. Proxying individual bank opaqueness with the inverse of the number of financial analysts covering the bank supports our results.

Hence, agency conservatism seems - at least partly - to play a role in the rating process, leading to downward-biased unsolicited ratings. Both market participants and regulators should take this effect into account, particularly when dealing with banks. While our results tentatively suggest that higher transparency may help to reduce the bias in unsolicited bank ratings, it seems less clear how it may be fully avoided. Given the inherent and partly even regulatory-induced opaqueness of the banking sector (Morgan, 2002), rating agencies can hardly be forced to announce more optimistic unsolicited bank ratings.

The remainder of the paper is organized as follows. Section 2 derives the hypothesis to be tested in the following. Section 3 uses empirical default observations for our sample of non-U.S. firms to examine the theoretical implications. It also discusses various robustness checks. Section 4 follows up on the role of opaqueness in banks, as one important sub-sample in our dataset. The last section concludes.

2 Derivation of hypothesis

Two lines of arguments may provide an explanation for the level differences between solicited and unsolicited ratings: either strategic behavior on the part of the rated borrowers or strategic actions on the part of the rating agencies. The first argument is based on a simple self-selection process: only those firms that perceive their unsolicited ratings to be too low will choose to commission proper ratings which should then correctly reflect their (better) credit quality. They do so, because they expect to profit from lower credit spreads requested by investors due to the rating improvement - otherwise there would be no compensation for paying the rating fee. In contrast, issuers who feel that their unsolicited rating correctly reflects (or even overstates) their credit quality, will not decide to pay for a solicited rating as this may not increase the rating level. Low-quality issuers will, thus, remain with their (relatively low) unsolicited rating. Self-selection should therefore lead to ratings that correctly reflect an issuer's credit quality, irrespective of the solicitation status. Hence, from an ex-post viewpoint, we should find that firms with an unsolicited rating are not less risky than companies with a solicited rating, controlling for rating level. A model deriving these effects can be found in the appendix.

Strategic rating behavior by the agencies, as the second argument, is particularly concerned with rating "errors" (Löffler, 2005). These fall in two categories with different consequences for bondholders as the main users of rating information. A type-I error ("overrating") occurs if an issuer is assessed as low risk and assigned a high rating, but defaults nonetheless. A type-II error ("underrating"), in contrast, refers to the non-default of a low-rated issuer. Particularly a conservative rater "by definition worries relatively more about overrating" (Morgan, 2002) than about assigning too pessimistic evaluations - an effect that should be magnified by a weak information basis to act upon, as is the case in unsolicited rating processes. As a consequence, given conservative rating agencies, the lack of soft information in unsolicited ratings should lead to a considerable difference vis-à-vis solicited ratings. Eventually, the downward bias of unsolicited ratings should be strongest for highly opaque issuers. Banks and insurance companies are often mentioned as the most opaque firms due to their complex asset and liability structures (Morgan, 2002; Iannotta, 2006; Hirtle,

2006; Haggard and Howe, 2007). Analyzing rating level differences based on solicitation status therefore seems to be of particular interest for the case of financial institutions.

Overall, strategic conservatism of rating agencies leads to the following testable consequence: firms of identical credit quality obtain different credit ratings, depending on their solicitation status, i.e., those who do not mandate a rating will receive a lower (unsolicited) rating than those who do ask and pay for a (solicited) rating. When comparing issuers with identical rating but different solicitation status, we should, therefore, find that issuers with unsolicited ratings are ex-post less risky than issuers with solicited ratings: the unsolicited status should be negatively correlated to a company's default incidence.[3] As this prediction is exactly opposite to the one stemming from the self-selection argument, we may easily test between the two explanatory lines by simply comparing the ex-post realized risk of firms with unsolicited and solicited ratings within each rating class. The following hypothesis is phrased in favor of the self-selection argument:

Hypothesis 1 *If firms self-select with respect to solicitation status, the fact that a company's rating is unsolicited should not have a discernible effect on its ex-post default risk, controlling for rating level.*

This hypothesis will be tested in the subsequent sections.

3 Main empirical analysis

3.1 Descriptive statistics

Our data set comprises solicited and unsolicited S&P rating data from January 1996 to December 2006, since S&P did not introduce the distinction between solicited and unsolicited ratings be-

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[3]Note that "blackmailing", i.e. trying to induce borrowers to solicit (and pay for) a rating by announcing an unjustifiably low unsolicited rating, should lead to the same projected outcome. The agencies' incentive to blackmail may also increase in the borrower's opaqueness: the more opaque an issuer, the easier it is for an agency to legitimate the subsequent increase in rating level due to the disclosure of private information in the solicitation process. Unfortunately, we are not able to differentiate between these two types of rating strategies in our empirical tests. To keep a neutral stance, we refer to strategic conservatism as driving the agencies' rating behavior in the rest of the paper. Given the empirical evidence (Covitz and Harrison, 2003) and theoretical arguments (Ramakrishnan and Thakor, 1984) regarding the dominance of reputational over financial concerns in the rating process, we believe this attitude to be justified.

fore January 1996. We did not include Moody's or Fitch's rating data as neither agency reliably discloses the solicitation status of the announced ratings. Rating data were extracted from S&P's credit ratings database provided by Wharton Research Data Services (WRDS). In accordance with Fenn (2000) and Morgan (2002), we translated the original ratings into numerical ratings scaled from 1 (AAA) to 18 (CC to C). All data on unsolicited ratings were manually cross-checked with S&P's RatingsDirect database. Additionally, we employed S&P's sovereign ratings to control for country-specific risk differences.

We first use actual default observations as an ex-post risk realization measure. For each sample firm we record the year-end rating and check whether the firm defaulted in the following one-year period, which we will refer to as the realization period. A default is registered upon the occurrence of a D (regular or full default), SD (selected default) or R rating in the realization period. An R rating indicates cases in which financial companies were regulated by national supervisory bodies. As regulated companies cannot freely decide to continue their debt repayments, we treat them as defaults. We cross-checked all recorded defaults with S&P's annual default reports.

<Insert Table 1 here>

Table 1 provides a descriptive overview of our sample. Panel I shows the sample's sectoral distribution. Overall, the sample contains 26,413 firm-year observations. Of these, 58.5% come from financial firms[4] and 41.5% from non-financial firms. In the group of financial firms, 63.7% are firm-year observations with solicited ratings and 36.3% with unsolicited ratings, reflecting the importance of unsolicited ratings in this industry. Particularly insurance companies receive unsolicited ratings relatively often, with the number of firm-year observations with solicited ratings roughly equaling those with unsolicited ratings. In the group of non-financial firms, in contrast, the proportion of companies with solicited ratings is much higher (89.3% versus 10.7% unsolicited ratings). With respect to default incidences, we find that 24 financial companies with a solicited and 18 with an unsolicited rating defaulted in the realization period, representing 0.24% and 0.32%, respectively, of the firm-year observations. In the group of non-financial firms, a much larger number

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[4]The category of "other financials" comprises asset managers, brokerage firms, and the like.

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of defaults is observed: 185 (1.89%) with a solicited and 11 (0.94%) with an unsolicited rating. In sum, the sample contains 238 defaults.

The regional distribution of our sample is depicted in Panel II. Interestingly, the Asia Pacific region displays the highest proportion of unsolicited ratings. The highest number of defaults is observed among firms with solicited ratings in Latin America, followed by Canada. The highest number of defaults with unsolicited ratings is found in the Asia Pacific region.

<Insert Table 2 here>

Table 2 shows the rating distribution per year. For financial firms, the ratio of unsolicited to solicited ratings has been varying over the years until it recently declined (from a maximum of 85.7% in 1999) to roughly one third, while for non-financial firms the ratio is much steadier and much lower at about 13.5%. The average solicited ratings of non-defaulting firms slightly deteriorated over the years (by approximately two rating notches). This is in line with Blume et al. (1998), who report a generally decreasing credit quality over time. With respect to the mean difference between unsolicited and solicited ratings, we observe a positive spread for both financial and non-financial firms. With almost 4 notches, the difference is particularly strong for financial firms that did not default within the observation period. A t-test of the difference between the mean solicited and unsolicited rating of non-defaulting firms shows that both the differences for financial firms (3.74 notches) and for non-financial firms (0.95 notches) are highly significant.

Note, however, that the relationship between the rating level and the default probability is highly non-linear. We therefore run the same univariate test on the long-term average default frequency. We use S&P's average default rates per rating class over the years 1981 to 2005 (S&P, 2006a) and smooth them exponentially. We find that differences in default frequencies of non-defaulting financial firms are still significantly positive on the 1%-level according to the t-test, but become zero for non-financial firms. For financial firms, the results are even more striking than before: The mean default frequency of surviving firms with solicited ratings equals 0.42%, and almost triples (to 1.24%) for those with unsolicited ratings.

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3.2 Multivariate analysis of empirical default observations

Our basic multivariate analysis is similar to the one used by Gan (2004). To test for differences in the probability of observing defaults we employ a pooled logit regression model. We define $default_i$ as a dummy variable indicating whether company i defaulted in the realization period (one for default, zero otherwise), and $default_i^*$ as the unobserved linking variable, which is continuous and ranges over the set of real numbers. Hence, we estimate

$$default_i^* = \alpha + \beta_1 \cdot rating_i + \beta_2 \cdot unsolicited_i + \gamma \cdot D + \epsilon_i \tag{1}$$

with

$$default_i = \begin{cases} 1 & if \quad default_i^* \; > 0, \\ 0 & if \quad default_i^* \; \leq 0, \end{cases}$$

where *rating* is the numerical rating level for company i that is followed by the realization period in which the default status is recorded. The rating level is the main variable to control for company-specific creditworthiness.[5] *Unsolicited* represents a dummy for the rating status (1 for unsolicited, 0 for solicited ratings). γ is a vector of coefficients for a vector of dummy variables, D, capturing nine realization periods, four business sectors, and four regions. Rating agencies claim that their rating assessments are consistent over all covered business sectors and regions, so that, actually, no further controls should be necessary. However, our univariate analysis in section 3.1 revealed striking differences, particularly between financial and non-financial firms. Therefore, we choose to control for business sector differences by classifying our observations into five sub-samples: banks, insurance companies, other financial firms, utilities, and industrials. The latter serves as the reference group. Additionally, since we also find considerable regional differences in the prevalence of unsolicited ratings, we use regional dummies for Asia Pacific, Australia/New

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[5]We do not control for any company-specific variables such as size, return on equity, liquidity, and the like, because the company's rating should be an aggregate measure of this information. Other suitable control variables, such as CDS spreads or bond yields, are not available for our observation period and sample.

Zealand, Europe/Middle East/Africa, Latin America and Canada as well. Again, the latter serves as the reference group. Furthermore, D includes S&P's sovereign ratings in order to control for varying macroeconomic environments that may affect firms' default probabilities.

The unsolicited dummy is our main variable of interest. If self-selection processes drove the downward bias in uncommissioned ratings, we should not observe any influence of the unsolicited dummy on the default variable. Irrespective of the solicitation status, ratings should then always correctly reflect a debtor's credit quality. However, if unsolicited ratings are too low because of strategic conservatism on the part of the agencies, they should be associated with lower probabilities of observing defaults than solicited ratings of the same level. In this case, the unsolicited dummy in the regression model should have a significantly negative effect on the default variable.

Note that although we use longitudinal data we do not employ a (logit) panel model. Given that - in the non-linear case - fixed effects models include only observations with a change in the dependent variable over time, this would have reduced our sample to the 238 companies with default events. Therefore, only random effects models seem to be feasible. These, however, would not have allowed us to conduct standard tests (except for the 238 firms) to analyze whether the coefficients of the random effects specification are consistent. We thus decide to employ a pooled logit regression and use standard errors that are robust to cluster correlation (Williams, 2000) on the firm level.[6]

<Insert Table 3 here>

Table 3 contains the results of the basic logit model. Regression model I displays the results for the total sample. As can be seen, the incidence of default is significantly higher for firms with a low rating, since the rating level variable exhibits a highly significant positive coefficient. The unsolicited dummy is significantly negative, indicating that strategic conservatism rather than self-selection seems to be driving the differences between solicited and unsolicited ratings. Interestingly, the country rating dummy is significantly negative, and not, as expected, positive. However,

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[6]In a recent paper, Petersen (2008) shows how crucial the choice of the correct standard errors is in order to derive meaningful conclusions. We also ran all analyses with Huber-White robust standard errors (results available on request). The results that we report in the remainder are robust to the use of these standard errors.

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this result is not robust to different model specifications (see below). Regarding the business sector dummies, other financial companies and utilities experience significantly higher probabilities of observing defaults than industrials. Defaults by banks, on the other hand, are less frequent. Also, defaults by firms in Latin America occur more often than in Canada.

Regression models II-IV contain the results for individual sub-samples. For industrial firms (model II), we find, again, a negative unsolicited dummy that is significant on the 1%-level. This result is in clear contrast to Gan (2004) on U.S. industrial firms, who suggests that - from an ex-post point of view - there are no differences between companies with solicited and unsolicited ratings. However, as will be seen below, our contrarian finding is not robust to different model specifications. Regression model III provides the results for the sub-sample of banks only. We exclude the region dummies for Asia Pacific and for Australia/New Zealand because we observe no defaults there. Again, we obtain a highly significant negative unsolicited dummy. For insurance companies, in contrast, regression model IV does not display any significant effect of the unsolicited dummy, even though its coefficient is still negative. Since insurance companies are allegedly very opaque businesses, this is a surprising result. It may either be the case that any conservatism on the part of the agencies is, indeed, outweighed by self-selection of the firms or that the insurance companies in our sample choose to be transparent (Park, 2008), so that even conservative agencies are not induced to announce biased unsolicited ratings. Regional aspects may play a role, too. Iannotta (2006), for instance, finds that European insurance companies are not very opaque due to required financial disclosures. Note that, contrary to the regression on the sub-sample of banks, the Europe/Mid East/Africa country dummy displays a significantly negative coefficient. For utilities and other financial firms we are not able to run separate regressions because of too few default observations.

3.3 Robustness tests

In order to take into account all available rating information, we modify the rating variable to include so-called rating outlooks and CreditWatch entries as well. Since these instruments are

rarely used in combination with unsolicited ratings,[7] one could imagine that the observed ex-post differences in default risk are simply due to the "coarseness" of our solicited rating information, caused by neglecting these rating refinements so far. S&P assigns an outlook to all long-term ratings to indicate the expected direction of a future rating change over a medium- to long-term period, typically 18 months. Rating watchlists, in contrast, are usually employed shortly after the announcement of a particular corporate event, for example, a merger. They are mostly concluded within 90 days (S&P, 2006b). Several empirical studies have shown that watchlist and outlook statements provide useful information to market participants about the companies' creditworthiness (Holthausen and Leftwich, 1986; Hand et al., 1992; Bannier and Hirsch, 2009).

In our dataset, we obtain (positive or negative) outlook information for 4,839 firm-year observations, and (positive or negative) CreditWatch entries for 1,433 firm-year observations. We follow Cantor and Mann (2006) in incorporating the additional rating information into our data and increase (decrease) the rating level by two numerical rating notches in the case of a negative (positive) CreditWatch entry. For outlooks we use an adjustment by plus (minus) one rating grade for negative (positive) outlooks. This approach has the advantage that it requires fewer degrees of freedom than employing dummy variables for positive and negative outlook and CreditWatch observations. The modified rating level is still in the interval 1 (AAA) to 18 (CC to C).

<Insert Table 4 here>

Table 4 contains the regression results using this refined rating information. Compared with our previous findings, there is hardly any change. Both for the total sample and for the sub-samples of industrial firms and banks the unsolicited dummy is still significantly negative. However, significance in the sub-samples is only found on the 5%-level. For insurance companies the unsolicited dummy is not significant, as before.

Finally, we check whether our previous results still hold after replacing the rating level with historical default frequencies. Given the highly non-linear empirical relationship between default

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[7]In our sample, only 1.3% (0.4%) of all firm-year observations with unsolicited ratings carry a negative (positive) outlook and only 0.1% (0.2%) negative (positive) CreditWatches.

risk and rating classes, the use of a non-linear measure of credit risk may be more appropriate. We therefore substitute the rating level in our regression models with S&P's long-term average default frequency as described above. Table 5 displays the respective results from pooled logit regressions. Interestingly, the results now do change. The unsolicited dummy is significantly negative only for the sub-sample of banks, but no longer for industrial firms.

<Insert Table 5 here>

Our results so far suggest that - with the exception of insurance companies - solicited and unsolicited ratings tend to differ not only from an ex-ante point of view as indicated by Poon (2003), Gan (2004) and Poon et al. (2009), but also from an ex-post perspective. While the findings for industrial firms are not robust to different model specifications, our results for banks show that strategic rating behavior by the agencies plays a significant role: ex-post default realizations differ strongly between banks with solicited and those with unsolicited ratings. But is this effect robust to further, bank-specific measures of default risk? And what causes this effect? Is it true that the opaqueness of financial institutions triggers excessive conservatism on the part of the rating agencies? The following section focuses on the sub-sample of banks solely, and tries to examine the effect of this industry's reported lack of transparency more closely.

4   Further analyses

Several studies have argued that financial institutions are relatively opaque (Morgan, 2002; Hirtle, 2006): fixed assets typically make up only a small fraction of their balance sheets, banks engage heavily in off-balance-sheet activities, they hold only small capital buffers and lend to information-intensive borrowers. Jones et al. (2008) analyze merger announcements in the banking industry and find that banks appear to have intentionally increased investments in opaque assets as a response to positive price signals around mergers. However, employing market-based indicators of opaqueness, Flannery et al. (2004) report that banking assets are not unusually opaque. Evidence pertaining to the opaqueness of insurance companies is even more mixed. While Morgan (2002)

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concludes from rating splits that insurance firms are even more opaque than banks, Iannotta (2006) finds that "European insurance firms' issues do not generate more uncertainty than banks".[8] Park (2008) examines information asymmetries in the insurance stock market and concludes that, while insurance companies are, indeed, inherently opaque, large firms are able to "disclose enough private information to have the same information asymmetry level as non-financial companies."

Close examination of the reason for the downward bias in financial firms' unsolicited ratings, hence, seems an important exercise. Unfortunately, severe data restrictions do not enable us to conduct further tests for insurance companies.[9] Therefore, we study solely banks in the following. First, we try to render our former results more robust by using alternative outcome measures for banks. Second, we analyze the role of bank opaqueness in the effect that the solicitation status has on the default outcome.

4.1 Alternative outcome measures

Even though our former results from the sub-sample of banks are highly supportive of strategic conservatism, they suffer from one major weakness: the number of defaults is very low. In order to increase the validity of our analyses, we construct alternative outcome measures as dependent variables. First, we employ a market-based estimate of default risk à la Merton (1974). Second, we use a purely accounting-based risk measure, the bank-individual z-score. As both dependent variables are continuous, we can now make use of panel estimations. For all regression models presented in this and in the next section, we run standard Hausman tests to determine whether a fixed or a random effects panel model is better suited. The results clearly suggest using a random effects panel estimation. We continue to employ standard errors that account for potential clustering on the firm level.

The computation of the market-based estimate of default risk rests on the seminal works by

[8]He attributes this effect to a directive enacted by the European Community in 1991 that improved the financial disclosures of European insurance companies.

[9]In contrast to the sub-sample of banks, we do not obtain sufficient firm-specific and market data to construct the necessary outcome measures for insurance companies. For example, we obtain balance sheet information for only 97 insurance firms and additional rating information from Moody's (Fitch) for only 33 (34) of them. Effectively, we lose almost 98% of the initial firm-year observations referring to insurance companies.

Black and Scholes (1973) and Merton (1974). According to Merton (1974), a firm's equity can be viewed as a call option on the value of the firm's assets. The call option expires at the debt's maturity, T. If, at T, the value of assets is below the face value of liabilities (i.e., the strike price), the call option is left unexercised and the bankrupt firm is turned over to its debt holders. The probability of default in this model is hence equal to the probability that the market value of assets falls below the face value of liabilities at maturity. We refer to this probability as the Merton PD.

The input parameters needed for computing the Merton PD are the face value of liabilities, proxied by the book value of total liabilities in U.S. dollars extracted from Compustat, the market value of equity in U.S. dollars extracted from Datastream, the volatility of the market value of equity over the whole fiscal year, the maturity of debt, which is assumed to be one year, and the risk-free rate of interest, which we proxy by using a one-year market yield obtained from the website of the Federal Reserve Board.[10] To compare the default risk prediction expressed by the rating level with the risk realization expressed by the Merton PD, we first record the end-of-year rating of a given bank and use the bank's Merton PD in year t+1. This PD then becomes our dependent variable. As we are not able to extract accounting and market data for all banks, our sample is considerably reduced to 1,537 firm-year observations.

The results of a random effects panel regression with the one-year Merton PD as dependent variable are depicted in Panel I, Table 6. In the first regression model we use the numerical rating, in the second the modified rating level which includes outlook information and CreditWatch entries, and in the third model we employ the default frequency as a control variable for S&P's assessment of default risk. The other control variables are as before, but we partially omit their results. In all three cases, the unsolicited dummy displays a negative coefficient that is significant on the 1%-level. The results of regression model III have to be interpreted with caution, though, because the default frequency itself is not significant. This indicates that the default frequency is not a good predictor for the realized Merton PD.

<Insert Table 6 here>

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[10]For a formal treatment of the necessary steps to compute the Merton PD, we refer to the original sources as well as to Hillegeist et al. (2004) or Vassalou and Xing (2004).

Second, we employ a purely accounting-based measure of default risk, the bank-specific z-score. It is defined as the return on average assets plus the capital-asset ratio divided by the standard deviation of asset returns. The z-score combines accounting measures of profitability, leverage and volatility. Specifically, z indicates the number of standard deviations that a bank's return on assets has to drop below its expected value before equity is depleted and the bank is insolvent (Roy, 1952; Hannan and Hanwick, 1988). The data for computing the z-score are extracted from Compustat. We calculate the standard deviation of asset returns for each bank over at least four years (Laeven and Levine, 2008). In contrast to our earlier tests, we now compare the risk prediction with the risk realization expressed by the bank z-score after two and three years. We choose this approach because we assume that it takes a considerable amount of time - more than a year - for a change in the company's risk, as reflected by a rating change, to affect the accounting figures. Thus, we first record the end-of-year rating of a given bank and use the bank's z-score in year t+2 (t+3 for the three-year horizon). This score then becomes our dependent variable. Due to the lack of accounting data, our sample is again reduced to 1,483 (1,287) firm-year observations in the case of the two-year (three-year) realization period.

Panel II of Table 6 shows the results for the two-year horizon. We find that the unsolicited dummy in model I and II has a positive coefficient that is significant on the 5%-level. This illustrates that banks with unsolicited ratings have significantly higher z-scores than otherwise comparable banks with solicited ratings. As higher z-scores reflect less risk, this result underlines our previous conclusion. The coefficient is not only statistically significant but its size also suggests a high economic significance. Furthermore, we see that the rating level coefficient is negative and significant. This mirrors the fact that banks with higher numerical ratings (higher risk) have lower z-scores in the realization period. The sign of the unsolicited dummy does not change when we use default frequencies (model III). However, the effect loses statistical significance. Panel III depicts the results for the three-year horizon. Results are a bit weaker, but qualitatively comparable to the two-year horizon.

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4.2 Does opaqueness drive the downward bias?

So far we have found robust evidence that the downward bias in unsolicited bank ratings is driven by strategic conservatism on the part of the agencies. In this section, we try to explore in more detail the role of bank opaqueness in explaining the ex-post differences between solicited and unsolicited ratings. To do so, we measure opaqueness in three ways. First, we introduce a new measure for opaqueness that gauges country-specific bank opaqueness, i.e., opaqueness that affects all banks in one country. Second, following Morgan (2002) we quantify bank opaqueness on the individual bank level by using the existence of a split rating, and, alternatively, by employing the numerical differences between the rating levels of two agencies. Third, we use the analyst coverage to capture individual bank opaqueness.

4.2.1 Country-specific opaqueness

If bank opaqueness is country-specific and if the level of opaqueness varies between countries, we should find that agencies' conservatism leads to more pronounced ex-post differences between solicited and unsolicited bank ratings in more opaque countries. To proxy country-specific bank opaqueness we introduce a new measure based on survey results collected by the World Bank. In 1999, the World Bank started to conduct a survey on bank regulation in more than 100 countries worldwide, with regular updates. Questionnaires were sent out to bank regulators, covering different areas of bank regulation, such as capital requirements, deposit insurance schemes, and - most important for our purpose - disclosure requirements. In order to compute the opaqueness indices, we use survey data from 1999, 2002 and 2006, accessible from the World Bank's website. We select eight survey questions associated with bank opaqueness and specify the answers as zero/one values. We then run a principal component analysis to compute the country-specific opaqueness indices. Since survey results are not available for every year in our observation period, we use the following mapping: For the time period 1996-2000 we use the information from 1999, for the period 2001-2003 we use information from 2002, and for the period 2004-2005 we use the information from 2006.

<Insert Table 7 here>

In the multivariate regression approach, we use the one-year Merton PD as our outcome mea-
sure and interact the bank opaqueness index with the unsolicited dummy.[11] If country-specific
opaqueness drove the results, we would expect to find a significantly positive coefficient for this
interaction term, reflecting more divergence in the outcome measure between solicited and unso-
licited bank ratings in countries with higher bank opaqueness. We include the level of the opaque-
ness index as a further control variable. Table 7 contains the results. Again, we use either the
rating level, the modified rating level, or the default frequency as explanatory variables (models I,
II, or III). The coefficient of the opaqueness index is negative and highly significant, indicating that
banks from more opaque countries are riskier. The unsolicited dummy is not significant, reflecting
no differences between solicited and unsolicited ratings at an opaqueness level of zero. The inter-
action term is also not significant. This result, therefore, does not support the hypothesis that the
differences between solicited and unsolicited ratings are particularly pronounced if bank opaque-
ness is high, i.e., index values are low. Hence, country-specific bank opaqueness does not seem to
drive our results.

4.2.2 Bank-specific opaqueness

In order to account for opaqueness on the individual bank level, we employ both rating-related
measures of opaqueness (Morgan, 2002; Drucker and Puri, 2009) and opinion-related ones (Flan-
nery et al., 2004; Livingston et al., 2007). For the latter, we employ the coverage via analysts'
earnings forecasts taken from the Institutional Brokers Estimates System (IBES). For the former,
we use Moody's and Fitch's in addition to S&P's ratings.[12] Moody's ratings are extracted from
Bloomberg, and Fitch ratings from Bankscope. We match all banks with S&P ratings with the re-
spective Moody's and Fitch ratings, using the numerical rating scale from 1 (Fitch: AAA, Moody's:
Aaa) to 18 (Fitch: CC to C, Moody's: Ca) as before. This matching leaves us with 2,103 firm-year

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[11]We cannot use the default observations as a dependent variable because the number of defaults is not large enough
to allow discrimination between banks from countries with different opaqueness levels.

[12]Note that we do not have access to information on the solicitation status of Fitch's or Moody's ratings.

observations with ratings by S&P and Fitch, and 1,890 firm-year observations with ratings by S&P and Moody's.

<Insert Table 8 here>

Table 8 shows the distribution of rating differences between S&P ratings and ratings from Fitch and Moody's, split into unsolicited and solicited ratings. A negative (positive) difference implies that the S&P rating reflects less risk (more risk) than the ratings from one of the other agencies. The unsolicited S&P ratings differ in 88.1% of all cases from the Fitch ratings, and in 87% from the Moody's ratings. For solicited ratings the ratios are 65.3% for Fitch and 59.9% for Moody's. A t-test shows that the ratios are significantly higher for unsolicited than for solicited ratings for both agencies, indicating that a higher opaqueness level is associated with unsolicited S&P ratings. However, the ratios between Fitch and Moody's do not differ significantly.

In the regression model, we first proxy bank opaqueness by the existence of a split rating, i.e., we interact the rating status with a dummy variable indicating whether or not S&P's rating differs from the rivals' rating. We do this separately for Fitch and Moody's. The generated dummy variable *Unsol*Split rating* indicates that the bank has an unsolicited rating from S&P and a different rating by the other rating agency at the end of the year. We use *Unsol*No split rating* as the reference and test whether *Unsol*Split rating* is significantly different from the reference. Again, we employ the Merton PD as our outcome measure. While this is consistent with the preceding analyses, it reduces our sample further because we cannot calculate the Merton PD for all banks for which we obtain a full set of ratings. If bank-specific opaqueness drove our results, we would expect the *Unsol*Split rating* variable to be significantly negative, indicating that the unsolicited effect is more pronounced if the agencies disagree over the rating level.

<Insert Table 9 here>

Panel I (II) of Table 9 contains the results for the case of Fitch (Moody's). Interestingly, *Unsol*Split rating* is not significant in any of the regression models. Hence, although banks with unsolicited ratings have lower Merton PDs than banks with solicited ratings (cf. section 4.1), the

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mere existence of a split rating between S&P and Fitch or S&P and Moody's cannot explain this result.

A potential shortcoming of measuring bank-specific opaqueness via the existence of a split rating is that this approach does not account for all the information contained in the agencies' disagreement. We therefore generate the variable *Rating difference*, which measures the absolute numerical rating difference in notches between S&P and each of the other agencies' rating levels. In the very few cases with differences larger than 5 notches, we set these to 5. Again, we interact the rating difference with the unsolicited dummy and include both the absolute level of rating differences and the interaction variable *Unsol*Rating diff* in our regressions. As before, if bank-specific opaqueness caused the differences between solicited and unsolicited ratings, we would expect *Unsol*Rating diff* to be significantly negative.

Panel III (IV) of Table 9 shows the results for Fitch (Moody's). The coefficient of the unsolicited dummy reflects the case in which a bank has an unsolicited rating by S&P and the same rating by the other rating agency. Regarding the rating differences vis-à-vis Fitch, we find (apart from regression model III, where we employ the default frequency as an explanatory variable) no significant effect for this unsolicited dummy. However, the interaction variable *Unsol*Rating diff* is significantly negative in all three regression models. This indicates that the Merton PD becomes smaller as the rating differences increase. Stated differently, the differences between solicited and unsolicited ratings are more pronounced when bank-specific opaqueness is higher. Interestingly, though, we cannot confirm these results for the case of Moody's. Even though the coefficient of the *Unsol*Rating diff* variable is negative in Panel IV, it is not significant.

In a final test, we proxy bank opaqueness by the number of analysts "following" the bank with their earnings' forecasts. We define analyst coverage as being *high* if the number of analysts covering a firm is above the median, and as *low* if it is below the median. In accordance with Flannery et al. (2004) and Livingston et al. (2007), we presume that opaqueness is negatively correlated with analyst coverage. Interacting the unsolicited dummy with the *low* analyst coverage dummy, we expect a negative coefficient as a sign that opaqueness drives the rating level difference. Panel V of

Table 9 presents the results. Indeed, we find that the interaction term displays the expected negative sign and is moreover highly significant. This leads us to conclude that the observed ex-post differences between solicited and unsolicited bank ratings seem - to a significant extent - to be driven by strategic conservatism on the part of the agencies, potentially fuelled by banks' opaqueness.

5 Conclusion

In this study, we analyze potential reasons for the downward bias in unsolicited ratings. Based on a data set of non-U.S. firms in the time period January 1996 to December 2006, we use an outcome test with various outcome measures. Our dataset allows us to differentiate between several industries. We find that for non-financial firms, agencies' conservatism seems to play a role, though we cannot unequivocally reject that self-selection causes downward-biased unsolicited ratings. Interestingly, we do not find evidence of excess conservatism affecting the unsolicited ratings of insurance companies, despite the recurrent accusations against the practice of announcing non-mandated assessments from this industry. Our results are, however, hampered by a relatively weak database in this industry and should, therefore, be interpreted with caution.

For the sub-sample of banks, in contrast, we obtain clear and robust evidence that unsolicited ratings differ from solicited ratings not only from an ex-ante but also from an ex-post point of view. In other words, identically rated issuers with differing solicitation status exhibit diverse ex-post risk realizations. This novel result suggests that the observed downward bias in unsolicited bank ratings is driven by strategic factors within the rating process, rather than by self-selection among banks. Rating strategies may be prominently driven by agencies' conservatism, fuelled by issuers' opaqueness. Additional tests taking bank opaqueness explicitly into account tentatively support this argument: while we do not find that country-specific bank opaqueness causes the differences between solicited and unsolicited ratings, opaqueness on the individual bank level seems to play a role.

Even though our study cannot give a general nor concluding answer to the overall question

what drives the downward bias in unsolicited ratings, the non-negligibility of rating conservatism in several of our sub-samples lets the criticism of unsolicited ratings appear at least partly justified. What is more, any ex-post biases between solicited and unsolicited ratings create a framework in which the two types of ratings are no longer comparable, as they essentially refer to different levels of default probability. In the long run, this should not be expected to leave the agencies' reputation unaffected.

Appendix

The following model is a simple extension of the model used in Bannier and Tyrell (2006). Consider a risk-neutral firm that continually conducts business projects and has to issue claims to raise the necessary finance. In order to keep the analysis as simple as possible, we assume that the firm conducts one independent project each period (the firm is hence equivalent to this project). After the publication of a rating (unsolicited or solicited) of the project's credit quality, risk-neutral market participants choose whether or not to invest, i.e., buy the issued claims. For simplicity we presume that the project will be successful only if sufficiently many investors donate financing. Otherwise it will be unsuccessful, yielding a payoff of zero (that does not suffice to repay investors).

The firm's project quality is represented by a random variable θ, which is normally distributed with mean y and variance $1/a$. The distribution of θ is assumed to be publicly known. The lower a is, the higher is the firm's fundamental risk, since quality θ may then deviate strongly from the ex-ante expected value y. While the distribution of θ is commonly known, the realization θ, however, is not observable to market participants. Yet, we assume that investors receive individual private information about firm quality: $x_I | \theta \sim N(\theta, 1/b)$. The higher b is, the more closely are investors' private signals distributed around the unknown quality θ. In this respect, b denotes the precision of investors' private information.[13] Similarly, a rating agency collects private information about the project (if the rating is solicited, this is based on a process of information sharing between firm and agency) that results in a private signal of $x_A | \theta \sim N(\theta, 1/c)$. Note that, conditional on θ, private signals are assumed to be independent of each other. The eventually announced rating, z, may be seen as an additional public signal about the realized value of θ.

Investors have one unit of capital and must decide whether to invest this unit in the firm's risky project or to invest in a safe asset. The safe asset may be thought of as a simple storage technology. For the project to be successful, a proportion of $1 - \theta$ of the total investment has to be financed externally, i.e., via the buying of claims by investors. Firm quality θ hence represents the firm's ability to internally finance part of the project. If at least $1 - \theta$ of all investors decide to buy the

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[13]We may conceive of institutional investors who are capable of collecting private information about the firm.

firm's securities, the project will be successful and delivers a payoff of $\bar{V}$ at maturity that allows a repayment of $R > 1$ to each of the investors. An unsuccessful project, in contrast, yields a payoff of zero and hence does not suffice to repay investors. Thus, the "better" the firm's quality, i.e., the higher θ, the higher is the proportion of total financing that the firm can bear internally and the higher is the probability that the project will be successful and the debt will be repaid. The agency's rating therefore refers directly to the firm's credit quality.

The sequence of events is then as follows:

- In t_{-2}, the firm announces its willingness to conduct a new business project and its need for debt financing. It offers a repayment of R per unit of debt at the project's maturity (t_{+1}). The firm additionally chooses whether or not to appoint a rating agency to assess the firm's quality.

- In t_{-1}, firm quality θ is realized. While its distribution is common knowledge in the market, the realization itself remains unobservable to market participants. Yet, investors observe individual private signals x_I about firm quality. The agency announces its rating, either solicited, z_S, or unsolicited, z_U.

- In t, investors decide whether to buy the firm's securities or the safe asset. Proceeds of the security's sale are invested in the firm's business project.

- In t_{+1}, if at least a proportion $1 - \theta$ of market participants decided to invest, the project yields a payoff of $\bar{V}$ and repayment of R is guaranteed. If the project is not successful, in contrast, a payoff of zero is realized and no repayment to investors takes place.[14]

Investors' payoffs are summarized in the following table. Here, l represents the aggregate amount of investment, i.e., the proportion of investors that decide to buy the firm's securities rather than the safe asset.

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[14]Consider, for instance, that the project requires a certain amount of administrative work that leads to relatively high sunk costs. The "real" investment process may start only after these costs are covered. If external financing is too low, the project cannot be successful and the initial amount of financing is "sunk" and cannot be recovered.

	Project successful	Project not successful
	$(l \geq 1 - \theta)$	$(l < 1 - \theta)$
Invest risky	R	0
Invest safe	1	1

Assuming that investors' private information is sufficiently precise, the model can be solved for a unique equilibrium, using the global games approach.[15] As has been shown by Morris and Shin (2003, 2004), the equilibrium will be in trigger strategies, i.e., investors will invest in the risky project if they expect the firm's quality to be sufficiently good (higher than a unique threshold θ^*) and invest in the safe asset otherwise. Equilibrium derivation will then rely on a marginal investor who is indifferent between investing safe or risky. The unique threshold quality θ^* may be translated into a unique private signal x_I^*, below which investors will optimally invest in the safe asset and above which they will optimally buy the risky security of the firm. Proceeding backwards, we will then solve for the optimal firm decision on whether or not to appoint a rating agency to assess the firm's credit quality.

Indifference on the part of investors requires identical expected payoffs from investing safe or risky. Expectations are based on each investor's private information x:

$$\pi_I(\text{safe}) = \pi_I(\text{risky})$$
$$1 = R \cdot \text{prob}(\theta \geq \theta^* | x) . \tag{2}$$

Investors' (posterior) expectations with regard to firm quality are based on private and public information about θ. If the firm decided to appoint a rating agency, we assume that the solicited rating will be equivalent to the agency's private information, i.e., $z_S = x_A$. Investors' posterior beliefs are

[15]A global game in the sense of Carlsson and van Damme (1973) is a game where each player noisily observes the game's payoff structure (i.e firm quality θ), which itself is determined by a random draw from a given class of games (in our case via a normal distribution). In the following, we will assume that investors' private information is always sufficiently precise, i.e., $b > \min\{(a+c)^2/(2\pi), 4a^2/(2\pi)\}$, so that a unique equilibrium is guaranteed. We may think of investors in our model as mainly institutional investors who usually maintain their own research departments that deliver sufficiently precise private information.

therefore distributed as follows

$$\theta | x_I, z_S \sim N\Big(\frac{ay + bx_I + cz_S}{a + b + c}, \frac{1}{a + b + c}\Big) .$$ (3)

If the rating is not solicited, in contrast, we may assume that on average the unsolicited rating is equivalent to the mean of public information, i.e., $z_U = y$, so that investors' posterior beliefs are given as

$$\theta | x_I, z_U \sim N\Big(\frac{2ay + bx_I}{2a + b}, \frac{1}{2a + b}\Big) .$$ (4)

Plugging these posterior beliefs into (2) delivers the respective indifference condition for the individual investor:

$$x_{I,S}^* = \frac{a + b + c}{b}\theta^* - \frac{a}{b}y - \frac{c}{b}z_S - \frac{\sqrt{a + b + c}}{b}\Phi^{-1}\Big(\frac{R - 1}{R}\Big) ,$$ (5)

or

$$x_{I,U}^* = \frac{2a + b}{b}\theta^* - 2\frac{a}{b}y - \frac{\sqrt{2a + b}}{b}\Phi^{-1}\Big(\frac{R - 1}{R}\Big) .$$ (6)

Each investor will invest in the safe asset as long as his private signal indicates a low credit quality of the firm, i.e., if $x_I \leq x_{I,S}^*$ or $x_{I,U}^*$. Only for sufficiently high private signals $x_I > x_{I,S}^*$ or $x_{I,U}^*$ will investors buy the risky security issued by the firm.

As the firm's project needs a critical mass of external financing in order to be successful, we can derive the equilibrium threshold θ^* that divides unsuccessful from successful projects. The project is on the brink of success if

$$
\begin{aligned}
1 - \theta &= l = \text{prob}(x \geq x_{I,S}^* | \theta) \\
\theta &= \Phi(\sqrt{b}(x_{I,S}^* - \theta)) ,
\end{aligned}
$$ (7)

or, in the case of an unsolicited rating,

$$\theta = \Phi(\sqrt{b}(x_{I,U}^* - \theta)) \ . \tag{8}$$

The condition considers that, due to the independence of private signals, the proportion of investors observing private signals above the threshold $x_{I,S}^*$ or $x_{I,U}^*$ (who therefore invest in the risky security) is equivalent to the probability with which an individual investor observes a signal above $x_{I,S}^*$ or $x_{I,U}^*$.

Combining indifference equations (2) and (7), respectively (8), delivers the equilibrium firm quality:

$$\theta_S^* = \Phi\left(\frac{1}{\sqrt{b}} \left(a(\theta_S^* - y) + c(\theta_S^* - z_S) - \sqrt{a+b+c}\Phi^{-1}\left(\frac{R-1}{R}\right) \right) \right) , \tag{9}$$

respectively

$$\theta_U^* = \Phi\left(\frac{1}{\sqrt{b}} \left(2a(\theta_U^* - y) - \sqrt{2a+b}\Phi^{-1}\left(\frac{R-1}{R}\right) \right) \right) . \tag{10}$$

In the following, we will distinguish between θ_S^* and θ_U^* only where necessary and write θ^* otherwise. Whenever a firm quality below θ^* is realized, the project cannot be successful because too few investors decide to invest in the risky project and the firm will default on the existing claims.

From an ex-ante viewpoint, the default probability of the firm may therefore be measured by the probability that the realized firm quality will be below θ^*, i.e.,

$$\text{prob(default)} = \text{prob}(\theta \leq \theta^*) = \Phi(\sqrt{a}(\theta^* - y)) \ . \tag{11}$$

As can be seen, the probability of default increases in threshold value θ^* and in all parameters that raise this threshold. This setup conveys a general functional idea that has also been derived elsewhere; see for instance Gordy (2003).

We may now proceed to solve for the firm's optimal decision on whether or not to appoint a rating agency. In order to keep the analysis as simple as possible, we assume that the firm aims at maximizing its expected payoff. While with a successful project the firm may claim the difference

$\bar{V} - R$ as its part of the project's profit, an unsuccessful project leaves the firm with a profit of zero. As long as $\bar{V} - R > 0$, the firm's expected profit therefore simply hinges on the probability of the project's success, or conversely the project's probability of default. Thus, the firm will appoint a rating agency if the announced solicited rating reduces the probability of default (i.e., threshold value θ^*) as compared to a situation where investors have to base their investment decision on an unsolicited rating (i.e., whenever $\theta_S^* < \theta_U^*$).

Proposition 1 (Rating solicitation as adverse selection process) *A rating agency will be appointed to announce a solicited rating only by those firms that feel they are publicly "undervalued", i.e., for which the precision-weighted difference between the presumed solicited rating and the prior publicly available information about firm quality is sufficiently high.*

Proof:

$$
\begin{aligned}
\theta_S^* &< \theta_U^* \\
a(\theta_S^* - y) + c(\theta_S^* - z_S) - \sqrt{a+b+c}\,\Phi^{-1}\Big(\frac{R-1}{R}\Big) &< 2a(\theta_U^* - y) - \sqrt{2a+b}\,\Phi^{-1}\Big(\frac{R-1}{R}\Big) \\
cz_S - ay &> (c+a)\theta_S^* - 2a\theta_U^* - (\sqrt{a+b+c} \\
&\quad -\sqrt{2a+b})\Phi^{-1}\Big(\frac{R-1}{R}\Big) \qquad (12)
\end{aligned}
$$

q.e.d.

Alternatively, we may assume that by soliciting a rating the firm influences the precision of the agency's private information, c. This is due to the fact that by sharing private information about the firm's credit quality with the rating agency, the firm may increase the agency's private information precision c as compared to a situation without any information sharing. If, by doing so, the firm could decrease the threshold θ_S^* up to which the firm defaults, it will have an interest in soliciting a rating. The following proposition shows that only those firms will want to solicit a rating, i.e., to increase the agency's private information precision, that are certain of disclosing sufficiently "good" information to the rating agency.

Proposition 2 *An appointment of a rating agency and the subsequent information sharing will be advantageous only for those firms that are able to disclose sufficiently optimistic information about their credit quality.*

Proof:

$$\frac{\partial \theta_S^*}{\partial c} = \frac{\phi(\cdot)\frac{1}{\sqrt{b}}}{1 - \phi(\cdot)\frac{a+c}{\sqrt{b}}}\left[\theta_S^* - z_S - \frac{1}{2\sqrt{a+b+c}}\Phi^{-1}\Big(\frac{R-1}{R}\Big)\right] \tag{13}$$

The latter partial derivative is negative (positive) if $z_S = x_A > (<)\theta_S^* - \frac{1}{2\sqrt{a+b+c}}\Phi^{-1}\big(\frac{R-1}{R}\big)$. q.e.d.

Obviously, therefore, only those firms will request a solicited rating that feel they are unfairly valued by the market and hence believe that their unsolicited rating is too low. If they are confident of being able to reveal much more optimistic private information to the agency, they will commission a solicited rating.

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Table 1: Sectoral (Panel I) and regional (Panel II) sample distribution

S&P rating and default data for the time period January 1996 to December 2006. Figures in brackets show proportions relative to line totals.

Panel I: Business sector	Banks	Insurance companies	Other financials	Industrials	Utilities	Sum
Survivors:						
Solicited rating	4,773	4,350	684	7,622	1,972	19,401
	(0.246)	(0.224)	(0.035)	(0.393)	(0.102)	
Unsolicited rating	1,202	4,407	6	1,121	38	6,774
	(0.177)	(0.651)	(0.001)	(0.165)	(0.006)	
Defaults:						
Solicited rating	12	4	8	161	24	209
	(0.057)	(0.019)	(0.038)	(0.77)	(0.115)	
Unsolicited rating	3	13	2	11	0	29
	(0.103)	(0.448)	(0.069)	(0.379)	(0)	
Sum	5,990	8,774	700	8,915	2,034	26,413
	(0.227)	(0.332)	(0.027)	(0.338)	(0.077)	
Panel II: Region	Asia Pacific	Australia/ NZ	Canada	Europe/Mid East/Africa	Latin America	Sum
Survivors:						
Solicited rating	2,632	2,410	2,056	10,354	1,949	19,401
	(0.136)	(0.124)	(0.106)	(0.534)	(0.100)	
Unsolicited rating	2,366	113	178	3,871	246	6,774
	(0.349)	(0.017)	(0.026)	(0.571)	(0.036)	
Defaults:						
Solicited rating	22	5	34	70	78	209
	(0.105)	(0.024)	(0.163)	(0.335)	(0.373)	
Unsolicited rating	22	0	0	5	2	29
	(0.759)	(0.000)	(0.000)	(0.172)	(0.069)	
Sum	5,042	2,528	2,268	14,300	2,275	26,413
	(0.191)	(0.096)	(0.086)	(0.541)	(0.086)	

Table 2: Mean year-end ratings

Ratings are translated into a numerical rating scale ranging from 1 (AAA) to 18 (CC to C). Panel I shows the mean numerical rating level at a certain point in time for all surviving (Panel Ia) and defaulting (Panel Ib) financial firms with solicited and unsolicited ratings. Panel II provides the respective information for non-financial firms.

| | Rating as of year-end | | | | | | | | | | | |
	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	Sum	Mean
Panel I: Financials												
Ia) Survivors:												
Number of solicited ratings	645	796	878	891	956	1,039	1,064	1,105	1,178	1,255	9,807	
Mean solicited rating	4.68	5.07	5.21	5.36	5.41	5.61	6.07	6.39	6.48	6.45		5.78
Number of unsolicited ratings	50	593	682	760	735	749	654	530	461	401	5,615	
Mean unsolicited rating	10.38	9.54	9.56	9.36	9.22	9.27	9.65	9.91	9.79	9.56		9.52
Ib) Defaults:												
Number of solicited ratings	0	6	2	2	6	5	2	0	0	1	24	
Mean solicited rating	0.00	14.50	16.50	16.00	12.33	14.20	13.00	0.00	0.00	16.00		14.13
Number of unsolicited ratings	0	0	2	5	5	2	3	0	1	0	18	
Mean unsolicited rating	0.00	0.00	16.00	15.00	13.80	14.00	15.00	0.00	17.00	0.00		14.78
Panel II: Non-financials												
IIa) Survivors:												
Number of solicited ratings	498	639	760	838	926	1,034	1,086	1,172	1,288	1,353	9,594	
Mean solicited rating	7.01	7.85	8.45	8.69	8.52	8.41	8.64	8.90	9.10	9.31		8.64
Number of unsolicited ratings	0	46	71	114	151	152	150	142	173	160	1,159	
Mean unsolicited rating	0.00	9.33	9.55	10.25	10.28	10.40	10.36	9.09	8.68	8.44		9.58
IIb) Defaults:												
Number of solicited ratings	1	4	22	12	36	71	24	4	5	6	185	
Mean solicited rating	17.00	12.00	15.64	15.75	15.44	15.21	15.58	16.25	15.00	14.17		15.31
Number of unsolicited ratings	0	0	1	2	1	2	2	2	1	0	11	
Mean unsolicited rating	0.00	0.00	15.00	17.00	15.00	16.00	18.00	16.00	17.00	0.00		16.45

Table 3: Ex-post analysis of empirical default observations using a pooled logit regression approach

The dependent variable takes the value one in the case of a default and zero if the firm has survived the next one-year realization period. The independent variables are: *unsolicited* as a dummy for rating solicitation (1 for unsolicited ratings, 0 otherwise), *rating* indicating the rating level in the estimation period expressed as a numerical value for company i between 1 (AAA) and 18 (CC to C) that is followed by the realization period in which the default status is recorded, and *country rating* as the country-specific sovereign rating. We further use sectoral and regional classifications and year dummies. The results for the latter are omitted. In regression model I, industrials and Canadian firms serve as references. In regression models II to IV we dropped the respective variable if we did not observe any default in a certain region or year. For utilities and other financial firms we were not able to perform a regression due to data limitations. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Total sample		(II) Industrials		(III) Banks		(IV) Insurance companies	
	Coefficient	p value	Coefficient	p value	Coefficient	p value	Coefficient	p value
Intercept	-13.995	0.000	-14.166	0.000	-17.614	0.000	-13.987	0.000
Unsolicited	-1.296	0.000	-1.333	0.005	-1.979	0.006	-0.659	0.357
Rating level	0.700	0.000	0.720	0.000	0.722	0.000	0.686	0.000
Country rating	-0.086	0.000	-0.068	0.001	-0.089	0.335	-0.340	0.069
Bank	-0.760	0.014						
Insurance	-0.425	0.186						
Other financial	1.134	0.011						
Utility	0.650	0.030						
Asia Pacific	0.440	0.124	0.282	0.454			0.900	0.315
Australia/New Zealand	-0.215	0.686	-0.056	0.924				
Europe/Mid East/Africa	-0.190	0.400	-0.307	0.203	1.534	0.162	-1.497	0.094
Latin America	1.168	0.000	0.957	0.002	2.899	0.006		
McFadden adj. R^2	0.442		0.390		0.364		0.367	
Number of observations	26,413		8,915		5,990		8,774	

Table 4: Ex-post analysis of empirical default observations using a pooled logit regression approach with additional outlook and CreditWatch rating information

The dependent variable takes the value one in the case of a default and zero if the firm has survived the next one-year realization period. The independent variables are: *unsolicited* as a dummy for rating solicitation (1 for unsolicited ratings, 0 otherwise), *rating level modified* indicating the rating level in the estimation period expressed as a numerical value for company i between 1 (AAA) and 18 (CC to C) that is followed by the realization period in which the default status is recorded. The rating level is increased (decreased) by two numerical rating notches in the case of a negative (positive) CreditWatch and increased (decreased) by one numerical rating notch in the case of a negative (positive) outlook. *Country rating* is the country-specific sovereign rating. We further use sectoral and regional classifications and year dummies. The results for the latter are omitted. In regression model I, industrials and Canadian firms serve as references. In regression models II to IV we dropped the respective variable if we did not observe any default in a certain region or year. For utilities and other financial firms we were not able to perform a regression due to data limitations. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Total sample		(II) Industrials		(III) Banks		(IV) Insurance companies	
	Coefficient	p value	Coefficient	p value	Coefficient	p value	Coefficient	p value
Intercept	-14.146	0.000	-14.430	0.000	-17.579	0.000	-13.873	0.000
Unsolicited	-0.877	0.001	-0.979	0.039	-1.686	0.025	-0.137	0.843
Rating level modified	0.701	0.000	0.731	0.000	0.672	0.000	0.647	0.000
Country rating	-0.079	0.000	-0.060	0.002	-0.077	0.430	-0.335	0.068
Bank	-0.795	0.013						
Insurance	-0.573	0.076						
Other financial	1.020	0.027						
Utility	0.596	0.034						
Asia Pacific	0.368	0.215	0.268	0.486			0.991	0.271
Australia/New Zealand	-0.170	0.751	-0.020	0.973				
Europe/Mid East/Africa	-0.087	0.709	-0.254	0.313	2.122	0.080	-1.433	0.112
Latin America	1.099	0.000	0.867	0.006	3.330	0.005		
McFadden adj. R^2	0.460		0.417		0.365		0.356	
Number of observations	26,413		8,915		5,990		8,774	

Table 5: Ex-post analysis of empirical default observations using a pooled logit regression approach with default frequency instead of rating level information

The dependent variable takes the value one in the case of a default and zero if the firm has survived the next one-year realization period. The independent variables are: *unsolicited* as a dummy for rating solicitation (1 for unsolicited ratings, 0 otherwise), *default frequency* indicating S&P's smoothed, long-term average default frequency corresponding to the rating class that is followed by the realization period in which the default status is recorded. *Country rating* is the country-specific sovereign rating. We further use sectoral and regional classifications and year dummies. The results for the latter are omitted. In regression model I, industrials and Canadian firms serve as references. In regression models II to IV we dropped the respective variable if we did not observe any default in a certain region or year. For utilities and other financial firms we were not able to perform a regression due to data limitations. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Total sample		(II) Industrials		(III) Banks		(IV) Insurance companies	
	Coefficient	p value	Coefficient	p value	Coefficient	p value	Coefficient	p value
Intercept	-6.236	0.000	-5.807	0.000	-12.973	0.000	-8.219	0.000
Unsolicited	-0.226	0.413	-0.736	0.140	-1.315	0.072	0.659	0.369
Default frequency	21.627	0.000	21.200	0.000	11.830	0.084	28.800	0.000
Country rating	-0.029	0.169	-0.056	0.042	0.285	0.004	-0.418	0.143
Bank	-1.395	0.000						
Insurance	-1.604	0.000						
Other financial	0.132	0.740						
Utility	-0.458	0.055						
Asia Pacific	-0.680	0.035	-0.619	0.136			1.307	0.124
Australia/New Zealand	-1.542	0.002	-1.241	0.013				
Europe/Mid East/Africa	-0.682	0.003	-0.678	0.005	1.835	0.120	-1.345	0.097
Latin America	0.822	0.003	0.900	0.007	2.661	0.025		
McFadden adj. R^2	0.364		0.322		0.294		0.264	
Number of observations	26,413		8,915		5,990		8,774	

Table 6: Ex-post analysis of alternative outcome measures using a random effects panel approach

The sample comprises only banks. Panel I refers to the Merton PD of the next one-year realization period as a dependent variable. Accounting data for computing the Merton PD are taken from Compustat, market values from DataStream. Panel II (III) refers to the bank-specific z-score in the second (third) year following the original rating as a dependent variable. Accounting data are taken from Compustat. The independent variables are: *unsolicited* as a dummy for rating solicitation (1 for unsolicited ratings, 0 otherwise), *rating* indicates the rating level of the estimation period expressed as numerical value for company i between 1 (AAA) and 18 (CC to C) that is followed by the respective realization period. The *modified rating* level is increased (decreased) by two numerical rating notches in the case of a negative (positive) CreditWatch and increased (decreased) by one numerical rating notch in the case of a negative (positive) outlook. *Default frequency* indicates S&P's smoothed, long-term average default frequency corresponding to the rating class of the estimation period's rating. *Country rating* is the country-specific sovereign rating from S&P. We further use regional classifications and year dummies. Results for both are omitted. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Coefficient	p value	(II) Coefficient	p value	(III) Coefficient	p value
Panel I: Merton PD, one-year horizon						
Intercept	0.000	0.992	-0.006	0.177	0.007	0.136
Unsolicited	-0.010	0.002	-0.011	0.001	-0.009	0.004
Rating level	0.002	0.041				
Rating level modified			0.003	0.000		
Default frequency					0.143	0.180
Country rating	0.002	0.010	0.001	0.071	0.002	0.000
No. of observations	1,537		1,537		1,537	
Panel II: Z-Score, two-year horizon						
Intercept	3.265	0.000	3.343	0.000	2.774	0.000
Unsolicited	0.276	0.031	0.264	0.037	0.131	0.314
Rating level	-0.136	0.000				
Rating level modified			-0.144	0.000		
Default frequency					-5.543	0.000
Country rating	0.036	0.057	0.040	0.022	-0.016	0.349
No. of observations	1,483		1,483		1,483	
Panel III: Z-Score, three-year horizon						
Intercept	3.046	0.000	3.169	0.000	2.689	0.000
Unsolicited	0.301	0.059	0.303	0.056	0.197	0.216
Rating level	-0.089	0.003				
Rating level modified			-0.115	0.000		
Default frequency					-6.739	0.002
Country rating	0.006	0.802	0.022	0.295	-0.014	0.512
No. of observations	1,287		1,287		1,287	

Table 7: Ex-post analysis of the influence of country-specific bank opaqueness using a random effects panel approach

The sample comprises only banks. The dependent variable is the Merton PD of the next one-year realization period. Accounting data for computing the Merton PD are taken from Compustat, market values from DataStream. *Unsolicited* is a dummy for rating solicitation (1 for unsolicited ratings, 0 otherwise). *Opaqueness index* is calculated using World Bank survey data from 1999, 2002, and 2006. We map the respective country-specific bank opaqueness index from 1999 to our rating data for the period 1996-2000, the index from 2002 to the rating data for 2001-2003, and the index from 2006 to the rating data for 2004-2005. *Unsol*opaqueness* is an interaction term of the unsolicited dummy and the opaqueness index. The remaining independent variables are: *rating* indicates the rating level of the estimation period expressed as numerical value for company i between 1 (AAA) and 18 (CC to C) that is followed by the respective realization period. The *modified rating* level is adjusted by increasing (decreasing) the rating level by two numerical rating notches in the case of a negative (positive) CreditWatch. In the case of outlooks we use an adjustment of plus (minus) one rating grade for negative (positive) outlooks. *Default frequency* indicates S&P's smoothed, long-term average default frequency corresponding to the rating class of the estimation period's rating. The *country rating* is the country-specific sovereign rating from S&P. We further use regional classifications and year dummies. Results for the latter are omitted. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Coefficient	(I) p value	(II) Coefficient	(II) p value	(III) Coefficient	(III) p value
Intercept	0.038	0.005	0.028	0.036	0.048	0.000
Unsolicited	-0.005	0.840	-0.004	0.867	-0.007	0.765
Opaqueness index	-0.004	0.002	-0.003	0.006	-0.004	0.001
Unsol*opaqueness	-0.001	0.831	-0.001	0.781	0.000	0.921
Rating level	0.002	0.054				
Rating level modified			0.003	0.000		
Default frequency					0.146	0.162
Country rating	0.002	0.006	0.001	0.046	0.002	0.000
Asia Pacific	0.004	0.168	0.003	0.266	0.007	0.033
Australia/NZ	-0.002	0.237	-0.001	0.761	-0.002	0.339
Europe/Mid East/Africa	0.001	0.569	0.002	0.262	0.002	0.408
Latin America	0.002	0.841	0.004	0.728	0.005	0.639
No. of observations	1,537		1,537		1,537	

Table 8: Rating differences between S&P and Fitch, and between S&P and Moody's

Negative (positive) rating differences (measured in notches) indicate that S&P ratings reflect less (more) risk than the rivals' ratings. Rating data for Fitch were extracted from Bankscope, rating data for Moody's from Bloomberg. Figures in brackets provide proportions relative to column totals.

| Rating difference | Unsolicited S&P ratings vs. | | Solicited S&P ratings vs. | |
in notches	Fitch	Moody's	Fitch	Moody's
<-4	5	0	0	1
	(0.016)	(0.000)	(0.000)	(0.001)
-4	0	0	2	6
	(0.000)	(0.000)	(0.001)	(0.003)
-3	2	3	8	10
	(0.006)	(0.021)	(0.004)	(0.006)
-2	20	9	22	60
	(0.064)	(0.062)	(0.012)	(0.034)
-1	26	20	104	195
	(0.083)	(0.137)	(0.058)	(0.112)
0	37	19	621	699
	(0.119)	(0.130)	(0.347)	(0.401)
1	80	28	776	559
	(0.256)	(0.192)	(0.433)	(0.321)
2	78	45	201	161
	(0.250)	(0.308)	(0.112)	(0.092)
3	37	9	50	30
	(0.119)	(0.062)	(0.028)	(0.017)
4	20	2	4	15
	(0.064)	(0.014)	(0.002)	(0.009)
>4	7	11	3	8
	(0.022)	(0.075)	(0.002)	(0.005)
Sum	312	146	1,791	1,744

Table 9: Ex-post analysis of the influence of bank-specific opaqueness using a random effects panel approach

The sample comprises only banks. The dependent variable is the Merton PD of the next one-year realization period. Accounting data for computing the Merton PD are taken from Compustat, market values from DataStream. Specification (I) uses the numerical rating level, (II) the CreditWatch/Outlook modified numerical rating level, and specification (III) employs the empirical default frequency. In Panel I (II), *Unsol*Split rating* is the interaction term between the unsolicited variable for rating solicitation (1 for unsolicited ratings, 0 otherwise) and the observation of a split rating vis-à-vis the Fitch (Moody's) rating. This interaction term is tested against the reference *Unsol*No split rating*. In Panel III (IV), *Unsol*Rating diff* is an interaction term between the unsolicited variable and the absolute numerical rating difference between S&P's and Fitch's (Moody's) rating. Rating data for Fitch are extracted from Bankscope, rating data for Moody's from Bloomberg. The remaining independent variables are: interaction terms between solicited rating and split (no split) ratings (Panels I and II) and the rating difference to Fitch (Panel III) or Moody's (Panel IV). All other control variables are as before. In Panel V, we proxy bank opaqueness by the number of analysts covering the bank with their earnings' forecasts. We use IBES data for that purpose. We define analyst coverage as being high if the number of analysts covering a firm is above the median, and as low if it is below the median and form four interaction terms the unsolicited dummy. Results for the intercept and the additional controls are omitted. P values are derived via robust standard errors which are adjusted for clustering on company level (Williams (2000)).

Independent variable	(I) Coefficient	p value	(II) Coefficient	p value	(III) Coefficient	p value
Panel I: Split rating vis-à-vis Fitch						
Unsol*Split rating	0.002	0.612	0.002	0.620	0.001	0.667
Sol*Split rating	0.003	0.535	0.003	0.470	0.001	0.630
Sol*No split rating	0.004	0.405	0.004	0.414	-0.001	0.830
No. of observations	835		835		835	
Panel II: Split rating vis-à-vis Moody's						
Unsol*Split rating	-0.028	0.382	-0.029	0.355	-0.027	0.430
Sol*Split rating	-0.012	0.684	-0.012	0.684	-0.014	0.652
Sol*No split rating	-0.013	0.656	-0.012	0.678	-0.016	0.598
No. of observations	691		691		691	
Panel III: Rating difference vis-à-vis Fitch						
Unsolicited	0.009	0.121	0.010	0.112	0.011	0.017
Rating difference	0.002	0.305	0.002	0.229	0.004	0.074
Unsol*Rating diff	-0.006	0.030	-0.007	0.020	-0.007	0.009
No. of observations	835		835		835	
Panel IV: Rating difference vis-à-vis Moody's						
Unsolicited	-0.007	0.410	-0.007	0.406	-0.007	0.507
Rating difference	0.001	0.565	0.000	0.836	0.002	0.237
Unsol*Rating diff	-0.004	0.422	-0.004	0.333	-0.002	0.753
No. of observations	691		691		691	
Panel V: Analyst coverage						
Unsolicited*Low	-0.038	0.018	-0.041	0.011	-0.036	0.038
Solicited*Low	-0.002	0.808	0.001	0.931	-0.002	0.749
Solicited*High	0.008	0.115	0.010	0.035	0.007	0.126
No. of observations	699		699		699	

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