The Information Content and Redistribution Effects of State and Municipal Rating Changes in Mexico

Purpose and strategy of the paper

The paper examines the impact of rating events regarding state and municipal bonds. The authors have two goals: (i) to distinguish between the signaling hypothesis and the wealth redistribution hypothesis; (ii) to examine the time series properties of States and municipial bond offerings.

They try to achieve these goals by using a time-series model rather than an event study. They use what they call a „market model" with the EGARCH model (Nelson 1991) specified as an EGARCH(1,1)-in-Mean (p. 10). The equation for bond returns (eq. 1) includes a risk aversion parameter as well as (eq. 2) two dummies for rating changes (one for the issuer, one for others). Regarding the error term they assume an iid Generalized Error Distribution featuring a Gamma function for flexible modeling tail shape.

Results

Estimating the main equation for bond returns they find a negative (and highly significant[1]) coefficient for the variance term. They interpret this as a negative correlation between risk premium and conditional variance (which is not how I read the coefficient). They also confuse me by claiming that their finding is (i) „in line with the seminal paper by Nelson (1991)", but (ii) still „counterintuitive", yet (iii) not necessarily in contradiction to Backus and Gregory (1993) as well as somes other literature (p. 16-17).

Further they find low Betas (correlation of state and municipial bond returns with market risk).

Finally, on their main issue, they find that rating changes mostly confirm the signaling hypothesis (ratings convey some information), but that often enough, the contrary redistribution effect overrides the signaling effect. They also find an impact on higher moments of the return distributions.

Comments

First of all, the topic and setup of the paper is interesting. I did also benefit from the descriptive part. There is an interesting observation on the „trust fund effect" in Mexico, which I would expect to cause some negative trend to ratings, but a positive trend to bond returns in the period under examination. I do not understand why the effect is introduced but not used in the empirical part of the paper.

Discussiong the bank regulation motive for having a rating the authors might mention the likely influence of the discussions on Basel 2 capital standards in the late nineties.

One problem I had was with the presentation of the **theoretical background**. I do understand the signaling hypothesis (good borrowers are keen to get ratings in order to signal their quality). But I do not understand the asset wealth redistribution hypothesis. I read the explanation on pp. 8 and 19 several times. I am also familiar with the elements, like shareholders' incentives to issue debt in order to redistribute wealth from existing debtholders to shareholders. Yet, with all best intentions, I fail to understand the redistribution story as it is explained in the paper.

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1   Note that the authors definition asterisks as *=1%, **=5% and ***=10% significance level, rather than the other way round.

The **empirical approach** is basically plausible if one thinks that fat tails and conditional volatility are an important phenomena in the present context. (A bit more discussion of tail shape of bond returns and empirical alternatives might have been helpful, though) .

But let me focus on the main critical issues:

Using equation (1), the authors perform some initial analysis of the data. They find that bond returns on *four* state offerings (among a total of 40 issues, see p. 5) „converge satisfactorily and do not exhibit correlation in the residuals nor squared residuals" (p. 13). Rather than rejecting the model, they reject the other data and restrict their further empirical analysis to the four bond issues that behaved well. I tend to consider this the crucial issue in the paper.

Further, I cannot judge the pros and cons of explaining the stochastic process for bond returns *and* the impact of rating events in one model. However, as far as rating changes may affect not only returns (the dependent variable) but also the volatility term (an explanatory variable) the method may call for some further explanation for the benefit of non-specialists like myself.

At one point, the authors start to refer to *expected* returns. They do not explain how they observe expected returns, though. Does expected refer to estimated returns from the model? Or does the term mean average returns (the authors refer to table 2)? On this issue, the reader would need some clarification.

The results are to a large part surprising (positively speaking) or implausible (negative view). Estimated bond returns are close to zero, the variance being 30 times higher than the returns. Would this suggest that investors in these bonds are not only satisfied with a zero (nominal) return but would also be risk-neutral? Or, given that the volatility parameter has a negative sign, do investors happily forego return, because they get some risk instead??? Note that the negative volatility parameter is all the more puzzling since the authors assume that the distribution of returns may have fat tails.

Final remark

While I am not a specialist regarding the particular empirical methods used, I do think it
is the authors' job to explain their paper in a way that a typical economist
can understand the more tricky issues it raises.