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Decision on submission to Finance Research Letters

1 message

Finance Research Letters <em@editorialmanager.com>
Reply-To: Finance Research Letters <support@elsevier.com>
To: Kai Wu <wukai8759@gmail.com>

Mon, Aug 14, 2023 at 3:46 AM

Manuscript Number: FRL-D-23-01902

Bankruptcy Court Establishment and Corporate Risk-Taking

Dear Dr. Wu,

Thank you for submitting your manuscript to Finance Research Letters.

I have completed my evaluation of your manuscript. The reviewers recommend reconsideration of your manuscript following minor revision and modification. I invite you to resubmit your manuscript after addressing the comments below. Please resubmit your revised manuscript by Sep 03, 2023.

When revising your manuscript, please consider all issues mentioned in the reviewers' comments carefully: please outline every change made in response to their comments and provide suitable rebuttals for any comments not addressed. Please note that your revised submission may need to be re-reviewed.

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Finance Research Letters values your contribution and I look forward to receiving your revised manuscript.

Kind regards,

Samuel Vigne

Editor-in-Chief

Finance Research Letters

Editor and Reviewer comments:

Reviewer #1: 1. On page 1, the authors claim that the number of bankruptcy courts in municipalities increased rapidly after 2014. The authors should add reference for this claim, or could include the graph to show the trend.

2. The authors use the sample of 2,889 firms over 17 years. Are they unique firms or firms that have been listed through the sample years? If it is the latter case, I could expect around 49,000 firm-year observation. After excluding financial firms, they authors obtain around 30,000 firm-year observation. Clarify if 2,889 firms are the unique firms. If additional filters were applied, clarify all. Footnote would be sufficient.

3. Include all primary variables in the summary statistics table (e.g., Risk2, KZ, Tobin's, etc.).

4. In Table 3, the coefficient estimated on CASH is positive. I expected the negative correlation between cash holding and risk taking. Provide brief insights on the positive association or if the sign of the coefficient is consistent with what

prior studies reported, the authors could include the reference instead.

5. For robustness, check if results continue to hold when standard errors are clustered at the firm level. Footnote would be sufficient.

6. Do results hold when further accounting for some managerial risk taking traits (e.g., Age, Gender, Tenure, and Ownership)? If so, report the results. Footnote would be sufficient.

7. To arrive the conclusion that sample firms in the regions with bankruptcy courts are likely to take excessive risk, I suggest the author utilize alternative proxy for the corporate risk taking. I understand earnings volatility gauges the uncertainty, and excessive risk taking might be one of many drivers for increasing earnings volatility. However, still it is concerned that increase in earnings volatility is the director receipt for the excessive risk taking. I would suggest that for the robustness check, the author employee more direct measure of corporate risk taking measures (if feasible). If not, include more references to support the validity of the risk taking proxy the authors mainly rely on.

Reviewer #2: Bankruptcy Court Establishment and Corporate Risk-Taking

Following are my comments

1. The first paragraph of the research paper is a strong statement without any relevant citations. The prior literature suggests that only secured creditors prefer liquidation over unsecured creditors due to collateral and absolute priority rights. Therefore, liquidation doesn't need to be advantageous. Even the firm might be saved through a revival scheme, but secured creditors might force the firm into bankruptcy. Furthermore, increased risk-taking does not necessarily lead to higher economic benefits because managers might start gambling. Therefore, I think the author should rephrase this statement with proper citation.

2. In the introduction section, the author has not provided any information on why we need to study the impact of the bankruptcy court on corporate risk-taking in China. There is sufficient literature on emerging, developed, and cross-country markets (Singh, Chauhan, and Jادیappa, 2022; Acharya et al., 2011; Favara et al., 2017). Therefore, there should be an explanation for why do author should study China.

3. There is no specific section about China's legal environment and judicial system. This might help the readers to understand the Chinese market better. Further, there should be a comparison between the judicial system before the establishment of the bankruptcy court and after the establishment of the bankruptcy court.

4. While differentiating the author's paper from Singh, Chauhan, and Jادیappa's (2023) paper, the author has argued that he has examined increased creditor rights, but Singh, Chauhan, and Jادیappa (2023) examined increased debtor rights. The author has misunderstood the paper because Indian bankruptcy reform has increased creditor rights except for the focus on reorganization provision. Therefore, Singh, Chauhan, and Jادیappa's (2023) paper has mentioned the balance between creditor and debtor rights.

5. The author has given both sides of the argument under the hypothesis section. However, the author has not mentioned any specific hypothesis. I think the author must mention the hypothesis section. Even the hypothesis is not adequately motivated because the author has not given any specific instance of the Chinese bankruptcy system.

6. The author has taken the standard deviation of ROA after adjusting for industry ROA as a measure of risk-taking. This calculation is based on a 3-year window. I think this is not an appropriate measure to examine the relationship because even after the establishment of the court, the risk-taking variable will incorporate before court establishment deviation while calculation, which might not give the correct picture of risk-taking (First rolling SD in the post reforms period is based on two pre-reforms observations). The author might use quarterly ROA standard deviation to measure the risk. Further, I did not understand the rationality for adjusting for industry ROA, and even the author has not given justification for that.

7. There is a difference in the number of observations in summary statistics (Table 1) and Bankruptcy Court Establishment and Risk-taking Behavior (Table 3). Even though the author mentioned that it removed the missing value observations. It must be the same in both the table.

8. In the cross-sectional heterogeneity section (Table 5), the author mentioned that it classified firms as low and high based on median value. There is no more detail about this classification. I think this classification is time-varying in nature. In my view, it is problematic in the causal relationship because establishing a bankruptcy court might have increased the financial constraint and market valuation. Therefore, it is advisable that this classification should be based on the year before the establishment of the bankruptcy court.

9. The author has interpreted the cross-sectional heterogeneity section (Table 5) incorrectly. For example, the author has written, "Column (2) shows that the coefficient of BC in the sample of firms with financial constraints is significantly negative, suggesting that the link between bankruptcy court establishment and corporate risk-taking is more pronounced in firms with greater financing constraints". In the above statement, the author says that corporate risk-taking is more pronounced in firms with greater financing constraints, which is incorrect. These kinds of statements can be made only after statistical examination of low and high financial constraints firms through an interaction term. Similarly, the author makes the same mistake in market valuation and local law. Before making such statements, the author should do statistical tests, preferably using an interaction term. The author can use either Seemingly unrelated regression or the Wald test to do that. Even double interaction can be helpful.

10. The author may use a specific table on the variable to conserve space. The author has explained the empirical design, which has taken up lots of space. I think apart from the dependent and BC variables, the author can make a table for other variables.

11. I did not find any novelty in the paper except the methodology. The prior literature has already proven these findings in other emerging markets.

12. In the regression equation, the author has controlled for the required variables. However, leverage is an important

factor affecting the corporate risk-taking of the firm. It is helpful if the analysis includes firm leverage. Furthermore, the author has mentioned the staggered adoption of bankruptcy court. But there are no details of the date.

I hope this helps

All the best

More information and support

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