Review of: "Corporate giving as earnings quality signal: some new evidence from Nigeria"

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Potential competing interests: No potential competing interests to declare.

The authors aim to analyze the effect on CSR disclosure on earnings quality. The thematic is relevant and the work is well written. Although, some items need to be improved.

In the introduction the authors say that before the interest was on the impact of CSR on performance and now it is on the impact of CSR on earnings quality. Although, the reference used to call the need to study of the second impact is from 2018 and of the first impact is 2020. I recommend the authors to include more recent references (since there is no reference from the year od 2021 and 2022) to explain the relevance of the paper analysis and the need to explore more it.

A similar study was done also to Nigeria by Uyagu and Dabor (2017) who study 52 listed firms from 2001 to 2015. The authors need to stress more its differences and innovations.

Through the paper the authors say that will analyze CSR but in fact what is analyzed is corporate giving. I can understand that it may be difficult to use other proxy of CSR, but the authors should realize that donations are not enough to measure CSR. Maybe the authors can do a robustness analysis using other variable.

Moreover, the authors say that will analyze the impact on earnings quality but in fact analyze the impact on earnings management. As the authors say earnings quality has several characteristics, and accruals is only one of them, but is not the same to analyze earnings quality. Maybe the authors can change the aim of the study to accruals quality that is what is analyzed in the study. Regarding discretionary accruals, it is not clear why the authors use Kothari et al model since several models can be used. The formula presented in appendix B is not clear as it not clear that the ratios are divided by total assets.

Model (3) and (4) appear in appendix B but should appear in separate as this analysis is done after the explanation of the variables. Till the moment the authors say to analyze the appendix the first model was not presented yet.

Regarding the control variables, the explanation in page 8 is quite confused and the idea of the reader is different when appendix A is analyzed. Here, why the authors choose the Zmijewski model to most common is Z-score.

In the abstract the authors say 300 firm-observations but in fact the final number of observations is 281.

I also recommend the authors to include the significance in the correlation matrix and present the VIF values (are only commented).

The conclusion and contributions are clear.

As I said before, the work is relevant but need to have some improvements to assure better quality. I wish the authors success for publication.