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PEER REVIEW REPORT

Peer review report for:

Zanandrea, G., Bitencourt, C., Müller, H. (2024). A (re)view of dynamic capabilities: Origins and future developments. *RAE-Revista de Administração de Empresas*, *64*(4), e2023-0216. http://dx.doi.org/10.1590/S0034-759020240401

Disclaimer: The content of the Peer Review Report is the full copy of reviewers and authors' reports. Typing and punctuation errors are not edited.

Reviewer: Edmundo Inácio Júnior . Universidade Estadual de Campinas, Faculdade de Ciências Aplicadas, Campinas, SP, Brazil. The second reviewer did not authorize disclosure of their identity and peer review report.

ROUND 1

Reviewer 1 Report

Reviewer: Edmundo Inácio Júnior

Date review returned: 23-Jun-2023

Recomendation: Major Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

I declare that there is no conflict of interest.

Comments to the Author

Thank you for the opportunity, please take the observations as suggestions for improving the article.

As for the data source: I suggest that you expand the data source to the publications in WoS, as well as, if they are Brazilian authors, to the national databases (Scielo and Spell), because there are good journals that exist in it and are neither in WoS nor Scopus.

As for the query criteria (parameters): search for articles that have cited TPS (1997) or EM (2000) for a reduction much with strong assumptions, because not every article that cites these two really deals with articles that the central theme has to do with dynamic capabilities. Therefore, the use of qualifiers (key words) such as "dynamic capabilit", for example, to check Title or key words, would be of great value in returning articles that focus on the theme in question.*

The division into six subgroups with the criterion of generating a similar number of articles per group does not seem interesting to me either. A cluster analysis, following some criteria (lenses/dimensions) coming from RSL, could be more useful and generate more knowledge, especially regarding the identification of latent patterns in the data.

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Another approach that could have been used was to run LDA with the 3 large groups: i) articles that cited only TPS(1997)= 7.120; ii) articles that cited only EM(2000)= 3.718; iii) articles that cited both= 2.396.

Analytical phase: separetared arbitrarily to generate groups with approximately 16xx to 19xx articles

Table 1: There is not even 1 paragraph discussing the numbers in table 1 and their practical or theoretical significance.

page 4, lines 40/51: the explanation of the concept/measure of perplexity was not clear, it could be better explored and exemplified.

page 4, lines 59/60: "... Coherence values range between 0 and 1"

page 5, lines 05/06: "... the practice has indicated values between 3 and 7 as being often found..."

Is it 0 to 1 or 0 to 10?

page 5, lines 46: Should readers assume that beta is the symbol representing the coherence measure, which ranges from 0 to 1?

page 8 to 10: TPS (1997); followers from 2013 to 2016: as the division of articles by periods follows a criterion of only generating a similar number of articles by groups, it occurred to have in this group 2 nodes that converge the themes of the previous group: Knowledge management and Firm Performance.

The section Relational dimension - interpretative phase , I believe, was harmed by the methodological choice of how the articles were separated in the groups. If the previous separation had been by another category than generating homogeneous groups in relation to the number of articles, the relations could have been different. As a form of triangulation, the authors could have verified the network of relationships of these 10,838 by their citations. Traditional applications do this. This way we would see if the themes would converge among the articles cited with the articles' keywords and titles.

In the section SUGGESTIONS FOR FUTURE RESEARCH the suggestions seem to have come from specific articles that were cited, i.e., they are suggestions from these cited authors and not suggestions coming from the LDA or interpretive analysis.

Reviewer 2 Report

The reviewer did not authorize the disclosure of the peer review report.

ROUND 2

Reviewer 1 Report

The reviewer did not authorize the disclosure of the peer review report.

Reviewer 2 Report

Reviewer: Edmundo Inácio Júnior
Date review returned: 24-Sep-2023
Reviewer: Reject

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Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

There is no conflict of interest.

Comments to the Author

Very little was (almost nothing) added regarding my suggestions on the way of the analysis of made, which were not few. I noticed a great resistance to look again at the data, run new analyses and even have them reported, even if their results were worse than the current ones presented, thus demonstrating a willingness to investigate new ways of analyzing the same data.

In that sense, I believe Mr. Editor can better condition to evaluate if it is the case of approval or not. I did not put in any new suggestions, as the previous ones weren't tested and/or used.

Thanks for the opportunity!

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PEER REVIEW REPORT

Peer review report for:

El Ghossain, A. M., Vieira, A. D. A. S. N., Godoi, A. S., & Mirapalheta, G. C. (2024). Influência de gênero nas recomendações de ações por analistas de Equity Research. *RAE-Revista de Administração de Empresas*, 64(4), e2023–0182. https://doi.org/10.1590/S0034-759020240403

Disclaimer: The content of the Peer Review Report is the full copy of reviewers and authors' reports. Typing and punctuation errors are not edited.

Reviewer:

Rogiene Batista dos Santos , Fundação Getulio Vargas, Escola de Administração de Empresas de São Paulo, São Paulo, SP, Brazil. Andréia Ribeiro da Luz , Pontifícia Universidade Católica do Paraná, Escola de Negócios, Curitiba, PR, Brazil, and she did not authorize the disclosure of their peer review report. The third reviewer did not authorize disclosure of their identity.

ROUND 1

Reviewer 1 Report

Reviewer: Rogiene Batista dos Santos

Date review returned: 12-Jun-2023

Recommendation: Major Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None

Comments to the Author

Gostaria de primeiro parabenizar os autores pelo esforço em realizar este trabalho. Acredito que eles buscaram fazer o melhor. Assim, farei considerações de forma que eles consigam colocar este artigo em outro patamar. O artigo apresenta deficiências que precisam ser sanadas. Certamente, os autores têm condições de trabalhar cada um dos pontos a serem apresentados. O objetivo de pesquisa: "testar se existe influência de gênero nas recomendações de Equity Research" é relevante e tem implicações práticas. Assim, esse estudo pode fornecer novos insights tanto para acadêmicos quanto para profissionais da área.

Pontos fortes

1. *Tema relevante e com implicação prática;*
2. *Amostra e quantidade de observações;*
3. *Tema que demanda por pesquisas.*

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Pontos fracos

1. *No resumo os autores afirmam: "encontrou-se um efeito estatisticamente significativo na direção oposta ao previsto: recomendações feitas por mulheres tenderam a ser mais otimistas". Por que se esperaria que as mulheres seriam menos otimistas?*
2. *No resumo os autores afirmam: "Possíveis explicações seriam uma menor propensão ao conflito por parte de mulheres ou um possível efeito de auto-seleção no mercado financeiro". Não compreendi o que os autores quiseram dizer.*
3. *Não consegui acompanhar essa conclusão apresentada no resumo: "Uma regressão logística multinominal demonstrou que o efeito desaparece ao se controlar pelo setor de cobertura e instituição a que o analista pertence, ou seja, sugere que diferença se deve mais ao fato de as mulheres, que ainda respondem por apenas 12,8% do total de recomendações analisadas, se concentrarem na cobertura de setores específicos, como consumo." Se o controle foi de setor por que isso teria relação com o gênero?*
4. *O objetivo apresentado no resumo está diferente do apresentado na introdução.*
5. *Tanto no resumo quanto na introdução, os autores precisam deixar claro qual foram os resultados encontrados e dizer qual é a relevância deles.*
6. *Será que a relação de risco e retorno para mulheres e homens não tem mudado ao longo do tempo? Hoje as mulheres têm acesso a estudo e têm renda. Diferente do que era no século passado. Isso talvez possa explicar os resultados encontrados pelos autores.*
7. *Quais são as hipóteses desta pesquisa? Como elas foram testadas?*
8. *Na minha opinião, o modelo de regressão logística não captura o efeito que os autores desejam capturar. O modelo apresenta probabilidades e não causalidade*
9. *Se os autores tiveram acesso aos nomes doa analistas, como foi tratada a questão da LGPD?*
10. *Seria interessante apresentar os dados do Gráfico 2 por gênero.*
11. *Sugiro alterar o título do Gráfico 5 pois não é possível compreender que ele se refere a proporção de mulheres e homens liderando equipes.*
12. *Os autores afirmam: "Foram utilizados modelos de regressão (linear e logística) pois o experimento se propõe a analisar o efeito de quatro variáveis de entrada (setor, instituição, gênero e data) na variável de saída (recomendação)." Mas, entendo que não foi realizado um experimento.*
13. *Qual é a real contribuição deste estudo?*
14. *Os autores realizaram algum protocolo de pesquisa de forma que esta pesquisa seja replicável?*
15. *Como os outliers foram tratados?*
16. *Como os autores trataram o problema de endogeneidade?*
17. *Sugiro que os autores realizem teste de robustez, bem como teste de sensibilidade de forma a tornar os resultados mais confiáveis.*
18. *O que os players deveriam fazer após lerem este artigo?*
19. *Quais as implicações reais dos resultados?*

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20. *Qual a magnitude econômica dos resultados encontrados?*
21. *Conclusões: Aqui é os autores precisam deixar claro qual foi a real contribuição do artigo e a lacuna preenchida. O que não se sabia, mas que agora sabe-se após este artigo? A quem interessa os resultados encontrados?*

Reviewer 2 Report

Reviewer: Andréia Ribeiro da Luz

She did not authorize the disclosure of their peer review report.

ROUND 2

Reviewer 1 Report

Reviewer: Rogiene Batista dos Santos

Date review returned: 05-Sep-2023

Recommendation: Accept

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None.

Comments to the Author

Os autores atenderam as sugestões de melhoria. Parabéns! Por este motivo, a minha sugestão é que o artigo seja aceito.

Reviewer 2 Report

Reviewer: He/she did not authorize disclosure of their identity.

Date review returned: 09-Oct-2023

Recommendation: Minor Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None.

Comments to the Author

Embora o manuscrito tenha sido cuidadosamente elaborado, tenho algumas críticas/sugestões que destaco a seguir.

1. *O manuscrito parece estar bem escrito do ponto de vista gramatical. Mas ainda é possível encontrar alguns pequenos problemas relacionados a typos, como "multinominal" em vez de "multinomial" na Conclusão e outros problemas como preposições a mais ou a menos. Também é recomendável revisar a seção de Referências, pelo menos uma delas não está no formado APA. Enfim, o manuscrito poderia beneficiar-se de uma revisão gramatical e tipográfica mais rigorosa.*

2. *As Tabelas 2 e 3 da regressão múltipla poderiam ser unidas numa só tabela.*
3. *Na página 12, os valores de R-quadrado e R-múltiplo devem ser corrigidos, já que um é o quadrado do outro.*
4. *Creio que uma outra opção, em vez da regressão múltipla com a dummy de gênero, seria mais simples apresentar um teste qui-quadrado para checar as proporções entre gêneros no geral (todos os anos) e depois separadamente para cada ano analisado. Isso poderia se feito caso o objetivo seja simplesmente verificar se há uma diferença significativa nas proporções de gênero ao longo dos anos, assim um teste qui-quadrado seria mais direto e fácil de interpretar. No entanto, se o objetivo é entender o efeito do gênero e do ano (além de outras variáveis independentes de controle, se houver) na variável dependente, então a regressão múltipla seria a abordagem mais apropriada. Ela permite estimar o efeito de cada variável controlando pelas outras.*
5. *As tabelas em geral, mas principalmente as Tabelas 4 e 6, deveriam ter uma "Nota", abaixo na própria tabela, explicando as abreviações das variáveis usadas nas regressões.*
6. *Quanto à amostra, ela parece ser suficiente, mas se poderia ter uma ideia melhor de seu tamanho se tivéssemos uma ideia do tamanho da população (quantos analistas brasileiros de sell side são reportados na Bloomberg). Além disto, é costume, e mandatório em muitos periódicos, colocar um link para que o leitor interessado possa acessar os dados utilizados na pesquisa.*
7. *Não está claro, para mim, como emitir recomendações de compra se relaciona com aversão ao risco. Do ponto de vista psicológico, recomendar a compra de uma ação a outra pessoa pode indicar menor aversão ao risco, mas isso não significa que você pessoalmente compraria a ação, caso fosse tomar uma decisão. É importante separar o que você aconselha que outros façam do que você mesmo faria, já que diferentes motivações psicológicas podem estar em jogo em cada cenário. Há também conflitos de interesse que podem influenciar essas decisões de recomendações etc. A literatura pode oferecer maior clareza sobre essa distinção. Isso mereceria um parágrafo em alguma seção do trabalho. Sugiro a consulta às seguintes referências para elucidar esse ponto ao leitor:*
    - Malmendier, U., & Shanthikumar, D. (2014). Do Security Analysts Speak in Two Tongues? Review of Financial Studies, 27(5), 1287-1322.
    - Hovakimian, A., & Saenyasiri, E. (2010). Conflicts of Interest and Analyst Behavior: Evidence from Recent Changes in Regulation. Financial Analysts Journal, 66(4), 96-107.
    - Clarke, J., Ferris, S. P., Jayaraman, N., & Lee, J. (2006). Are Analyst Recommendations Biased? Evidence from Corporate Bankruptcies. Journal of Financial and Quantitative Analysis, 41(1), 169-196.

ROUND 3

Reviewer 1 Report

Reviewer: Rogiene Batista dos Santos
Date review returned: 25-Dec-2023
Recommendation: Accept

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Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None.

Comments to the Author

Parabéns pela evolução no artigo!

Reviewer 2 Report

Reviewer: He/she did not authorize disclosure of their identity.

Date review returned: 31-Jan-2024

Recommendation: Accept

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None.

Comments to the Author

I believe that after the requested revisions the manuscript is suitable.

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PEER REVIEW REPORT

Peer review report for:

Araujo, M. M. de., Jhunior, R. de O. S., Uchoa, M. T., Boaventura, J. M. G. (2024). Value distribution to stakeholders: A study on power and strategic importance on the Toronto stock exchange. *RAE-Revista de Administração de Empresas*, *64*(4), e2023-0116. https://doi.org/10.1590/S0034-759020240405

Disclaimer: The content of the Peer Review Report is the full copy of reviewers and authors' reports. Typing and punctuation errors are not edited.

Reviewer:

Ricardo Rocha de Azevedo, Universidade de São Paulo, Faculdade de Economia, Administração e Contabilidade de Ribeirão Preto, Ribeirão Preto, SP, Brazil. Mohit Srivastava, EM Normandie Business School, Metis Lab, Le Havre, France.

ROUND 1

Reviewer 1 Report

Reviewer: Ricardo Rocha de Azevedo

Date review returned: 22-Jul-2023

Recommendation: Minor Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

No conflicts to report.

Comments to the Author

I appreciate the opportunity to review and contribute to the discussion presented in the manuscript "Value distribution to stakeholders: a study on power and strategic importance in Toronto Stock Exchange".

Below, I present my considerations about each section of the manuscript and a general overview of points that should be considered before approving the paper.

The main constructs seem correctly articulated and defined based on the literature: power and strategic importance. The theoretical construction of the relationship between the constructs seems to be adequately presented, bringing both general definitions and a definition for the article.

Overall, the manuscript presents interesting empirical results, based on adequate prior literature. There is a need of adjustments for a publication, according to the comments presented below.

The main criticism is related to the similarity of the research by Boaventura et al., (2020), which analyzed IPOs of companies in Brazil. The manuscript follows practically the same research design, and they have great similarities. Why would an analysis in another context (Canada) be relevant? What does it bring to literature?

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I understand that it is superficial just to argue that the results "maintains the argument of Boaventura et al. (2020)". I suggest discussing and articulating better, justifying a publication.

I suggest adjusting the first paragraph of the "Stakeholder Theory" topic, making it clearer how stakeholder approach is "an alternative" to strategic and economic theorizing.

It is necessary to justify the period adopted between 1993 and 2019. Why were the most recent periods not included (2020 to 2022)? Would the results change when changing the period?

I suggest better justifying the methodology as the independent variables were elaborated (power and strategic importance). Was this methodology based on previous literature? I suggest specifying better, justifying. The reader should be given greater assurance that the methodology is able to adequately assess the proposed variables.

The most recent literature presented in the discussions of the results is from the year 2020. Considering the publication in 2023, are the authors asked if there is no more recent literature with results to be included in the discussion?

Reviewer 2 Report

Reviewer: Mohit Srivastava

Date review returned: 13-Jul-2023

Recommendation: Major Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None

Comments to the Author

This paper, VALUE DISTRIBUTION TO STAKEHOLDERS: A STUDY ON POWER AND STRATEGIC IMPORTANCE IN TORONTO STOCK EXCHANGE, investigates the association between stakeholders' power and strategic importance and the distribution of value to them by publicly traded companies on the Toronto Stock Exchange (TSX). Overall, the manuscript presents an interesting study. However, I recommend some suggestions to improve the quality of the paper.

In the Introduction section, the discussion regarding the context of the study (Canadian business environment) requires strong arguments. Why should we study Canadian firms? You have added some info (such as the top 10 strongest economies etc.), but it still needs more work. So, I suggest re-looking at this part and rephrasing it more coherently.

The literature review section has unnecessary details which need to be precise. Stakeholder theory should be better aligned as it poses ambiguity for the reader, given that the theory has been connoted in a varying manner in the literature. Generating the two hypotheses also requires a strong backup of recent references. They seemed rushed to me.

The discussion section also needs improvements, linking the study with existing literature. Please include the theoretical and practical implications of the study. I could not find limitations of the study. This can be added in the conclusion section. Can you give an example of a real company supporting the findings of your paper?

The authors should be more careful with the paper's writing style. The text needs writing revisions, and typing errors must be corrected. The use of complex sentences often makes the interpretation ambiguous. Especially in the discussion section, the sentence structures must be revisited.

All the best.

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ROUND 2

Reviewer 1 Report

Reviewer: Ricardo Rocha de Azevedo
Date review returned: 12-Oct-2023
Recommendation: Minor Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

No conflict.

Comments to the Author

First of all, I congratulate the authors for the effort made to resolve the questions presented in the previous review, indicating the changes made in the new version. In the evaluator's opinion, the adjustments made the new version presented much clearer. I am grateful for the opportunity to read the research again, aiming to make contributions to its improvement.

I have two main questions.

My main question in this version is regarding the presentation of the literature gap. This version presents the following reflection: "empirical studies contradict the theoretical proposal that power has more influence on value distribution than strategic importance (Boaventura et al., 2020)". First, it is not clear what the contradiction presented is. Second, the argument is based only on Boaventura et al., (2020). Are there other published empirical results? The argument needs to be clear, as the gap is what opens up space for the publication of the research.

Additionally, in research we need to seek clarity in the arguments and constructs used. What is obvious to authors (I include myself in this criticism) may not be clear to readers. In this sense, I raise some questions about clarity and readability.

What would be "value allocation"? Is it the same as "value distribution"? If it has the same meaning, why not use the same term? Furthermore, a definition of this construct could help the reader.

Still, for greater clarity, I suggest expressly mentioning in the "Dependent Variable" topic what the dependent variable is. For example: "The dependent variable used in the research is "value distribution to a stakeholder". To measure it, x steps were followed. First... second...

I had some difficulty understanding. Although I think I understand, I think it's better to make the text clearer.

Likewise, in the topic "Independent variables" expressly mention in the opening: "The research used two independent variables: power and strategic importance. The measurement of the variables is presented below...".

It is not clear to me how these variables were measured.

Still seeking greater clarity, I suggest including a note in table 5 that presents the results of the regressions. Can the note clearly mention what the dependent variable is, and the meaning of the variables? For example, what is CommMedia? Is it a dummy that means "Communications and Media"? Likewise, what is the dependent variable in the regressions in table 6?

In short, the readability of the text can improve, helping the reader to understand it more easily.

Finally, I suggest a change to the first paragraph of the conclusion:

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The purpose of the research was to empirically verify the association between stakeholders' power and strategic importance and the distribution of value to them by publicly traded companies on the Toronto Stock Exchange

I suggest changing it to: (please think about your agreement).

"The purpose of the research was to verify the effect of stakeholders' power and strategic importance on the distribution of value to stakeholders by publicly traded companies on the Toronto Stock Exchange".

I leave final comments to the authors.

1. Despite understanding the journal rule of considering non-textual elements (tables, for example) as figures, I find it strange to consider the content of "Figures" 1 to 3 as Figures. My suggestion would be to transform it into tables, and only use tables in the text. But this is just a suggestion, for the authors to decide.

2. Descriptive tables 1 and 2 could provide more content that would be useful to readers, going beyond presenting frequencies. Is there no more information about the companies?

I'm sure that with these additional improvements, the text will gain a lot in clarity.

Reviewer 2 Report

Reviewer: Mohit Srivastava

Date review returned: 21-Oct-2023

Recommendation: Accept

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

I have no conflict of interest related to this manuscript. **Comments to the Author**

I am satisfied with the changes. The author/s has incorporated all the suggestions well.

ROUND 3

Reviewer 1 Report

Reviewer: Ricardo Rocha de Azevedo

Date review returned: 12-Oct-2023

Recommendation: Accept

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

No conflict to report.

Comments to the Author

I congratulate the authors for the effort made to resolve the questions presented in the previous review, indicating the changes made in the new version. In the evaluator's opinion, the adjustments made the new version presented much clearer. I am grateful for the opportunity to read the research again, aiming to make contributions to its improvement.

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I thank the authors for sending a detailed review letter with comments. This made the evaluation work easier.

Both the presentation of the results in the tables and the definition of the dependent and independent variables became much clearer.

Although they did not meet all the recommendations, I understand that the main ones were met.

I wish the authors success in their publication and hope that the research brings contributions to the literature

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PEER REVIEW REPORT

Peer review report for:

Resende Junior, P. C., Pereira da Silva, L. F. C., Santana, R. S., Fujihara, R. K., Viana, W. S., (2024). Process integration model and artifacts consistency assessment in management systems. *RAE-Revista de Administração de Empresas*, *64*(4), e2023-0012. https://doi.org/10.1590/S0034-759020240406

Disclaimer: The content of the Peer Review Report is the full copy of reviewers and authors' reports. Typing and punctuation errors are not edited.

Reviewer: Mercy Ejovweoghene Isiavwe-Ogbar[https://orcid.org/0000-0002-8086-4460] , Covenant University, Department Business Management, Nigeria. The other reviewers did not authorize disclosure of their identity and peer review report.

ROUND 1

Reviewer 1 Report

Reviewer: Mercy Ejovweoghene Isiavwe-Ogbar

Date review returned: 05-Jul-2023

Recomendation: Minor Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None

Comments to the Author

My observations are:

1. *Introduction needs to be reworked so as to capture the attention of the readers. This can be achieved by defining and explaining the variables of the study. This is very important.*
2. *The method adopted for the study as documented in the manuscript should be overhauled and represented in a manner that policy makers and other researchers can easily understand it and thereby apply, adopt and adapt it as case may be. The method should be free of the findings of the study.*
3. *Vmax used in the study was not defined anywhere in the study.*
4. *Conclusion of the study should be free of discussion of findings. The authors are expected to come up with a conclusion or conclusions based on the findings of the study and if possible policy implications of the findings.*
5. *References: Two set of authorities used or cited in the study were not properly referenced. These are Cox e Spencer (2002) and Goldratt and Cox (1995).*

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ROUND 2

Reviewer 3 Report

Reviewer: Mercy Ejovweoghene Isiavwe-Ogbar
Date review returned: 22-Dec-2023
Recomendation: Minor Revision

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

None

Comments to the Author

- *the work is commended for its rich contents.*

however, authors could consider the following issues for corresponding actions.

1. *the abstract is vaguely written; repeated ambiguity should be removed and redrafted with simplicity.*
2. *theory of constraints applied should be elaborated enough to reflect its impact and relevance to the study.*
3. *the conceptual clarifications were not explicit enough in the terms simple comprehension, authors wrote as though they were addressing previous audiences who had followed this particular study before now and its a development on the previous work. i advise that the language should be simple enough.*
4. *the empirical clarification were not robust enough, i suggest for a work as this, more recent works could have been incorporated to give the model reality in the context of organizations, their practices and the role of the model integration as practiced in order to show the essence of the study as it relates to the artifacts consistency assessment in management systems.*
5. *the justification for the mixed method approached used is commended and quite appreciated, however, the authors would need to clarify the criteria for*
   i. the population,
   ii. the sample size of 48, and how it was determined?
   iii. what informed the 16 subsystem that were analyzed?
   iv. justification for he time frame between April and June, 2021?
   v. further explanation on the descriptive nature of the research would be appreciated
   vi. the criteria for the specialists / experts engaged in the values chain process would be appreciated as well to enhance the quality of the work.
6. *the results are ok*
7. *the discussions could be more elaborated upon as well implications to practice.*
8. *the language is highly technical but would advise that authors are a bit more simple in their presentations for enhanced comprehension by users.*

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ROUND 3

Reviewer 3 Report

Reviewer: Mercy Ejovweoghene Isiavwe-Ogbar
Date review returned: 09-Mar-2024
Recomendation: Accept

Please state any conflict(s) of interest that you have in relation to the review of this paper (state "none" if this is not applicable).

I have no conflict of interest

Comments to the Author

The authors have done significant review on the paper and its commendable.