

Review of: "The Application of PROMETHEE with the recalculated weight method as a more accurate measurement for the selection of the best Hybrid Renewable Energy Technology for a slum building"

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Potential competing interests: The author(s) declared that no potential competing interests exist.

The study applies the PROMETHEE method in combination with the recalculated weight method, which – in comparison to other weight derivation methods – is claimed to be a more accurate measurement for the selection of the best Hybrid Renewable Energy Technology for a slum building. It also considers the combined weight method, which integrates subjective and objective weights. The recalculated weight method is derived from the application of the Bayes theorem. The paper is interesting and relevant, but it has several flaws. It should hence be improved considerably.

1. It is not clear whether the paper provides any substantial contribution. It is mainly an application of the PROMETHEE method with the recalculated weight method. The results of the study do not clearly indicate an improvement in the efficiency. It is not properly explained if the research introduces any methodological advancements and novelty. The contribution of the paper to the field of MCDM should therefore be better explained.
2. The paper should be better structured and written, in particular Sections 3 and 4. See also comments below.
3. Some acronyms are not defined before they are firstly used, e.g. MCDM, GKUA, etc.
4. There are several places where an acronym should be put into brackets, i.e. »Kemeny Median Indicator Ranks Accordance KEMIRA« should be consistently written as »Kemeny Median Indicator Ranks Accordance (KEMIRA)«, »Criteria Importance Through Intercriteria Correlation CRITIC« should be written as »Criteria Importance Through Intercriteria Correlation (CRITIC)«, etc.
5. References should be uniform throughout the paper. In version 1, the bracket style is mostly used, but there are also occurrences of the APA format, such as »Vinogradov, et al.,(2018)«.
6. References to figures, equations, etc. are not well organized. Figures (e.g. figures 1, 2 and 3) should be referred to in the text before they are presented. For example, there should not be a description of table 1 between the reference to equation 1 and the equation itself as it makes the paper difficult to follow.
7. There are several grammatical errors in the paper. Also, some sentences are ill-formed (e.g. »Apply the PROMETHEE method to rank the HRES alternatives with the most accurate [weights].«, »Convert the crisp numeric values from the scale of relative importance are converted into fuzzy numbers.«, etc.). These errors should be corrected.
8. Several existing MCDM methods (AHP, PROMETHEE, etc.) are described in Section 3, but no references to the literature are provided from where descriptions, definitions and equations are taken (since none of these methods are newly defined/introduced by the paper, they are merely adopted and applied).
9. In Section 3, most applied MCDM methods are poorly presented.

10. It is not well justified/explained why specific MCDM methods are applied. Why is AHP selected to infer subjective weights, and not some other weight derivation method, such as SMART, GRAPA, ROC, Simos' procedure (which is particularly designed to be combined with outranking methods, such as ELECTRE and PROMETHEE), etc.? There is also no explanation why TOPSIS and VIKOR methods are used for comparison.
11. In step 1 in subsections 3.1.1 and 3.2.1, the decision matrix X that is obtained with simulation from the HOMER system should be better defined and/or explained. What criteria and alternatives are represented? What are the domains/scales of criteria? In particular, it is not clear how alternatives are initially evaluated because it is not explained which preference model is used by HOMER (e.g. outranking relations, utility functions, etc.). The only information that is provided is that the decision matrix is obtained from the results of the simulation conducted in a slum settlement in Nigeria with the use of the HOMER software. However, this does not suffice to fully comprehend the procedure and the results presented in the paper.
12. The entropy method is very weakly explained – insufficient explanations, badly structured, missing equation for the calculation of weight W_j , etc.
13. Also, the description of AHP should be improved. Too much focus is given on the consistency and the consistency index, which is just one aspect of AHP. Moreover, no indication of the recommended consistency is provided (what level is required and why), especially not in relation to the applied combined and recalculated weights.
14. Step 1 of the PROMETHEE method is very poorly written and is useless. In PROMETHEE, we have thresholds (preference and indifference thresholds) and preference functions (usual, U-shape, V-shape, S-shape, etc.). Which functions are used and what thresholds are set to obtain the matrix from HOMER? Why is the concept of preference functions not explained?
15. From step 3 of PROMETHEE it follows that, for example, AHP is used to derive weights for the purpose of PROMETHEE aggregation of preferences. How is this justified, as these are two different MCDM methods that exhibit different preference models and structures, as well as different conceptual interpretations of weights? That is, there is a distinction between a ratio based approach (AHP) and an outranking approach (PROMETHEE).
16. In step 4 of PROMETHEE, the preference index can also be between 0 and 1 (not just two crisp numbers as presented). Moreover, H is not defined in equation 28.
17. Subsection 3.5 is not titled correctly.
18. Subsections 3.6, 3.8 and 3.9 should be considerably improved.
19. Criteria are not well defined. Subsection 2.1 identifies some inputs and outputs of the HOMER software (TNPC, energy demand, etc.), but these parameters are not correlated with criteria C_1 to C_{10} which are referred to in Section 4. What do criteria C_1 to C_{10} represent? What do values in table 2 (decision matrix in Section 4) represent? No interpretation of their meaning and scales is provided.
20. In tables in Section 4, formatting of numbers is not appropriate for a scientific paper. Some numbers have no decimal places, other have 1, 2, or up to 8 decimal places, respectively. Format of all numbers must be fully consistent. According to the results of the study, I suggest that 3 or 4 decimal places are uniformly used for all numbers throughout all tables.
21. Most tables in Section 4 are not properly referred to. Each table must have a reference in the text, and each reference

(with a description) must be provided before the corresponding table.

22. Descriptions of steps (especially steps 2 to 5) in Section 4 are very weak. Moreover, the fuzzy AHP is mentioned in step 4, although results in this step pertain to the classic AHP. Results of the fuzzy AHP are then listed in step 5. How are fuzzy ratios (l, m, u) in table 9 obtained? It seems that they are just slightly extended ratios from table 7 – with only slight/minor deviations of lower and upper bounds of triangles from median values which are taken from table 7.
23. The premise of the paper is that the recalculated weights are the most accurate, but from table 13 they do not appear particularly valid as C1 stands out overly extremely – its weight is higher (0.5718) than the sum of weights of all other criteria (C2 to C10) combined (0.4282). This means that only criterion C1 is effective in the ranking of alternatives. This is also evident from tables 14 and 15 because alternatives HR3, HR4 and HR7 with good evaluations on criterion C1 are ranked as the best (first, second and third).
24. It is the presumption that the recalculated weights are the most valid because they result in the most similar rankings for all three compared methods – PROMETHEE, TOPSIS and VIKOR. But this is a direct consequence of the fact that the recalculated weight of criterion C1 (weight W1, which is 0.5718) is too extreme, so that all methods strongly prefer the same alternatives. Hence, rankings are the same for the best three alternatives HR3, HR4 and HR7. This is a big limitation of the study. Hence, the results of the paper cannot be considered sufficiently relevant. I suggest that several additional examples are provided which will produce different sets of weights, in order to improve the meaning of results. A simulation study may also be performed to improve the validity of the research.
25. In Section 4, there is no justification why only the complete ranking (net flow) is considered since there are also some incomparabilities between alternatives that can be observed. For example, alternatives HR5 and HR2, which are ranked 5th and 6th, respectively, are incomparable. How does incomparability relate to the results of VIKOR and TOPSIS which are used for comparison?
26. In equation 5, the symbol (Greek letter) is undefined, incorrectly defined or incorrectly used.
27. In the description of fuzzy AHP, it is not clearly explained what A1 and A2, respectively, stand for.
28. Presented results in table 13 (recalculated weights) and table 16 (combined weights) are the same.
29. In table 15, the net flow of HR7 is incorrect. It should be 0.183 instead of –0.183.
30. It is written that informal settlements are an integral part of a city with an estimated population of 1 billion inhabitants in 2018, with a future projection of 3 billion in 2030. This is probably an error since the population of a city is usually a magnitude of millions, not billions.