

Interactive comment on “The evaluation and enhancement of quality, environmental protection and safety in seaports” by D. Tadic et al.

Anonymous Referee #1

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General comments

The manuscript presents an application of Analytic Hierarchy Process (AHP, (Saaty, 2008)) for the ranking of "business processes" in the context of harbour management. Qualitative expert judgements are represented in terms of triangular fuzzy numbers (TFN), i.e. triples of conventional (or: crisp) numbers. Derived weights of the pairwise comparison judgements are employed for ranking priorities for the harbour management processes.

AHP has been successfully applied to various cases of multicriteria optimization problems, such as strategic and military actions, customer satisfaction, development of new products (Saaty, 2008). In this respect, I find interesting the attempt by the authors to use AHP in combination with fuzzy numbers for modeling the decisional process in a harbour. However, in order to accomplish the goal of an enhanced objectiveness and

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transparency, AHP should be employed using high methodological standards and with a clear analysis of the primary sources of judgment. This is actually the main criticism I feel to move to the paper, that in fact ends up with the opposite result of making the decisional process in the harbour even more obscure and more subjective than without AHP. My concern is declined into two major issues that I have with the manuscript. Formally, it is not well organised, making it quite hard to read and verify. Concerning the contents, I find the methodology not completely sound and not capable of supporting the main conclusions. I will address both issues in detail in the specific and technical comments below.

I think that in order the manuscript to match the minimum requirements for publication in a peer reviewed journal, the authors should first analytically address all issues listed in the following.

Specific comments

A - Formal structure

A1 Acronyms.

Several acronyms are used without any previous definition, such as: QMS, TQM, FAHP (given 13 lines after its first use in terms of AHP), AHP (that, apart from a reference within the abstract to "modified fuzzy extended analytic hierarchy process" or MFAHP, is nowhere directly defined), TFN (given just in the abstract, where it is not used, but not in rest of the paper), APQC.

A2 Organization.

The review of literature is spread among different sections and this is not justified by the reference to it done in the rest of the contents. Literature reports appear

not just in the introduction (Sect.1) but also in the initial part of Sect.2 called "Materials and methods" and in whole Sect.3. The material in Sect.3 could be introduced before Sect.2.1. The main algorithm is kind of repeated in two versions: one on P4-5 and the other on P6-7. Sect.3 contains a description of KPIs that would deserve an indentation. Furthermore, no reference to the application done later on, matrices at P12-15, is done, where instead a)-e) letters are employed for sorting the various KPIs. I suggest to use the same letters in the list of KPI in Sect.3.

A3 Notation.

- Sect.2.2 is highly repetitive and does not help in reading and memorising key quantities. I suggest to replace the contents of Sect.2.2 with a table as Tab. 1 of this review and to simplify the symbol for the fuzzy numbers: do not use x or y and just give the triple of crisp numbers making the TFN. E.g. $(x; 2, 3, 4) \rightarrow (2, 3, 4)$.
- Most of these symbols introduced on Sect.2.1 (e.g. $\varepsilon, \kappa, \varphi_k, \iota, E, K, J_k, I$) are not at all or just poorly used in the following of the manuscript.
- there is a confusing nomenclature about "weights vector of performance", "weights vector of KPI", and "preference vector of business process".
- Since I do not see any reason for breaking the alphabetical order, I would replace χ with γ in Eq.2.3 at P6

Table 1. Suggested table to replace material in Sect.2.2. Please consider note on "business processes" expression in A4 item.

	set symbol	running index	set size symbol	set size
experts	ε	e	E	4?
performances	κ	k	K	3
KPI of k th performance	φ_k	j	J_k	4
"business processes"	ι	i	I	5

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A4 Other.

- do not use the word "business" both in the collective expression "business process" and for one of its actual implementations ($p = 5$: "business activities in seaport")! This is a highly confusing linguistic choice made by the authors, I really cannot approve it.
- write matrices at P11-15 as equations whose l.h.s. is some meaningful combination of symbols with pedices or apices related to the actual contents of the matrix (consider symbols introduced in Tab. 1 of this review)
- Fig.1 is quite complex and not entirely related to the text. It could be simplified, highlighting (i.e., numbering) the steps of the proposed methodology;
- caption of Fig.2 could explain more directly that the horizontal axis contains the performances, detailed per KPI. Also, the notation $1^1, 2^1, \dots$ is quite confusing at first sight.
- Sentence at the end of P4 ("Value 1, and value 0 denote that one performance or KPI is as important, or unimportant, as any identified performances or KPIs under each treated performance") does not add any understanding and can be removed.

A5 Figures and Tables.

The list of processes in the legend of both Fig.1 and 2 is referenced both in Tab.1,2,3 and in the manuscript. Thus, it deserves an independent presentation in a specific table.

A6 English.

Specific sentences are really badly formulated. E.g. "In the course of easier understanding of the proposed Algorithm, in this Section the notation is given" (P5, row14). Revision by a professional translator of technical manuscripts is

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B - Actual Contents

B1 Abstract.

The proposed model is far from being "verified", demonstrated or validated within this paper. Instead a simple numerical evaluation of the "proposed algorithm" is carried out. Furthermore, the conclusions are quite surprising, see item B5.

B2 Problem statement and methodology.

- First of all, see all comments done in **A**, since the actual scientific contents of a paper can be hardly detached from their presentation style.

- It should be more clearly stated what the input data for all subsequent elaborations are. In particular, the weights $w_e = (.4, .3, .2, .1)$ of the experts used are present in the example of line 14 of P10. I -and I think most readers too- would like to see a table where these weights are clearly associated to the 4 experts (not sure if in this order, but they seem to be: seaport owner, main manager, local government, operational management of the seaport).

- Furthermore, the most influential expert overweights by 4 times the least influential one. How were the w_e assessed? This raises the more fundamental question "who is judging the judges?". This information about expert judgement is quite crucial for the actual numerical outcomes, see B5.

- as from the definitions of the base TFNs (P4), the authors use a linear scale $[\frac{1}{\sigma}, \sigma]$ with $\sigma = 5$. The type of scale (Ishizaka and Labib, 2009) and the quantity σ are keys in a pairwise comparison matrix, representing the accuracy of the judgements and indirectly affecting matrix consistency, see e.g. (Ramík, 2009). It is usually taken $\sigma = 9$ (Saaty, 2008). In my opinion, the actual choice of the quality and extent of the scale deserves a dedicated comment by the authors.

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- why are there so many crisp numbers (1, 1, 1) in the off-diagonal elements of the pairwise comparison matrixes at P11-15 ? The authors make a big point about modeling uncertainty in terms of fuzzy numbers, and then it turns out that several specific processes can be assessed to have exactly the same relative importance (such is in fact the meaning of (1, 1, 1) in the matrixes). I find it odd that there is not even a comment on this.

B3 Pairwise comparison matrices.

- The numerical case study (Sect. 4) starts all of a sudden with a pairwise comparison matrix, whose relevance to the method (which is great) is never mentioned but in Fig.1.
- The consistency of this matrix (Ramík, 2009) is never evaluated nor discussed. Given the qualitative nature of the expert judgements, consistency is a quite relevant concern of an AHP investigation (Saaty, 2008). Thus, I believe some measure of consistency should be computed and provided for all comparison matrixes in the manuscript. E.g. is the consistency ratio below the classical threshold of 10%?

B4 Missing originality.

- The specificity of the claimed "modified" FAHP (MFAHP) method proposed by the authors is not demonstrated nor stated. The core of the proposed algorithm (steps # 5-8 of Sect.2.1) is just a few standard rules taken from the literature, while the rest (steps # 1-4 of Sect.2.1) is just definitions. Unless the authors clearly state where the originality of the proposed algorithm is, I think they cannot claim to have developed a new method: they just made an application of an existing one, and the use of the dedicated acronym MFAHP is not justified, in my opinion.

B5 Not fully justified conclusions.

- According to Tab.1 and Tab.3 the "business activities in seaport" process ($p = 5$)

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gets rank 1 for both the quality and the safety performance. How can a business activity be the most crucial action for enhancing safety of a harbour? The authors comment this surprising finding by stating that "the level of customers' satisfaction mostly depends on quality of this business process realisation, so the obtained result is expected" (P15, rows 23-24). I actually thought that the focus of the paper was to establish priorities for the port management without a specific perspective on customers, but in view of multi-criteria optimization. If instead the authors mean that the whole analysis is just functional to enhance customers' satisfaction, then the title, abstract and scope of the paper should be consequently restricted. In any case, I cannot easily accept that business activities will enhance safety of a harbour. I think that either there is some numerical manipulation mistake or the initial expert assessments (including their relative weights) were biased. This leads me back to the observation about expert weights (B2) and missing analysis of consistency of the pairwise comparison matrices (B3).

Technical comments

C1 For a symmetry reason, on P4 it seems to me much more natural to define $VL=(1,2,2)$ and not $VL=(1,1,2)$: just plot the 5 fuzzy numbers VL, L, M, H, VH and see why. Actually it would help the reader in having this plot as a Figure of the manuscript.

C2 Matrix on P10, row 10 (please, use symbols for identifying mathematical objects more easily!):

- I guess the 3×3 matrix refers to the $K = 3$ performances and each fuzzy number

in the 4-tuples refers to an expert judgement. If this is correct, it should be clearly stated. Furthermore, for consistency of notation, the diagonal elements should be 4-tuples of crisp numbers, something like $(1, 1, 1), (1, 1, 1), (1, 1, 1), (1, 1, 1)$ that could be conveniently replaced by a convenient multi-dimensional identity symbol such as the one expressed in LaTeX by `\mathbb{1}`.

- In the following, (P12-15), also 4×4 (P11) and 5×5 appear. It would be good to always state what this dimensionality refers to. I suppose that they refer to $J_k = 4$ KPIs of each performance, and to the $I = 5$ "business processes", see Tab. 1 of this review.

C3 - on P4, row21: replace "consensus" by "group consensus" and make reference to Step 5 (P5) of the algorithm.

- it is unnecessary to define again $\tilde{W}_{kk'}^e = (\dots)$ and $\tilde{W}_{jj'}^e = (\dots)$ on P6, row10, after they were introduced in Sect2.2
- remove range of indexes ($i = \dots j = \dots k = ..$) in both Eq.(4) and Eq.(5): they were already introduced in Sect.2.2;
- Eq.(5) could be better rewritten as

$$\tilde{a}_i = \sum_{k=1}^K \tilde{w}_k \tilde{a}_i^k = \sum_{k=1}^K \sum_{j=1}^{J_k} p_{ij}^k$$

- there is a logical need to insert a separation (new subsection) on P10, row8.

References

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