Review of « Coastal vulnerability assessment of Puducherry coast, India, using Analytical Hierarchical process » by R. Mani Murali, M. Ankita, S. Amrita and P. Vethamony

General comments

This paper presents a coastal vulnerability multicriteria mapping method based on the Analytical Hierarchical Process (AHP). The method is applied along the Puducherry coast. The method is useful as it helps in defining weights in multicriteria mapping when no purely deterministic assessment can be drawn. In addition, with respect to previous studies that also used AHP for mapping coastal vulnerability, this application integrates social factors. The paper is timely addressing an important topic, which is within the field of NHESS. Therefore, I think it is worth being published.

However, I have several comments and questions:

1. My main concern in the application as it is presented here is the lack of justifications for the bins in the vulnerability ranking criteria (Table 2). I recommend the authors discuss and justify how they chose the boundaries between the vulnerability classes.
2. The choice of the criteria to be evaluated should be justified. Implicitly, a conceptual model of coastal processes is adopted when selecting those criteria.
3. I recommend the authors write more explicitly what problems the AHP helps to solve in the context of vulnerability mapping.
4. I recommend that the authors explain more clearly how their results are constrained by their data and by some subjective choices made during the method implementation. Previous AHP applications address this issue through a sensitivity analysis and/or a discussion on how the results are realistic.
5. A review of previous works that used the Analytical Hierarchy Process for coastal risk or vulnerability mapping is lacking. This review is necessary to clearly show the novelty of this research.
6. Finally, these exercises of coastal vulnerability mapping are only useful in the context of their potential use for coastal prevention or for adaptation to coastal changes. I recommend the authors include a discussion on the potential use of their results (or eventually the barriers to this use) by coastal planners.

Detailed comments:

Abstract: page 510: line 10. The paper should not “advocate” but demonstrate that AHP helps solving some specific problems or explains where it fails to support the analyst.

Page 511:

- Line 15: 1,5m of sea level rise by the end of the century is the upper limit of sea level projections. This sentence should be rephrased or make reference to previous studies.

Page 512:

- Line 1 to 5: I don’t understand the point in these two sentences
- Line 11: I recommend rephrasing *“(i) in terms of” <u>potential</u> (…)*; By the way, Romieu et al. (2010), cited by the authors, discuss the double meaning of the world vulnerability in the two

fields. They states that in the climate community, vulnerability is equal to the potential impacts of climate change minus the benefits of adaption, whereas vulnerability is a component of risk (=hazard*vulnerability of exposed stakes). Am I right to understand that the authors want to point out the same here?

- Line 20: I don't understand the relation with management here.

Page 513

- Line 21: As stated by the authors, the most used coastal vulnerability index (CVI, Gornitz, 1991) uses a geometric aggregation function. This is justified by the fact that this aggregation is the most resilient to the lack of data. Conversely, AHP has a completely different approach as it helps experts in formalizing their subjective judgement and to translate it into a numerical aggregation function. I understand this is the main justification in using AHP here. The authors should explain here the specific problems in coastal vulnerability mapping that AHP is expected to solve.
- Line 21: in addition to a stronger justification of using AHP in this context), a review of previous applications of AHP for coastal vulnerability mapping is necessary to explain the novelty of this paper.

Page 514

- Lines 19/21: the authors should justify why AHP deduced weights produce "better" estimations. This could be done by responding to the two comments above.

Page 517

- See general comment 1: the bins (upper and lower limits in each class) should be better justified. This is most important since the results are highly dependent on this. For example: why is sea level change (1 to 2mm/year) considered as corresponding to a high vulnerability, while 0 to 1mm/year is considered to low vulnerability? Other could state that high vulnerability corresponds to sea level rise in the order of at least one centimetre per year. Similarly, it is considered that higher tidal ranges account for higher vulnerability. Conversely, the classical Coastal Vulnerability Index (Gornitz, 1991) considers that the lower tidal range account for greater vulnerability (e.g. a 2 days storm would reach the upper beach all the time in a microtidal environment. These choices should be justified (e.g. by knowledge of the field, by reference to previous studies or publications, by a subjective choice of experts) as all the results depend on them. If some choices are subjective, this should be indicated too.
- I also strongly recommend justifying the criteria chosen here. For example: why did you choose the "observed sea level rise" and not "expected future sea level rise" as a criterion?

Page 521

- Line 9 to 14 are unclear for me: what is the reference period for the rates of 1.29mm/yr? I recommend explaining more clearly what components of relative sea level rise (with respect to a terrestrial framework at the coast) are assessed in each approach and the timeperiods.
- Lines 9 to 14: It would be necessary that the authors discuss here the likelihood of more local vertical ground motions (subsidence or uplift) affecting the coast. Levelling data, if available,

could provide insight to this issue. If not, are they any geological evidences (footmarks) or any other geodetic information?

- Line 21: Equation 1 is actually homogeneous to an energy flux (J/m^2). In this context, except if all waves periods are similar, I would suggest using the the total wave energy in one wavelength per unit crest as indicator.

Page 522

- Line 5: please indicate the unit of the resolution (degree °?)
- Given the resolution of the NCEP/NCAR wind model, I would be quite surprised that the modelled cyclonic waves are realistic. This should be discussed.

Page 524

- Land use/land cover: What is actually assessed? Land use or land use changes?
- Road network: why are the values 250m/500m/1km2km chosen?

Page 529

- Lines 20/21: I am not sure I understand the point
- All along the result section, a discussion on how the results are realistic is missing. Also, to which extent do the results reflect the actual coastal vulnerability and to which extent is it dependant of subjective choices done in the previous steps?

Page 532

- Lines 8-10: this point would be relevant earlier in the paper
- Line 15: susceptibility or vulnerability?

Tables

Page 538: GIS data are not sources but the format; LISS III is not self-explicit here.

Page 539: see major comment above

Page 542: “column total” is not useful here. Conversely the resulting priority index would be useful. (i.e. as derived by calculating the eigenvector associated with the principal eigenvalue of each comparison matrix).

Page 543: Table 6 is not useful (equal to normalised table 5). Same remark for table 8.

Minor (wording)

Page 511:

- Line 6: “in lieu” or “in light”?

Page 513:

- Line 10: delete the space between sea and -level

- Line 17: “multi-hazard vulnerability” may be unclear: I would suggest instead: “the vulnerability of the coast to multiple types of adverse event”
- Line 20: I suggest to clarify what is meant with “geo-spatial technologies” in this context

Page 516

- Line 1: “Methodology” refers to the science of methods. What is presented here is a “method”.
- Line 16: I would delete the word risk (“risk variables”) which is confusing in this context.

Page 517

- Coastal slope definition: “perpendicular to the shoreline” is missing.
- What is ETOPO5?

Page 521

- Line 4/5: please rephrase (“the study (…) has been studied (…)”)

Page 523

- Use “Disaster” instead of “calamity” (?)

Page 525

- Delete caused (?)

Page 526

- Lines 9 to 18 are actually redundant with the introduction

Page 527

- This part could be slightly reduced as the AHP method itself is clearly explained in Saaty’s publications

Page 528

- Lines 22/23: I disagree: water pumping can affect even smaller areas and therefore, the fact that the coastal zone of interest is small is not a good justification for considering equal sea level rise

Page 531

- Line 24: is it meant: “the rising number of coastal disasters” (?)

Page 532

- Line 7: acronyms should be defined earlier

Typing errors in the References:

- Thieler and Hamm**a**r-Klose (A instead of E)

- Vittal Hegde and Reju (2007) is actually Hegde and Reju according to my reference list (Hegde, A. V., and Reju, V. R.: Development of coastal vulnerability index for Mangalore coast, India, Journal of Coastal Research, 23, 1106-1111, 10.2112/04-0259.1, 2007.)
- Szlafsz**tein**
- Ramesh et al. 2011 is not quoted (therefore delete)