Hydrology and Earth System Sciences Discussions

Interactive comment on "**Ecological adaptation as an important factor in environmental flow assessments based on an integrated multi-objective method**" *by* **T. Sun et al.**

X. Desmit (Referee)

xdesmit@gmail.com

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General Comment

The authors propose to couple a 2D-hydrodynamical model of the Yellow River estuary to a habitat model to test how the habitat areas of four key-species change under different river discharge regimes. The modifications in habitat areas are assumed for each of the four species on the basis of their resilience to salinity and water depth variations. The general idea is interesting and may certainly be useful for other applications and decision making. In general the ecological understanding of the system by the authors

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seems very good, including the assumption that recruitment and juvenile periods are crucial to ensure the future health of a species.

However, the objectives of the study and the methodology are not clear to me, especially regarding the habitat modelling and the "ecological adaptation". The habitat model appears to be a simple projection of tolerance thresholds on salinity and depth outputs from the hydrodynamical model, without any response relationship from the biota. It is not clear to me what is the difference between the present manuscript and the one the authors just published in Estuaries and Coasts, "Objective-based method for environmental flow assessment in estuaries and its application to the Yellow River estuary, China", 2012, Estuar. Coast. 35: 892-903. Both papers present similar graphs, tables and conclusions.

I would only recommend accepting the paper at the condition that the Major Comments are addressed by the authors, with a special attention at clarifying the Methodology.

Major Comments (requested modifications)

1) The Methodology is unclear and must be clarified before publication. The habitat model is not well described and the reader must "guess" essential information about the environmental factors under study (i.e. salinity and depth). The first occurrence of the word salinity is found page 8. At line 24, the reader guesses that "acceptable salinity and depth" are the environmental factors under study. I advice to clarify that point from the beginning (i.e. in the Abstract, and in the Introduction p.4 line 15, and in the Methodology), and make clear that the only environmental factors influencing the habitat area in the present study are salinity and depth (just as the authors did in their paper in Estuar. Coast., 2012).

Equation (2) does not make much sense in this study, according to me. When you know that Si is the salinity or the depth, what is the function g(Q) "the relationship between ecological processes and flow regime"? Where is the relationship defined or referenced? Beside, I would suggest that g(Q) is changed into another symbol (e.g.

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j(Q)) because g is further allocated to the gravitational acceleration in Eq (4).

It should be explained somewhere how to derive habitat areas from salinity and depth. The reader can only guess the following (without being sure): the 2D-hydrodynamical model produces results of salinity and depth distributions in 2D-space at every time step. Then, the authors delimit the habitat area by projecting on these salinity and depth results the tolerance thresholds of the species. The habitat area of one species is derived from these projections by taking the smallest intersection of both salinity- and depth-derived areas. This is only a supposition from me, and I request from the authors that they clarify this point in their manuscript.

The two primary objectives (i.e. habitat area and habitat area variability) that are found in the Abstract (p.2 line 9) should be well described in the Methodology, including the way they are estimated. For instance, Figure 4 (p.24) shows the "Amplitude of habitat variability". How do you calculate the variability? It is difficult to understand how most graphs are produced. How do the authors derive the maximum habitat area (p. 9 lines 22-24 and Fig.4)? Figure 2 (p.22) shows the "Temporal variation objectives for environmental flows". What is it? How is it calculated? Why is it an "objective" or "objectives"? Where is it explained in the Methodology? Table 2 (p.20): how do the authors derive the "Annual environmental flows (109 m3)"?

(Optional) In the Methodology, it may be useful also to present the study area first, then the hydrodynamical model, the habitat model, and then how they are coupled by specifying in the Methodology how outputs of the hydrodynamical model are used in the habitat model. A figure presenting the spatial grid of the model might also be useful.

2) As far as I can see, the manuscript does not address "ecological adaptation". The title suggests that "ecological adaptation" is the central idea of the manuscript (see e.g. also p.4 lines 11 and 13). In the Methodology, some mechanisms of adaptation are well explained in the text. It is mentioned how species may migrate into new areas under changing water flows and how they may sometimes adapt their habitat (p.5 line

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2, p.7 lines 6-7). However, I do not see how this is translated into the equations, how this ecological knowledge is tested in the present study.

According to what I understand from the methodology of the authors, the boundaries of habitat areas are estimated with the tolerance thresholds per species. These boundaries then draw the contour of a "potential" habitat area based only on salinity and depth. At most the authors can calculate the geographical shift in potential habitat boundaries related to shifts in salinity and depth. The relationships between habitat and salinity or depth is hidden in the function fi in eq.1, but I suppose that the authors have simply projected the upper and lower tolerance thresholds for each species. This is not a biological response but merely a scaling of salinity and depth results. From my point of view, this is not a study on ecological adaptation. A study on ecological adaptation would be a study that relates the presence and survival of a species to environmental factors, like salinity, with a biological response function that is validated, not just the displacement of potential boundaries. Eq. (1) and (2) suggest such a biological relationship, but these equations are not used. The function fi in Eq. (1) is neither defined nor referenced. Unless proven wrong, I would recommend that the authors use the terms "potential habitat area" (or similar terms) instead of "adaptive habitat area" or "habitat area", and do not use the term "ecological adaptation" or "adaptable relationship" in their title, objectives, methodology or conclusions.

3) A comparison between the present manuscript and the paper from the same authors i.e. Sun et al. (2012) in Estuaries and Coasts (35) shows that both papers study the same phenomenum with slightly different methodologies and with different results. These differences are not discussed in the present manuscript. In Table 2 (p.20) of the present manuscript, the authors present the environmental flows in the Yellow River Estuary for four species (minimum and maximum tolerable flows in m3 s-1). They present the same flows (min and max) for the same species in the same estuary in their paper Estuar. Coast. (p.899, Table 2). The values are different (in some case by a factor 5). Idem for the conclusions: in the present manuscript the authors conclude that

the river discharge must be comprised between 25% and 112% of its annual average, versus 15% and 101% in Estuar. Coast. Why is it so different? A contrario, in the present manuscript, Figure 7 (p.27) is a very interesting figure. However, it seems to be another version of the Figure 11 (p.901) in Estuar. Coast. Then the question: what has improved since the publication of the Estuar. Coast. paper? Shouldn't these differences be discussed by the authors as a result of the differences in methodologies?

4) Several crucial terms are confusing to the point that the manuscript remains long misunderstood by the reader. For example, the "multi-objective method" (omnipresent in the paper) is merely known as the "multi-objective optimization method". It is the process of simultaneously optimizing two or more conflicting objectives subject to certain constraints. There is no single solution but several tentative solutions that allow quantifying the trade-offs and make decisions. The authors must know this method as they cite Yang W (2011) who uses such a method to evaluate environmental flows in the Yellow River delta. However, the optimization method is not used in the present manuscript, as far as I can judge. Therefore, unless the authors prove me wrong, I strongly recommend the use of other terms in order to dispel the confusion.

With the aim to help the authors clarify their text, I give three additional examples: 1) the term "objectives" (p.22 Fig.2) is never really defined. Is it the objective of the study, or the objective of an optimization, or something else? 2) The term "temporal variation in objectives" (p.9 line 9 and p.22 Fig.2) seems abusive to me. A temporal variation is a rate of change (ïĄĎx/ïĄĎt or dx/dt), not the ratio between a monthly value and an annual value of the same variable. 3) The term "integrated" as part of the name of the method (see e.g. title, abstract) is unclear to me. Does it mean that the hydrodynamical and habitat models are coupled, as suggested in the abstract (p.2 line 7)?

Minor Comments

1) p.2 line 5: suggest replace "migrated" with "migratory" 2) p.2 line 9: "low variability." ... of what? of the habitat area? 3) p.2 line 13: replace "data" with "results" 4)

p.2 line 17: replace "are compensated" with "may be compensated" 5) p.3 line 28: suggest replace "population effect" with "effect on the population" 6) p.4 line 4: remove "As with many biotic and abiotic factors" 7) p.5 line 12: suggest replace "Si is the environmental" with "Si is the distribution of the environmental" 8) p.5 line 13: replace "factor of number i" with "factor number i" 9) p.5 line 23: suggest replace "presence" with "occurrence" 10) A question out of curiosity: what is the explanation, if any, of the increase in dry events in the Yellow River since the 1990's? (p.8 lines 7-9). It may be interesting to mention it. 11) Remark about the enhancement of ecosystem biodiversity with fluctuating environment (p.7 line 14): the authors might be interested to read (if not yet done) the paper of Huisman and Weissing (1999) Biodiversity of plankton by species oscillations and chaos, Nature 402.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C4669/2012/hessd-9-C4669-2012-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6753, 2012.

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