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# *Interactive comment on* "An approach for coupling higher and lower levels in marine ecosystem models and its application to the North Sea" *by* J. A. Beecham et al.

**Anonymous Referee #2**

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This paper presents how to use CouplerLib to link a bio-physical model (low trophic levels) with an Ecopath with Ecosim model. Coupling such models is challenging, and very useful for further scenarios of both environmental change (including climate change) and fishing pressure. Whereas the study underlying this manuscript is clearly of interest for the scientific community, the objectives and thus the structure of the paper needs to be revised before being published. In its current state, the manuscript addresses 1) a very detailed methodology of how to couple a LTL and HTL model; 2) some ecological interpretation of how the ecosystem functions; and 3) scenarios of climate change. From my point of view, the main interest of this work lies in the coupling, so I would suggest to the authors to leave out the scenarios part, and focus on what are

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the methods and effects obtained when coupling models, and how CouplerLib could be used by the community. This would be achieved by describing more the effects of the coupling, and comparing some specific groups: e.g. benthos is separately modelled in each model, thus is its dynamics comparable?, or what is the level of primary production simulated with ERSEM compared to the Phytoplankton group present in Ecosim? Finally, the discussion would need to focus on the advantages and drawbacks of this kind of coupling, and of using CouplerLib rather than a direct coupling such has the EwE-NEMURO one.

Abstract: a final sentence on the interest of the coupling presented here is missing (if the objective of the paper is to focus on the coupling)

Introduction: In the introduction, coupling LTL and HTL models appears as a way to better represent and understand the mechanisms underlying the recruitment process and the zooplankton dynamics. However, the results presented in the paper do not focus on these fast-growing populations. A better match between the introduction/results and discussion section is needed. P5579-L8: the MSFD relies a lot of monitoring, end-to-end model might be more useful for predictions of policies and/or climate change in a management context. P5579-L19: It is not clear what the authors are referring to with "multiple models" – does it means the coupled submodels or a single framework of end-to-end modelling (such as Atlantis for instance). P5580-L3: "sociological aspects": this must be clarified, the link with previous sentences is not clear P5580-L6: if work-packages of the Meece project are to be quoted, then it must add something to the text, which is not the case here. P5582-L5-6: It is not clear here, neither elsewhere in the paper, if CouplerLib was existing beforehand or if it has been developed specifically to couple HTL and LTL models. Please clarify that.

Methodology: According to the refined objectives of the paper (see general comments), the methods section should be revised to focus on the main message of the paper. The dictionary is mentioned several times and seems of importance. Putting it (or a subset of it) in appendix would help better apprehend what it contains. P5583-L21: figure1

should not be quoted here as fig 1 mentions network (thus it seems to fit better in c) P5584-L18: It is not clear what "define the linkages before the simulation is run" means, and thus what is the big difference between CouplerLib and the other existing examples of model coupling? P5589-L16-17: This sentence (among others) would better fit in a userguide than in a paper. P5590: Section 2.5 is not about the LTL and HTL model (i.e. ERSEM and EwE) but is more about their application, i.e. to what they have been applied (the period of time represented by both model is missing) P5590- L11-12 : is there 67 or 68 groups ? P5591- L15-16: The way how the 2 benthic communities are handled is not clear and need more details. P5592-L1-5: This has already been mentioned. P5592: Have you compared the level of primary production/biomass between the models. How different were they? Some changes are made in the model, but not enough to be considered as a proper tuning : please clarify how the changes have been chosen (what to change and what to keep constant even if the coupled model does not produce realistic patterns)

Results: It would be interesting to mention the running time of the coupled model, as including a physical model can increase very strongly the time required to run a simulation. P5595-L24-26: If there is only one phytoplankton group in EwE, how the transition between diatoms-dominated community towards dinoflagellates-dominated community can be modelled? P5596-L9: The reference to the 1990 calibration data is not clear. Some details are missing here to really understand the period of time covered by the models, and the period to which the models are calibrated to (if they are). P5597: As the model has not been extensively recalibrated, the long term scenarios are less easy to interpret. As mentioned before, I would suggest to remove this part.

Discussion: Several issues deserved discussion, as it is currently the case, but some of them could be included in any complex model effort (e.g. global versus regional parameterization). I would suggest the authors to focus the discussion on the issues that are directly derived from the coupling. The advantages and drawbacks of such coupling exercise versus other studies should be stated. Some sentence need to be

clarified (ex P5603-L11-12; P5604-L4)

Figure 2 was not readable in the pdf I got.

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