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Interactive comment on “Population modelling of *Acartia* spp. in a water column ecosystem model for the Southern Baltic Sea” by L. Dzierzbicka-Glowacka et al.

Anonymous Referee #2

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Population modelling of *Acartia* spp. in a water column ecosystem model for the Southern Baltic Sea.

The authors devised a coupled bio-physical model to characterize the population dynamics of calanoid copepods from the genera *Acartia* in the Gulf of Gdansk, in the southern Baltic Sea. A stage-based model previously developed by the authors for another species of copepod was enhanced and parameterized for *Acartia* species with data already published by the authors. This model was coupled to an NPZD-type water column model as a refinement for the Z state variable. The final bio-physical model was forced by physical fields generated by a circulation model of the area.

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Interactive Discussion

Discussion Paper



General comments:

This study follows the current and valuable trend of adding some relevant details to the zooplankton communities considered in biogeochemical models. The authors employed appropriate numerical methods and contributed new information to the domain, which however do not compensate for the lack of novelty and, above all, clarity of the work presented in this manuscript. Several major caveats lead me to recommend that the editor reconsider the publication of a substantially altered version of this manuscript after the authors convincingly address the following general points. I find the overall subject compelling, and I am convinced in the ability of the authors to provide conclusive results. Hence I provide hereafter some general suggestions on both style and substance, but I don't think that at this stage the manuscript requires a line by line detailed review. There will not be specific comments in my review.

Style: Ideas are presented in a muddled manner throughout the manuscript. The general impression left by this manuscript is that it was difficult to follow, objectives were not clear, not identified in the results and not logically addressed and developed in the discussion. The organization of the whole manuscript is puzzling (e.g. Parts of both the results and the discussion should be either in the introduction or in the material & method section). Finally, the level of writing is not suitable to an international scientific paper. Part is structure, and part is English. For structure, please be concise, don't provide irrelevant information, and do not repeat the same (relevant) information at several places throughout the manuscript. Try not to finish the paper with a sentence like this: "Generally, the biomasses of total zooplankton and *Acartia* spp. biomass are in agreement with observations". Your work produced for sure much more interesting results deserving to be the take-home message. For English, it is not my mother language either, and as such I strongly recommend the use of a professional scientific translator at the final step before submission, as I often have to, in order to ensure the readability of the paper which then should make justice to the quality of the scientific message it conveys. Figures are generally good, but be careful to provide enough

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7, C152–C155, 2010

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Comment

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Interactive Discussion

Discussion Paper



information in the caption (for example in Fig. 2, is the model result integrated over the whole water column ?). Before the manuscript could be considered for publication, the authors need to provide a substantial effort in clarifying the objectives of the study and the description of the results, in organizing its sections, and improving the overall quality of the writing.

Substance: This paper can easily be perceived as a sequel of a previous one (Dzierzbicka-Glowacka 2005, JMS 53:19-36) developed for *Pseudocalanus elongatus*. I am aware of the answer to reviewer #1, and I noticed some enhancements too, but the point is not made clear enough how the current work does anything else beyond a technical application of the model to another species. The scope of this manuscript is too restricted to make a paper. A possible objective worth of publishing could be, for example, to add into the model several species of calanoid copepods presenting contrasted life-cycle strategies and compare their respective population dynamics, and the benefits (and/or drawbacks) of trading the bulk Z for a more detailed and complex zooplankton community (e.g. Ji et al. 2009, MEPS 384:187-205). Building up on what the authors already did for *Pseudocalanus* and now *Acartia*, *Temora longicornis* would be an interesting candidate with plenty of data available in the literature (This last sentence is only a suggestion, I don't expect the authors to build a new model in order to publish their work...). In a similar way, I am convinced that the authors could insist more on how much the Gdansk area is representative of the southern Baltic ecosystem, in the context of the ecological shifts already documented in the area. This could strengthen the justification and usefulness of their modeling work. A striking example of what is hard to follow and deceiving in this manuscript is the recurrent discussion about the importance of several zooplankton taxa not studied explicitly in the present study but buried instead into the Z state variable of the biogeochemical model. Why is that so ? They should be focusing their discussion on *Acartia* population dynamics, how much it differs/resembles the dynamics of *Pseudocalanus* they already modelled, what is the information their cleverly designed numerical model provides which cannot be found otherwise... They should emphasize in their discussion how numerical models

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Interactive Discussion

Discussion Paper



are actually one of the most efficient way to integrate and summarize large amount of observation data collected through much money, effort and patience, as well as generating new knowledge about the subject of the study.

I will be really interested in reviewing an upgraded version of this work.

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