

Interactive comment on “Modelling the population dynamics of *Temora longicornis* in the Basin Gdańsk (southern Baltic Sea)” by L. Dzierzbicka-Glowacka et al.

Anonymous Referee #2

Received and published: 18 September 2013

The submitted manuscript reports on the set up of a population model of *Temora* in the Baltic Sea which is ‘tested’ against existing near-shore and offshore data from the Gdansk Bay. I agree with the authors that formulation of model can give insights into the environmental factors driving the dynamics of the population. However, I have some problems with the sound scientific basis if the model and the procedure how the model results were analyzed.

The model is largely based on a formulation of published model on *Acartia* spp and was adjusted to fit the biology of *Temora*. This adjustment has only partly done in the re-formulation of the reproductive response, while development – as far as I understood – was basically adopted from *Acartia*. Apart from the fact that the reproductive model

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is largely described in a paper which is not assessable yet, the reproductive model was formulated - as outlined below in the detailed comments - with published temperature and salinity responses which are erroneous because they ignore copepod mass. Mass – alone and in interaction with environmental variables - is one of the major drivers of seasonal variation in egg production in the Baltic and in fully marine waters, while the published sources relate in-situ and experimental data only on the environment (= the formulated response functions are misleading). I agree with the authors that limited knowledge on the physiology of Baltic copepods hampers the formulation of a 'perfect' model. However, the imperfect model should not be based on wrong assumptions. There are more limitations to the formulation of the model, for instance it is unclear how temperature and salinity effects on development are incorporated into the model or which weights were used to model the transfer (nauplii weights are not available for the Baltic).

In addition, I have problems with procedure to test the model. As the authors describe it, following the formulation of the model it was tested against data and then adjusted several times to fit the data better. I wonder what can be gained then out of the final comparison how well the model describes the real data? As a biologist, I would like to know what adjustments were done and if they have a biological meaning. Apart from this major criticism, the manuscript has further limitations outlined in the detailed comments. The introduction lacks a real objective, the material and methods apparently repeat a lot of information provided in other, already published (partly not accessible) papers. In contrast, other important information on procedures is missing. The results are generally too long and repeat also already published results from field studies in detail. Part of the results discussing differences between model and field should be moved to the discussion in which also the general value/applicability of the model beyond the Gdansk Bay should be evaluated in relation to recent field and long-term studies that tried to identify the drivers behind Temora population dynamics. Otherwise the paper is only of regional interest. In summary, the paper should be re-submitted after revision.

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Line 1: Some of the 'miscellaneous transformations' could be named here.

Line 4: What is the 'very negative' impact?

Line 10: Please explain 'due to the distinction. . . should be considered as organisms. . . What do you mean?

Line 13-14: Stegert not Stengert

Line 17: Dzierzbicka-Glowacka et al. 2013: which article? Citation 2013e is also not available yet, but is essential for the present paper (egg production modeling). All necessary information should be included in the present paper, as the reference cannot be cited yet (and should therefore be deleted)

Line 19 following: many of the statements here need references.

Line 26: Why are studies insufficient? What is missing and to be assessed? What is the missing information regarding Temora?

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Line 4: The authors should provide a sound introduction why they address especially Temora longicornis and what are the objectives in a context of a changing Baltic Sea. What can be learned from a local modeling study in the Gdansk Bay in order to address the issues raised in the introduction (e.g., efficient management. . .)

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Line 8: Please specify how the webpage demonstrate the 'correct' performance of the model. Figure 1 just shows results, but does not illustrate the correctness and is therefore unnecessary.

Line 24: This needs explanation: why was the non-feeding stage N1 grouped together with a feeding stage N2?

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Line 4: The citation is on *Acartia*. This implies that a similar model has been developed for this species and instead of disrobing the details, authors could refer to the published version. I wonder, however, how the authors set the critical mass for moulting, as far as I know, there is no published data for *Temora* in the Baltic Sea. Figure 3: What is the y-axis? Reproduction not repredaction. How can egg production saturate when ingestion saturates, but egestion increases? The different figures need explanation, but I wonder if they are not already published in the *Temora* reproduction paper which is not available yet. It is also left open on which 'currency' the model was run (e.g., nitrogen?) and where does the stage specific data come from?

Line 9: 'The total biomass for each individual. . .', this doesn't make sense.

Line 13 following: More interesting than the formulations of physiological processes is the way they were parameterized. This should be described as well. Dzierzbicka-Glowacka et al. 2009 is missing in the references and DE in Table 1. From where was the Belehraddek function obtained?

Line 4: Where does the value of 15 degrees come from? Dzierzbicka-Gowacka et al. 2009 refers to *Acartia*.

Line 6: There is no 'critical mass of exuviae'.

Line 8: Again, how was the feeding parameterized (food0, kfood)?

Line 11: N2 is the first feeding stage in *Temora* (as it is in *Acartia*).

Line 16, 21: An exuviae are defined as the remains of an exoskeleton after moulting. The use of the term should be checked throughout the manuscript.

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Line 5: Again, the authors apply derived (even not measured) data for *Acartia* to *Temora*, which is not convincing as both groups differ considerably. The q_{10} of Klein Breteler is derived in a fully marine study, in which costs for the much lower salinity are not included. There is compelling evidence in the literature, that energetic expenses are high due to osmoregulation, and therefore, the data cannot just be applied to Baltic *Temora*.

Line 11: Ingestion is related to body weight. How was the much smaller weight of Baltic *Temora* incorporated into the model?

Line 16: Where does the 20% loss rate come from? Reference? Low salinity increase the respiration rate (see Calliari papers on marine *Acartia*).

Line 28: Dzierzbicka-Gowacka et al. 2013e is not available. Still I wonder, how data for food concentration was obtained from Peters 2006 or Holste et al. 2009. Both do not contain egg production in relation to food. Moreover, both are misleading with regard to temperature because egg production is mainly determined by weight for which both papers do not correct and therefore cannot provide essential data for the parameterization of egg production (Dutz et al. 2012 provides a discussion on the main factors that influence *Temora* egg production in the Baltic)

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Line 2: The effect of T is also interacting with the effect of weight, as weight is a function of T and potentially of S (Castellani & Altunbas 2006, Dutz et al. 2012). Thus, egg production cannot realistically modeled with equation 4. Where do the coefficients come from?

Line 7: What is meant by efficiency? Of what?

Line 12: Herrroth 1985 provide data on copepodite weights. How was data on nauplii obtained?

Line 14: From which data was the mortality rate 'determined' from? Or do the authors

assume that mortality is exponential like in Aksnes and Ohman? This is confusing. And one wonders if this is realistic regarding the salinity influence and sensitivity of the species.

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Line 6: It should be listed which parameter need to be adjusted to 'fit' the observation. I think, readers could extract most out of an comparison between originally used and finally 'fitted' coefficients.

Page 12357:

Line 1: Please explain. If I understood correctly, the procedure was the following: you build a model based on published knowledge, and afterwards you fit it to observation data by adjusting the coefficients to obtain the best fit. Then, you test the correlation of the simulation with the observation using Pearson's linear correlation coefficient? This does not make sense to me. That the model results are consistent after such a procedure is no surprise. It would actually be more valuable to know which coefficients were adjusted and how in order to evaluate which processes need to be better understood.

Line 2: Field data is not experimental data.

Line 5: After reading it twice, I still cannot extract what is meant here. Please specify.

Line 11: Use 'simulate' instead of 'determine'.

Page 12358:

Line 4: Figure number missing. For this species it is quite well known that some life history traits correlate better with larger food than others. Therefore, since large and small phytoplankton was modeled anyways, the two size classes could be presented instead of the bulk.

Line 20: In Fig 6, the spring bloom starts around day 60, the increase in egg/nauplii numbers is starting on day 90 with maximum around day 100. This is a discrepancy

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of about one month and this does not fit field observations, which shows an immediate response of an overwintering population (for the Baltic: Dutz et al. 2012, for other areas: Castellani & Altunbas 2006; and references therein). This needs explanation.

Page 12359

Line 1: 'stage duration' instead of 'residence time'

Line 2: What justifies using Acartia development times (as the 2009 paper is about Acartia (not in the references)) for Temora? And why is individual growth declining at 15 degrees? Does this fit with experimental evidence?

Page 12360

Line 1: Evidence suggest that Temora overwinters in an active state and develops into adulthood (Dutz et al. 2010)

Line 2: What was the salinity difference between the two stations? Does this justify the large difference in egg production? Data on salinity should be presented. I assume that the numerical differences are based on the observations by Holste et al, which did not correct for weight differences in their experiments and, therefore, overestimated the effect of salinity. Therefore, Holste's data (from a population at Kiel Bay) cannot be simply applied to any other location without correcting for weight (Temora from the Baltic proper is considerably smaller than from Kiel Bight).

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Line 16: In the field egg production is normally not associated with increasing temperature, but with food (the authors should check literature on reproduction of this species in the field). This presumably is related to the use of data from Holste. However, as explained above, this relationship is wrong because their relationship does not include weight as a major factor determining egg production. Unfortunately, this happens when people copy-paste data of others.

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Line 20: The material and methods leave open how the vertical position was modeled. Copepodite stages of *Temora* are capable of intense diel vertical migration and thus are not bound to a specific layer.

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Line 4: Phytoplankton is given in mmol C, zooplankton in 20-40 mg C. This should be consistent. Microzooplankton is also important food for the species and should also be shown.

Line 19: The paragraph on the seasonal variation is repeating published results and can be reduced to the comparison. As already stated earlier, I find such a comparison redundant when the model is fine-tuned to replicate the seasonal variation. Because the model is expected to perform not very well due to several reasons outlined above, it would be more interesting for modelers and field biologists to see where it failed and how the fine-tuning was performed.

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Line 7: Why are toxic algal blooms discussed in a chapter comparing modeling with data? Is there any evidence for such blooms in May in the Baltic? How important is advection in the area? Fig 10 suggests continuous sampling as implied by the smoothed curve; however, the data is discontinuous and should be accordingly presented.

Page 12364:

Line 4: The salinity effect is interpretation and should not presented here but in the discussion. Also, the salinity differences should be given.

Line 14: Again, presenting the results from the period 2006/2007 is repeating published results and should be avoided, similar to the following discussion on observations of other studies. This has to go into the discussion. Much of the cited literature are reports in Polish, and I wonder if they are assessable for the public.

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Line 19: Möllmann et al 2000 provide no information on the function of zooplankton in the food web. Original literature should be cited and not the paper in which one has read the information. Cury et al. 2008 is lacking in the references. What does the ranking of the abundance of different groups has to do with the topic of the paper?

Page 12366:

Line 1-9: Similarly, the paragraph has little to do with the discussion of the results on the comparison of field and modeled variation in *Temora*. A link to the existing knowledge on ideas on environmental control and to those studies actually investigating the environmental control of population dynamics in the BALTIC is generally lacking. Many statements also are without any reference, but do not originate from this investigation (role as food, producers of fecal pellets)

Line 13: I don't agree. This is what actually hasn't been done in the paper: identification of processes describing the development...and relationship with environmental parameters. It would have been nice though. That the differences in population development in both areas were related to salinity as a factor is the only exception. Since in this case no data was presented the relevance cannot be evaluated.

Line 23: It is nice to see that 'fine-tuning' has led to similar results than in the field. But since the model is based on some unrealistic assumptions it would be nice to see how the 'fine-tuning' has been done. This is left completely open in the following paragraph.

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