All the comments of the reviewer were exhausted in detail in corrected in the manuscript.

Review response:

Reviewer #2:

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 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).

Yes, the model is largely based on a formulation of published model on *Acartia* spp but was parameterised according to the population dynamics of Temora.
Both, reproduction and development as refers to the species studied Temora.
I'm sorry for a mistake by referring to the wrong article.
We corrected the citation to Dzierzbicka-Głowacka et al. (2011) instead of Dzierzbicka-Głowacka et al., (2009).
We do not ignore copepod mass (=weight). In the article, the egg production depends on the female mass and egg mass (see Dzierzbicka-Głowacka et al., 2013).
Unfortunately we do not have data on the weight of Temora in the bay itself, therefore we had to use available data from different Baltic regions, mostly those given by Henroth. I agree that this could be the erogenous, however, it is the best we could possibly get, if in the future we will hopefully be able to obtain more accurate data on the weight of Temora in the bay and the Gdansk Deep, and of course, this will allow as improve the performance of the model.

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).

Nauplii weight came from the work of Arndt and Heidecke (1973), which is, moreover, quoted by Hernroth (Table 8, page 9), this work was oversight in the references for which we are very sorry. Reported by them values probably differ from those actually present in the

Gulf, but as we mentioned earlier this is the best what we were able to obtain. But if it comes to implementation of salinity and temperature influence, we relied on available the available knowledge on those matters, and I think we did the best we could.

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.

The procedure was performed to determine how much parameters are sensitive and susceptible to changes in the range of their values. The procedure consisted of checking the sensitivity of the model.
The parameter values are in the range of their variation found in the literature.
We added a Table giving details.
Our goal was to develop a working model capable of modeling development of Temora population in the Gulf of Gdansk and Gdansk Deep. We already have models for Pseudocalanus and Acartia, and we hope that in conjunction with the present model they will allow us to fully model copepods development, we also hope to apply the model to other parts of the Baltic Sea, and eventually the southern Baltic Sea.
The results are presented in such a way as we wanted to show precisely what data were obtained from field studies and how they relate to the data model. We feared that otherwise the readers could have false impression that the model was not thoroughly tested. But I do agree that they are too extensive and should be slightly shortened.

We hope that the corrected paper became more clear and understandable.

Page 12349
Line 1: Some of the 'miscellanous transformations' could be named here.
I agree that the wording used here is too vague, we mainly meant the long-term changes in zooplankton biomass and structure that were observed in the Baltic by various authors especially decrease of *P.acuspes* stocks (Renz and Hirche 2006, Renz et al 2007). Which are associated with a decrease in salinity in the central Baltic Sea and the coastal zone due to meteorological factors (Lauiainen and Vihma 1990, Malmberg and Svansson 1982, Matthaus and Franck 1992, Mathaus and Schinke 1994, Ojaveer et al. 1998). This paragraph will be corrected to be more specific.

Line 4: What is the 'very negative' impact?
The wording hear is unfortunate, the whole paragraph will be modified, see above

Line 10: Please explain ‘due to the distinction
should be considered as organisms
What do you mean?
Indeed this sentence seems to be confusing, we mean the treatment of zooplankton in a more comprehensive manner and not just as a single variable. I agree that the sentence should be rebuilt to be more understandable.

Line 13-14: Stegert not Stengert
Corrected

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)
Corrected
We meant 2013a. Dzierzbicka-Glowacka et al. 2013e is already in online access. We thought the article will be available earlier, sorry for the inconvenience.

Line 19 following: many of the statements here need references.
Indeed this part lack of proper citation, they were meant to be: Mollmann and Koster 2002 for the section on the role as a food source, and: Chojnacki et al. 1984, Siudzinski 1977, Wiktor and Zmijewska 1985, Wiktor et al. 1982, Line 1979, Line 1984 for the role of *T. longicornis*.

Line 26: Why are studies insufficient? What is missing and to be assessed? What is the missing information regarding Temora?
Studies conducting long-term variability of zooplankton in the southern Baltic are certainly insufficient, available data are often fragmentary. As for *T.longicornis* for example data on the wet weights of the development stages are largely insufficient. Due to the large variation of hydrological conditions in different regions of the Baltic it is difficult to estimate data from other regions, leading to potential errors.

Page 12350
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: : :)
Models are working under a set of strict assumptions, the authors claim that we can simulate the spatial and temporal dynamics of important copepod species.
This is one of the most important points about modeling. It allows us to test our assumptions in a way no simple statistical analysis can equal. Especially to test our understanding of the processes which mathematical representation is embedded in the models.
Ultimately only field data are the source of on information on the environment. However the scarcity of empirical data makes it tempting to fill the gaps with modeling.

Temora is a subdominant copepod species in the Gulf of Gdańsk fulfilling an important role in local ecosystem and therefore we believe that research designed to modeling of this species populations are deliberate. Our ultimate goal is to create a more holistic model for the Baltic copepods, we already have a working model for *Pseudocalanus* sp. and *Acartia* spp., preparing a model for *Temora longicornis* is a next step in this direction. The experience

gained in creating a model for the Gulf of Gdansk will allow us to gradually expand its coverage to the southern Baltic.

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.

We completed the references :

Dzierzbicka-Głowacka L., et al. 2012b A new marine ecosystem 3D CEMBS model (version 2) for the Baltic Sea, IEEE Conference Publications, Complex Systems (ICCS), Article number 6458601, 2012 (10.1109/ICoCS.2012.6458601).

Dzierzbicka-Głowacka L., et al. 2013c i 2013d

The preliminary validation of the model for the hydrodynamic module and the ecosystem module has been presented in two other papers (Dzierzbicka-Glowacka et al. 2012b, 2013c respectively).

The model results indicate that the pre-validated version describing KPP-parameterization and the long-term surface temperature distributions is operating correctly. The initial model validation of the main hydrodynamic parameters for 2000 is given in Dzierzbicka-Glowacka et al. (2013c). The mean correlation coefficient for the sea surface temperature at four points was 0.97035 for 1963-2007. These results indicate excellent conformity of the newly adopted model, both with other models and with the experimental data. The results also show that the parameterization of horizontal mixing needs improvement (Dzierzbicka-Glowacka et al. 2013c). This will be done in the next step of the work on the CEMBS model.

A relationship between the measurements of the main biogeochemical parameters and their calculated levels was obtained (Dzierzbicka-Glowacka et al. 2012b). The correlation coefficients for five selected points are presented in Dzierzbicka-Glowacka et al. (2012b – Table 1). The modelled values resembled the observed ones, with mean (for five points) correlation coefficients of 0.9634 for temperature, 0.9083 for nutrients, 0.6067 for chlorophyll *a*, 0.6189 for phytoplankton and 0.6404 for oxygen. The spatial and temporal variability of plankton is usually so great that any model with the right orders of magnitude in its outputs will fit the data. So even if the correlation coefficient is ca 0.6, this is still a good value. (Dzierzbicka-Glowacka et al. 2013d).

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

According to our research laboratory, N1 and N2 is very difficult to distinguish from each other.In our model the non-feeding naupliar stages were combined with the state variable for eggs. In the numerical calculations it does not really matter whether the non-feeding stage we call egg-N1 or egg-N1-N2. However, it is important that the weights are correct.

We agree with the reviewer about the difference between N1 and N2 and therefore in the revised manuscript we call the non-feeding stage egg-N1. However this does not change our calculation and conclusions.

Page 12352

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.

The authors cite the article for *Acartia* (see Dzierzbicka-Glowacka et al. 2010), because, both processes, ingestion and transfer, depend on the individual mass of specimens at different

stages of their development, using critical mass for moulting W_m ,as described by Carlotti and Sciandra (1989) and Moll and Stegert (2007).
We've added references: Carlotti and Sciandra (1989) and Moll and Stegert (2007).

In this paper, the critical moulting mass is obtained by: $W_m = \left(W_k + \sqrt{2}W_r\right)/\left(1+\sqrt{2}\right)$, assuming that the half saturation value is equal $W_h = 2W_m - W_r$ (Moll and Stegert, 2007), which ensures that ingestion is not reduced before transfer starts and that the function fm_i describing the limitation of ingestion rate as molting weight is $fm_i(W_h) = 0.5$. The transfer rate TRN_i from stage *i* to the next *i+1* is given by a sigmoidal function depending on the W_i and W_m with a reference weight, W_r, as a threshold mass, below which no transfer takes place and $TRN_i(W_m) = 0.5$. (see Dzierzbicka-Glowacka et al. 2010, Fig.3b). This aspect is explained below in this article.

Figure 3: What is the y-axis?
Fig. 3. Corrected.

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?
Corrected.
See the article Dzierzbicka-Głowacka et al. (2013e) for egg production *Egg*.
The different figures in Fig.3 are explained later in this article.
Copepod model was run for "carbon".
The data are taken from the literature and based on our observations, research and numerical calculations.
We added a table giving details.

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

Corrected.

For each of the model stages, biomasses as the product of weights and abundances were calculated

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 Belehradek function obtained?
We added a table giving details.
Line 20 page 12352: corrected; should be Dzierzbicka-Głowacka et al. (2011) instead of Dzierzbicka-Głowacka et al., (2009).
Belehradek J., 1957, Physiological aspects of heat and cold. A. Rev. Phys., 19, 59-82

Page 12353
Line 4: Where does the value of 15 degrees come from? Dzierzbicka-Gowacka et al. 2009 refers to Acartia.
I'm sorry for the wrong information.

Corrected; should be Dzierzbicka-Głowacka et al. (2011) instead of Dzierzbicka-Głowacka et al., (2009).
Our numerical studies, based mainly on experimental data by Klein Breteler and Gonzalez (1986), show that the optimum temperature for the development of Temora is slightly higher than 15^{o}C. In the real environment during summer, in the 15-20^{o}C temperature range, and probably with limited food availability, an increase in temperature reduces growth of almost all developmental stages.
Generally, growth follows an exponential curve up to the optimal temperature (with 15 ^{o}C for Temora and 19 ^{o}C for Acartia and 14 ^{o}C for Pseudocalanus) and decreases for higher temperature.

Line 6: There is no 'critical mass of exuviae'.
I'm sorry for the wrong information. Corrected; should be critical moulting mass

Line 8: Again, how was the feeding parameterized (food0, kfood)?
This information, for $Food_o$ i k_{Food}, is provided in the article Dzierzbicka-Głowacka et al. (2011).

Line 11: N2 is the first feeding stage in Temora (as it is in Acartia).
I agree with the reviewer. N2 is the first feeding stage in Temora, the mistype has been corrected.

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.
Corrected.

Page 12354
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 q10 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 apliied to Baltic Temora.
I'm sorry for the wrong information.
Corrected; should be Dzierzbicka-Głowacka et al. (2011) instead of Dzierzbicka-Głowacka et al., (2009).
I agree with the reviewer. Other values are for Q10 and other parameters (t1, t2 , To…) of the function of temperature *fte* than for *Acartia.*
Also our observations do not exhibit much influence salinity on the development time of the species. See also the explanation below. (Calliari, D., et al. 2006).

Line 11: Ingestion is related to body weight. How was the much smaller weight of Baltic Temora incorporated into the model?
Weights were taken for the southern Baltic Sea by Hernroth (1985) and own data (Mudrak 2004 PhD thesis).
Ackefors 1972 and Arndt and Heidecke 1973, which also should be put in references, sorry for oversight.

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

I'm sorry for the wrong information.Corrected;
Respiration rates are difficult to obtain experimentally. Therefore we assumed that metabolic rate is represented by excretion rate and that excretion can be separated into 2 terms (see Steele and Mullin 1977, Wroblewski 1984, Carlotti and Sciandra 1989). The first term *Ms* represents the basic metabolism and is proportional to weight; the second *Ma* refers to the active metabolism and is proportional to ingestion rate. Here we assumed that juvenile stages, which do not feed, lose 6% of their mass per day as a result of basic metabolism.

Calliari, D., Andersen, C.M., Thor, P., Gorokhova, E. and Tiselius, P. 2006. Salinity modulates the energy balance and reproductive success of co-occurring copepods *Acartia tonsa* and *A. clausi* in different ways. Marine Ecology Progress Series 312: 177-188.

Citation from the above article:
"Our present results do not suggest a high energetic cost of osmoregulation at low salinities. Previous studies have suggested that *Acartia tonsa* could only weakly (if at all) regulate extra cellular water balance at salinities below 31, but the evidence is inconclusive (Lance 1965). In our experiments, respiration did not increase with decreasing salinity, revealing that osmoregulation represented, at most, only a small fraction of the energy budget, as has been observed for other crustaceans (0.4 and 1.3% in *Astacus* and *Eriochir* respectively; Potts 1954, Schmidt-Nielsen 1991), and for the copepod *Eurytemora affinis* (Roddie et al. 1984)."

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)
Dzierzbicka-Glowacka et al. (2013e) (DOI 10.2478/s13545-013-0084-9) is available. In this paper the reviewer finds the answer to all questions. We also added Dutz et al 2012 and Castellani and Altunbas 2006 to refferences.

The hypothesis that the food-saturated rate of production of egg matter is equivalent to the maximal specific growth rate of copepods $ProdEgg = \exp g_{max} - 1$ (μg $\mu g^{-1} d^{-1}$) was used for calculation of the number of eggs produced by each female during one day $Egg = W_{female} / W_{egg}$ *ProdEgg* (no. eggs $female^{-1}$ d^{-1}) (Sekiguchi et al., 1980; McLaren and Leonard, 1995). Here, the egg production rate was obtained, assuming the growth rate for the naupliar stage – feeding stage. Hence, taking into consideration growth for the specific development nauplii stage (see Dzierzbicka-Glowacka et al., 2011), *ProdEgg* for females of *T. longicornis* was computed as a function of food concentration and temperature. The values of *ProdEgg* and W_{female} / W_{egg} were used to determine the number of eggs produced per day by one female. Transformation of these data yields a relationship between the temperature *T* and the number of eggs *Egg* at seven food concentrations (25, 50, 100, 200, 300, 400 and 500 mgC m^{-3}): $Egg = a \exp(b\ T\ ft_2)$, where ft_2 is the parabolic threshold function of temperature.

In the case of the Baltic Sea, egg production of *T. longicornis* is largely dependent on salinity (Peters, 2006; Holste et al., 2009) and decreases with diminishing salinity. According to the findings of a study by Mudrak (2004), the best conditions for the development of *T. longicornis* in the southern Baltic Sea (Gulf of Gdansk) are at a salinity above 7 psu.

Therefore, in this paper, the egg production is made dependent on salinity by a function *fs* including a salinity threshold for egg production of 7 psu.
This means that the egg production rate as a function of three variables – food concentration, temperature and salinity – is given by a non-linear regression : $Egg = a \exp(b\ T\ ft_2)\ fs$,
where coefficients *a* and *b* are functions of the food concentration *Food* and *fs* is a function of salinity *S*. *fs* was defined by exponential function in the shape $fs=1–\exp(\alpha(S–7))$, where a coefficient $\alpha = –0.3$ was adopted numerically including values of *Egg* which were obtained by equations given by (Holste et al., 2009: table 2) that describe the effect of salinity on reproductive success at $T=14^oC$ within unlimited feeding conditions.
As a result of modelled and experimental data the simulated new-mean $Egg = f(Food, T, S)$ is affected by food concentration, temperature and salinity. Both temperature and food concentration are controlling factors; salinity is a masking factor, however, i.e. the decrease in salinity limits the production of eggs (see Dzierzbicka-Glowacka et al. 2013e).
The number of eggs produced per day by one female is ca 1.6 eggs in the range from 4.5 to 2^oC at the beginning of the year, in January and February, when the population is starving. However, *Egg* increases to ca 11 eggs at 4.5^oC when the food concentration increases to a high value ($Food$=280 mgC m^{-3}), at which the growth rate tends to become constant during the spring bloom. At low temperatures and food concentrations, a female produces only about 2 eggs per day or about 70 eggs in the period of egg production, assuming that this period is about 35 days for *T. longicornis* from the southern Baltic Sea. This situation is observed in the winter. However, at high temperatures and medium food concentrations in August ($T=18^oC$ and $Food$=150 mgC m^{-3}), individuals can reach maturity after just 40 days and females produce about 6.5 eggs per day or about 90 eggs during the egg production period, assuming that this period is about 2 weeks long. In the spring bloom, a female produces ca 1.6 times more eggs per day than in summer, when the rate of reproduction is about three times higher than in winter. (see Dzierzbicka-Glowacka et al. 2013e).

Similar values of Egg were obtained by Dutz et al. (2012). The seasonal cycle of reproduction in *Temora longicornis* was investigated in the Bornholm Basin, Baltic Sea, from March 2002 to May 2003. Females reproduced year round with maxima of 9.8 to 12.3 eggs $female^{-1}\ d^{-1}$ in spring and low to moderate egg production during the remaining seasons (Dutz et al. 2012). - it was added in this article.
The egg production rate as a function of three variables – food concentration, temperature and salinity – is given by a non-linear regression : $Egg = a \exp(b\ T\ ft_2)\ fs$.
In this paper, the hypothesis that the rate of production of egg matter is equivalent to the specific growth rate of copepods $ProdEgg = \exp GROWTH_{Nauplii} - 1$ was used for calculation of the number of eggs produced by each female during one day $Egg = X\ W_{female}\ /W_{egg}\ ProdEgg$ (Sekiguchi et al., 1980; McLaren and Leonard, 1995).

Page 12355
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 coeffcients come from?
See above (Dzierzbicka-Glowacka et al. 2013e); the coefficients *a* and *b* were numerical obtained.
In the paper (Dzierzbicka-Glowacka et al. 2013e) was shown that egg production can realistically modeled, as far as is possible.

The factors controlling the temporal dynamics of egg production rate (EPR) in *Temora*

longicornis were investigated during a 3 yr study by Castellani & Altunbas (2006). Their analysis showed that Egg was positively related to copepod body weight and chlorophyll-a concentration, whereas it was negatively related to suspended particulate matter and temperature.

Our numerical simulations show, that Egg increases with increasing available food concentration (including mainly phytoplankton). Temperature has also the effect on the Egg and Egg increases with increasing temperature to 15^{o}C, according to function ft_2; for temperature above 15^{o}C , Egg declines by ft_2.
The studies in the Gulf of Gdansk after Mudrak (2004) and data unpublished for 2006 and 2007 showed that the highest number of *T. longicornis* in the naupliar stage was observed during the spring bloom. This may suggest that egg production to a large degree depends more on food concentration than temperature.

The relationships between food concentration, composition, temperature and egg production are difficult to quantify in natural food conditions.
The relationship between egg production (*Egg)* and chlorophyll-*a* concentration (*Chl-a*) and the conclusion that egg production is food limited are in general agreement with studies of *Temora longicornis* fecundity in Long Island Sound (Peterson and Bellantoni, 1987), the Skagerrak (Peterson et al., 1991) and the English Channel (Bautista et al., 1994). A positive correlation between water temperature and the rate of egg production was demonstrated by Halsband and Hirche (2001) and Lee et al. (2003). Halsband and Hirche (2001) (southern North Sea) concluded that female body size, as a function of temperature during development, rather than present food conditions, was the primary factor controlling egg production in *T. longicornis* during the spring and early summer. But Kiørboe and Nielsen (1994) (the southern Kattegat, Denmark) suggested that temperature did not significantly affect egg production or feeding rates.
The discussion was carried out in the paper Dzierzbicka-Glowacka et al. (2013e).

Line 7: What is meant by efficiency? Of what?
As not all females are reproductive, we considered X=50% of adults laying eggs (=efficiency), i.e. 50% being males or non-productive females.
Based on our experimental studies, 60% were females and 40% - males (see Lemieszek 2013, PhD thesis, unpublished data). Here we assumed a non-productive females of 10%.

Line 12: Hernroth 1985 provide data on copepodite weights. How was data on nauplii obtained?
Weights were taken for the southern Baltic Sea by Hernrotth (1985) for copepodite and own data (Mudrak 2004 PhD thesis, after Arndt, E.A. and Heidecke, D. 1973) for nauplii.

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.
Knowing the values of Di (development time) and Zi (abundance) in particularly development stages we could determine the mortality rate *mz* using the estimation Aksnes and Ohman (1996). Numerical calculations were made using the iterative method.
Di and Zi were the calculated; Di as a function of food concentration Food, temperature T and salinity S and Zi as a variable of model. Food, T are from the ecosystem model.

The authors also performed calculations taking into account the *in situ* data and received that the average mortality rate for CI / CII was in the range 0.10-0.25d^{-1} and older stages was 0.05 - 0.10 d^{-1} (paper in press). *mz* determined from numerical and field data are in the same range of values.

I agree with the reviewer that the salinity influence but it is considered by the parameter Di and Zi.
I believe that this equation can be used, because it does not contain any numerical coefficients, and only depends on Di and Zi, which in turn depend on the parameters of the environment.

Page 12356
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.
We added a table giving details.
In Table given the parameter values for Temora in the present model and their grade of sensitivity.

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.
The procedure (*simulation-analysis of results-modification of the model*) is done in the same way as for other numerical models.
Calibration of the model was done for different years than those discussed in the article.
The procedure was performed to determine how much parameters are sensitive and susceptible to changes in the range of their values. The procedure consisted of checking the sensitivity of the model.
The parameter values are in the range of their variation found in the literature.

We feel there is not enough data yet to get good statistics, anyway. Besides, we use the field data only to validate the model and the data themselves are not the point of the paper.

We added a Table giving details.

Line 2: Field data is not experimental data
Corrected

Line 5: After reading it twice, I still cannot extract what is meant here. Please specify.
Indeed this sentence is confusing, we just wanted to say that the discrepancy between the data obtained from the model is usually in the variation observed in different field studies.

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

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.
Corrected. Fig. 6.
Concentration of available food (*Food* = 50%*Phyt* +25%*Zoop* + 25%*Detr*) (Figure 6) – a mixture of phytoplankton, microzooplankton and pelagic detritus used in the population model as the input data for the model, are values from the 3D CEMBS ecosystem model. In addition, phytoplankton biomass consists of three variables: the biomass of small phytoplankton and large phytoplankton such as diatoms and cyanobaceria.
So, in order to present all the components of food, we would need to prepare ten additional figures.
We cannot add these figures, such decision need to be made by an editor.

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 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.
The numerical simulation starts from the wintering population of adult specimens. Production of eggs starts late March, with the beginning of spring phytoplankton bloom and a temperature increase, with maximum around day 100 when the food concentration also reaches the maximum value. It is hard to see in the Fig. 6; which shows average in the water column, it is more visible on Fig. 9. with vertical distribution. The model does not show a rapid change in the number of Temora with a change in environmental conditions, but a gradual shift over time.
We completed literature: (Dutz et al. 2012 and Castellani & Altunbas 2006)

Page 12359
Line 1: ‘stage duration’ instead of ‘residence time’
Corrected.

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?
I'm sorry for the wrong information.
Corrected; should be Dzierzbicka-Głowacka et al. (2011) instead of Dzierzbicka-Głowacka et al., (2009). It has already been explained above.

Page 12360
Line 1: Evidence suggest that Temora overwinters in an active state and develops into adulthood (Dutz et al. 2010)
The numerical simulation starts from the wintering population of adult specimens; this assumption was made on the basis of our research.
We addend Dutz et al. 2010 to references, as it was mentioned earlier.
We are aware that our model requires a lot of work, and that there many publications that we have accidentally omitted.

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).
Corrected. Added figure for salinity.
In this paper, the hypothesis that $ProdEgg = \exp Growth - 1$ was used for calculation of the number of eggs produced by each female during one day $Egg = W_{female}/W_{egg}\ ProdEgg$, assuming that W_{egg} and W_{female} are defined for the southern Baltic Sea.
We took into account Holste's data (Holste et al., 2009) to improve our equation for *Egg.*

The Holste's et al., (2009) paper concerns the Baltic Sea:
Holste, L. and John, M.A. St.: The effects of temperature and salinity on reproductive success of *Temora longicornis* in the Baltic Sea: a copepod coping with a tough situation, Mar. Biol., 156, 527-540, 2009.

Page 12361
Line 16: In the field egg production in 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.
All these aspects have been already discussed in another article (Dzierzbicka-Glowacka et al. 2013e: figure 3).
This relationship Egg is not wrong because it includes weight (W_{egg} and W_{female}) as a major factor determining egg production; the value of W_{female}/W_{egg} is concealed in the coefficient of *a*.
The Holste's et al., (2009) paper concerns the Baltic Sea, as it was mentioned abowe.

Line 20: The material and methods leave open how the vertical position was modeled. Copepodite stages of Temora are capable of intense dial vertical migration and thus are not bound to a specific layer.
I agree with the reviewer that Copepodite stages of Temora are capable of intense dial vertical migration and thus are not bound to a specific layer.
Corrected. The equation (2) was completed..
Simulated weighs and abundances are connected as function of time and depth (t, z).

Page 12362
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.
Corrected units.
Concentration of available food ($Food = 50\%Phyt + 25\%Zoop + 25\%Detr$) (Figure 6) – a mixture of phytoplankton, microzooplankton and pelagic detritus used in the population model as the input data for the model, are values from the 3D CEMBS ecosystem model.
In addition, phytoplankton biomass consists of three variables: the biomass of small phytoplankton and large phytoplankton such as diatoms and cyanobacteria.

So, in order to present all the components of food, it should be presented at ten additional figures, such decision is need to be made by an editor.

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.
Results for Temora species were not published in 2010 and 2011. Calibration of the model was carried out on another set of data. So you can not say that the model was fine-tuned to the data on which the validation was done.
Explanations are given above.

Page 12363
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.

Advection is important especially in the more coastline area as regional upwelling can occur, and of course at a time when there is no fixed water stratification. Algal blooms can occur in the bay of Gdansk in May, but unfortunately we do not have precise data on phytoplankton growth during this period. Although we are aware that during investigated period there was a significant river inflow to the bay associated with flood. Which resulted in a significant algal blooms and increase of zooplankton biomass. This event, however, was not reflected in the model as it does not include actual inflow from rivers, only the average values (see description of the 3D model CEMBS -Dzierzbicka-Glowacka 2013 c,d). Therefore we thought that algal blooms should be mentioned.

Corrected Fig. 10.

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.
Corrected.
Added figure for salinity.
Difference in salinity between the depth of the bay is quite significant and can go up to 4-5 PSU in the deeper parts, which certainly can have an effect on egg production.

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.
The Copepod model results for the Temora were not published anywhere.
Only the data field for Temora for 2006 and 2007 have been published (Dzierzbicka-Glowacka 2013b).

Indeed, some of these publications: Gaj (1999), Guzera (2002) and Rakowski (1997) may not be available and will be removed, sorry for inconvenient. Other publications as Siudziński (1977) and Szaniawska (1977) are assessable, but due to the time of issue only in Polish.

Page 12365:
Discussion
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. Curry 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?
I am sorry for miss citation, Cury et al 2008 have been added. I agree that paragraph about different groups of zooplankton is unnecessary, and will be removed.

Cury P.M., Shin Y.J., Planque B., Durant J.M., Fromentin J.M., Kramer-Schadt S., Stenesth N.C., Travers M., Grimm V., 2008, Ecosystem oceanography for global change in fisheries, Trends in Ecology and Evolution, 23(6): 338-346.

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 knowl-edge 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)
In this paragraph we wanted to highlight that copepods play an important role in the ecosystem, and that our research have a deeper purpose.
I agree that some of the statements in this section requires additional citations.
(Vuorien et al. 1998, Mollmann and Koster 1999, Urrere and Knauer 1981)

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.
Effect of salinity on the development of the studied species Temora in our numerical simulations has been included by: suitable weight individuals for the southern Baltic Sea, determination the appropriate values of maximum ingestion rate and egg production is also dependent on salinity.

I can agree that our goal was not fully achieved, but it is certainly a step in the right direction and we hope that our work can be used in future assessments in this mater.

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.
The "fine-tuning" has been described for *Acartia* spp in previous articles. The "fine-tuning" of Temora was made in similar way (differing only in taken parameters), but as requested by the reviewer we will briefly present it in the corrected paper.

The previous work has been cited to make a statement of what is actually new in the present. There is no need to summarize the previous work. However if the papers were not cited (it may lead to false assumption that we are trying to sell the same research) someone might have the incorrect impression that the authors (us) try to sell the same research twice.

We would like to express our thanks to Reviewer for his/her very instructive and profound comments.

Additional papers will be added to the references:

Ackefors, H. 1972, The amount of zooplankton expressed as numbers, wet weight and carbon content in Askö area (The Northern Baltic proper), Meddel. Havsfiskelab. Lysekil 129: 1-10

Arndt, E.A. and Heidecke, D. 1973, Zooplanktonuntersuchungen im Küstenbereich der Mecklenburger Bucht, Wiss. Zeitschr. Univ. Rostock, 22 Mat. Nat. R., H 6-7: 599-616

Belehradek J., 1957, Physiological aspects of heat and cold. A. Rev. Phys., 19, 59-82

Castellani, C. and Altunbas, Y. 2006, Factors controlling the temporal dynamics of egg production in the copepod Temora longicornis, Mar. Ecol. Prog. Ser. 308: 143-153

Chojnacki, J., Drzycimski,I. and Siudzinski, K. 1984, The ecological characteristics of the main species of crustacean in plankton of the southern Baltic. In Articles on Biological productivity of the Baltic sea. Moscow, vol. 2, pp. 148-171 (in Russian)

Dutz, J., Mohrholz, V., van Beusekom, J.E.E. 2010, Life cycle and spring phenology of Temora longicornis in the Baltic. Sea. Mar Ecol Prog Ser 406: 223−238

Dutz, J., van Beusekom, J.E.E. and Hinrishs, R. 2012, Seasonal dynamics of fecundity and recruitment of Temora longicornis in the Baltic Sea, Mar. Ecol. Prog. Ser. 462: 51-66

Launiainen, J. and Vihma, T. 1990, Meteorological, ice and water exchange conditions. Second periodic assessment of state of the marine environment of the Baltic Sea, 1984-1988. Baltic Sea Environ. Proc., 35 B, 22-33

Malmberg, S.A. and Svanson, A. 1982,Variations in the physical marine environment in relation to climate. ICES C.M. 1982/Gen: 4. Mini Symposium

Matthäus, W. and Franck, H. 1992, Characteristics of major Baltic inflows-a statistical analysis. Cont. Shelf Res., 12, 1375-1400

Matthäus, W. and Schinke, H. 1994, Mean atmospheric circulation patterns associated with major Baltic inflows. Deutsche Hydrograph. Z., 46, 321-339

Ojaveer, E., Lumberg, A., and Ojaveer, H.1998, Highlights of zooplankton dynamics in Estonian waters (Baltic Sea). ICES J. Mar. Sci., 55, 644-654

Urrère, M.A. Knauer, G.A. 1981, Zooplankton fecal pellet fluxes and vertical transport of particulate organic material in the pelagic environment, Journal of Plankton Research, 3(3) pp. 369-387

Vuorinen, I., Hänninen, J., Viitasalo, M., Helminen, U. and Kuosa, H. 1998, Proportion of copepod biomass declines with decreasing salinity in the Baltic Sea. ICES J. Mar. Sci. 55, 767-774