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

Review response:

Reviewer #1:

The main objective of this study is to determine the population dynamics of the T. longicornis in the southern Baltic Sea based on the numerical analysis. Much similar studies have already been published for *Pseudocalanus* sp. and *Acartia* spp. from the Baltic Sea by the same authors. So evaluation of this study, as well their previous ones, depends on the feasibility of the numerical model.
Of the three parts of the study, i.e. (1) determination of the functional relationships between physiological processes and environmental parameters,(2) determination of the population model for T. longicornis connected with the ecosystem model 3-D CEMBS and (3) emperical verification of the population model based on in-situ data. I concern mainly with the above (2) and (3), and have several comments and question as below.
1) In the section of Baltic ecosystem model 3-D CEMBS, for example, Fig. 1 presents the results from the model for hydrodynamic and biogeochemical variables on 2 May 2012. I wonder how the population model for T. longicornis is connected with 3-D CEMBS, because 3-S CEMBS incorporate one of the variables such as zooplankton. I feel difficult to understand how the above zooplankton in 3-D CEMBS is equivalent to T. longicornis in the population model, or whether the above zooplankton was replaced with T. longicornis in the population model.

There is only one zooplankton class (microzooplankton) in the 3D-CEMBS model, which grazes on the all three phytoplankton classes. The microzooplankton consists of ciliates and other heterotrophic protists, which are filter-feeders, feeding on phytoplankton.
Zooplankton growth rates vary with the food source: it is higher when small phytoplankton is the food source, and lower when diatoms are grazed. The growth of the microzooplankton is correlated exactly with that of the phytoplankton generally speaking, the numbers of microzooplankton in the upper layer were the highest, when the algal biomass there was large. Represented as passive particles, the microzooplankton is assumed to be immobile. This is not realistic, but its speed is very, very low – c. 0.5 body length. Therefore, the speed of microzooplankton can be assumed to be zero. The detrital class mentioned above does not sink (SDetr), and thus represents dissolved organic carbon as well as very small particles.

We added a new figure (Conceptual flow diagram of the 3D CEMBS model with copepod model) presenting in a schematic way different biological processes controlling growth and population dynamics for the investigated species in the model.
The population model was coupled to 3D ecosystem model 3D CEMBS to simulate the annual life cycles of *Temora longicornis* during 2010 and 2011 on a 3D grid into sub-basin – Gulf of Gdańsk.

Although we present the results only for two stations representing two different environments, the model calculates the parameters on a 3D grid making it possible to have an actual "spatial-temporal" estimation of the parameters, something that will be needed for any long-term forecasting as soon as we have reliable estimates of the future forcings (winds, precipitation, nutrient river inflows etc).

Only with a 3D circulation model it is possible to calculate the temporal variability of the studied parameters (not to mention the spatial one).

2) For the sections dealing with empirical verification of the population model, I do not agree to author's discussion. Particularly the verification based on in-situ data at each station (P1 and P2) is of much problem, needing to confirm their representatives or average values of the field situation and I do not agree to without such confirming.
On the contrary, I ask the authors to make clear the proposal or field-sampling design in future in order to verify their model using in-situ data. And further I like to ask the comparison of seasonal dynamics of *Pseudocalanus* and *Acartia* spp. in their previous studies and Temora in this study based on their numerical analysis, so that the authors are able to evaluate the validity of the model by judging whether seasonal and spatial variations in the rank-in-dominance of each of the above species is simulated in the Baltic Sea.

Methodology used in field studies was based on HELCOM recommendation for monitoring and guidelines developed for zooplankton sampling in the Gulf Gdańsk in 70's and 80's (Siudzinski 1977, Wiktor et all 1982, Wiktor and Żmijewska 1985). Of course it is impossible to have model and field data fit perfectly due to model processing and limitation of field sampling. In the ligation of the above, we have assumed that our field studies were representative enough for aforementioned model.
Of course, I agree that increasing the area and frequency of sampling would improve quality of our research and this is what we are planning to do in future investigations. Although we were not able to do so in this study due to our technical limitations.

I agree that such a comparison would allow for a broader validation of the model, however the aim of this study was to show the operation of the model specifically for *Temora longicirnis*. I assure, however, that in the future we are planning more detailed work featuring comparison of all our working models.

Models are working under a set of strict assumptions. 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.

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