Biogeosciences Discuss., 12, C5826–C5829, 2015
www.biogeosciences-discuss.net/12/C5826/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Projected climate change impacts on North Sea and Baltic Sea: CMIP3 and CMIP5 model based scenarios" *by* D. Pushpadas et al.

Anonymous Referee #3

Received and published: 28 September 2015

General comments

I am sorry to say that my recommendation is that this paper be rejected in this current form. This is on the grounds of a lack of substantial new scientific insight to the question at hand. The question of downscaling climate impacts to the regional marine environment is important and interesting, but it is not really clear from the text what the significant new findings from this work are. This paper has the potential to make a significant contribution, but this potential is not realised. The authors have at their disposal the novel resource of a small ensemble of simulations drawing from both the CMIP3 and CMIP5 models. These could be used to explore in quantitative detail the spread of responses of different aspects of the marine ecosystem, either comparing

the CMIP3 and 5 runs or treating them as a single ensemble. The latter would mix model and scenario uncertainty, but this is not such a big problem. This could be used to make real progress on understanding how reliable future projections are for a much wider range of ecosystem parameters. As its stands we are simply presented with quite basic diagnostics from a limited range of model variables (mainly SST, SSS, netPP), some ensembles means and variances, but very little in the way of quantitative analysis relating the forcing aspects to the response. Alongside this I have to criticize the lack of mechanistic insight offered by this work. Something that is missing throughout the work is 'why' (in a quantitative sense) one model forcing gives a particular response, whereas a difference model forcing gives a different response. Where explanations are offered, they are drawn quite loosely from the literature and some are questionable (see below). This body of work could be developed into a good paper in either of these directions (uncertainty in projections or mechanisms or both), however I would anticipate it would require a further round of revision/review prior to being acceptable, hence my recommendation.

Detailed comments

1.1 The motivation for doing this work, the purpose of the study and what we will learn from it needs to be clearly stated up front; presently this is missing

12231 line 25 'favours short characteristics time scale' Need to be more specific here – what properties and what time scales (e.g. temperature varies on weeks to months, salinity varies on months to years')

2.1 A bit more detail on the physical model is needed, e.g. horizontal/vertical resolution, type of sub-grid scale parameterisations used, surface/lateral boundary condition approaches. And a bit more detail on the ecosystem model, particularly what temperature dependencies are included.

2.3 Some discussion of the disadvantages of the delta method is needed, e.g. relating to changes in inter-annual variability and non-linearities.

1237 line 19 If a purely passive temperature/salinity boundary condition then no response to changing oceanic T and S can be included, this limitation and its implications should be made clear (if this is the case).

12237 line 24 onwards – need to clearly explain notation.

12238 line 11 Again specifics of the time scales of the North Sea are required, with relation to spin up. It is no clear what period of adjustment is being allowed for spin up in these experiments, and what the sensitivity of the results are to the choice of this period.

12241 Line 12 Sentence beginning "Projected. . .." Needs clarifying

12241 The comparison to Holt 2014, is incorrect as that paper only considers IPSL-CM4

12244 line 25 on Some explanation of why MLD is chosen as a metric here? Are the changes in wind stress sufficient to explain the changes in MLD (calculating change in Ekman depth would be helpful).

12246 line 15 Similarly - is the change in wind stress sufficient to increase upwelling/primary production here? There's an opportunity to develop the mechanistic response, e.g. by referring to Ekman dynamics

12246 line 25 The North sea does not show a uniform reduction in nutrients and primary production, but also shows coastal increases in both, that are in places statistically significant. Some explanation for this is needed.

12247 line, the model with the smallest increase in netPP (IPSLCM5) appears (by eye) to have a large drop in winter nutrients at the boundaries – it would be insightful to quantify the relationship between winter nutrient bc change and netPP change for each ensemble member.

12247 line 13 if the changes in cyanobacteria are not going to be discussed further

and explained, it is probably best not to mention them

12447 line 24 onward. This contradict the general statement about the North Sea made earlier, and some of the positive changes are statistically significant, so some explanation is needed. As far as I can see from Holt 2014 figure 10 the effects of temperature on biological rates are small and not sufficient to account for the increase in near coastal production. Of course the treatment of temperature effects on biological rates in ECOSMO is most likely different than in ERSEM, but no details are provided. The comment that the temperature effects on production cancel out across the North Sea needs to be explained (ie which temperature effects), and justified.

12248 line 11 onward. Fig 12 suggests that the air temperature effects on netPP in the North Sea are nowhere statistically significance, is this true of each member? How significance testing is carried out across an ensemble needs to be clearly explained.

12249 line 1 onwards. It is not clear what the present work adds to help with the (quantitative) disagreement between Groger (2013) and Holt (2012).

12249 The point of the discussion around fig 13 is not clear – no explanation for the differences between North Sea and Baltic Sea in terms of trophic response are offered.

Section 4 Only the first three paragraphs of this section summarise the conclusions of this work, and as they stand these are not very substantial - see general comments.

The remainder of this section is largely a discussion on methodology and is full of opinion and conjecture. This material is not appropriate for a conclusion section – it really belongs in a discussion section or introduction. It needs to clearly state the evidence that backs up the various statements around experiment design.

Interactive comment on Biogeosciences Discuss., 12, 12229, 2015.