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Interactive comment on "Simulation of factors affecting *E.huxleyi* blooms in arctic and subarctic seas by CMIP5 climate models: model validation and selection" *by* Natalia Gnatiuk et al.

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Dear Prof. Heinze,

Thank you very much for your assistance with our manuscript: "Simulation of factors affecting E.huxleyi blooms in arctic and subarctic seas by CMIP5 climate models: model validation and selection" by N. Gnatiuk, I. Radchenko, R. Davy, E. Morozov, and L. Bobylev. We are very thankful to the anonymous reviewers and greatly appreciate their valuable comments and suggestions that considerably improve our manuscript. Regarding the comments from both reviewers, we decided to refuse from focusing on the E. huxleyi problematics per se and concentrate on the main goal of the manuscript,

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which is a selection of those climate models that simulate most efficiently the state of abiotic parameters relevant to living conditions of the phytoplankton communities inherent in a number of seas at subpolar and polar latitudes. Accordingly, the Introduction is thoroughly recast. As both reviewers suggested us to improve the "Results and Discussion" section, we will move Figures 4, 6, and 7 to section "Materials and Methods" in order to better describe our methodology of climate models selection. Also, Figures 3, 5, and 8 are deleted as they are either a mere modification of presentations of some other akin figures or their presence in the manuscript is not so important. We also decided to add a new figure (#Figure 7) to section "Results and Discussion" as both reviewers suggested to cover in discussions all studied parameters and seas. So, Figure 7 displays a spatial distribution of biases in five parameters between models and reanalyses in six target seas. The biases are averaged over the vegetation season and 1979/1993-2005 period. Here we give an example of that figure illustrating sea surface salinity. We further will improve the section "Results and Discussion" following the comments from both reviewers. Please kindly find attached the responses to the reviewers and effected revisions, as well as a detailed specification of the changes we introduced. We are looking forward to hearing from you considering these changes and await further instructions. On behalf of the paper's co-authors

Best regards, Natalia Gnatiuk (and co-authors)

Response to Reviewers and Proposed Revisions:

We are very grateful to both reviewers for their constructive and valuable comments and very useful suggestions, which will greatly help in improving our manuscript. Concerning the comments of both reviewers on the choice of factors controlling phytoplankton blooms in general, and coccolithophore in particular, and other reviewers' comments directly related to the coccolithophore blooms, we fully agree with the arguments provided by reviewer #2 in section "general comments": "By not having a primary focus on E.huxleyi blooms in the Introduction, the reader will be able to recognize the wider implications of this extensive intercomparison of climate models – it will also alleviate

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some of the major issues of neglecting “what else” underpins coccolithophore blooms and their occurrences.”

Actually, the main objective of the study was to analyze how CMIP5 climate models reproduce different oceanographic and meteorological parameters in the arctic and subarctic seas as well as to form a methodology for validation and selection of the optimal model sub-set. To have practical use of the results we have chosen for case study oceanographic and meteorological parameters that influence coccolithophores blooms in studied arctic and subarctic seas. Due to the fact that we did not consider in the article all the factors (including biotic ones) that influence coccolithophores bloom, we mistakenly paid too much attention to coccolithophores and the factors affecting their blooms. This resulted in shifting the paper’s focus away from the main goal of the study, i.e. to develop a methodology of validation and selection of climate models that simulate most accurately the abiotic conditions within the target marine areas. To mend the situation, we decided to refuse from focusing specifically upon the issue of coccolithophore blooms and put at the forefront the methodology of validation and selection of climate models. In the absence of a close connection to coccolithophores, the article indeed gains greater clarity and becomes focused on the substance of the research done on the comparative effectiveness of global climate models for specific marine objects. We corrected the manuscript according to the recommendations of the reviewers and tried to make the goal of our research clear and precise. Below we have presented all the answers to the comments and all text changes. We earnestly thank the reviewers for their critical comments.

Reviewer #2

General comments

This study evaluates (and ranks) the performance of 34 climate models in simulating 5 physical parameters [namely, sea surface temperature (SST) and salinity averaged over 0-30 m (SSS); surface wind speed at a height of 10 m (WS); ocean surface current

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speed (OCS); shortwave downwelling solar radiation (SDSR)] on a sub-regional scale in the Arctic and Subarctic regions. These 5 parameters are selected as "forcing factors (FFs) controlling E. huxleyi blooms in arctic and subarctic seas" (p. 2, line 21) and tested in six seas (Barents, Bering, Greenland, Labrador, North and Norwegian seas) where the coccolithophore Emiliania huxleyi is known to form blooms.

I believe the core of the study is interesting and merits publication, but I think the authors could and should do a better job at discussing (all) the results. By not having a primary focus on E. huxleyi blooms in the Introduction, the reader will be able to recognize the wider implications of this extensive intercomparison of climate models – it will also alleviate some of the major issues of neglecting "what else" underpins coccolithophore blooms and their occurrences. Nothing wrong with mentioning your motivation for selecting the regions of interest and the potentially relevant abiotic parameters, but as is, the reader is expecting more than is actually presented re. coccolithophore blooms (see below).

Specific comments

The authors explain that this study is a precursor for another study (in prep/planned by Kondrik et al.), in which these FFs will be applied to "model the future dynamics of E.huxleyi blooms" – so in fact, the current study has very little to do with E. huxleyi blooms apart from being the motivation for the presented set-up. There is no objection to test the model performance of the selected parameters, but the authors should do a better job at explaining why these factors were selected, and others ignored (i.e. because they cannot be assessed in the models? I wonder). Because it could be easily argued that the authors miss a crucial parameter in their line-up of FFs – nutrient availability – that arguably underpins any phytoplankton bloom (i.e. sustained exponential growth). Any biotic factors (e.g. grazing pressure) are ignored herein. Indeed, it is unclear what correlations are sought between the various FFs and E. huxleyi blooms – what do you mean with "affecting"? – e.g., the onset (triggers), the duration/maintenance of blooms, other affects?

After reading the ms, I felt that the study is a valid and interesting intercomparison of climate models, raising important issues in simulating abiotic parameters, but that the initial focus on E. huxleyi seems too specific here; i.e. without it, all results could be presented just as well – or even better as these parameters surely affect more than just E. huxleyi, thus giving the study a wider relevance. In fact the authors conclude rather generally, without discussing specific implications for the next / planned study by Kondrik et al. – so that also gives the impression that the initial motivation need not take central stage in this ms (or the title). Still, I don't fully understand what the strategy would be in "selecting the best models" for such follow-up study, given the multivariate outcomes, this could/should be better explained in the final discussion and conclusion.

Figure 9 ("heat map") is a good visual representation of the amount of work performed and the complexity of the outcomes; not only does it show the range in performance between the listed models (1-34), but also how within one model the chosen parameters are simulated at different strengths – and, possibly even more intriguing (disconcerting?) that a model that performs very well for one sea, does not in another (for example, compare model 1, ACCESS1-3 in Barents and Bering Seas). Indeed, the authors conclude that the results "show that there is no optimal model ensemble or one top-model which could best simulate all factors across all of the study regions. Despite the fact that the Arctic is often considered as one single region in many studies, our results show that CMIP5 climate models do not have consistent performance across such a large area" (p.8, L. 12-15).

What I miss, is an in-depth discussion why these inter-model, inter-parameter and inter-subregional differences exist – is this due to issues of spatial resolution, initial parameterization of each model (what it was built for) or real physical differences between the seas that models cannot address/capture? Again, I don't know, but would be interested to learn what factors could underpin the results in Fig. 9. Currently, the "results and discussion" section reads as a list of figure descriptions rather than highlighting the main take-home messages (while figure captions could do with more information).

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Moreover, only one of the 5 factors is highlighted (SST) as “an example” – I believe the paper would have a much greater impact if the other parameters get equal treatment or at least their highlights mentioned and discussed in the main text, not only in a supplement.

Reply to Reviewer # 2:

We thank Reviewer #2 for very helpful comments. We fully agree with the arguments regarding the suitability of application the percentile-score based method climate models’ selection based on several important parameters that impact blooms not only E. huxleyi but also other phytoplankton in the study areas. Therefore, we decided to change the focus from the coccolithophore blooms and concentrate on the methodology of choosing climate models. This step is fully justified, since this work, in its essence, is certainly not connected with the work performed by Kondrik et al. (2019) of simulating conditions that modulate the intensity of coccolithophore blooms. Meanwhile, this was an issue that caused the criticism of the reviewers. In the absence of a close connection to coccolithophores, the article indeed gains greater clarity and becomes focused on the substance of the research done on the comparative effectiveness of global climate models for specific marine objects.

We will improve the section “Results and Discussion”, to cover other parameters and to discuss the inter-model, inter-parameter and inter-sub-regional differences.

Thus, we decided to add to the section Results and Discussion a new figure displaying the spatial distribution of biases in five parameters between the models and reanalysis data in target studied seas; the biases are averaged over the vegetation season and the time period 1979/1993-2005. So far we present an example of this kind of figure using sea surface salinity (PSU), averaged over 30 m, for 6 studied seas: “Figure 7. Spatial distribution of biases in five parameters between models and reanalysis in six studied seas averaged over the vegetation season and the time period 1979/1993-2005. ” We added an example of the text: “In order to analyse how well the selected ensemble

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models represent the five studied parameters, we calculated the spatial distribution of biases in the parameters, which were calculated as the difference between selected ensemble models and reanalysis data, in six studied seas - Barents, Bering, Labrador, Greenland, Norwegian and North Seas (Figure 7). Thus, less biases in SSS are determined in the case of the Labrador, Greenland and Norwegian Seas ($\pm$0.5 PSU); low biases proved to be often in the Bering Sea next to the coastline - up to 1.5-4 PSU, this overestimation is possibly due to the river runoff from the Alaska mountain Kuskokwim mountains, the Alaska and Aleutian ranges SSS is underestimated in waters next to the coastline in the Barents and North Seas (1.5-2.5 PSU), which is probably due to the overestimation of river runoff or underestimation of salty Atlantic water."

We consider to a) move Figures 4, 6, 7 to section Methodology section as they mainly describe our methodology of model selection, and b) delete Figures 3, 5, 8 due to either their resemblance to some akin figures or because their presence is not so important in the manuscript.

Abstract: first sentence, shortly name the reasons; why only carbon cycle mentioned here, as opposed to carbon and sulphur cycles in first sentence of Intro?

Answer:

We removed the first paragraph of Abstract in order to concentrate on the main goal of our manuscript. We changed the first paragraph as follows: "Currently, there are a large number of climate models that give projections for various oceanic and meteorological parameters in the Arctic. However, their estimates often differ in absolute values in individual sea areas in comparison with the historical reanalysis data. The main goal of this research was to find out the optimal ensemble models that most accurately reproduce the abiotic parameters inherent in six selected seas, viz. the Barents, Bering, Greenland, Labrador, North and Norwegian Seas."

Line 25 (last paragraph): too much information (and acronyms) for abstract. Remove.

Answer:

We removed the last paragraph.

Intro, p. 4. Lines 8 -14 - this paragraph "goes without saying"; what follows is generic order of methods, results, discussion.

Answer:

We will either delete this paragraph or reword it.

Intro/Methods: What is a CMIP5 climate model / the CMIP5 project? Define and describe – currently not done anywhere.

Answer:

We introduced “CMIP5” in Introduction as follows: “Today, there are a large number of CMIP5 (Coupled Model Intercomparison Project phase 5) models that differ in the reliable representation of specific parameters, which mainly characterize the abiotic factors, which together with other variables determine the habitat of biota in surface waters of the target seas at a regional scale. ”

Figure 1 (if kept as motivation for selected regions), please state what type of data are shown and cite data sources in caption.

Answer:

We corrected it as follows: “Figure 1: Spatial distribution of the frequency of E. huxleyi blooms based on Ocean Colour Climate Change Initiative dataset version 3.0 (Kazakov et al., 2018) for the Barents, Bering, Labrador, Greenland, North, and Norwegian Seas.”

General: Many figure captions need more details for reader to follow or identify data Sources.

Answer: We improved the figures captions as follows:

Figure 2: A schematic representation of the percentile score-based model ranking

method (Division of RMSD values distribution of 28 models into four groups that are limited by 25th, 50th and 75th percentiles and relative assignment of scores from 3 to 0 for each group accordingly - very good, good, satisfactory and unsatisfactory).

Figure 3: Box plots of the spatial variability of SST biases, which are calculated as the difference between model and reanalysis data, in the Barents Sea over the vegetation season and the time period 1979-2005. Each box spreads from the lower quartile Q1 to the upper quartile Q3 of biases, the orange lines represent the medians. The lower "whiskers" are represented as Q1-1.5 Standard deviation, and the upper "whiskers" are represented as Q3+1.5 Standard deviation.

Figure 4: Spatial distribution of biases in SST (K) between models and reanalysis in the Barents Sea; the biases are averaged over June-September.

Figure 5: Spatial distribution of errors, which are calculated as the difference between model and reanalysis, in annual SST trends (K yr-1) in the Barents Sea (June-September)

Figure 6: Heat map with the final model scores obtained using the percentile score-based model ranking method for five parameters (sea surface temperature (SST, K) and salinity averaged over 0-30 m (SSS, PSU), surface wind speed at 10 m (WS, m s-1), ocean surface current speed (OCS, m s-1), and shortwave downwelling solar radiation (SDSR, W m-2) for the Barents, Bering, Greenland, Labrador, North, and Norwegian seas based on different statistical metrics (Figure 2, Table 2). White areas indicate that the model was not considered due to partial or complete unavailability for historical, future projections (RCP4.5, RCP8.5) data.

Technical corrections

If you decide to keep Emiliania huxleyi in, know to write the full species name the first time the species is introduced in the text, as well as any time you start a sentence with "E. huxleyi" (change to "Emiliania huxleyi"). Also put space between E. and huxleyi.

Alternatively, as motivation you could mention “coccolithophore blooms” as a more generic way – and comment that many of the blooms in the Arctic and Subarctic are indeed formed by one species. Check: Winter et al., publication year is 2014?

Answer:

We thank the reviewer, the correct form is Winter et. al (2013)

p. 5, Line 7: delete “the” between “under” and “study” / and consider replacing as “under investigation”. Add “seas” after list of sea names.

Answer:

We corrected the sentence as follows: “The regions under study are six arctic and subarctic seas: the Barents, Bering, Greenland, Labrador, North and Norwegian Seas. The areas of study were selected for each sea on the basis of the results reported by Kazakov et al. (2018) for the E.huxleyi blooms based on the Ocean Colour Climate Change Initiative dataset version 3.0 (Europea Space Agency) for the period from 1998 to 2016 (Fig. 1).”

Line 18: add: “The” seasonal cycle

Answer:

We corrected it - The seasonal cycle was analyzed using multi-year averaged monthly variables for all months of the year (i.e., a sample size of 12), but the interannual variability of the parameters was analysed based on monthly variables solely for blooming periods (the sample size varied according to subregion and parameter combination, e.g., a sample size for SST in the Barents Sea was 108 – monthly variables from June to September during 1979-2005).

p. 7, Line 32: models (plural)

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We corrected it: “However, some climate models show good results for many cases, e.g., ACCESS1-3; ACCESS1-0; HadGEM2-AO; HadGEM2-CC; HadGEM2-ES; GFDL-CM3; INMCM4; GISS-E2-R; GISS-E2-R-CC.”

p. 8, Line 28: add “the” before proposed methodology

Answer:

We corrected the sentence as follows: “We can conclude that the range of different factors is important for model selection, including the spatial pattern of model biases, and that the proposed methodology is a way of enhancing the model selection procedures sophistication that promises a better chance to identify more skilful models for the features we are interested in. Thus, the proposed method can be used for analyses regarding other regions with the purpose to evaluate climate model performance with respect to various atmospheric and oceanic parameters at different scales.”

References (AR2)

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Winter, A., Henderiks, J., Beaufort, L., Rickaby, R. E. M. and Brown, C. W.: Poleward expansion of the coccolithophore Emiliania huxleyi, J. Plankton Res., 36(2), 316–325, doi:10.1093/plankt/fbt110, 2013.

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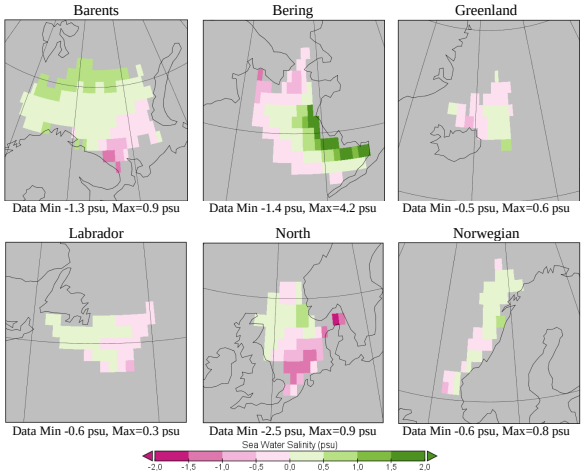

Figure 7. Spatial distribution of biases in five parameters between models and reanalysis in six studied seas averaged over the vegetation season and the time period 1979/1993-2005.

Fig. 1.

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