Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-305-RC3, 2021

Geoscientific
Model Development
Discussions
Open Access

GMDD

Interactive
comment

Interactive comment on "Ice Algae Model Intercomparison Project phase 2 (IAMIP2)" *by* Hakase Hayashida et al.

Anonymous Referee #3

Received and published: 19 April 2021

This paper describes a model intercomparison project with the main goal of evaluating the influence of ice algae at regional and global scales. As the authors note, usually, ice algae are not included in Earth System Models (ESMs). In fact, none of the ESMs that are part of CMIP6 includes sea ice biogeochemistry. Therefore, it is necessary to know how important their inclusion in future ESMs might be. Whilst I fully agree about the importance of this experiment, there are several issues that I think authors should consider in revising this paper. This paper describes an experiment without presenting its results. The only "results" presented are about decisions regarding the atmospheric forcing used across all models included in the experiment and some other technicalities. Therefore, I wonder how this paper fits into the scope of a scientific journal. I could see it as a short comment but not so much as a full scientific paper at its present

Printer-friendly version

Discussion paper

stage. Looking into the Aims & Scope of Geoscientific Model Development (GMD) I can see that one of the possible types of manuscript is about “model experiment descriptions, including experimental details and project protocols”. I am not sure if the idea behind this “model experiment descriptions” includes papers with only such descriptions and without corresponding results. So, I leave this up to the editor to decide. Now focusing on the experiment, itself. I think the idea is fine and extremely useful as I implied above. However, I think that some details should be addressed. For example, authors emphasize the evaluation of ice algae at regional and global scales. Their experiment is forced by atmospheric conditions from either a data assimilation product or ESMs. Their simulations do not include feedback from ice algae towards the atmosphere. Therefore, they cannot evaluate directly the influence of ice algae on atmospheric behavior. However, they can perhaps attempt to do so indirectly, through changes in carbon and DMS fluxes, dependent on including/excluding ice algae in their simulations. They can also evaluate the importance of ice algae on the ocean physical and biogeochemical processes, on the biological carbon pump and on the sink/source role of Arctic and Antarctic seas. I am sure authors know this quite well, but they should share it with the readers – specify the protocols they will use to evaluate the role of ice algae: which variables will be used from each model and what processes will they use to quantify their role at regional and global scales. Some models include dynamic elemental ratios, other do not include them but in the end, they will need to bring results to common currencies under some simplifying assumptions. In the text they merely write that the historical experiment is “designed to simulate changes in ice algae abundance and distribution”, the “projection experiment is designed to simulate the projected changes in ice algae abundance and distribution”, the “exclusion experiment is designed to simulate ocean biogeochemistry in the absence of ice algae” and the “control experiment is designed to diagnose artificial model drifts and to distinguish anthropogenic effects from natural variability”. None of these sentences clarifies how the abundance and distribution of ice algae will be used to evaluate their regional and global significance. This should be clearly addressed in the text. By the way, I suggest when referring to the various simulations that are part of the experiment to call them “simulations” or “treatments” and not “experiments” for the purposes of clarity. Whilst the overall experimental design seems fine, I see one aspect that I think authors should reconsider. When it comes to the projection simulation authors adopted the worst-case scenario - Shared Socioeconomic Pathway 5-8.5. Why this worst scenario which, probably, is not very likely to occur? The utility of this extreme scenario is a matter of debate (e.g. Hausfather and Peters, 2020). If I planned to use only one scenario, I think I would pick up the most likely one. The authors start the abstract by stating that “Ice algae play a fundamental role in shaping polar marine ecosystems and biogeochemistry”. Moreover, at the beginning of the Introduction they write that “ice algae are the foundation of polar marine food webs”. After these sentences one could ask: why then an experiment that attempts to evaluate the influence of ice algae at regional and global scales? Ice algae are quite likely foundation species for the ice-associated ecosystem, but I am not sure one may say they are foundation species for polar marine food webs “in general” before their quantitative contribution is compared to that of phytoplankton. Estimates of ice algal production are generally much lower than those of phytoplankton (see e.g. Jine at al. (2012) one of the co-authors of this study). These authors estimated an average Arctic ice-algal primary production of 21.7 Tg C year-1 for the period 1992–2007 corresponding to $\sim$5% of Arctic primary production. According to Arrigo et al. (2010) a limited number of studies have indicated that ice algal annual production is similar in the Arctic and Antarctic, ranging from 2 to 15 g C m-2 y-1 and from 0.3 to 38 g C m-2 y-1, and estimated to amount to 2–24% of total production in sea-ice covered marine areas (Arrigo et al., 2017). I am not implying that ice algae are not important, I am merely implying that they may be not so important as to call them the foundation of polar systems. A recent study by Kohlbach et al. (2021) suggest that in summer and winter pelagic calanoid copepods and amphipods rely much more on food from pelagic origin. These conclusions may differ if a similar study is conducted in spring-early summer, but they also emphasize the importance of pelagic food for organisms that often are referred as strongly linked with the sea ice. When it comes to thermodynamics, Kauko et al. (2017) estimated melting rates of ∼1 cm per week for a refrozen lead in the drift ice of the Arctic Ocean, as a result of shortwave radiation absorbed by ice algae. Are these values relevant? Arctic-wide these correspond to a lot of ice of course but these values seem rather low when compared with melting rates resulting merely from physical processes. I guess that these arguments are by themselves a good reason for experiments like the one proposed in this paper, but we need their result to properly judge about the quantitative role of ice algae. The term “skeletal layer” is used in the text when referring to the bottom layer where most of ice algae are found in land-fast ice and that is considered in the models used in this experiment. I suggest using the term “bottom ice” or “bottom layer” and not skeletal layer which is present only during periods of ice growth (e.g. Hunke et al., 2015). I see a major limitation in this intercomparison project: it relies on models assuming ice algae only at bottom ice. Whereas this may be the dominant "picture" in land-fast ice, it does not seem to be the case in the much larger fraction of drift ice in the deep ocean, where often there is no bottom maximum of ice algal biomass. I quote here a sentence by Gradinger (1999) based on a study conducted in the Central Arctic and the Greenland Sea: "The lowermost 20 to 40 cm contained between 4 and 62% of the entire algal biomass. Consequently, ice biological studies, which deal only with the bottom few centimeters of the ice floes, will underestimate algal biomass and production by factors of up to 25”. Studies by Melhikov based on the Sheba experiment and more recent studies based on the N-ICE2015 expedition confirm such findings. This issue is only slightly addressed in this paper, in the first paragraph of the Discussion where authors anticipate that “this limitation has a negligible effect on the estimation of depth-integrated biomass and primary production as demonstrated by field observations”, quoting a study based on land-fast ice, when most of the ice in the Arctic and Antarctic is not land-fast. I finish this comment by merely emphasizing that the Los Alamos Sea Ice Model (used by some of the models included in this experiment) includes vertically resolved sea ice biogeochemistry besides the bottom layer approach. I am aware of the cpu costs associated with its usage in comparison with a simpler approach, but this is no scientific reason to choose an approach which is, at least, “highly debatable”. The authors justify the selection of ESMs atmospheric output based in part on whether they have or not the temporal resolutions needed for simulating high-frequency variability (e.g. line 209). However, I found no info about what is understood here as “high-frequency variability”. I suggest specifying such details. I am not a native English speaker so I will not comment much on the language, which I think is clear. However, I have the impression that the text may be slightly improved, and I would recommend a revision by a native English speaker. I also find some repetitive sentences in various parts of the text about the rationale behind this experimental approach and its protocol that may be removed to avoid redundancies. In my view, if it is acceptable for GMD to publish a paper that merely describes an experiment, the acceptance of this paper should depend on the authors addressing the points above providing an in-depth description of the missing details and justifying convincingly the choice of the projection scenario. References Arrigo K.R., Mock T., Lizotte M.P., 2010. Primary producers and sea ice. In: David N. Thomas Gerhard S. Dieckmann (eds.). Wiley. https://doi.org/10.1002/9781444317145.ch8. Arrigo, K.R., 2017. Sea ice as a habitat for primary producers., In: Thomas, DN (ed.), Sea Ice, 3rd Edition, 52–369. Oxford, UK: Wiley-Blackwell. DOI: https://doi.org/10.1002/9781118778371.ch14. Gradinger, R., 1999. Vertical fine structure of the biomass and composition of algal communities in Arctic pack ice. Marine Biology 133: 745-754. Hausfather, Z., Peters, G.P., 2020. Emissions – the ‘business as usual’ story is misleading. Nature 577, 618-620. doi:https://doi.org/10.1038/d41586-020-00177-3. Hunke, E.C., Lipscomb, W.H., Turner, A.K., Jeffery, N., Elliot, S., 2015. CICE: the Los Alamos Sea Ice Model. Documentation and User’s Manual Version 5. Jin, M., Deal, C., Lee, S.H., Elliot, S., Hunke, E., Maltrud, M., Jeffery, N., 2012. Investigation of Arctic sea ice and oceanic primary production for the period 1992–2007 using a 3-D global ice-ocean ecosystem model. Deep-Sea Res. II Top. Stud. Oceanogr. 81–84. Kauko, H. M., Taskjelle, T., Assmy, P., Pavlov, A. K., Mundy, C. J., Duarte, P.,

. . . Granskog, M. A. (2017). Windows in Arctic sea ice: Light transmission and ice algae in a refrozen lead. Journal of Geophysical Research-Biogeosciences, 122(6), 1486-1505. doi: 10.1002/2016JG003626. Kohlbach et al., 2021. Winter Carnivory and Diapause Counteract the Reliance on Ice Algae by Barents Sea Zooplankton. Front. Mar. Sci., 8:640050. doi: 10.3389/fmars.2021.640050.

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2020-305/gmd-2020-305-RC3-supplement.pdf

---

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-305, 2020.