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Geoscientific Model Development Discussions

Interactive comment on "CSIB v1: a sea-ice biogeochemical model for the NEMO community ocean modelling framework" *by* Hakase Hayashida et al.

Anonymous Referee #2

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This paper describes one more Pan-Arctic coupled model. I think it is a well-written paper and it seems to fit the scope of the journal. I have a few general comments/questions (below) and several minor comments/corrections made directly on the paper pdf (attached). I think this paper may be accepted after minor to moderate modifications. I suggest that authors address my general comments below to help the reader understanding better some of the modeling options taken here. This can be done with some small addition of text to the original manuscript. I also suggest that authors have a look at my minor comments/questions and choose the best way to address them. In general these should be quite easy to handle. General comments/questions 1) I think that the effort made here to test the model and compare it with observations is quite im-

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portant. This is frequently lacking in modeling studies that emphasize obtained results without a proper assessment of model performance. The modes implemented here is compared with observations temporarily and spatially (both horizontally and vertically). I think this is a good example. I guess authors could improve a bit the comparison by including some statistical measures of model performance such, as for example, the Nash Sutcliffe model efficiency and the Percentage model bias synthesized in Allen et al. (2017). In this case they could perhaps make comparisons across time and space simultaneously and come up with some objective qualitative assessment of model performance. 2) Why a "new" Pan-Arctic model? I think it would help if authors justified the reasons for selecting a specific sea-ice biogeochemistry model, especially considering that the selected model simulates only bottom-ice biogeochemistry while, since the 90s, several authors adopted vertically resolved sea-ice biogeochemistry models, suggesting the importance of the stocks of algae, nutrients, etc., in upper ice layers through their contribution to vertically integrated production (e.g. Arrigo et al., 1993; Vancoppenolle et al., 2010; Pogson et al., 2011; Duarte et al., 2015). I have the impression that the emphasis on bottom sea-ice biogeochemistry comes from the larger availability of studies on land-fast ice, with a typical large accumulation of ice algae at the bottom few centimeters. However, studies in the pack ice over the open ocean show quite a different picture, where maximum may occur at various depths (e.g. Melnikov et al., 2002; Olsen et al., 2017). 3) Why testing the model for a period when available data is much less than in recent years and, therefore, it becomes much more difficult to properly evaluate model performance? In fact and with regard to the biogeochemical data, author's comparisons with other data sources may be biased by the differences in the temporal frames of various studies.

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Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-191/gmd-2018-191-RC2-supplement.pdf

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