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Biogeosciences Discussions

Interactive comment on "Prioritization of the vector factors controlling *Emiliania huxleyi* blooms in subarctic and arctic seas: A multidimensional statistical approach" *by* Dmitry Kondrik et al.

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Associate Editor Decision: Reconsider after major revisions (28 May 2019) by Jean-Pierre Gattuso Comments to the Author: Dear Author,

The referees rated your manuscript below the level of quality expected in Biogeosciences papers. Both reviewers recommend to reconsider the manuscript after major revisions. I urge you to address their comments thoroughly. For example, the issue of the possible contribution of the carbonate chemistry in driving blooms and distributional

changes of Emiliania huxlehi cannot be summarily dismissed as is done in your reply to the comments. There are ways to estimate changes in the carbonate chemistry when observational data sets are poor, for example Bittig et al. (Frontiers in Marine Science) and Denvil-Sommer et al. (Geoscientific Model Development).

The revised manuscript will undergo a second round of review to ascertain that all comments and concerns have been satisfactorily addressed.

Sincerely, Jean-Pierre Gattuso BG editor

Dear Prof. Gattuso, First of all, thank you for your close consideration of our manuscript applied to the Biogeosciences journal (Kondrik et al., Prioritization of the vector factors controlling Emiliania huxleyi blooms in subarctic and arctic seas: A multidimensional statistical approach, https://doi.org/10.5194/bg-2019-104). In this letter we will try to clarify the question emerged from the public discussion of our manuscript related to the use of data on carbonate chemistry. This question was initially raised by Dr. Neukermans (referee #1). Indeed, our studied variables did not include the parameters, directly connected to the carbonate chemistry system, as this work was done with the use of spaceborne data. Of course, addition of such parameters, as background pCO2, would be preferable. Datasets/approaches containing this parameter were proposed by Dr. Neukermans (Takahashi pCO2 climatology) and you (Bittig et al., 2018; Denvil-Sommer et al., 2018). The first dataset was discussed by us in the reply to the Dr. Neukermans, so here we will focus on the latter two approaches. The proposed approaches are indeed very interesting in terms of both methodology and results, but meet some difficulties inherent in our specific study. In particular, work performed by Bittig et al. (2018) has some limitations described by the authors themselves in (Bittig et al., 2018), which appear to be crucial when implementing its results to our RFC algorithm: 1) The results have an increased bias at the surface layer (p. 15). 2) The decoupling is noticeable following intense blooms or long bloom periods (p. 15). It is

also worthwhile to mention that this approach has not been specifically tested in the coccolithophore bloom regions, which are known to alter the carbonate chemistry very significantly (this issue will be discussed below). 3) It is clearly stated that CANYON-B and CONTENT algorithms have a clear focus on the water column and ocean interior variable estimation (p. 15), which, in turn, can lead to the uncertainties in some results (they can be seen in, e.g. Fig. 8 of (Bittig et al., 2018), constituting up to 50 μatm in comparison to the Polarstern vessel data). 4) In caption to Figure 9 of (Bittig et al., 2018) it is also stated that CONTENT pCO2 estimations can be lower than actual SOCAT data (and even climatology) in the high latitude North Pacific during Summer, which supports our statement that in our study regions (polar and subpolar latitudes of Atlantic, Pacific and Arctic ocean) practically all climatologies/algorithms can give much higher errors due to low amount of testing in situ data. Other work proposed by you is also very interesting, but also has its limitations specifically for our study. This approach is very similar to reanalysis, as it employs the climatological data as the first step and then implements the neural networks to assimilate the SOCAT pCO2 data. It leads us to the same conclusion: whereas this approach has, no doubt, a lot of applications on global scale, in subpolar and polar regions it practically shifts to the same climatological dataset, with only slight local changes. Thus, the discussed datasets/approaches have their limitations/uncertainties, which due to their nature of data availability tend to increase poleward, which, in turn, lead us to question their adequacy for our study. But, again, these limitations do not lower the significance of discussed works, but only underpin the difficulties of studying the polar and subpolar regions in terms of available and reliable data. But even more important obstacle for application of discussed approaches aimed at estimation of changes in the carbonate system lies in the fact that in our case E. huxleyi, as calcifying alga can, drastically change the pCO2 in water - up to hundreds of μatm, which relates to 60% of background pCO2, according to our estimates in (Kondrik et al., 2018). In addition, Shutler et al. (2013) report on an average reduction in the monthly air-sea CO2 flux by about 55% across the marine tracts encompassing extensive E. huxleyi blooms in the North Atlantic, whereas the

maximum reduction over the time period 1998–2007 was registered at 155%. Due to the fact that RFC is developed for estimation of weights/importances of forcing factors without the determination of (in terms of, e.g. pCO2) sources of changes in these parameters (background or caused by the calcification process), we can't interpret the resulting importance of this parameter in terms of carbonate system's status quo during the bloom period. In simple words, the RFC just can't tell us whether it is pCO2 changes are influencing on the E. huxleyi blooms or vice versa. Moreover, the "artificially" increased importance of pCO2 (we are confident that it will be high taking into account above stated facts) can significantly decrease the importance of all other parameters, as the resultant importances are relative and always have 100% in total, which, in turn, will lead us to much worse modelling results. This means that pCO2 (as well as the other variables related to the carbonate chemistry described in (Bittig et al., 2018)) can't be employed for the RFC training because the E. huxleyi blooms phenomena itself has a very strong influence (if not prevailing) on the carbonate system state inside the E. huxleyi bloom areas. At the same time, being initially consistent (as we showed through a 2-decadal time series) our spaceborne variables can arguably give access to the carbonate system characterization through the water chemistry theories, as it equally refers to the four variables discussed by Bittig et al. (2018). We thank you again for all your efforts made up to this point for improving our manuscript (as well as the two referee's) and hope that all above stated arguments can help to solve this whole situation.

On behalf of the co-authors, Prof. Dmitry Pozdnyakov

Dear Colleague, I quickly reply to your message. The decision to request a major revision is based on the reviewers' evaluation and my own and remains unchanged. I have forwarded to you the scores, which are below the level of quality expected in Biogeosciences. As my decision letter mentions, the issue of the carbonate system is not the only argument. Other criticisms and suggestions from the reviewers must be satisfac-

torily addressed, that is in a better way than in your replies to the comments. The style also needs a lot of attention. As you acknowledge, considering pCO2 (I would say the status of the carbonate system) is highly relevant. It is known to be a driver for coccolithophores, even though species and strains do seem to exhibit different sensitivities. The data coverage may be inadequate in space in time to be used in your statistical and modelling approaches but that is not a good reason to dismiss these variables. It makes your conclusion that "the adequacy of the developed models for FFs prioritization with regard to E. huxleyi blooms" very weak and questionable. Your study aims at identifying the variables which control the extent and magnitude of E. huxleyi blooms. The carbonate chemistry may play a big role in the onset of the bloom. Your argument (II) that Emiliania raises pCO2 does not apply there. I hope this helps. Kind regards, Jean-Pierre Gattuso

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Dear Colleague,

We send our manuscript with the additions and changers required by reviewers. As far as in your letter of June 1 you practically ignored/waved away our reasonings without any real consideration, we rely on it that the revised text will be given full consideration by the two reviewers who have already commented on our study and were given our responses/clarifications. We earnestly hope that their criticism, if there is any, will be seriously argued but not reduced to a mere dismissal as it was in your letter. Together with the revised manuscript we also submit our previous letter to you using the option “Authors’ comments” just to let the two reviewers get aware of our argumentation given to you regarding the inappropriateness of using in our analyses the datasets by Bittig et al, 2018) and Denvil-Sommer et al. 2018). Kind regards, Co-authors

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