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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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We appreciate the reviewer's interest in our research, and below there are our reciprocate comments.

1. The first two bullets of the review regard the use of PAR vertical profiles within MLD in light of 8-day MLD data availability on a global scale as it was done by e.g. Lacour et al., 2015. However, Lacour et al. employed to this end monthly climatological data interpolated to an 8 day time period (incidentally, previously we published a descrip-

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tion of such databases with a specification of their merits and shortcomings: Kondrik et al., 2017). Nevertheless, the use of the aforementioned data is at odds with the very essence of the methodology of bloom causal factors identification: climatological parameters do not vary from year to year, and thus do not reflect their status on concrete years. Moreover, as the intraannual timing of E. huxleyi blooming varies within 1-2 weeks falling on one and the same month(s), the application of climatological data on PAR vertical distributions would mean that ones and same MLD profiles would be used (e.g. August-September in the case of the Barents Sea) with the only varying parameter -PAR at the surface, but this is the parameter that we actually exploited. This parameter veritably serves the major purpose of our research, namely, the establishment of interannual variations in E. huxleyi blooms for each target sea. A climatological approach would not be appropriate.

It should be also noted that even in a hypothetical case of actual MLD data availability, the utilization of such data for restoration of PAR vertical profiles would be highly problematic if not impossible. Indeed, to restore the vertical profile of PAR within MLD requires in turn data on all optically active components co-occurring with E. huxleyi cells and scales/coccoliths. Understandably, such a requirement is impractical.

2. Regarding the issue of E. huxleyi morphotypes. We agree with the reviewer that such data on morphotypes would be useful for interpretations. However, such information is unavailable for all seas targeted in our study, and we abstained from discussing this option, confining our discussion exclusively to those factors the data on which was confidently established.

3. The issue of using the Takashashi data on pCO2. We are certainly aware of these data as well as those in LDEO and SOCAT data (Kondrik et al., 2018). However, again the Takashashi data are climatological, and hence no interannual variations in pCO2 could be retrieved from them. As to the original/ in situ data on pCO2, on which the SOCAT database is developed, their size reduces drastically upon extraction of the information on our concrete seas and bloom periods. The resultant amount of data

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becomes insufficient for use in machine learning technology applied in this study.

4. The reviewer is absolutely right that typically E. huxleyi blooms are preceded by some intense growth of native non-calcifying alga like diatoms (most often cases). The moments between the outbursts (i.e. maximum growth) of such cold-water photosynthesising alga and E. huxleyi vary but usually are about three to four weeks (sometimes a bit longer). In some cases the onset of E. huxleyi blooms occur while the "tail" of preceding bloom (for simplicity reasons, let's call it "diatomic bloom") overlaps the beginning of the E. huxleyi bloom period (e.g. Pozdnyakov, et al., 2017); in such circumstances the concentration of diatoms might constitute about 10% of the concentration of E. huxleyi) [e.g. Lavender et al., 2008]. Our observations reported in the submitted paper have actually revealed (through the concentration of chlorophyll as a proxy) the presence of "diatomic" alga within a two or four week period prior to the maxim of E. huxleyi growth. However, no indications were revealed that the preceding "diatomic" bloom somehow affected the extent/intensity of the following bloom of E. huxleyi. So that, let us repeat for clarity: it is not the question that our observations failed to detect the presence of "diatomic" phytoplankton, but that there was no evidence that the chemical "preparation" of surface water (in terms of N:P ratio) by the preceding bloom was appreciably consequential for the development of E. huxleyi bloom.

All other critical remarks contained in Section "Specific comments" will be accepted in the final revised version of our paper.

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