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Interactive comment on "A synthetic satellite dataset of *E. huxleyi* spatio-temporal distributions and their impacts on Arctic and Subarctic marine environments (1998–2016)" *by* Dmitry Kondrik et al.

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Kondrik and collaborators present a 19-year satellite time series of Emiliania huxleyi bloom area, calcite content, and associated increase in in-water pCO2 in four selected areas of the high-latitude northern hemisphere. The dataset is only partly unique, in the sense that a 19-year global remote sensing dataset of E. huxleyi bloom extent, coccolith concentration, and PIC content can also be easily obtained elsewhere. Therefore uniqueness only applies to pCO2. This dataset could be useful, but I request a few substantial modifications that I believe are necessary to improve understanding and

quality of the dataset: (1) some flaws in the dataset (pointed out below, 1a and 1b) will need to be fixed, (2) error estimates for remotely sensed quantities must be provided, and (3) in its present form, the study/data is not correctly positioned within the state-of-the-art literature and other available datasets.

(1a) It appears from Fig. 4 that the E. huxleyi bloom dataset includes false positives, a problem that is particularly evident in the Bering Sea (1998-2001) where the authors have detected blooms initiating in winter and lasting about 10 months as previously reported from ocean colour remote sensing data (Iida et al., 2002). However, ship-borne measurements have identified resuspended diatom frustules as the cause of these bright waters in winter-spring instead of E. huxleyi blooms (Broerse et al., 2003). This invalidates the authorial E. huxleyi bloom detection algorithm and all derived products in the Bering Sea from late fall to spring. I further fail to see how the algorithms used by the authors (Kondrik et al. 2017; Kondrik et al. 2018) to detect E. huxleyi blooms present an advance to NASA's standard method of E. huxleyi bloom classification (Brown and Yoder, 1994), and many other subsequent bloom detection methods (Iglesias-Rodriguez et al., 2002; Iida et al., 2002; Iida et al., 2012; Moore et al., 2012). (1b) The remote sensing algorithm for pCO2 estimation is a simple linear regression between observations of Delta_pCO2 and remote sensing reflectance Rrs in a blue waveband. This relationship is strictly empirical and does not appear to have theoretical grounds; I believe the user should be aware of this. Not surprisingly, there is an enormous spread along this regression line such that for a given reflectance value the estimated Delta_pCO2 has a confidence interval with a width of 50 ppm and even wider for denser blooms. Furthermore, the residuals of the regression are clearly unevenly distributed, with a strong tendency to underestimate Delta_pCO2 at higher reflectances. This relationship should be explicitly stated, which is not presently the case, including all relevant regression statistics, and especially a figure showing the observations and the fitted line so that the user can better grasp the errors of the algorithm. (2) Whereas the statistics of the validation of the retrieved coccolith concentration are given in section 2.2, the accompanying figure is missing.

No uncertainty assessment is given for pCO2 (see previous comment). (3) A 19-year global remote sensing dataset of PIC concentration merging all ocean colour satellite missions can be obtained here: http://www.globcolour.info/ in temporal resolutions ranging from daily to monthly, spatial resolution ranging from 4km to 100km, and various geographical projections. From PIC concentration, coccolith concentration can be derived using a fixed mass per coccolith (as you do too), and PIC content can also be easily derived by combining with a climatology for Mixed layer depth available here http://www.ifremer.fr/cerweb/deboyer/mld/Surface_Mixed_Layer_Depth.php. I therefore suggest you remove all statements of uniqueness of your PIC dataset (e.g., page 2, lines 24-26). The statements on page 2 lines 11-16, "Prior to the publication of Kondrik et al. (2018), no attempts have been undertaken to retrieve from space… No concatenated time series data are available to date on the associated bloom intensity…" are thus simply incorrect. I also suggest you appropriately reference the work of (Shutler et al., 2013) entitled "Coccolithophore surface distributions in the North Atlantic and their modulation of the air-sea flux of CO2 from 10 years of satellite Earth observation data Âż, which is very similar to your work on remote sensing of pCO2 in Ehux blooms, but is mentioned nowhere. Page2 Line 8-10: "Until recently, only few satellite studies were published on the typical locations of E. huxleyi blooms and associated concentrations of PIC in surface waters within the bloom area". It appears to me you missed a vast body of literature: (Balch et al., 1991; Balch et al., 1996; Gordon et al., 2001; Smyth et al., 2004; Signorini and McClain, 2009; Moore et al., 2012; Hopkins et al., 2015; Balch et al., 2016; Neukermans et al., 2018) etc.

Further comments : Title : add "blooms" after "E. huxleyi" Abstract : delete "detailed information on E. huxleyi impacts within the bloom area on marine environments", as this suggests that you are detailing ecological impacts

P1, L16 : "Ongoing climate change is a background of numerous emerging hot topics." is a rather meaningless opening sentence. P1 L25 : Rivero-Calle is not the right reference for poleward expansion of coccolithophores, instead use (Winter et al., 2014;

Neukermans et al., 2018). "gradually propagating in the poleward direction" ; the poleward expansion is not gradual, as expansion rates exhibit stark jumps as demonstrated in (Neukermans et al., 2018). P2, L1-4 : a lot of statements for only one reference. P2, L23 : replace 1918-2016 by 1998-2016 P2, L20 : remove "original" P3 L1 : spell out OC CCI P6 L1 : "in the cause of satellite processing" ?, rephrase P7 L10-15 and L24-28 : same paragraph appears twice. P7 L31 :"1,105,6800 km2" commas are in the wrong place

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