

Review: „Simulation of factors affecting *E. huxleyi* blooms in arctic and subarctic seas by CMIP5 climate models: model validation and selection“ (Gnatiuk et al., 2019)

5 Summary

In this study, Gnatiuk et al. evaluate the simulated fields of environmental drivers of *Emiliana huxleyi* blooms (sea surface temperature and salinity, ocean current speeds, wind speed at 10m, and shortwave downwelling radiation) in arctic and subarctic regions in historical CMIP5 model simulations. The authors assess the agreement of the simulated fields with available reanalysis products in terms of e.g. their correlation coefficient or the bias in several subregions, e.g. the Barents Sea. Gnatiuk et al. rank the models according to a percentile score-based model ranking, with the ultimate goal to identify the best subset of models for each of the considered drivers and subregions. Future projections of the respective models are then planned to be used in a follow-up study to feed a statistical model identified elsewhere (Kondrik et al., 2019, Biogeosciences Discussions). The study at hand is certainly a valuable starting point for the assessment of potential future changes in coccolithophore dynamics and in principle suitable for publication in Biogeosciences. However, I don't think neither the analysis nor the presentation thereof is currently thorough enough for the manuscript to be accepted for publication. I suggest major revisions as outlined below.

General comments

Below I list several general comments that should be addressed before this manuscript can be considered for publication:

- In my opinion, the choice of the variables to be validated in the models is incomplete and the chosen subset is not obvious when thinking about the factors controlling phytoplankton blooms. First, all possible drivers should be carefully introduced in the introduction (it is nowhere clearly stated in the manuscript what factors impact phytoplankton dynamics in general). Important variables such as (macro/micro)nutrients and carbonate chemistry are not considered at all and it is not even thoroughly discussed why. Motivating the choice of drivers to be analyzed in this paper simply by referring to an individual study (Kondrik et al., 2019) which is currently in review for publication in *Biogeosciences* is not sufficient in my opinion. Additionally, reviewers of the manuscript by Kondrik et al. (2019) have raised similar concerns with regard to the chosen drivers considered in the analysis. In my view, the choice made for the manuscript at hand is a missed opportunity as the evaluated models can provide more comprehensive information on factors impacting phytoplankton/coccolithophore growth than the variables the authors chose here.
- In the current version of the manuscript, a discussion of the results is completely missing. While section 3 is called “Results & Discussion”, it currently only represents a description of the Figures, without putting the results into the context of previous literature or how the results impact the overall motivation for the study (assessing the potential future development of *E. huxleyi* blooms). It is not clear to me e.g. what modelers should take away from their analysis. In a revised version of the

manuscript, I suggest to include a thorough discussion on e.g. the sensitivity of the resulting model combinations on chosen thresholds in the ranking, the impact of the identified model biases on coccolithophore blooms, the impact of neglecting important forcing factors (nutrients, carbonate chemistry) or biotic interactions (which cannot be assessed with this approach as opposed to when coccolithophores are included as an explicit functional type to the model).

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- Regarding choices of the presentation of the results, I would personally like to see more than just temperature in the Barents Sea to be included in the main text (the choice of the figures could be reconsidered, especially given the title of the manuscript). The current choice makes it very hard for the reader to assess how the representation of present-day coccolithophore blooms in these models is potentially affected by biases of all variables impacting phytoplankton dynamics (and not just temperature). Including more detail in the study at hand will also make the assessment in a follow-up paper on future changes easier.

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- Overall, I think that the literature review in the introduction on factors controlling coccolithophore blooms in the arctic/subarctic (or North Atlantic) and possible drivers for observed changes in their distributions is not comprehensive enough in its current form. In my detailed comments below, I suggested a few papers that could be considered in my view – a result of a very brief literature search I have done (as I am not 100% familiar with the literature of the arctic/subarctic), but this list is by no means exhaustive. The authors should revise the manuscript accordingly, as this might also help to motivate why certain variables are (or are not) considered in their study.

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- Throughout the manuscript, the writing needs to be more concise and to the point. Often, it is not clear to the reader why a certain information is given, i.e. what the relevance is for the study at hand or what the take-away message is (see detailed comments below of e.g. the introduction). The authors should especially revise the result section, which is currently a list of brief descriptions of the Figures without making it clear enough why they were chosen to be included and what the key message for each Figure is, which makes this section quite hard to read in its current form. Ultimately, all figure captions are currently incomplete as they do not describe what is actually shown in the respective figures.

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Detailed comments

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Abstract:

p. 1, L. 9: Please add ocean acidification here. It is not only the effect of global warming that can be expected to impact future coccolithophore blooms.

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p. 1, L. 10: I find this statement on the aim of the paper misleading, as none of these models (or very few) include an explicit parametrization of coccolithophores to the best of my knowledge. I think it you need to state more clearly what exactly you do here. You don't actually assess the blooms, but only how well the models reproduce the present-day

95 environmental conditions that favor coccolithophore blooms. Also, please be precise here what you mean by “optimum combination”.

100 p. 1, L. 11: This last statement of the paragraph is misleading because you don’t actually address this in the study. Please point out that this is future work that can/will be done following this study. Additionally, please add a “potential” in the last part of the sentence “[...] *potential* future changes [...] can be assessed.”

105 p. 1, L. 14: Please delete the “complex” or describe what methodology you’re using. I suggest to rephrase to something along these lines “Here, we present the validation of 34 CMIP models over the historical period. Furthermore, we present the procedure for model selection, which is based on their skill to represent important forcing factors for coccolithophore blooms.”

110 p. 1, L. 15: Are these five factors really known to be the dominant factors impacting coccolithophore dynamics in the arctic and subarctic?

115 p. 1, L. 16: The chosen set of environmental factors to be validated in the models is not obvious to me. There is no rationale from my point of view as to why one would completely neglect nutrient fields and carbonate chemistry in the validation of the models (see general comments and detailed comments further down). Furthermore, I suggest to include a brief description what the environmental conditions wind speed, current speed, and salinity are proxies for as phytoplankton growth in these models is not a direct function of these variables.

120 p. 1, L. 16: Please check throughout the text: are you using sea surface salinity (as stated e.g. in the abstract and introduction) or the salinity averaged over the top 30m (as e.g. stated in section 2.1 or the caption of Figure 9)?

p. 1, L. 20: “best models” in what respect? Please be precise here.

125 p. 1, L. 22: I don’t understand this the statement about “30 combinations of most-skillful models were selected”. Selected for what? Additionally, I suggest to state how many models are considered within each combination.

130 p. 1, L. 23: “common” is used in what sense here? I don’t understand this. How do you define “high skill”? This is rather subjective. I suggest to rephrase.

135 p. 1, L. 25: What should e.g. modelers conclude from your analysis? I miss a statement on the broader implications of your study in the abstract.

Introduction:

140 p. 2, L. 2-5: Please include a brief description on how exactly coccolithophores impact the carbon cycle (as done for the sulphur cycle). Additionally, I don’t think Rivero-Calle et al. (2015) and Winter et al. (2013) are appropriate references for the biogeochemical impact of coccolithophores here, as these only describe changes in the biogeography and occurrence

over time. Check e.g. Iglesias-Rodriguez et al. (2002) or Balch (2018) (and references therein) for the biogeochemical imprint of this phytoplankton group.

145 p. 2, L. 3: Please delete the “additionally”. You describe the impact on the sulphur cycle here.

p. 2, L. 6: It is not only essential to study *E. hux.* blooms, but coccolithophore blooms in general. *E. huxleyi* has not yet been introduced in this line of the text. Please change to
150 “coccolithophores” instead of “*E. huxleyi*”

p. 2, L. 7: Please introduce the abbreviation “*E. huxleyi*” here.

p. 2, L. 9: Please add a reference to the temperature and salinity tolerance.
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p. 2, L. 10: I suggest to add the more recent reference “Krumhardt et al. (2017)” here, as they provide the most recent compilation of the global present-day distribution of coccolithophores (to my knowledge).

160 p. 2, L. 11: Have coccolithophores really expanded because of ecosystem changes in the Arctic? Don’t you mean “as a result of recent changes in environmental conditions, coccolithophores have expanded poleward”? Please revise the logic in this sentence.

p. 2, L. 12: Henson et al. (2018) is not an appropriate reference here (they don’t talk about the changes in *E. huxleyi* blooms in the cited paper). Please consider adding e.g. Rivero-Calle et al. (2015) here.
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p. 2, L. 12: Winter et al. (2013) suggest that that the poleward expansion is mainly driven by temperature, salinity, or nutrients, but Rivero-Calle et al. (2015) and Krumhardt et al. (2016)
170 suggest that carbonate chemistry matters as well. Please be more comprehensive in the discussion of possible drivers for the expansion.

p. 2, L. 14: Please be more precise here: When you say “*E. huxleyi* blooms have a high positive correlation with [...]”, do you mean the occurrence, their size, their duration...?
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p. 2, L. 14: The description of controls on *E. huxleyi* blooms (and causes for its changes) is not comprehensive enough. I only did a very brief 10-minute search in the literature and found a number of papers that could be relevant for the introduction of this paper (only focusing on those of the northern hemisphere, i.e. disregarding the wealth of recent literature on
180 Southern Ocean coccolithophore dynamics, see e.g. Balch et al., 2016, Nissen et al., 2018 and references therein): please have a look at e.g. Daniels et al. (2015), Harada et al. (2012), Oziel et al. (2017), and Smyth et al. (2004) (and references therein). I suggest to first describe the factors that contribute to phytoplankton/coccolithophore blooms in general (these are currently not introduced) and to then discuss what has been suggested for coccolithophores
185 in general and in the (sub)arctic in particular. Please motivate why you think nutrients and carbonate chemistry are not important as this is not at all obvious.

190 p. 2, L. 12-20: Please clearly differentiate between discussing drivers of present-day coccolithophore blooms as opposed to possible drivers of observed/future changes in coccolithophore distributions and bloom dynamics.

195 p. 2, L. 20-32: I find it problematic to focus so much on a single paper here, especially as the discussed paper by Kondrik et al. (2019) has not yet been accepted. One of the main criticisms by the reviewers of that paper was the neglect of important variables as potential drivers of coccolithophore blooms (such as e.g. carbonate chemistry). I think the study at hand can be much more generally motivated, without going into the details of this specific one. To that aim, and similarly to the points raised in the review of Kondrik et al. (2019), the analysis in the manuscript by Gnatiuk et al. should be more comprehensive in the assessment of potential drivers of coccolithophore blooms, especially because the output from models is assessed here, which can provide information on all environmental variables impacting phytoplankton growth. There should not be a a-priori-restriction to the drivers assessed here without giving a good reason to do so. Please revise the introduction and the analysis in that respect.

205 p. 2, L. 2-32: The whole first part of the introduction does not provide a comprehensive summary of what is known about drivers of coccolithophore bloom dynamics and does not naturally result in the knowledge gap that will be assessed in this study. From my point of view, it should be substantially revised following the comments made above. Additionally, the models are not properly introduced. E.g. no reference to the CMIP is given. 210 Furthermore, it should be clearly stated that none (or maximum a few, to be double-checked) of the CMIP5 models includes an explicit parametrization of coccolithophores, which is why it is currently only possible to project potential changes of their blooms based on changes in environmental conditions (but note the recent paper by Krumhardt et al., 2019). This comes with the limitation that biotic interactions cannot be assessed, which should be clearly stated in the discussion section (see also Krumhardt et al., 2017). 215

220 p. 3: The first paragraph does not link well with the above. Please work on your flow in the introduction. Additionally, this whole page reads like it should be in the method section. Please revise and consider moving at least parts of it to the method section.

p. 3, L. 7: How are the “best models” defined here? Please be precise what you mean.

225 p. 3, L. 8: Please revise the sentence “These two approaches usually give a good result”. Good in what respect? Please add references.

p. 3, L.11: Choosing this method implies the assumption that whatever model is representing present-day conditions best will also do the best job in projecting these into the future, doesn't it? I think this is important to state here.

230 p. 3, L. 16-21: It is not clear to me what the take-away message of this paragraph is. How does the first approach, assessing how well models do in representing air temperature, sea level pressure, and precipitation help in the assessment of environmental factors impacting phytoplankton/coccolithophore growth? Please work on this paragraph and make it more specific to the goal of your work. Consider combining it with the next to avoid having a 2-sentence paragraph with no clear take-away message. 235

p. 3, L. 21 – p. 4, L.2: Again, the take-away message in context of your specific goals for the paper are not clear. Please re-write.

240 p. 4, L. 3: Why do you conclude that? This is not clear to me from what you have presented so far in the introduction.

p. 4, L. 3-5: You have not presented the differences in environmental conditions of the different focus areas. Please revise the introduction accordingly. I don't understand what the
245 second half of the sentence means: How can areas have a wide range of parameters?

Methods

250 p. 4, L. 17-19: As mentioned before, I don't understand based on what grounds you neglect the assessment of nutrients and the carbonate chemistry in the models.

p. 4, L. 23-25: Did you include regional models, e.g. CORDEX? I can't find it in Table 1. If you
255 didn't include those models, don't make that statement here. I am bit confused. Please distinguish between regional and global models and state which kind you considered.

p. 4, L. 25: Did you only consider global models in the end? This is not clear from your description in this section. Please clarify.

260 p. 4, L. 25-26: I suggest to give the range of models available: number available for FFs ranged from X1 for variable Y1 to X2 for variable Y2 (see Table 1). What do you mean by "main characteristics"? Please rephrase.

265 p. 5, L. 4: replace "has been shown" by "have been shown".

p. 5, L. 6: Please choose a better description in the title than simply "methods", maybe something like "model evaluation metrics"?

270 p. 5, L. 7: Please rewrite "regions under the study". Add "Sea" behind "Norwegian".

p. 5, L. 7-8: How do you define a bloom here? Please state this and add references. Additionally, you don't state what data you base Fig. 1 on to define the blooms. Please clarify in the text and the figure caption. I suggest to draw the study regions into Fig. 1. to help the reader localize the different subregions.

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p. 5, L. 11: Do you mean model output here when you say "data? Please clarify.

p. 5, L. 12: I have a hard time believing that the blooming period lasts from January-December in the Bering Sea. What bloom definition is used for this?

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p. 5, L. 10-14: I don't fully understand why you're restricting the analysis to the times and locations of identified *E. huxleyi* blooms under present-day environmental conditions for each sea (if the models do not necessarily reproduce the environmental conditions at these

285 exact locations and times). Don't you want to restrict the analysis to the observed
environmental conditions at the times/locations of the blooms (i.e. the observed
environmental niche)? As a consequence, I am wondering why don't you define each
subregion as a slightly larger area than currently done.

290 p. 5, L. 17: The interannual variability of what exactly? The seasonal cycle/amplitude,
summer average, average over blooming period, ...? Please be precise here.

p. 5, L. 18: *The* seasonal cycles [...]

295 p. 5, L. 19: "but *the* interannual variability " of what?

p. 5, L. 19: Replace "sea" by "subregion"

300 p. 5, L. 23: Can you rephrase "RMSD-observations standard deviation ratio"? I have a hard
time understanding what you mean here. Please consider to add the formulas to make it
very clear.

p. 5, L. 25-26: Rephrase to something like "For the assessment/evaluation of the interannual
variability [...]"

305 p. 5, L. 26: Do you mean the difference in the spatial distribution of temporal trends
between the model output and the reanalysis data? This sentence is not clear to me. Please
rephrase to clarify.

310 p. 5, L. 26-27: What exactly is "your percentile score-based model ranking method"? This
method is defined nowhere in the method section up to this point. In particular, the
description of this ranking method should be very clear (e.g. by including an overview listing
the metrics are included in the ranking), as the main result of your study is based on this
ranking.

315 p. 5, L. 31: Less than 25% of what? Please be precise. What do you base these thresholds on?
It seems rather subjective to me. What is the effect of the choice on the outcome? This
needs to be discussed somewhere in the text.

320 p. 6, L. 4: Again, choosing 25% seems random to me (see previous comment).

Results & Discussion

325 p. 6, L. 6: Personally, I find it a bit unfortunate that only results for temperature and the
Barents Sea are presented in the main text. Isn't there a better way to synthesize the results
and present more than just one tiny subarea and one forcing factor?

330 p. 6, L. 9-11: I don't think a Taylor diagram needs to be explained in the result section. I
suggest to rather briefly explain when the agreement between model and reference data set
is good (i.e. how to interpret the plot) instead of simply stating what can be seen (see
comment on L. 15-17).

335 p. 6, L. 11/12: Please add “[...] capture the *climatological* seasonal cycle [...]”. Furthermore, please explain how it can be seen from the plot that the seasonal cycle is represented better than the interannual variability (see previous comment).

340 p. 6, L. 14: Are these numbers really unitless? If so, define somewhere that you plot normalized SD and RSMD (method section, consider adding formula there) and state that here by saying e.g. “the SD and RSMD normalized by XX are between ...”. This will help the reader to follow.

345 p. 6, L. 15-17: This is the information you should start your paragraph with (see previous comments). First explain to the reader how to interpret the plot. However, the statement that “the closer the model data is to the x-axis, the better the correlation coefficient” is not entirely correct, as the correlation coefficient is shown on the radial axis. A point with RMSD/SD/CorrCoeff of 0.1/0.1/0.1 is closer to the x-axis than a point with 1.0/0.8/0.9 (note that this is under the assumption that RMSD is on the x-axis, SD on the y-axis and the correlation coefficient on the radial axis, see comment on the Figure further down)– but the correlation coefficient of the second point is higher. Please be precise in the description.
350 Also, a correlation coefficient is high/low and not good/bad.

p. 6, L. 17: Replace “climate parameter” by “e.g. SST”.

355 p.6, L. 17-18: Please revise the grammar of this sentence.

p. 6, L. 18-20: This statement is redundant with the method section.

360 p. 6, L. 9-20: For the whole description of the Taylor diagram, please add the names of models here that show the highest/lowest correlation coefficients, RMSD etc. to make it easier for the reader to extract the information from the plot.

365 p. 6, L. 21-23: If you say you show the “spatial distribution”, I expect maps. Do you mean the spatial variability of the climatological SST bias across the subregion? Please be more precise throughout the description.

p. 6, L. 22: I see median biases that are >0 (e.g. for the model 2). Please double-check.

370 p. 6, L. 24: Do you mean the maximum bias? I don’t understand “amplitude bias” (throughout paragraph). Similar to above, please add the names of the models showing the numbers you’re stating to make it easier for the reader to find the information you’re stating in the plot.

375 p. 6, L. 24-25: Please revise this statement, e.g. simply stating that the comparison shows large variability across the models.

p. 6, L. 27-29: Where is this seen? This is not included in Fig. 4. If you’re referring to a different plot here, please add the reference.

380 p. 6, L. 29: Similar to above, be more precise in your description. From just the wording “spatial distribution of annual trends”) the reader expects maps here, not box plots.

385 p. 6, L. 31: How are “significant trends” defined here? How can that be seen in the plot? Please be precise. What models show a significant trend? What is an “unrealistic trend” for you here?

390 p. 7, L. 1: How do you know that? As mentioned above, I think it is important to state in the method section that this is the assumption you make (a model that reproduces the observations best over the historical period (however you define “best”), also gives the “best” projections for the future).

395 p. 7, L. 9-12: Is the +/- 1K the average over the domain? Currently, the reader at this point has totally forgotten why you’re doing this exercise. I suggest to always relate your analysis back to your goal of projecting potential future changes in coccolithophore blooms. I understand that this will be a follow-up paper, but this paper would gain a lot if you speculated at least. How can these biases be expected to impact these estimates? You could do some basic calculations using a Q10 function (see e.g. Nissen et al., 2018) or a temperature optimum function (see e.g. Krumhardt et al., 2017) describing the impact of temperature on phytoplankton growth.

400 p. 7, L. 13: What error do you mean here? Please be precise and make sure that all the metrics you present are carefully introduced in the method section.

p. 7, L. 15-16: Please revise the grammar of this sentence.

405 p. 7, L. 13-17: Similar to above, I don’t understand from the current presentation of the results what these mean.

410 p. 7, L. 24-28: This is repetitive with the method section and what should be in the figure caption. There is no need to state it this detailed in the main text.

p. 7, L. 28-30: Does it surprise you that the model combinations vary?

415 p. 8, L. 3-5: How is “better performance” defined here? Is not clear to me how you conclude this.

Conclusions

420 p. 8, L. 13-16: The statement that the Arctic is often considered as a single region in other studies is never made in the introduction, but should be included there as a motivation to look at subregions. Furthermore, you don’t actually assess the whole area, so I suggest to revise this statement, as you don’t actually compare the performance over the whole area to the smaller subareas.

425 p. 8, L. 18: What about the temporal development of the environmental conditions?

p. 8, L. 18-21: Are more important than what? Please be precise. I cannot follow your logic here. Please revise to clarify, taking also into account the comments I made in the result section.

430 p. 8, L. 24: And the time series is even shorter for SSS and ocean currents, isn't it? What is "out-of-sample" testing? Please try to avoid introducing concepts in the conclusion section which were not discussed before. Why did you not test by excluding certain time periods from the analysis?

435 p. 8, L. 27: important for what? Please be precise.

p. 8, L. 31: Why only at regional scales?

440 **Figures/Tables**

Fig. 1: You don't simply show the "locations of the blooming areas" here, but the spatial distribution of the frequency of blooms. Please be more precise. I suggest to show the subregions in the plot directly (put names and add e.g. a black contour to show the extent).
445 Please add to the caption what data this map is based on and how you define a bloom.

Fig. 2: Be more precise in caption, a lot of information on what is seen in the plot is missing. What is the unit of the RMSD?

450 Fig. 3: The way I know it, a Taylor diagram shows the RMSD (normalized by the standard deviation of the reference data set) on the x-axis, the standard deviation (normalized) on the y-axis and the correlation coefficient on the radial axis. It is not clear to me what exactly you're showing. Please add labels to the plot (y-axis, grey circles) and also say what you're showing in the caption (including units or state if you normalize by something). Also, please
455 add panel labels to the plot and the caption.

Fig. 4 & 5: Possibly replace "distribution" by "variability"? Be precise in what you show. What are the orange line and the whiskers? How is the bias defined (Fig. 4)? What trend is shown Fig. 5; trend in average over blooming period averaged over subregion?)?

460 Fig. 6: Restate blooming period in caption, add unit of SST bias.

Fig. 7: What error are you showing here? Please add the unit of the SST trend in the caption. The colorbar currently states that you're showing SST (K) – please double-check. Please
465 restate the blooming period.

Fig. 8: In my view, it is not really common to plot SST in Kelvin, consider changing it to °C. Please add the units of the variables in the figure caption. Explain what the fit is, exchange "x" and "y" by the actual variables you fitted. Please don't use black/dashed for all fits, I
470 suggest to change the color of each fit to the color of the respective full time series.

Fig. 9: Please briefly summarize what the numbers for each model-variable combination represent and refer back to the method section and Fig. 2. Please also explain in the caption

475 what the white areas are and refer back to Table 1. Please add the units to the variables in
the Figure caption.

Table 1: Replace “concrete” by “respective. Please define all abbreviations in the Figure
caption (e.g. SST, WS...) and add units.

480 Table 2: Please add units in the Figure caption. What is SD_{dif} ? This is never explained in the
text (method section). Please be consistent with the use of underscores in caption and Table
(e.g. Tr_m vs Tr_m). What does “modulus of standard deviation difference” mean? I don’t
understand this. Please use the exact same names as introduced in the method section.

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References

Balch, W. M., Bates, N. R., Lam, P. J., Twining, B. S., Rosengard, S. Z., Bowler, B. C., ...
Rauschenberg, S. (2016). Factors regulating the Great Calcite Belt in the Southern Ocean
and its biogeochemical significance. *Global Biogeochemical Cycles*, *30*(8), 1124–1144.
490 <https://doi.org/10.1002/2016GB005414>

Balch, W. M. (2018). The Ecology, Biogeochemistry, and Optical Properties of
Coccolithophores. *Annual Review of Marine Science*, *10*(1), 71–98.
<https://doi.org/10.1146/annurev-marine-121916-063319>

Daniels, C. J., Poulton, A. J., Esposito, M., Paulsen, M. L., Bellerby, R., St John, M., &
495 Martin, A. P. (2015). Phytoplankton dynamics in contrasting early stage North Atlantic
spring blooms: composition, succession, and potential drivers. *Biogeosciences*, *12*(8),
2395–2409. <https://doi.org/10.5194/bg-12-2395-2015>

Harada, N., Sato, M., Oguri, K., Hagino, K., Okazaki, Y., Katsuki, K., ... Grebmeier, J.
(2012). Enhancement of coccolithophorid blooms in the Bering Sea by recent
500 environmental changes. *Global Biogeochemical Cycles*, *26*(2), 1–13.
<https://doi.org/10.1029/2011GB004177>

Iglesias-Rodríguez, M. D., Armstrong, R., Feely, R., Hood, R., Kleypas, J., Milliman, J. D.,
... Sarmiento, J. (2002). Progress made in study of ocean’s calcium carbonate budget.
Eos, Transactions American Geophysical Union, *83*(34), 365–375.
505 <https://doi.org/10.1029/2002EO000267>

Kondrik, D., Kazakov, E., Chepikova, S., and Pozdnyakov, D.: Prioritization of the vector
factors controlling *Emiliana huxleyi* blooms in subarctic and arctic seas: A multidimensional
statistical approach, *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2019-104>, in review,
2019.

510 Krumhardt, K. M., Lovenduski, N. S., Freeman, N. M., & Bates, N. R. (2016). Apparent
increase in coccolithophore abundance in the subtropical North Atlantic from 1990 to
2014. *Biogeosciences*, *13*(4), 1163–1177. <https://doi.org/10.5194/bg-13-1163-2016>

Krumhardt, K. M., Lovenduski, N. S., Iglesias-Rodríguez, M. D., & Kleypas, J. A. (2017).
Coccolithophore growth and calcification in a changing ocean. *Progress in
515 Oceanography*, *159*, 276–295. <https://doi.org/10.1016/j.pocean.2017.10.007>

- Krumhardt, K. M., & Lovenduski, N. S. (2019). Coccolithophore Growth and Calcification in an Acidified Ocean : Insights From Community Earth System Model Simulations. *Journal of Advances in Modeling Earth Systems*, *11*, 1–20. <https://doi.org/10.1029/2018MS001483>
- 520 Nissen, C., Vogt, M., Münnich, M., Gruber, N., & Haumann, F. A. (2018). Factors controlling coccolithophore biogeography in the Southern Ocean. *Biogeosciences*, *15*(22), 6997–7024. <https://doi.org/10.5194/bg-15-6997-2018>
- Oziel, L., Neukermans, G., Ardyna, M., Lancelot, C., Tison, J.-L., Wassmann, P., ... Gascard, J.-C. (2017). Role for Atlantic inflows and sea ice loss on shifting phytoplankton blooms in the Barents Sea. *Journal of Geophysical Research: Oceans*, *122*(6), 5121–5139. <https://doi.org/10.1002/2016JC012582>
- 525 Smyth, T. J., Tyrrell, T., & Tarrant, B. (2004). Time series of coccolithophore activity in the Barents Sea, from twenty years of satellite imagery. *Geophysical Research Letters*, *31*(11), n/a-n/a. <https://doi.org/10.1029/2004GL019735>