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Geoscientific
Model Development
Discussions

# *Interactive comment on* "The role of phytoplankton dynamics in the seasonal variability of carbon in the subpolar North Atlantic – a modeling study" *by* S. R. Signorini et al.

**Anonymous Referee #2**

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Review of: The role of phytoplankton dynamics in the seasonal variability of carbon in the subpolar North Atlantic – a modelling study

By: S.R. Signorini et al.

Summary

In this manuscript a 1D water column model, which includes a complex biogeochemical submodel and is forced with 3D model output, is used to study the seasonality of coccolithophorid populations in the northern North Atlantic, and to attribute their role in air-sea CO2 exchange. The model is validated using a diverse range of observational data, and reproduces many aspects of the plankton ecosystem of this region. Analysis

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of simulations in which the coccolithophorids are omitted reveals significant changes in the carbonate chemistry of surface waters, with consequences for the air-sea flux of CO2.

General comments

Though it does not pertain to the scientific content of this manuscript, my first comment would be that the current draft of the manuscript does not seem well-suited to GMD. While the manuscript does involve a model, and there is something of development in it, the manuscript appears poorly tailored for the journal. The model description, while extensive, still has omissions, and the development of the model for the research described is largely undocumented. In fact, at surface level, the manuscript has made very few concessions for the journal to which it has been submitted. On the contrary, in its current form the manuscript seems far better suited to, say, GMD's sister journal Biogeosciences.

Regarding the manuscript's scientific content, I have a number of serious questions about aspects of the modelling undertaken. For instance: the use of a 1D model in an advective regime; the use of empirical DIC/ALK relationships; the use of strong relaxation. The main result of the manuscript, that the presence of coccolithophores strongly influences air-sea CO2 flux, is obscured by the change in primary production that results when they are removed from the model, and this is not properly explored. That said, I believe that most of these concerns could be addressed by further analysis or through sensitivity analyses.

However, as this would require substantial extra work, and because the manuscript appears to be to be ill-suited to GMD in its current form, my recommendation is that the manuscript is rejected. I would encourage the authors to resubmit to a different journal (e.g. Biogeosciences), but I would also advise significant revision along the lines I identify below.

I include a number of specific remarks below, and hope that these help the authors revise the manuscript.

Specific comments

Pg. 291-293: This is less an introduction to the science of biogeochemical dynamics in the northern North Atlantic than it is to the physical model used in the manuscript (and even then it's a poor job). This section should more clearly frame the context for this study. The material describing the physical model should be moved later in the manuscript, possibly into the methods section.

Pg. 291, ln. 25: Although the authors describe the physical model's performance, it is never given a name, and is currently only cited as "pers. comm." – this is not really acceptable when it plays a critical role in forcing the biogeochemical model (and is definitely not acceptable in GMD).

Pg. 292, ln. 13: The formatting of author name Hakkinen (cf. its trema) is not consistent.

Pg. 292: It would be helpful if the performance of the physical model was more quantitatively described. Perhaps some reference to any biases or systematic errors. Also, given the location, something concerning sea-ice (e.g. area, extent) would be very helpful – at present the manuscript merely notes the rather obvious fact that sea-ice extends in the winter and contracts in the summer. The description is also lacking basic information concerning the physical model's domain, surface forcing, parameterisation (e.g. MLD), etc. Not all of this is pertinent, but it's difficult to judge whether the model is sensible, or performing sensibly, from what little is currently presented.

Pg. 293, ln. 10-11: The authors cite primary production values, but it's unclear whether these are in situ or satellite. I presume the former because of the date of the associated citation (i.e. close to the start of the SeaWiFS period). In which case, I would want to know what the corresponding satellite values are – since, per Figure 8, the biogeochemical model appears anti-correlated with in situ observations.

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Pg. 293: The introduction of a GMD paper might reasonably be expected to set the scene re: model development.

Pg. 294: A GMD paper might be expected to start with model description before moving onto validating data, not least because the choice of model representation will dictate what is relevant for validation.

Pg. 294, ln. 6-7: The authors use the CbPM productivity model – did they use any of the standard rivals available at the Ocean Productivity website? VGPM, Eppley-VGPM? What about more high-latitude specific ones like Pabi et al. (2008)? These models can differ markedly at local scales (and don't even agree on global totals). This is particularly important since the model does not appear to agree strongly with in situ observations.

Pg. 295, ln. 6-14: The 1D model is described extremely rudimentarily here. More importantly, its relationship with corresponding properties in the 3D model that is somehow used to force it is entirely glossed over. It's conceivable that the 3D and 1D models would suggest quite different physical situations for the same forcing. On which point, it's not even clear that the 3D and 1D models are using the same surface forcing. In general, the relationships between the unnamed 3D model, the predecessor 1D model (ECO1D) and the current 1D model (TCMLM) are unclear and confusing.

Pg. 295: More generally, given that the 1D model is sitting in a location where there appears (from the figures) to be a modest flow field, it would be useful for the authors to somehow quantify, or at least discuss, residency in the water column. That is, while forcing (surface and at depth) is obviously location specific, the trends in biogeochemistry that occur at the location are also strongly affected by horizontal processes. If these occur fast enough, the signal one discerns is less a function of the biogeochemistry than it is a function of the flow field. I don't think that the authors explore this enough.

Pg. 295, ln. 15-20: This seems extremely meagre pickings for a manuscript that ostensibly deals with model development. There's no attempt to explain, justify or evaluate the changes to the biogeochemical model here. Making a model more complex is not the same as "upgrading" it.

Pg. 296, ln. 5-9: This material about the 1D framework would be better positioned earlier when the framework is first described. I'd also reiterate my point about whether the 1D model's forcing is the same as the 3D model's forcing. If not, then connecting them introduces yet another level of mismatch/complexity.

Pg. 296, ln. 10: Nutrients are restored below the MLD? Even in the summer when the MLD is shallow but short-term wind-mixing events can penetrate it? Such restoration is liable to strongly force any model back towards observations, and to decrease the importance of model dynamics. Also, why are nutrients handled differently to DIC/ALK? They're only restored below 200 m.

Pg. 296, ln. 19: Are DIC/ALK restored when MLD is deeper than 200 m? This appears a regular occurrence in the modelled winter, and is liable to strongly affect results. At the very least the authors should try to evaluate how important this is (e.g. what happens in: 1. the no relaxation case; 2. the abiotic ocean case; 3. both).

Pg. 296, ln. 20: How good are these DIC/ALK vs. T/S relationships? My experience with the CARINA dataset (which I've tried to use to fill in an Arctic-sized gap in the GLODAP dataset) is that they're flaky.

Pg. 296, ln. 21: How important is this "decadal trend"? What happens if it is not included? I note that it's sourced to an "in prep." manuscript.

Pg. 297: Might it not be an idea to compare with satellite PP here? Also, we've switched back to validation data again – the manuscript should really be more structured into model description then validation data. Mixing them up randomly makes things needlessly difficult to follow.

Pg. 298, ln. 1: What happened to the "results" section? We seem to have dived immediately to “discussion”.

Pg. 301, ln. 8-10: “... an indication that the calcite-forming coccolithophores require much more light ...”? Surely the authors know this from their parameterisation of the model (i.e. cocco alpha = 0.033, which is the lowest of the three groups represented)? Further, is this a well-established relationship? Further still, I note from Figure 6b that the model coccolithophores peak around 2 months later than in observations, suggesting that however the model is doing things, it’s doing them inaccurately.

Pg. 302-303: This introductory material concerning the carbonate system might be better off in the introduction.

Pg. 304, ln. 6: The authors note the consequences of nitrate uptake for proton consumption but I can’t see this reflected in their equations for ALK. While many models neglect this minor term (often because they do not resolve the nitrate/ammonium split), the authors draw attention to it here, but don’t appear to act on it. Note that it has a reverse effect (usually at depth) when organic nitrogen is remineralised and nitrified back to nitrate.

Pg. 304-305: No explanation is given for why the absence of coccos in the model causes such a large change in primary production. My naive expectation would be that the other two groups of phytoplankton would take up the slack (= uptake the spare nutrient), but this does not appear to happen. The authors should investigate why. Merely noting “This result highlights the importance of including all major functional groups” is insufficient – the coccos, at least as they are defined in this model, are not special and do not have access to resources that other phytoplankton do not. Diazotrophs, by contrast, would be an example of a group that have access to a pool of nutrient (dissolved dinitrogen) that other functional types do not (and who, as it happens, may or may not be in this model – Figure 4 suggests that they are).

Pg. 307, eq. A1: It isn’t necessary to repeat all of the physical operators in each and every one of the PDEs – it makes the equations ugly and may obscure the more important differences. It could be shown once at the head of this section for a generic passive tracer.

Pg. 307, eq. A1: What is the final term on line 1 of this equation ( P_{i,0} )? It does not appear to be defined or assigned a value.

Pg. 307, eq. A1: The “R” and “M” portions of this equation both appear to be linear loss rates on phytoplankton biomass – any need to specify them separately?

Pg. 307, eq. A2: Where is there no Z_{0} term in the quadratic loss term? I presume the intention is to create some sort of “refuge” for the zooplankton, but why is it not carried over both loss terms?

Pg. 310, eq. A16: Per my previous remarks, I don’t see anything in here regarding the effect of nitrate assimilation on proton consumption (and, thus, alkalinity).

Pg. 312, eq. A22: I can’t find a K_{NH4} in the parameters table. Is this different between phytoplankton functional types?

Pg. 313, eq. A31: Nitrification is parameterised as light-limited, and therefore depth-dependent – there seem to be a number of studies contradicting this now. Bearing in mind my earlier point about missing K_{NH4} values for different functional types, could this be important?

Pg. 328, Table A2: Could the authors indicate which of the parameters values are “defaults” for the baseline model, which are new parameters for the additions to the model, and how these new parameters were assigned values.

Pg. 328, Table A2: The diatoms appear not to have a sinking rate.

Pg. 330, Figure 1: Over-busy plot. I’d be inclined to just show bathymetry, ice extent and observations. The surface currents make the plot too complicated and, anyway, are repeated better (or should be) in Figure 3.

Pg. 331, Figure 2: Again, too much information. Showing SST and SSS together with ice extent is probably sufficient. Combining a contour plot with arrow vectors is too much.

Pg. 332, Figure 3: Again, too much information. This plot is important since it shows the surface currents in high resolution, but they're obscured by the overlaying of data. Further, we only get to see them this close as an annual average.

Pg. 333, Figure 4: Plain ugly. There's got to be a better way to make this plot interpretable. Also, CaCO3 seems detached from the biological variables. Also, we seem to have diazotrophs suddenly – where did they come from? They are not mentioned previously.

Pg. 334, Figure 5: Why is primary production in the region north-west of Iceland so smooth? It's much, much smoother (a near-uniform 300 mg C / m2 / d) than in the corresponding image for calcite production. The authors should also note that the colour scales are non-linear.

Pg. 335, Figure 6: I can't think of any reason why better-than-monthly data can't be used here, at least for the model. Among other things, it might better resolve the seeming discrepancy between observed and modelled calcite production (a 2 month lag in the latter).

Pg. 336, Figure 7: Again, far too much information on this plot. Also, it's impossible to determine whether the model is or isn't a good fit for primary production. About all that can be said is that the range is about right – and that's partially obscured by the use of a non-linear scale.

Pg. 337, Figure 8: Is this plot based on an inter-comparison of surface data and model values? The caption doesn't say. If it includes deep data, what is done about depths where restoration occurs? The caption also doesn't say what's going on with in situ PP – it appears to be anti-correlated, but is the position of the data point accurate, or has it been chosen so that the point remains visible?

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Pg. 338, Figure 9: See my earlier remarks about monthly model output for Figure 6.

Pg. 339, Figure 10: Given that there's no comparison with data, and that it's practically impossible to discern any signals in these highly compressed plots, I'm not sure what purpose they serve. The authors appear to wish them to show (a) trends, and (b) interannual variability, but there has to be better ways of doing this.

Page 340, Figure 11: See my remarks for Figure 10.

Page 341, Figure 12: As per Figure 6. Also, per my earlier remarks, it would be useful if the authors investigated, or at least speculated, on why the absence of coccos makes such a difference to primary production. My naive first guess would be that their absence would merely alter the pattern of production since the slack would be taken up by one of the other phytoplankton types (e.g. the diatoms; or the dinos if silicic acid has been depleted). The explanation for why this doesn't happen may lie with the abundance of zooplankton, or subtle shifts in vertical nutrient distribution, but it does seem significant to me, and it certainly requires more explanation (see earlier remarks).

Page 342, Figure 13: As per Figure 12. The authors need to establish how much of this change is due to the drop in primary production and how much is due to the fact that this production is driven by coccos in the default model.

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