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Earth System Dynamics
Discussions

Interactive comment on “Resolving ecological feedbacks on the ocean carbon sink in Earth system models” *by* David I. Armstrong McKay et al.

Anonymous Referee #1

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Summary

The manuscript uses two biogeochemical models (one simple, one complex) and two remineralisation schemes (one invariant in temperature, one temperature-dependent) in a factorial experiment to investigate how export production and carbon storage of the ocean change over the 21st century (and beyond). They find that: 1. Export production declines more in the complex model because of structural foodweb changes; 2. Export production declines less when temperature dependent remineralisation is used, regardless of ecosystem complexity; 3. There is a complex relationship with ocean uptake of carbon, mediated in part by model calcification. The authors argue

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these results underscore the need for model complexity (realistic dynamic ecology and temperature-dependent remineralisation) to properly represent the ocean's carbon cycle feedbacks.

Major comments and assessment

I have a number of minor points across the manuscript and supplementary (see below), as well as several major comments about the study:

1. Considering that this is a study focused on export production, the authors skip over how exactly this is defined in their model, and how this relates to what other researchers conventionally understand by the term. More generally, the paper's analysis largely avoids what the different models and parameterisations mean for the patterns (geographical, temporal) and magnitudes of export flux, and even how this varies with depth. The major statistics chosen for analysis deal simply with export at some undefined horizon and with how this affects air-sea exchange, and doesn't really look at spatiotemporal patterns of ocean DIC content (e.g. depth distributions).

2. The authors do not adequately describe either the physical or biogeochemical models that they are using. Given this study is focused on the vertical transfer of organic (and inorganic) material, I would expect some description of the physical domain (e.g. vertical grid), and reassurance that important vertical processes are represented (e.g. mixing). Similarly, key results appear to depend on model parameterisations of calcification, but details of this are entirely omitted. These omissions make it difficult to determine the validity of some of the conclusions reached. (Even the precise nature of the scenario simulations, e.g. emissions- vs. concentration-driven is not made clear.)

3. On a related point, the manuscript's evaluation of model performance (physical and biogeochemical) is insufficient. There is nothing on how physically realistic the preindustrial and future scenarios are; for instance, how aspects such as the magnitude and pattern of temperature and mixing change compare to other more physically-realistic models. These are of key importance to the biogeochemistry models used here, and

specifically to the temperature and nutrient responses investigated.

4. While deficiencies with models are always inevitable, it is important that these are addressed in manuscripts. The noted lack of model description and evaluation are joined by a lack of appropriate discussion and caveats. Combined with the authors strong (over-confident?) conclusions, these absences make it difficult for readers to accurately assess the significance of this manuscript.

5. When presenting its key findings of change in export production between the various combinations of model and parameterisations, the manuscript does not any provide contextualising information. How, for instance, do the modelled changes compare to those of the CMIP models mentioned elsewhere? Are the changes found here within or outside the ranges found by these other (less sophisticated) models? This omission, in particular, makes it difficult to understand this work in context. Table 1, for example, could be expanded to include such metrics from CMIP5.

All that said, the manuscript is otherwise relatively straightforward to understand and follow. And I am fairly confident that their findings are accurate in identifying certain processes as important, and therefore relevant to both observational and modelling scientists. However, it remains the case that the manuscript's omissions (both in background and analysis) undermine its credibility, and I recommend major revisions before it can be accepted for publication.

Minor comments

Ln. 28: saying "gradually" in the same sentence as "centuries to millennia" might convey the wrong impression; I might be inclined to just delete "gradually"

Ln. 32: what do you mean by "sink feedbacks"?; this could be enhanced stratification decreasing mixing and / or nutrient supply; be clearer please

Ln. 40: what about AR6?; quite a lot of model output is available, together with a lot of supported information about model formulation; you might also wish to consult

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doi:10.1007/s40641-020-00160-0 or doi:10.5194/bg-17-3439-2020 on AR6

Ln. 52: a "this manuscript is structured as follows" (or similar) might help here

Ln. 55: erm, what about a straight decline fed by a decreased amount of production?

Ln. 60-61: I seriously doubt this; there is a large body of work focused exclusively on the soft pump whereas there is far less focus on the hard and silicic acid pumps; in part because, unlike the soft pump, the activity of these latter pumps is less ecologically-driven and more physiochemically-driven (cf. the importance of the CCD)

Ln. 72: note that Kwiatkowski et al. (2014) describes 6 models, of which only 1 has a single size class; the rest have at least 2 size classes

Ln. 74-76: as opposed to some other "class of plankton model" which is not limited ...

Ln. 84: can you give an idea why such evidently superior (as described here) models still fail to capture BGC and "large-scale" dynamics?; do you mean physical resolution problems?

Ln. 94: "shoaling" rather than "raising"?

Ln. 94: “the point at which most POC is remineralised” - is this a formal definition you’re using in this study?; it might be good to give it a name (e.g. R_50, or something) if so

Ln. 95: at some point it might be nice to demonstrate that faster remin. leads to greater export despite the remin.; maybe that’s yet to come

Ln. 98-99: cf. my previous remark, you’ll need to demonstrate that faster remin. doesn’t act as a *positive* feedback mechanism (i.e. warming -> faster remin. -> less C export -> more atmospheric CO2 -> more warming -> ...)

Ln. 107: “will not substantially affect productivity in existing oligotrophic regions” - is this actually important?; the important aspect of oligotrophication is that formerly productive regions of the ocean become less productive; that unproductive regions remain equally

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unproductive is not obviously important

Ln. 107: “the depth rather than the intensity” - my understanding is that most research already focuses on the depth and not the intensity of stratification; usually studies focus on change in mixed layer depth - key for resetting surface nutrient conditions during periods of deeper seasonal mixing (e.g. winter)

Ln. 111: “result in ocean interior deoxygenation” - as does warmer surface temperatures that control the oxygen concentration of water ventilated into the interior

Ln. 111: “reduce nitrogen availability” - are you referring to denitrification here?; you should be specific if you are; otherwise one might assume you mean reduced nitrogen (and other nutrients) from reduced mixing

Ln. 114-117: it might be helpful to note the significant uncertainties in OA feedbacks on marine BGC; something like doi:10.1146/annurev-environ-012320-083019 might help here

Ln. 116: “by which POC sticks to denser falling PIC” - this kind-of reads as POC is somehow "attracted" to CaCO3; instead, the ballast hypothesis is more about POC *already associated* with CaCO3 somehow being "protected" by it to reach further into the ocean interior

Ln. 124: “IPCC AR5” - as already mentioned, the archive of simulations used in AR6 is already quite well-stocked with models

Ln. 130: “(Bendtsen et al., 2015; Dunne et al., 2007; Martin et al., 1987)” - this list of citations is ambiguous; are these submodels of remineralisation used in specific models in Table 1 or what?

Ln. 130: Given that Dunne et al. invokes a ballast model that dynamically alters the remineralisation profile, this characterisation seems inaccurate

Ln. 131: this statement is characteristic of many in this manuscript; i.e. bombastic on how deficient existing models are; given this manuscript is using a reduced-physics ocean model with extremely poor vertical resolution (<= 16 levels; it's unclear, see below), a more measured toned might seem appropriate

Ln. 141-154: this description must be augmented by a description of the underlying physical model so that readers understand that it imposes its own limitations on this study; while pointing to other uses of the model is fine, a minimal description that notes model resolution (horizontal, vertical, temporal) and the reduced physics nature of this model is key; noting what the model does around processes relevant to export production (e.g. mixing, convection) would help readers unfamiliar with this particular EMIC

Ln. 151: "have lower spatiotemporal resolution" - cf. my previous remark, expand on what's meant by "lower"

Ln. 151-154: ". . . well-suited for investigating more complex biogeochemical dynamics . . . " - only if the physics they represent is up to the job, of course; and that may depend on the specific application; a reader should be interested in how reduced vertical resolution impacts a model of vertical POC transfer

Ln. 155: the history of the models can probably be skipped to focus on what's being used here; unless it's important for this particular study (which it might be)

Ln. 161: what's the relationship between the size classes?; does each predator graze everything smaller than it is, or is there another scheme?

Ln. 161: it would be helpful to have some idea (e.g. a sentence or two) on how the different size classes differ from one another; e.g. photosynthesis parameters, nutrient uptake, growth rates, grazing rates, mortality rates

Ln. 168: "16 layers" - ah, finally!

Ln. 169: does the model resolve seasonal mixing of different ecological regimes?

Ln. 170: is there information about what "not quite as well" means?; e.g. which properties were examined to determine this?; ones directly relevant to this study, or more indirect?

Ln. 176: ah-ha; we do need to know about BIOGEM; good!

Ln. 186: “POC export” - how is this defined?; it is the flux at a particular depth, or just out of the model’s uppermost level?; in cGENIE this can be quite deep (∼80m?), but it may not conform to field norms; e.g. Martin et al. (1987) used 100m

Ln. 186: “Gt C” - The SI unit is Pg C rather than Gt C

Ln. 191: “shorter sub-overturning timescales” - this is opaque; I presume you mean that, as this model doesn’t include sediments, DIC is conserved within the ocean regardless of export production (which might not be true if it were "lost" to sediments); but I don’t understand the reference to timescales; not least because a 10,000 y simulation should be enough for several complete overturnings of the ocean

Ln. 193: “total CO2 emission scenarios” - is the model being run in emissions mode (i.e. a time-varying amount of CO2 is being added to the model atmosphere and then redistributed, including into the ocean) or in concentrations mode (i.e. a time-varying atmospheric concentration is specified but cannot be affected by different ocean uptake responses); make this clear

Ln. 193: this list of scenarios seems to omit the low scenario RCP 2.6; any reason?; this unlikely, low emissions scenario (as SSP126) is still used in CMIP6

Ln. 193: “3PD, 4p5, 6p0, and 8p5” - these scenarios are more typically referred to as e.g. RCP 8.5, where the period is a decimal point

Ln. 199: do you evaluate the performance of your modelled warming relative to other models (e.g. CMIP5, CMIP6)?; as the marine BGC models you’re using are sensitive to warming, it would be useful to know how realistic this is in the model; it should be straightforward to compare model output to, say, corresponding CMIP5 / CMIP6 output;

e.g. change in magnitude / pattern of ocean temperature, mixing, etc.

Ln. 200: cf. my last remark, maybe say something here about what happens physically in your scenario simulations

Ln. 200: you should also perhaps begin by discussing how the different models represent the pre-industrial situation; some of this is covered (I think) in the supplementary material, but I have comments there too

Ln. 217: “0.3%” - I’ve mentioned over at the table itself, but you might like to add such relative stats there

Ln. 221: how is the mixed layer handled in cGENIE?; older versions of the model don’t really have a mixed layer

Ln. 223: do you really mean "new production" here?; the distinction may be confused by changes in mixed layer depth itself across the scenario period

Ln. 268: this part of the manuscript is confusing without some clarity on how the hard pump works in this model; both in terms of $CaCO_3$ production (and controls on this), and how it dissolves down the water column

Ln. 277: it’s difficult to tell, but this just sounds like the two models differ in the strength of their hard pumps (probably relative to the soft pump), with the result that they have different hard pump changes relative to soft pump ones into the future; and because the models are not well-described on this point, it’s hard to decide what’s going on

Ln. 282: why "initial"?

Ln. 282: “the importance of incorporating multiple dimensions of ecological complexity” - the paper doesn’t really present anything concerning the modelled ecological complexity; passing comment is made on shifts in size structure, but nothing is shown, not even as supplementary information

Ln. 284-287: the modelled change in export production is presented without any contextualised reference to other work on this; there is a large body of published work on how export may change, ranging from studies using individual models through to meta-analyses using, for instance, CMIP output; to make clear the significance of the distinction the authors are highlighting, the existing span of estimates needs to be clear

Ln. 286: "a much simpler NPZD-based ecosystem" - much simpler, perhaps, but possibly different in an important way for the hard pump

Ln. 307-309: I wouldn't necessarily expect TDR to increase the production of diatoms because opal is dissolved and not remineralised; if anything, one might expect diatoms to do less well as time passed because - although N and P might be becoming more available (because they're getting remineralised faster) Si would not be; there's probably some subtlety I'm missing here, however; expand to make clearer

Ln. 311: "a subsequent increase in calcifying plankton and PIC export" - this result may be quite dependent on how calcifiers are modelled; this already isn't clear in this study, so I'd suggest drawing the parallels out more fully

Ln. 314: "which we have shown is critical" - I don't think this has been clearly shown here; the model is too incompletely described, and the physical model probably leaves something to be desired

Ln. 320: there are no caveats in this paper about the quality of the model, physical or biogeochemical; the only caveats seem to relate to making the biogeochemical model even more complex without any consideration of whether the physical framework is adequate; work has emphasised the potential importance of physical frameworks for BGC models, e.g. doi:10.1016/j.pocean.2009.10.003

Ln. 320: things left undiscussed include: 1. how realistic is this model's response under climate change (e.g. pattern and magnitude of temperature change; compare with CMIP5 / CMIP6); 2. can this model realistically represent mixing; 3. can this model realistically represent vertical gradients of properties given grid cell thicknesses (even close to the surface); 4. how dependent are results on (undescribed) hard pump submodel

Ln. 320: some of the above points cannot easily be addressed here; but they should be properly acknowledged and discussed, and they should temper the conclusions drawn here; it may well be that these are accurate, but the physical and BGC models used here should give some pause for thought

Ln. 320: per my comment on Table 2, the different scenarios get pretty short shrift here; they're just stand-ins for different amounts of warming / emissions; it's not clear that they couldn't be thinned to small, medium and large warming

Ln. 320: equally, it's not made very clear what the differences between the atmospheric CO2 concentrations across the scenarios mean for the ocean uptake numbers here; we should expect larger numbers for higher RCPs, but does efficiency of uptake of CO2 change (or is that too far for this paper?)

Ln. 322: "critical"? - while there may well be feedbacks such as those described here, I think the authors are arguably exaggerating their importance, especially given the magnitude of the numbers they find; I'd suggest "may be important" is more suitable wording

Ln. 328: “as expected” -> "than expected"?

Ln. 326-329: this sentence is too long to be parsed well; it's important so make it clear

Ln. 333: "post-CMIP5 projections"? - you might need to explain what you mean by this

Fig. 1: "surface layer" = cGENIE's top box?

Fig. 2: perhaps use different colours rather than linestyles to separate the four different models?; then save solid and dashed styles for the two scenarios

Ln. 651: it might be helpful to note the distinction you're drawing between "N/A", "No", and "No mention" here; also, assuming "no mention" means you couldn't find any reference to this in the model descriptions, have you considered contacting the model authors to ask?

Table 2: while this sort of summary is of key importance, it might also be useful to see how these numbers change in time (beyond the single supplementary figure)

Table 2: it occurs that the manuscript does not clearly address the different scenarios; it might be better to reorganise so that the results are organised by model first and then by scenario; that way the span of results between scenarios (i.e. the effect on the properties for different degrees of warming) are clearer to see

Table 2: this kind-of omits what happens for the period 1850-2000; it might not be important, mind

Table 2: might it be useful to note what the changes in this table represent in relative terms as well (e.g. what's this delta as a percent of the total flux over this period?)

Table 2: columns 4 and 6 - "default cGENIE" = "BIO+FPR", so perhaps just say that?

Table 2: thinking about the ocean uptake column, what about the efficiency of ocean uptake and how this varies with scenario and time?; this may require information about emissions (see my previous remark about emissions vs. concentration simulations)

Supplementary material

Ln. 17: perhaps show the observational field as well so that the relative size of these errors is clear

Fig. S1: which time point is being compared here?; presumably near-present day given the choice of observational product

Fig. S1, caption: when you say "surface" are you comparing the concentration in the uppermost layer of GLODAP with the uppermost layer of your model, or are you depth-averaging so that the intercomparison is fairer?; if not, you will need to explain why the intercomparison you're doing is the right one; this applies to alkalinity too (and nutrients if you plot them)

Fig. S1, caption: why not write this as 2 mmol / kg?; ditto for the graphs; "E-0X" notation is a little annoying when we've got scientific prefixes available to us

Figure S3: this pattern looks interesting; is it salinity-related?; i.e. does it reflect a bias in model salinity?

Fig. S3: alkalinity is usually given in equivalents rather than mols

Fig. S3, colour scale: "1.00E-04" - see previous remark about units

Fig. S5: I'm assuming annual mean chlorophyll here; although I note that the model's Arctic is negatively biased to almost 1 mg chl / m3 - that implies quite a high annual mean observational chlorophyll; has it been time-averaged correctly?

Fig. S5: chlorophyll is not usually a brilliant metric to compare models to; I'd suggest using nutrients

Fig. S5: also, you could compare to productivity; that possibly is even more relevant to the problem at hand

Fig. S5, colour scale: I see the "milli" prefix is getting used here! ;-)

Fig. S6: I presume these colour scales are being used for parity with the previous delta plots?; I understand that, but it might be more informative to use a more relevant colour scale to help readers delineate where models differ geographically

Figure S19, colour scale: "5.8E-07" - as well as "milli", there is also "micro"

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