Biogeosciences Discuss., 11, C4364–C4370, 2014
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Biogeosciences Discussions
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Interactive comment on "iMarNet: an ocean biogeochemistry model inter-comparison project within a common physical ocean modelling framework" *by* L. Kwiatkowski et al.

Anonymous Referee #1

Received and published: 13 August 2014

This is a well written and professionally presented paper that I consider publishable with minor to moderate revision. It is arguably more suitable for GMD but that is the editor's decision.

I agree that showing the full suite of maps and associated Taylor diagrams for individual fields would be overwhelming and relegating some of these to the Supplementary information is a good idea. However, I think Figure S5, or a similar one for annual mean data, could be incorporated into the main text. The paper has only 6 figures and I think an additional one summarizing the various models' skill in a Taylor diagram for each of the fields considered (except O2: see below point 4) is a good idea.

There are a few things missing from the model description:

(a) The grid resolution should be stated. This is highly relevant to issues discussed, such as computational cost and deficiencies in the modelled ocean circulation. NEMO at e.g. 1 or 2 degrees resolution gives a very different circulation.

(b) There should be a brief description of the algorithms used for carbon chemistry and gas exchange (e.g., which equations were used to calculate the equilibrium constants). These models are fairly mature and not the main source of error in ocean biogeochemistry models (and I assume they were standardized across the six models used here although this is not actually stated), but a brief description is nonetheless required.

(c) None of the ecosystem model descriptions say anything about calcification or calcite dissolution. This relates directly to interpretation of the modelled vertical profiles of DIC and alkalinity, and to the anomalous distribution of pCO2 in the equatorial zone in some of the models (see below points 1 and 5).

Main conceptual points:

(1) When the errors are relatively uniform across models and are therefore attributed to errors in circulation there is little discussion of the underlying physical processes. Vertical gradients of DIC and alkalinity are weak in the Southern Ocean, which could conceivably be attributed to excessive vertical mixing. But I think there is a biological element that is not considered here. Modelled vertical gradients are much stronger for DIC than for alkalinity, which I would attribute to the ecosystem models exporting POC but negligible PIC. If it were purely due to circulation I doubt there would be such a difference between the two.

I also think that the x axes on Figures 5 and 6 (and S6 and S7, but see below note Re: 10550/12) should be rescaled to reduce white space. This is particularly true for the case of DIC in the equatorial Pacific. Some of these profiles don't show much vertical structure, so wasting half of the available space is a bad idea. The boxes themselves

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could also be made a bit wider. (Also the vertical axes are nonlinear and need some explanation. If it is a logarithmic scale, say so. If it is an arbitrary 'telescoping' this needs to be stated explicitly.)

(2) The Conclusion does an admirable job of spelling out the implications of different strategies for model formulation, and the arguments for continuing development of more complex models even if they do not have greater skill with respect to e.g. DIC and pCO2. But I have two caveats here:

(a) One issue that is not mentioned is model diversity. Given that no model is shown to be the most skillful by all metrics, and all are most or least skillful by at least one metric, a central conclusion that can be drawn from this work is that it is important that the international climate modelling community maintain a diverse suite of models and do not 'converge' on a few similar ones.

(b) I don't care for the false dichotomy of improved climate simulations vs "scientific exploration" in the final paragraph. Adequately addressing some issues previously raised with respect to unresolved climate feedbacks (e.g., DMS) will certainly require more complex ocean biology models.

(3) I think the conclusion that no model is demonstrably better or worse than any other is not really consistent with the data. In Table 3 (see also Figure S5), not only does ERSEM show the weakest correlation for pCO2, chlorophyll and primary production, but these correlation coefficients are consistently the smallest by a wide margin and are in all cases not meaningfully different from zero. It does better for nitrate, DIC and alkalinity but these are weak diagnostics for the reasons discussed (e.g. 10547/18-19). I don't think the claim made on 10551/23-27 that in some cases "models of greater biological complexity tend to equate to improved model skill" is justified by ERSEM having (marginally) higher skill for surface nitrate.

(4) I don't think surface O2 is a useful diagnostic, and the authors should consider removing it entirely (e.g., Table 3, Figure S5 and especially Figure 4). At the surface,

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biological processes play a negligible role in the distribution of O2, as is noted in the text (10548/21-23). Figure 4 summarizes the rank order of model skill on different metrics, with no consideration of how large the differences are. Do they really want this analysis to be biased by inclusion of an essentially meaningless diagnostic for which the differences among models are negligible?

(5) The pCO2 fields in the tropical upwelling zones in the more complex models (ERSEM, PlankTOM) look almost like a mirror image of the expected pattern, with lower pCO2 associated with recently upwelled waters (Figure 1). I agree that this probably results from excessive alkalinity in the upwelled water (10550/10-11, Figure 6). But these authors do not go into much depth about the underlying processes. Clearly these models are not removing alkalinity from the surface layer by biogenic sedimentation at anything like real-world rates. By failing to consider (or even describe) the calcification and calcite dissolution models and by too casually dismissing the Southern Ocean alkalinity errors as deriving from circulation, they miss an opportunity to delve into the source of errors that are on the surface quite pathological. No one is going to accept a model in which cold, DIC-rich water upwelled to the surface in the tropics has a pCO2 below atmospheric.

Some details:

10539/6 "Dynamic Green Ocean Models" Is this really a class of models? I thought it was just the name that a particular group gave to their own model (which may have since evolved into a suite of related models, but that still doesn't really justify calling it a class or type of model). Anyway the abbreviation is never used and is not necessary (see also 10544/1-2).

10540/6 "direct human exploitation of the seas" I don't think there is any evidence for such top-down forcing of the kind of fields considered in this paper.

10540/23 "What controlled the variations in atmospheric trace gas over the geological past including those measured by isotopes?" What controlled variations in atmospheric

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trace gas concentrations and isotopic composition over the geological past?

10540/28 I don't think it's accurate to say that IPCC 'produced' the data archive.

10541/4 "how will climate change affect oceanic primary production" ocean

10541/8 I would consider citing the more recent and more comprehensive paper by Harvey 2008 (10.1029/2007JC004373) in place of or in addition to Khesghi 1995. The older paper is in a somewhat obscure journal and is cited in the more recent one.

10541/21 "following the same experiment protocol" experimental

10543/14 "a dimethyl sulphide (DMS) sub-model for cloud feedbacks" I would delete "for cloud feedbacks" as it is not relevant to the present experiment.

10544/2 add "level" after "trophic"

10545/3 "the marine biology" biota

10545/16-17 makes it sound like the pCO2 data came from SeaWiFS

10545/25 the GLODAP data product is not a climatology

10546/3 "the biogeochemical pathway through which the vast majority of marine ecosystems ultimately obtain energy" I would not word it like this. Phytoplankton photosynthesis represents the vast majority of the primary energy source to marine ecosystems. But I have trouble envisioning what is meant by a majority of ecosystems.

10546/10 delete "and in part related to preceding points"

10546/25 "circumference axis" I have not heard this term before and Googling it turns up only a few marginally relevant examples. Taylor calls it the azimuthal position.

10548/24 "Figure 4 summarises Table 3" Figure 4 summarizes the data in Table 3

10548/28-29 "field metric" Another jargony and probably unnecessary term. I would just delete "field". (see also 10552/1, 7)

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10549/22 "much shallower gradients with depth" Not clear what "shallower" means here. Weaker? I don't think it means there is a 'cline' at a shallower depth, although that is true in some cases. Please reword and clarify.

10549/27 "ocean physics deficiencies" errors in ocean circulation

10550/6 delete "values"

10550/6 "MONSooN" I don't think the name of the machine is relevant here and anyway the acronym is never used.

10550/12 and 20 There are two supplemental figures numbered S7

10550/21-22 "This unsurprisingly reflects the significant cost of performing ocean physics operations on biogeochemical tracers." I'm not sure this sentence is necessary at all, but maybe it could be modified to something like "reflecting the significant cost of applying advection and mixing terms to each tracer" and appended to the previous one.

10550/26 It looks to me like "computational cost" means something other than total CPU time or wall-clock time here but I can't tell exactly what.

10551/11,14 delete "of"

10551/12 "shown to generally have higher" shown to have generally higher

10551/20 delete "the oceanographic regions of"

10551/21 "possibly because their biological export production can more easily be tuned to maintain the observed vertical gradients" Is there any reason to believe that these models were tuned to reproduce depth profiles in these specific regions?

10552/7 add a comma after "(Table 4)"

10552/10-11 "depths of 1000 m" less than?

10552/13 "discrepancies within the physical ocean model" errors?

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10552/15 "For alternative fields such as DIN in the Southern Ocean and Equatorial Pacific (Supplement Fig. S7), however, models have both positive and negative biases" For other fields, such as DIN in the Southern Ocean and Equatorial Pacific (Supplement Fig. S7), models have both positive and negative biases

10552/21-22 "also tend to represent additional factors" are also able to represent additional factors

10553/5 "Specifically, the HadOCC and MEDUSA-2 models that were previously implemented within NEMO v3.2 were “familiar” with this ocean model’s configuration and flaws." Meaning, I assume, that the developers of these models were familiar with NEMO and had some opportunity to tune the ecosystem to a circulation similar to that used in this experiment. Please be more specific. Models of this sort do not learn on their own.

10553/7-8 "the ERSEM model ... had a distinct disadvantage" which is what?

10553/9 delete "found"

10553/10 change "settings" to "values"

10553/18-19 "a bottom-up approach to model skill assessment" I can’t tell what this means, and the term does not appear to have been used by Vetter et al.

Table 2 I would change "Prokaryotes" to "Heterotrophic bacteria" (assuming that is what it means). Prokaryotes is a (mostly obsolete) taxonomic category rather than a functional/biogeochemical one, and some other groups in this table are mostly made up of prokaryotes.

Interactive comment on Biogeosciences Discuss., 11, 10537, 2014.