

Interactive comment on “Improved routines to model the ocean carbonate system: mocsy 1.0” by J. C. Orr and J.-M Epitalon

Anonymous Referee #3

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Summary

The manuscript presents a revised and updated oceanic carbonate chemistry scheme, MOCSY, intended for use by both observational scientists and carbon cycle modellers. Updates include a number of amendments to existing schemes, some new parameterisations and a general effort to permit a range of possible inputs that will suit most researchers (e.g. units, in situ vs. potential temperature). The manuscript also includes instructions to allow readers to download the scheme.

Overall, the manuscript is clear and well-written, though I have a number of minor criticisms described below. The associated Fortran code is easy to access, compile and run, both with the included test case and as part of custom code (essential if it is to be adopted by modellers).

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Please note that I am a user of carbonate chemistry calculators like MOCSY rather than a chemical oceanographer. As such, my comments deal more with use of MOCSY than its fundamentals.

Specific comments

The following are minor comments relating mostly to edits that may slightly improve the manuscript. They are followed by a few remarks concerning my experience building and running MOCYS.

Pg. 2878, ln. 28: Careless readers (such as this one) may misinterpret “nutrient contributions” to refer to the consequences for proton concentration (and thus pH) of the use of nitrate and ammonium by phytoplankton; probably later rather than here, a “not-to-be-confused-with” statement might head this off

Pg. 2879, ln. 7: “In many models ...” – it might be helpful if the authors gave a few examples, more so that readers are aware of the diversity of uses to which the OCMIP code has been put; citing examples of use of the code by OCMIP itself might be useful too

Pg. 2881, ln. 3-4: “... from only one input pair ...” – this is slightly confusing because the code makes use of other inputs (which are mentioned later); perhaps this needs to be qualified with a specific reference to these being carbonate inputs – my reading of this is the authors are hinting at possible (but unstated) alternative inputs that are sometimes used to constrain the carbonate system (e.g. pH)

Pg. 2883, ln. 26: “[HF]” is not defined; please make sure that all chemical terms are defined when first used (or, probably better here, in a table); this work will likely be used by non-specialists involved in carbon and climate research and it is important that they are not waylaid by abbreviated terms that they are unfamiliar with

Pg. 2884, section 3.1: the authors present this section almost as a “disagreement” between “gentlemen models” and do not make it clear whether one model is better or

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worse than the other; perhaps this cannot readily be assessed to the required accuracy, but the authors could simply say so if this is the case

Pg. 2885, section 3.2: this section refers, correctly, to the errors caused by making the assumptions mentioned, but associated figure 3 instead presents these errors in the context of “corrections”; the implication being that one could keep using the approximations and just adjust the answers appropriately – surely we don’t want people to do this?; it would be better if the paper was consistent in its branding of these differences as errors to be avoided (by, for example, using mocys-1.0!)

Pg. 2885, ln. 25: as the authors present the magnitude of errors or differences elsewhere I would expect them to give some notional (quantitative) idea of why the differences caused by the Kf option can be neglected

Pg. 2886, ln. 2: “substantial differences” caused by 1. total boron and 2. K1 and K2 are both mentioned here, but while numbers are presented in the following paragraph for boron, K1 and K2 go unmentioned; is Figure 4 doing all of the talking?; a clarifying remark might help

Pg. 2886, ln. 3-4: is this tantamount to saying that models using the GLODAP climatology of alkalinity are effectively setting the oceanic inventory of this property wrongly?; I’m sure that I’m misreading that, but it may give readers this idea

Pg. 2888, ln. 19-22: given the strong linear correlation between dissolved inorganic nitrogen and dissolved inorganic phosphorus, models that include N but not P could use a Redfield-scaled N as a substitute for P; this is likely to be a temptation for a number of models and this may be something that the authors could comment on here

Pg. 2890, Code availability: I would imagine that mocsy performs – in a computational sense – similarly to previous iterations (or rival packages), but it would be useful if the authors reported on any comparisons that they have made on it; for instance, does it typically converge in the same number of iterations?; I would expect so, but the addi-

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tions to the complexity of the modelled chemistry may impact on this; one of the main groups of scientists who will be interested in this are ocean biogeochemical modellers, and reassuring them that the code performs comparably to what they currently use would doubtless help with its uptake by the community

Pg. 2897, figure 3: the legend is ambiguous about the identity of the black dotted line; panel 1 labels it up, but it wouldn't hurt if the legend did too

Experience with code:

- Code successfully downloaded via OCMIP5 website (git has passed me by)
- Code initially failed to compile using ifort ...

```
sw_adtg.f90:11.19:
```

```
USE msingledouble
```

```
1
```

```
Fatal Error: File 'msingledouble.mod' opened at (1) is not a  
GFORTRAN module file
```

- However, switching to gfortran in the makefile fixed this and was successfully able to build test_mocsy; the above error is most likely caused by local environment configuration

- test_mocsy successfully runs producing sample output for “model” and “observational” test datasets; I was also able to easily incorporate it within an existing Fortran subprogram ready for adoption within our local BGC model

Interactive comment on Geosci. Model Dev. Discuss., 7, 2877, 2014.

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