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Interactive comment on "Evaluating CaCO₃-cycle modules in coupled global biogeochemical ocean models" *by* W. Koeve et al.

J. Dunne (Referee)

john.dunne@noaa.gov

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General Comments

The manuscript "Evaluating CaCO3-cycle modules in coupled global biogeochemical ocean models" by Koeve et al. assesses the utility of a variety of derived water column metrics of alkalinity for calcium carbonate cycling, describing the difficulty of deconvolving biases in physical and biogeochemical pathways, and concluding that the Alk* method is considerably more robust than others in the ability to parse out the preformed (Alk0), remineralized organic (Alkr), and dissolved CaCO3 (Alk*) contributions through an uncertainty analysis in an offline model. The authors then apply the method to

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three coupled physical-biogeochemical models to assess their representation of Alkr and Alk* and find that while the models all represent Alkr fairly well, they have large differences in their representation of Alk* both compared to the observational estimate and each other. The major strengths of the manuscript are in the detailed fashion the authors explore the complications associated with interpreting alkalinity variations in observations as they confound model verification and how they arrive at a concrete recommendation. The major weaknesses of the paper is that the authors fail to incorporate into their analysis comparative assessment of a common approach (Alk-DIC, or 'excess alkalinity') used to verify CMIP5 class models (e.g. Séférian et al., 2013; Dunne et al., 2013), choose a CaCO3 dissolution scheme that puts most dissolution in the upper water column rather than below the saturation horizon, fail to describe the factors leading to the differing Alk* distributions in the three OCMIP5 models, and similarly fail to come to any conclusions as to the relative strengths and weaknesses of these model formulations. These weaknesses aside, I think the present manuscript provides a very thoughtful and helpful contribution to advance the verification and future development of ocean CaCO3 models and would hope that the authors are planning on a more targeted application of this method to assess OCMIP5 and CMIP5 models in a following manuscript.

Specific comments

6120,5 - Where does the 45% come from, a specific depth horizon? For a maximum, I get more like >95% remineralized PO4 in the N Pacific.

6120,15 - suggest replacing 'are not from the' with 'reflect not only' and removing 'alone'.

6121,13 - This is where I would be interested in seeing a comparison with the Alk-DIC metric used in Figure 9 of (Séférian R., Bopp L., Gehlen M., Orr J. C., Ethe C., Cadule P., Aumont Olivier, Melia D. S. Y., Voldoire A., Madec G. Skill assessment of three earth system models with common marine biogeochemistry. In : Presentation

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and analysis of the IPSL and CNRM climate models used in CMIP5. Climate Dynamics, 2013, 40 (9-10), p. 2549-2573.) and Figure 5 of (Dunne, J. P., and Coauthors, 2013: GFDL's ESM2 Global Coupled Climate–Carbon Earth System Models. Part II: Carbon System Formulation and Baseline Simulation Characteristics*. J. Climate, 26, 2247–2267. doi: http://dx.doi.org/10.1175/JCLI-D-12-00150.1). Admittedly these assessments where more generally configured for evaluation of anthropogenic carbon uptake rather than specifically the CaCO3 models within them, but it would provide a helpful critical assessment for feedback to these groups.

6122, 2 and 11 - Can you provide the r2 for these to help the reader quantitatively assess the pattern similarity.

6123,5,9 - I'm having trouble interpreting what the authors mean by 'spurious' and why 'the salinity-normalized TA0-anomaly should be constant everywhere' Do they simply mean that the distribution is not helpful for CaCO3 cycling assessment as it reflects surface compositional differences determined other than through Salinity differences? It is clear that 6a and 6c look similar and 6b and 6d look similar and that some is preformed, but I'm not sure how any of the distributions are 'spurious'.

6125,21 and 6130,1 - In acknowledging that undersaturation during water mass formation leads to AOU overestimating oxygen utilization by 20-25%, more detail is warranted here, particularly with respect to why it was ignored and the consequences to the underlying uncertainties in Alk*... i.e., that it leads to an overestimation of Alk* of about 3 mmol/m-3 or 5%.

6127,8 - Representation of dissolution as an exponential scaling is extremely crude in ignoring the role of saturation state and is a potentially important limitation of the error analysis in its applicability to the real world. This structural limitation should be acknowledged.

6130,9 - Discussion of Figure 7 - What is TA0ub? Why are their no observational lines in the regional plots?

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6132, 10 - Some description of the CaCO3 dissolution formulations in these models is warranted.

6132, 21 - Table 4 versus Figure 11 - Table 4 has the UVIC model as slightly higher than GLODAP, but in Figure 11, it looks like the UVIC line is always at or below the GLODAP line, how does that work out?

6133, 21 - What is causing these differences?

6133, 24 - suggest replacing 'compared with the representation of the organic tissue pump' with 'than their organic tissue pump modules'

Interactive comment on Geosci. Model Dev. Discuss., 6, 6117, 2013.