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Interactive comment on "Sensitivity of pelagic CaCO₃ dissolution to ocean acidification in an ocean biogeochemical model" *by* A. Regenberg et al.

Anonymous Referee #1

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This paper compares a range of parameters in models for dissolution of calcium carbonate in offline simulations with an ocean biogeochemistry model, with circulation fields generated by two different ocean models (BERN and NEMO). The experiment is worthwhile and generated some interesting results, but the paper is not well written and requires quite substantial revision before publication.

The paper combines Results and Discussion, which is somewhat verbose although the actual results presented are quite limited, and ends with "Summary and Conclusions", much of which is about topics that are not closely related to the results presented. I often recommend that authors try rewriting a paper in the standard I-M-R-D format, especially when the key messages are buried under a lot of questionably relevant de-

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Interactive Discussion

Discussion Paper

tails. I am not opposed in principle to combining Results and Discussion, but when a paper's key message is not clear it is almost always worthwhile to try to separate them. The Discussion could then include, for example, all of the discussion of ballasting, which was not part of the experimental design and regarding which no results are presented. The rather grandiose claim in the Summary that "All diagnosed differences in the distribution of alkalinity can be qualitatively and straightforwardly explained from the expected effects arising from the use of either Omega or Delta and the accompanying reaction rate orders" is not supported by the results presented.

As a further topic for Discussion, they might acknowledge that the assumption that dissolution only occurs below the saturation horizon is probably wrong. The models used clearly only apply when Omega<1 (about which more below), but it is not very explicitly acknowledged that this assumption is being made, or that it is inconsistent with a wide variety of observations and analyses (e.g., Milliman et al 1999 DSRI 46:1653; Berelson et al 2007 GBC).

The most significant result of this paper is the sensitivity analysis with respect to the exponent n in equation (5). But they could do a better job of stating exactly what S represents in this equation, and how its definition relates to equations 1 and 2. It is implied (11348/14) that for the ratio (Omega) approach it is 1-Omega, which makes sense as long as the equation is only applied where Omega<1, which should be explicitly stated. (There should also be square brackets around CaCO3 in the equation to signify that it is a concentration and not an (undefined) symbol.)

The effort expended on comparing the Omega and Delta based formulations (equations 1 and 2) seems unproductive given that (1) it makes almost no difference to the overall behaviour of the model (e.g., Figures 2 and 3), (2) it is not connected to any discussion of physical processes, and (3) it is not clear that the differences are not due to arbitrary choices about which parameters were optimized and how. Why is it necessary that the order be the same in each case, and only the rate constant is optimized? Why not have e.g. an order of 4.5 in one case and 4.4 in the other, if it fit the data bet-

ter? Would the already small differences between the two approaches shrink further if they did this? I find the paragraph following equations 3 and 4 (11347/8-20) verbose and mostly unnecessary. I think the authors should try to condense this down to half its length or less. If they can not state in a clear, concise manner what the point of this comparison of the competing formulae is, they should consider dropping this whole topic. There is little inherent interest in comparing a difference and a ratio of the same physical quantities; what is of interest is the models' sensitivity to parameterizations with different mathematical properties. At the very end of the paper (final paragraph), the authors hint at why this sensitivity analysis of competing models may be useful, but in general they do not do a good job of explaining what they are trying to achieve by comparing these two formulations, and do not present any results that seem likely to be useful in helping other modellers decide which to use.

The methods used for optimizing the parameters to the trap data, and the data themselves, are also not well explained. It is claimed that the models were optimized to "observed dissolution values" (11352/6), but clearly dissolution rates are not observed but only inferred from observed rates of particle flux at different depths. There is no description of the trap data set (where? what depths? what kind of traps? how long a deployment?); the reference given is an AWI technical report.

I do not find the assertion that the vertical gradient in alkalinity determines cumulative C uptake (11354/18-20) convincing, because in a fully equilibrated model it should depend primarily on the total inventory of alkalinity. It is possible that there is a slight dependence on the vertical gradient, but that should be a second order effect compared to the total inventory if the model is fully equilibrated. Also the authors need to state exactly what are the boundary conditions with regard to alkalinity. Is there burial? Redissolution from sediments? Inputs from rivers? Is the total ocean inventory conserved? The text seems to state that in NEMO there is a fair amount of burial (top p. 11358) and what the compensating inputs are is not stated. If there is burial but no compensating source of alkalinity, won't the alkalinity eventually just go to zero?

I also don't really understand how accelerated dissolution can cause the vertical gradient of alkalinity to reverse (11358/24-25) unless there is a shallow external source from e.g. rivers or shelf sediments. The increasingly shallow dissolution expected under this high-CO2 scenario will homogenize the vertical profile, but it is difficult to see how it can reverse if the only source to the surface ocean is mixing and upwelling of subsurface water. An inverted profile implies an external source, which underscores the fact that the boundary conditions are not explained.

I actually find almost all of the discussion of vertical gradients rather confusing, even in the discussion of the reaction-order sensitivity experiment in Section 3.1.1, although that part is at least physically plausible. But most of the discussion of the underlying mechanisms is vague. For example, on 11354/27-28 it states that "the deviation from the GLODAP data may also result from different temperature and salinity distributions as well as circulation fields (solubility pump) and it is also likely that they are affected by differences in the vertical flux of organic carbon (soft tissue pump)". This a very broad and vague generalization that says little or nothing about the underlying processes. The reference given is just a definition of the respective "pumps". What about looking at salinity-normalized alkalinity? This can be a useful metric of the relative contributions of circulation and biogeochemistry to errors in modelled alkalinity. Similarly, the processes that cause the saturation horizon to shoal in the North Pacific while deepening in the North Atlantic (11355/19-20) are not explained. Up to this point, the only processes discussed should have a more or less uniform effect across regions, and the only results shown are for the global mean.

The discussion of the overturning circulation in Section 3.1.3 (first paragraph on 11357) is similarly unconvincing. There is no particular reason why global aggregate primary production should be related to the AMOC, and the fact that the ratio of these two quantities is similar in the two models probably doesn't mean anything. The relationship to AOU is more plausible, but a lot of the AOU is in the weakly ventilated middepth layers, particularly in the Pacific, so perhaps it is less surprising than implied that the expected

relationship fails to materialize (especially given the coarse horizontal resolution of the BERN model).

Some details:

11344/24 "followed by a decrease in pH" not clear why changes in one species 'follow' the other

11345/1 "predicted to proceed" projected

11345/7-14 "While sensitivities . . . carbonate system" This sentence is very long, convoluted and awkward (although more or less grammatically correct). Reword and preferably break into two sentences.

11345/17 "About half of the anthropogenic carbon emissions" There seems to be a persistent confusion (not just in this paper, I have been noticing this for several years) between the fraction taken up on an annual basis and the cumulative fraction. First we are told that the ocean has taken up about 25% of cumulative emissions (11344/24), and that this will increase to 50% in the future. The sign of the "potential repercussions on atmospheric CO2" (of ocean acidification, 11345/6) is not stated, but I think it is generally accepted that ocean CO2 uptake as a fraction of total emissions is likely to decline rather than increase. I think the problem here is confusion between current and cumulative emissions. I think the 25% figure is the "real time" uptake (say, 2 Pg out of 8 Pg emissions, although the actual fraction seems to be a bit higher, see Table 7.1 in the AR4 (WG1) report). The cumulative uptake fraction is higher (e.g., estimated as 48% by Sabine et al 2004).

11345/23 "are particularly concerned" are of particularly concern

11346/1-2 "Consequently . . . from below" I see what they are trying to say here but the wording needs work. Regardless of the direction of vertical migration of the saturation horizon, CaCO3 in the sediments will dissolve from the top down. Nor is it universally true that the saturation horizon migrates monotonically from greater to lesser depths.

11347/12 "differently spoken" stated differently

11348/14-18 I don't see what is necessarily counterintuitive about having a weaker dissolution response to an incremental decline in [CO3–] in an environment with a lower (baseline) saturation state.

11351/5 "the pool of particulate CaCO3 pool"

11352/10 "higher order expressions are less sensitive to low values of undersaturation" not clear what this means

11352/15 "a CO2 of 286 ppm" atmospheric CO2

11354/17 "the CaCO3 counter pump" This is a jargony term that I am not familiar with, and requires some explanation.

11359/2-4 "Until year 1000 CO2 uptake further increases but now the higher rate order experiments of the Delta series exhibit significantly less uptake than all other experiments in both models." C_anth inventory in Table 2 is higher in Delta4.5 than in Delta1 in the BERN model. It's lower in NEMO but the difference is <1%; by what criterion is this significant?

11359/20-21 "devastating impact on calcite particles"??? (see also 11360/14)

11359/23-24 "where still the highest saturation values are yielded"? reword

11360/15-16 "calcite fluxes at shallower depths (e.g. 1000m) are clearly fanning out" not clear what "fanning out" means

11360/20 "impact on the oceanic sink or anthropogenic CO2" for

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