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Discussions

Interactive comment on "A reactivity continuum of particulate organic matter in a global ocean biogeochemical model" *by* Olivier Aumont et al.

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The manuscript "A reactivity continuum of particulate organic matter in a global ocean biogeochemical model" by Aumont et al describe the implementation and implications of a relatively sophisticated representation of sinking particle degradation into a global biogeochemical model. In this representation by Boudreau and Ruddick (1991) the decreasing lability of particles over time is represented as a series of particles with a continuum representation of remineralization rate constant. As this formulation is relatively numerically intensive, its effects have generally been either ignored or approximated simply by an increasing sinking velocity or power-law remineralization rate constant dependence with depth. Thus, the present manuscript makes a powerful contribution to the ocean biogeochemical literature by reconciling all of: past contradictions between particle concentration and flux, particle age distributions, and regional

variability in both empirical power law attenuation coefficients. There are four topics on which I would like a bit more information added to the manuscript: 1) What generates the regional variability in the transfer efficiency in Figure 11? 2) How good is the representation of transfer efficiency as a power in Figure 11? The authors should show some representative profiles of particle flux and the inferred power law fit to go along with the concentration profiles in Figures 3-6. A plot of particle weighted sinking velocity and lability would be most helpful. 3) Is the relationship between initial composition and final remineralization profile amenable to the creation of a numerically efficient metamodel that would avoid the addition of the extra particle tracers? 4) Does this result finally solve the challenge of distinguishing between the two hypotheses of increasing sinking velocity with depth and decreasing lability with depth leading to the fidelity of the Martin curve? The model seems to include both factors, and this reconciliation should be highlighted. Specific comments (For line numbers, I am using the guide on the left rather than counting from the top): P3, ln13 – "explicitly" should be added before "taken into account" since the Martin curve is an implicit implementation of this. P4, ln 3, 10, and p6, ln 6 – "big" should be "large" – also, the nominal size cutoff should be provided. P7, ln 14 – Wilf Gardner has a database (http://people.tamu.edu/∼wgardner/∼pdgroup//SMP_prj/DataDir/SMP-data.html). Jim Bishop may have one as well. P7, ln 25 – The authors could also consult the Honjo dataset for 2000m values (http://usjgofs.whoi.edu/mzweb/smppi/honjo.html) P8, ln 18 – "does represent" should be "represents", and the two "the" should be removed. P8, ln 21 – What are the units of "0.1 to 0.4"? P8, ln 24 – Remove "associates to a relatively strong remineralization" P9, ln 29 – What are these modeled and observed C14 ages in confict? P9, ln 15 – add "a" before "result" P9, ln 16 – Add "variable" before "lability" P9, ln 19-20 – To make the case the the model is good, one compares with observation, but if both models are not good, the improvement in r2 relative to observations will not necessary be high. . . to show the result of the parameterization has changed the model distributions, one should show the r2 for both mod-obs and mod-mod. P9, ln 23 – What is the third simulation "in all three simulations" P10, ln 1-10 – The authors need not be concerned that their estimates to not agree with the anomalously low Henson et al. (2012) and follow on Guidi et al., (2015) relative to the previous literature by Yamanaka and Tajika (1996), Schlitzer (2000), Laws (2000), Muller-Karger et al (2005) and Dunne et al (2007). While more recent, these “revised” Henson estimates are deeply flawed and nicely refuted empirically in Weber et al., (2016; PNAS). The problem with the Henson et al. (2012) and follow on Guidi et al., (2015) papers is that they restrict their calibration to particle export estimates that have the advantage of being geographically broad through the tropics but were found to suffer from strong analytical biases. This was first identified by Quay (1997) who performed a carbon budget for the equator at 140 as part of the EqPac synthesis and demonstrated that the Buesseler method of measuring C:234Th ratios via in-situ filtration led to inferred carbon fluxes a factor of two lower than necessary to achieve a steady state carbon budget for the region. It turned out that the filters were directly absorbing dissolved 234Th, leading to artificially low C:234Th ratios and inferred carbon export fluxes. Unfortunately, this analytical bias was not fully appreciated until the field program was complete and an extensive database was developed. While Buesseler soon discontinued use of this method and critically championed the development of neutrally buoyant sediment traps to achieve more accurate estimates, these filter-based C:234Th ratio particle export estimates remain in the JGOFS-era database. As a result, scientists continue to publish syntheses based on these data. If these are to be used, they should only used in combination with the suite of other estimates that are all double the global flux with much more tropical contribution. P11, ln 30 – “relied on” should be “tests” P12, ln 28 – What are the small particle ages in these simulations? P12, ln 5 – Again, don’t just trust the Henson numbers. P13, ln 2 – Some discussion of the modeled ecological factors driving the regional varaibilty in transfer efficiency is warranted. P15, ln 10-14 – Provide the r2 or RMSE comparison for these runs. P16, ln 10 – It would be extremely helpful to the ocean biogeochemical modeling community to have a parameterization of the transfer efficiency based on the ecological structure to avoid the addition of the extra particle tracers. P17, ln 24 – A note on the implications for variability in sinking

particle weighted lability and velocities as a function of depth Figure 4 – This should be combined with Figure 3 into a single figure. Figure 6 – This should be combined with Figure 5 into a single figure. Figure 11 – Missing colorbar

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