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Biogeosciences Discussions

Interactive comment on "The ocean response to volcanic iron fertilisation after the eruption of Kasatochi volcano: a regional scale biogeochemical ocean model study" *by* A. Lindenthal et al.

Anonymous Referee #1

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This paper presents the first attempt to model the 2008 volcanic ash fertilization event from the Kasatochi eruption. Such work is important and sensitivity studies from models like this could be very useful in elucidating the mechanisms behind these processes and when the ecosystem might be most responsive. However, the model does not capture the most important feature of this region (lack of a spring bloom). This has to be corrected before results from the model can be used to understand this event. I hope that the authors do correct this problem in the model and present new model runs, because I think the results would be quite interesting.

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Major issues:

This model fails to represent the basic seasonal cycle at station Papa. It shows a distinct and large spring bloom, while the main feature of the subarctic NE Pacific is an absence of a spring phytoplankton bloom. Chlorophyll at Station Papa has been shown to be low all year long with occasional, brief blooms at any time of year. See for example the early surface chlorophyll measurements presented by Parslaw (PhD thesis 1981) and reprinted in Boyd et al. (Global Biogeochem. Cycles 1998). Also, Whitney and Freeland (Deep-Sea Research II 1999) present nutrient cycles at Station Papa that show a fairly steady decrease in surface nitrate from March to September, in contrast to the sharper nitrate drawdown in spring in this model. Only silicic acid has a sharp decrease in about May through July. The authors state that macronutrient mixed layer concentrations stay at a constant level during summer in this location, but this is not true. While I sympathize with the authors' desire to avoid a full evaluation of the biogeochemical model results in this paper, the model has to at least reproduce the most important feature of the annual cycle in this region for other results from it to be believable. Two possible issues seem likely. First, the model may fail to represent iron concentrations or uptake kinetics properly (see next paragraph). Second, zooplankton in the model may not be exerting enough grazing pressure on the small phytoplankton whose growth is less limited by low iron concentrations. In this region, small zooplankton reproduce quickly to keep populations of small phytoplankton in check.

The paper should present the modeled iron cycle as it does for the other nutrients. In particular, the initial iron concentration of 0.2 nmol Fe L-1 is about three times too large for this region (see Johnson et al., Deep-Sea Res. II, 2005). Also, more details are required regarding mixing rates at the base of the mixed layer (is iron being transported upward at too fast a rate?) and whether there is some other source of iron in the model / whether it is restored to some set value. The model assumes that all dissolved iron in the surface is bioavailable, while many studies have shown it is not. This might be okay, because the iron uptake kinetics used appear to be scaled to dissolved iron not soluble iron, but the authors should clarify.

The inappropriate presence of a spring bloom in the model also affects the timing sensitivity studies. The authors show that the modelled ecosystem does not respond as strongly to volcanic ash input during the spring as during the late summer, presumably because there is already a bloom going on in the spring. However, in the real subarctic NE Pacific that lacks a spring bloom, the region seems likely to respond as strongly in the spring as late summer.

The paper makes a number of statements regarding the link between the Kasatochi eruption and record Fraser River salmon returns two years later that are not supported by data / analysis in this paper or cited research. I agree that the model results could be used to support enhanced zooplankton production from the eruption event, but the model (since it does not include salmon) provides no evidence that this could significantly increase salmon survival rates. No evidence is presented for the statement that two other eruptions can be connected to large salmon runs. The model also does not demonstrate that a relatively small additional zooplankton population in late summer represents optimal feeding conditions for salmon. A more appropriate citation for the Kasatochi salmon link than a Nature news article is Parsons and Whitney, Fish. Oceanogr. 21:5, 374–377, 2012.

Moderate concerns:

The abstract does not summarize the results fully or make clear what new insights are presented in the paper. I suggest that the 2/3 of the abstract that summarizes the introduction be reduced and the portion of the abstract summarizing the results of the paper be expanded.

Please expand on the comment at the end of section 3 that "comparing temporal and horizontal scales it appears appropriate to neglect horizontal (processes)..." Station Papa is situated right in the geostrophic flow to the east, so horizontal processes are an important process there.

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It's surprising that the volcanic ash run has nearly identical silicic acid concentrations to the no eruption run. The annual drawdown in silicic acid in the model is also much smaller than in the observations. Are the modelled diatoms not responding dramatically to the iron addition?

That the pCO2 drawdown is faster with larger iron additions is a pretty interesting result that is not well highlighted in the paper. One might have expected the bloom to become larger or last longer, but to start faster is cool. What about the model creates this situation? Does it arise from the iron uptake kinetics?

The statement near the end of section 5.2 that a 30 times higher iron supply creates a modelled pCO2 that best resembles the measured one seems somewhat misleading. The model begins with a much higher pCO2 than the observations, so it's really the total drawdown or change that should be compared rather than the final pCO2 value.

Minor comments:

Section 2 suggests an iron concentration at Station Papa resulting from the Kasatochi eruption of 1-2 nmol L-1, but Langmann et al. (2010b) estimate 0.3-0.7 nmol L-1. Please clarify the reason for the increased estimate.

End of section 2: One of the SERIES iron enrichment experiment papers might be a better reference here than Wells (2003), which reports on iron measurements during IronExII in the equatorial Pacific.

Given that the paper presents pCO2 and pH results, some brief details about the carbon cycle in the model might be appropriate. Does the ecosystem include calcification? What gas exchange parameterization and atmospheric pCO2 are used? What starting values of DIC and Alkalinity are used? Poor initial conditions or representation of some controlling processes may explain the higher model pCO2 than observed.

In the pCO2 discussion at the end of section 4, the authors should cite and acknowledge the source of the pCO2 data.

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