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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 #2

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General Comments

This paper describes a simulation of the fertilizing effect of volcanic ash deposited in the aftermath of the Kasatochi 2008 eruption to the iron-limited NE Pacific Ocean. The model simulation indicates that iron, contained in the volcanic ash, triggered a phytoplankton bloom. A comparison with observed carbon dioxide drawdown at the surface suggests that the model is in agreement with a set of observations.

It is, to-date, a relevant scientific question if, or to what extent, the current generation of

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biogeochemical models is capable to simulate the limiting effect of iron. The approach to simulate a "natural" iron fertilization experiment, such as a massive deposition of volcanic ash, is up-and-coming. To this end, I think that this is a very important paper! As regards the present form of the manuscript, however, I feel that it lacks essential information. Bluntly put, it is not clear what kind of model the authors are using and, second, if the model is realistic. I suggest to complete the model description and to append additional model evaluation.

Specific Comments

- The model description is incomplete or at least convoluted: According to pg. 9234, ln. 9: "regional scale ocean ..." a regional scale model is applied. pg. 9238, ln.1 raises some doubt about that: "We focus here on one-dimensional column applications around the buoy Papa (50N, 145W) although the model is set-up in three dimensions." And finally, pg. 9238 ln. 9 seems to contradict that: "it appears appropriate to neglect horizontal advection and diffusion in this column model study." Hence the reader is left uncertain on the issue if it is a 3-D model or not. I was also baffled by the comment that the seasonal cycle of the mixed layer depth is prescribed (pg. 9238, ln. 5). In summary, I have the impression that the physical model is rather special. This calls for (1) an exhaustive documentation including all underlying equations and in (and out) going fluxes and data, and (2) a comparison of the simulated physics with observations (e.g., sea surface temperature measurements from space).

A vital element of the paper is the simulated effect of iron. As regards the iron cycle, however, the information given is very generic (pg. 9237, ln. 4) and does not allow for a reproduction of the results. In order to be of interest to other scientist the paper must contain (or reference) all model equations and associated parameters.

- Additional model evaluation needs to be appended: The only model evaluation

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there is, is a comparison with surface pCO₂. Because there is so much more data available at station PAPA, a mean reviewer might conclude that the model-data comparison is cherry-picked. One way to proceed could be: (1) Run the model to equilibrium (without the addition of iron contained in volcanic ash) and compare it with a climatological seasonal cycle of macronutrients. (2) The Hamme et al. (2010) paper compares chlorophyll, gross primary production, net community production and phytoplankton concentrations measured during the bloom with data from other years. A comparison of this data with simulations would be very meaningful.

Technical Corrections (incomplete list)

Some of the figures are of very poor quality, e.g., axis and colorbar labels of figure 5 are not readable.

I think the reference to the sockeye salmons is not really needed to make the paper work.

Replace "near surface" in the title of figure 7 with actual depth.

Figure 8: add the 2007 simulation of ECOHAM;

Figure 10: a measure of the quality of the respective model simulations, such as correlation or RMS, would be nice (to be put into the text). Also, I would replace "factor" with actual deposition.

Figure 9: I agree that the exact timing of the ash deposition is uncertain to some extent. Hence, I like the idea of the authors to test a suite of deposition scenarios. However, because the volcano erupted in August, I would skip all deposition scenarios prior to this date.

Figure 2b: Is it possible to give this in units nmol iron /m².

Figure 3: What about remineralization of detritus in the water column?

Figure 1: How is the "ash cloud position" defined? What is a "coast buffer"?

pg. 9243, ln. 11: "planed" => planned

5.2 "Amount of volcanic ash and associated ...": Add a discussion of that uncertainty, that is related to processes in the water column which partition iron into its bioavailable and unavailable forms.

pg. 9238, ln. 13: why is the model not run into a quasi equilibrium?

Reference

Hamme et al. (2010), Volcanic ash fuels anomalous plankton bloom in subarctic north-east Pacific. *Geophysical Research Letters*, 37, L19604.

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