Biogeosciences Discuss., 6, C4356–C4359, 2010
www.biogeosciences-discuss.net/6/C4356/2010/

Biogeosciences Discussions

Interactive comment on "The role of airborne volcanic ash for the surface ocean biogeochemical iron-cycle: a review" *by* S. Duggen et al.

S. Duggen et al.

sduggen@ifm-geomar.de

Received and published: 5 February 2010

Reply to Morgan Jones comments.

Morgan Jones emphasizes that our manuscript is easy to read, informative, thorough, and will be of use to scientists from a wide range of disciplines. He stresses that our review paper is both warranted and acceptable for publication in Biogeosciences, as it deals with an emerging field of scientific study. He only has a few minor suggestions and comments, which we reply to below. We thank Morgan Jones for his efforts and constructiveness.

Abstract. MJ points to the timing at which fine ash can settle through the atmospheric gravitational sedimentation. We agree with his comment and reworded the abstract

accordingly by phrasing "…fine ash may stay aloft for days to weeks, thereby reaching even the remotest and most iron-starved oceanic regions.". For this we cite Niemeier et al. (2009) in the main text rather than Robock (2000).

Abstract. MJ also argues that volcanism was present before the ocean and suggests to phrase that the possibility of iron injection to the oceans has occurred throughout Earth's history (instead of "much of"). We, however, prefer to keep the phrase "much of" as 1) little is known about how long back in the Earths history Fe-iron fertilisation was relevant for the marine primary productivity (although to our knowledge the current literature points to events as far back as the early Tertiary (see citations in our manuscript)) and 2) because Fe-fertilisation linked to volcanic activity is unlikely to have been a relevant process prior to the rise of atmospheric oxygen content in the early Proterozoic, which is argued to be associated with a significant drop in oceanic dissolved Fe-concentrations and the formation of the banded iron formations (Klein et al. 2005, Am. Mineral.).

Introduction. MJ suggests citing the literature for iron fertilization experiments either be in alphabetical or date order. Please note that we use Endnote and the Copernicus_Publications library for citations, which organizes the citations in the main text.

Introduction. The referee points to Sarmiento (1993) and the suggestion that onset of an El Niño after the eruption could have also assisted in the relative drawdown of CO2. He also mentions that whether the change in Pacific conditions had anything to do with the Pinatubo eruption is unclear, but that larger eruptions are predicted to cause changes in overturning and ocean circulation (see Jones et al., 2005; Jones et al., 2007). Please note that Sarmiento stresses that the net effect of an El Nino event on atmospheric CO2 is an increase rather than decrease. Components in the budget are an increase caused by a terrestrial effect that is larger than a decrease caused by a change in Eastern Pacific upwelling. This supports the idea that the atmospheric CO2-drawdown in the early 90s is triggered by the Pinatubo eruption rather than an El Nino event. As most of the ash of the Pinatubo eruption was deposited in the South

China Sea (Wiesner et al. 2004, Bull. Volc.) a causual connection of the atmospheric CO2-drawdown to possible changes in overturning in the Pacific are highly unlikely.

MJ mentions that the possible cause of increased volcanism from deglaciation (aside from sea level change) is changes to crustal stresses associated with inland ice removal (see Zielinski et al., 1997). We appreciate the comment but think it is not necessary mention the mechanism in this manuscript.

The referee argues that the satellite evidence for a phytoplankton bloom is still somewhat inconclusive. For the Montserrat case he questions that there is enough evidence from the Montserrat data to separate the chlorophyll signature from the background signal. In the revised version of the manuscript we discuss the possibility that mineral dust, may it be airborne or suspended in surface ocean water, could add a pseudo-Chl signal to the satellite data (which, for instance, was discussed in Duggen et al. 2007). In this section in the manuscript at hand we added new and very recent evidence for a link between ash fall-out and Chl levels, measured offshore Sicily during the large-scale Etna 2001 eruption (Randazzo et al. 2009). The Langmann et al. paper providing evidence for a causal link between the large-scale phytoplankton bloom in the northeast subarctic Pacific and ash fall-out from the Kasatochi volcano in the Aleutian in August 2008 has in the meantime been published (Langmann et al. 2010).

MJ questions if generalised distinctions can be made between ash samples from a subduction zone setting and from hotspot settings, given the range of compositions in each category. We appreciate the comment and, in order not to over-emphasize the distinction between SZVA and HSVA Fe-release, omitted the discussion from the introduction. We, however, chose to distinguish between SZVA and HSVA at some other place in the paper since, due to the differences in gas systems of volcanoes in these tectonic setting, volcanic ash from SZV and HSV may have different potential to release Fe on contact with seawater. Moreover, the few existing pristine HSVA, for which Fe-release was determined, show a much higher Fe-release than SZVA. Whether this reflects a systematic difference between SZVA- and HSVA-release is unclear. A cur-

rent key problem is the relatively low number of Fe-release data for HSVA samples and, by pointing to potential differences between SZVA and HSVA samples we hope to stimulate further research in this direction. Knowledge about a possible general difference in the Fe-release of SZVA and HSVA may be important for future estimates of the flux of Fe into the surface ocean. The Olgun et al. manuscript, by the way, is currently in review at Global Biogeochemical Cycles.

MJ argues that our assertion that the decay of HSVA samples with time does not occur is unfounded. This seems to be a misunderstanding arising from the way we phrased the paragraph. We agree that decay of salt coatings is possible for volcanic ash samples in general and provide own evidence in Olgun et al. (in review, GBC), which is discussed in our review paper. We also agree with MJs argument that “the change in the Fe-release between the two experiments appears to be greater than the change in relative surface area caused by sieving slightly different fractions”, which also argues for a decay of the salt coatings. We rephrased section 4.1.1 Decay of soluble salt coatings ? in the revised manusctipt accordingly.

Finally, we dealt with the few typos pointed to and the rephrasing of some sentences as suggested by the referee.

Interactive comment on Biogeosciences Discuss., 6, 6441, 2009.