Reply to Reviewers and Editor (our reply highlighted in red)

Before addressing specific suggestions of referees and the editor we would like to comment that we made some overall tightening and clarification of the text throughout the paper, augmenting the material in a few places where we deemed it important. The largest changes involved a one-paragraph introduction to help place the study in perspective and a rewrite of the section concerning the comparison with the Gao et al. (2008) reconstruction. In our opinion, the new paper by Plummer et al. (2012) in a Copernicus sister-journal (Climates of the Past) justified adding some additional material about the 1450's Kuwae eruption.

We also added some information on the new category of 'unipolar' tropical eruptions (Section 3.1), as we believe this important category needed some more clarification and discussion.

Finally, in the section on estimating time of year of eruption, we felt it important to add a figure on the dating of the great 1257/1258 eruption to illustrate our point better (the discussion was already there, just not the figure). We also added tables of our summary composites for each hemisphere, as we think this will be useful to future investigators.

We believe that between the additions/clarification of our making, and the helpful prompts from the reviewer and editor, the referees and editor will consider our paper more satisfactory and hopefully recommend publication of the present version.

Anonymous Referee #1

This technical report describes a very interesting reconstruction, based on sulfate records in Antarctic and Greenland ice cores. The article is well structured and has important findings of interest for linking climatic and volcanic episodes. The architecture of the paper is very clear, including three main chapters (with different subchapters),one table and eight figures. In my opinion, the technical report is scientifically andtechnically sound, and it matches well the scope of ESSD Discussions.

My only remarks are related to three points:

1) subchapter 1.1 (Determination of volcanic peaks) requires further explanation;

We thank the reviewer for their comments.

We rewrote the section to be clearer about how the volcano component was estimated. We state that: "The magnitude of a specific peak was determined by comparing it to the previous years of background variability, choosing the peak background variability as the baseline point for estimating excess flux for a particular volcanic event. Sometimes the baseline period can extend for several decades, in which case a confident assessment of volcano peak amplitude can be obtained. Sometimes there are of clusters of sulphate spikes that make assessment of an

individual peak more uncertain. In such cases the post-eruption variability was used as a further constraint on appropriate background levels."

2) the use of sulfate records should be complemented with more data about current sulfur emissions from selected volcanic areas (incorporating a more detailed analysis).

We thank the reviewer for their comment but are reluctant to engage in this exercise. Emissions from volcanoes that don't inject material into the stratosphere are besides the point (with respect to our paper). Thus more than 95% of 20th century eruptions are excluded. Thanks to satellite coverage (two different instruments) we have by far the best information on total emissions for the 1991 Pinatubo eruption. We tried to assess the uncertainty in our estimates by examining other eruptions for which we have independent aerosol optical depth information – from our analysis both in this paper and the cited Crowley et al. (2008) paper, our approach seems to be consistent with data from other volcanoes – within the uncertainties of both our approach and the other data. This is about the best that can be done at this stage.

3) the connection of the volcanic activity and the possible variations of the atmospheric dynamics should be tackledmore in depth and discussed by the authors.

While this is an interesting topic it has been addressed in many other studies and is beyond the scope of the journal we have submitted to, which focuses on elaboration of the methodology used in development of a data base. The editor of the journal also seems to agree with us. We therefore decline to elaborate on the implications for dynamics. However, we did write a short introduction that places some of our results in perspective of other cited climate studies on volcanoes, so in a sense this was one attempt to reach a compromise with the reviewer's recommendation.

End of reply to reviwer 1.

Anonymous Referee #2

This report shows an interesting reconstruction of the link between volcanic eruptions and climatic episodes. The reconstruction is based on integrated sulphate records from several Greenland and Antarctic ice cores and covers the last 1200 years. In my opinion the manuscript is clearly written and well structured and deserves publication on ESSD after few minor revisions. My only remarks are listed here:

Section 1.1. You should give some details on the "iterative approach" adopted for volcanic eruptions found in periods of high background variability.

We thank the reviewer for their comments.

The wording in the first draft was unfortunate and we rewrote that section as following: "Sometimes the baseline period can extend for several decades, in which case a confident assessment of volcano peak amplitude can be obtained. Sometimes there are of clusters of sulphate spikes that make assessment of an individual peak more uncertain. In such cases the post-eruption variability was used as a further constraint on appropriate background levels."

Table 1: Is the Dome C record used in the study the one obtained from the EPICA drilling or is it the old Dome C perforation? If the old Dome C ice core is the one usedin your study, why you didn't use the EPICA Dome C data?

Thank you for catching this – it is EPICA Dome C of course and we clarified that in the table.

Explicit the AOD abbreviation not only in the abstract but also in the text, the first time it appears.

This has now been done.

Page 3 line 21: change to (Vinther et al. . ..) This has now been done

Page 10 line 10: change to "hemispheric" This has now been done
Page 10 line 22: change to "it was" This has now been corrected.

Page 11 line 1: you write "biweekly-scale ECM measurement", but I think that the realresolution is bi-monthly as you write in the caption of figure 4. Please change the text accordingly.

Thank you for catching this; we may have made the appropriate correction.

Page 13 line 26: change to "Intercomparison"

This sentence has been deleted because the editor requests all appropriate data be posted with the journal. However, if the revised paper is accepted and published we will make certain the data are also available in other cites (e.g, PMIP, NGDC, etc).

In the list of references there is a paper by Castellano et al. (2005) that is not cited in the text, please find a place to insert the citation to this paper.

It is now cited in Section 3.7 and Table One.

Editor recommendations to revise listing of data:

We have deleted the section at the end of the text concerning the PMIP cite and moved discussion data repository to the base of the abstract, as recommended by the Editor.

End of Reply by Authors