

Interactive comment on “Towards a quasi-complete reconstruction of past atmospheric aerosol load and composition (organic and inorganic) over Europe since 1920 inferred from Alpine ice cores” by S. Preunkert and M. Legrand

Anonymous Referee #2

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The authors review previous geochemical data from alpine ice cores recovered from Mt. Blanc, and summarize what is known about various aerosol deposition trends in central Europe from prior to World War II to present. This portion of the paper is comprehensive, which is not a surprise given that the authors have been involved in most of the data collection over the past 15 years. Given the extensive and excellent publication record resulting from these datasets, I have no real concern about the quality of data, the interpretations that are summarized in the paper, or the figures

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that are adapted from previously published work. If the authors intend simply a review paper, then I imagine they could leave the summary as is and provide a few key insights into where knowledge is still lacking and how it might be addressed in future studies. Unfortunately, the objectives of this paper in terms of providing a quantitative analysis are not clear to me. The authors attempt a semi-quantitative inversion of snow chemical concentrations to atmospheric aerosol concentrations, but this analysis is based on some rather poorly constrained assumptions in section 5. I don't see any new data collection, or numerical analysis here, but rather the application of more previous work to previously collected data. In the end, it is not clear to me how the conclusions reached from the semi-quantitative inversion are any different that what has already been published without the inversion. Perhaps I am missing a major piece of the paper, but if so I would appreciate the authors documenting in much clearer terms in the introduction what the paper is intended to do. Is it primarily a review, or is the main point the inversion? If the inversion is indeed the central point, then a much more rigorous treatment of the inversion techniques and assumptions needs to be presented to be able to evaluate it. So in essence I suggest that the authors either restrict the paper to a comprehensive review, or scale the length of review back considerably and focus instead on the data inversion, with a view towards conclusions that provide a clear addition to the knowledge of this important topic.

Please also note the supplement to this comment:
<http://www.clim-past-discuss.net/9/C593/2013/cpd-9-C593-2013-supplement.pdf>

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