Clim. Past Discuss., 9, C1974–C1976, 2013
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Climate of the Past
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Interactive comment on "Trace elements and cathodoluminescence of detrital quartz in Arctic marine sediments – a new ice-rafted debris provenance proxy" *by* A. Müller and J. Knies

Anonymous Referee #2

Received and published: 13 September 2013

This manuscript evaluates a new approach to trace the provenance of IRD that is based on the detailed analyses of individual quartz grains. This might be a valuable addition to the available set of approaches to deduce the provenance of IRD found in marine sediments. However, there are still some open questions regarding its applicability, which might need a more thorough discussion.

In the Introduction the authors refer to the review paper of Hemming (2004, focusing on Heinrich layers in the North Atlantic!) for outlining the importance of IRD provenance studies. This statement is followed by a brief listing of other proxies mainly applied to central Arctic Ocean sediments. However, as this manuscript focuses on the western Svalbard margin, a bit more detailed review of the large number of studies (e.g., Elver-

hoi et al. 1995, Andersen et al. 1996, Hebbeln et al. 1994, papers by Jens Bischof and by the Tromso group and others) dealing with IRD provenance from exactly this region definitely would improve the paper. It would be especially good to know, which proxies (lithology, mineralogy, clay minerals, but also those referred to in the above mentioned listing: Nd isotopes etc.) have been successfully applied and where we still have major gaps in tracking IRD provenance. Also sediment trap studies from the region have been used to link sea ice transported material to sea ice advection from eastern Svalbard by the East Spitsbergen Current (Berner & Wefer 1994, Hebbeln 2000).

Although I trust in their conclusion that most of the IRD comes from eastern Svalbard (as also other studies have shown before, see above), I am a bit skeptical regarding the reasoning. With their method the authors identify typical populations of the quartz types A-E related to specific onshore source areas. At the end they link these populations to those populations found in the sea floor samples. However, after uptake by sea ice and icebergs and after transport, the final deposition of the IRD will largely take place as individual grains. So, I wonder how the "artificial" population made up by these individual grains coming from a variety of icebergs/sea ice deposition events can be compared to populations being derived from a discrete rock sample? Maybe that works out, but to be convinced I would like to see some statistics.

Some more detailed questions regarding the interpretations:

If the high amount of Type D quartz in sample 1265 is indicative for a local source, where is the considerable amount of type B quartz in this sample coming from as it is not found in the surrounding onshore samples?

The type E grains in sea floor sample 1244 are linked to the only onshore sample containing this type of grains: DH7A-1. However, according to Fig. 5 this sample is located close to the Isfjord, where this group of samples has been used to explain the high contribution of type D sediments to the Isfjord sample 1265 (see above). Maybe this is a matter of clear presentation in the figure. But as it is presented now, it is somewhat confusing.

Sample 1246 from the southern Storfjord that is right in the proposed main IRD transport path, contains quite some Type C grains that are not common in the proposed source area on Edgeoya. A comment on this observation would be great.

Finally, the entire region has been glaciated several times with the last glaciation being just 20 ka ago. Glacial erosion and transport probably have distributed quartz grains from various source regions to all around the north-eastern Barents Sea. And, of course, also these glacigenic sediments can be picked up by icefloes and icebergs probably delivering a quite variable quartz signal to the western Svalbard margin. In which way would such a "reworked source" signal interfere with the conclusions drawn in the manuscript?

To sum up, this is a bit a difficult manuscript. The new approach presented might bear quite some potential and, thus, I think it is worth to explore. However, there are quite some open questions to me as they are outlined above. To my opinion, this manuscript needs a major revision before it might be ready for publication.

Interactive comment on Clim. Past Discuss., 9, 4145, 2013.