

## ***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***

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In terms of my own “expertise” I would claim both an interest and knowledge of ice-rafting processes, but I have had very little to no experience in the tracers used in this study. One small point before I discuss some major issues—my colleague at the University of Colorado is Lang Farmer, i.e. his 1<sup>st</sup> name is Lang (see references—they should be Farmer, L.).

I certainly support the publication of this paper as it has the potential to address a critical issue in many IRD studies, and that is trying to better identify source(s) for that the ubiquitous mineral—quartz. Because this is all about quartz then I suggest they

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should reference

*Kolla, V., Biscaye, P.E., Hanley, A.F., 1979. Distribution of quartz in Late Quaternary sediments in relation to climate. Quaternary Research 11, 261-277*

and also Bond's efforts to identify source(s) based on the hematite stains on sand-size quartz grains. Nevertheless, their assertion that characterizing the provenance of quartz in Quaternary sediments is an important objective for IRD studies is totally correct.

I think the authors might say a little at the beginning about the grain-size spectra of their sediments. I think many researchers have the mistaken belief that when "we" talk about IRD that the sediments contain a great wt% of coarse sand. In my experience this is not the case and even in this study it appears that quartz grains were not all that plentiful. What was the fraction of quartz versus other minerals in this size range?

However, when considering any laboratory method and its research application there are two vital issues that need to be discussed "up front" (at least in my mind)—they are the time taken to process samples, and then what is the cost. I do not think more than a paragraph is needed but with a new method then I suggest that these are important questions. For example, how long did it take to garner the 198 quartz grains etc etc. These issues become critical if someone was thinking of using this method in order to obtain multi-decadal records over a 1-2 cal ka interval. This is where the potential costs will also come in—what would be the cost for example of processing say 100 samples?

Another major concern I had was with the identification of the quartz groups (p. 4152) i.e. "...*offshore samples were classified into five major types...*" It is not clear to me 1) how these groups were initially defined, and 2) are they indeed "unique", i.e. what is the probability of assigning a grain to only one group? The criteria for placing a quartz grain in one of the 5 groups, A to E, are complex and not necessarily numeric but when I look at the plots on Fig. 6 my first question is: how distinct are the designated groups? This could be tested by Discriminant Function Analysis (DFA) under the null

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hypothesis that there are no differences between the groups based on the element analysis. However, in Table 3 (p. 4169) the distinction between groups A, B and C re primary and secondary rock types appears marginal at best—or do I misunderstand?

The authors demonstrate the power of the method in terms of a relatively small (i.e. small in the context of the NH ocean which would today or in the past be subject to IRD) region. The underlying question that I pose to them is this: does your characterization of the quartz grains carry enough information that we are likely (probably?) to be able to discriminate between quartz grains of similar origins (e.g. Table 3) from say NE Greenland versus SW Spitsbergen? They might legitimately answer that this is the next step, but. . . .they probably know enough of the regional geology of these areas as to hazard a guess or a best case scenario because after-all, for this method to have widespread utility then it needs to be able to differentiate, if possible, between rocks of similar ages and origins (the Old Red Sandstone) but which are now disjunct. I am certainly not advocating that this needs to be done for this paper, but this is the larger and more important issue that this paper, and methods, raise.

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

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