

## ***Interactive comment on “An inter-laboratory investigation of the Arctic sea ice biomarker proxy IP<sub>25</sub> in marine sediments: key outcomes and recommendations” by et al.***

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This is very well written account of an interlaboratory intercomparison exercise to measure a single isoprenoid hydrocarbon termed IP<sub>25</sub> in marine sediments originating from sea-ice. The compound is important since it has good prospects of being a reliable indicator of sea-ice conditions in paleoclimate studies. The study is somewhat limited by the small number of laboratories taking part, but the observations provide some salutary lessons for any laboratory undertaking analytical measurements of organic compounds in seawater or sediments. Indeed, there is a strong need for more of these types of exercises both to ensure the quality of the data being produced and identifica-

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tion of any analytical problems but also to ensure that data from different laboratories can be reliably compared.

The results presented are generally reassuring with reliable data being produced by most of the laboratories as might be expected since these were all experienced with these types of analyses. However, several laboratories presented anomalous data for some analyses and one might wonder whether less experienced laboratories would also have problems. The measurement of IP<sub>25</sub> in Antarctic samples that should not contain this compound by some laboratories is clearly of concern and this needs to be emphasized as a potential trap for new entrants in this type of analysis. It is nice to see the paper describing issues of partial overlap with C<sub>25</sub>:2 and mass spectrometric interference from its m/z 350 ion in any analysis of IP<sub>25</sub>.

The importance of accurate response factors is highlighted by this study and it appears that further work is required here. Given the increasing use of ASE for lipid extraction from a variety of matrices it is a pity that possible problems with recovery of some of the more unsaturated HBI isomers and the internal standard could not be discussed in more detail due to lack of data. This study highlights that whenever any new method is introduced it is incumbent on the authors to verify that the results are comparable to more standard methods.

The paper does not attempt to discuss the application of the IP<sub>25</sub> proxy and possible caveats on its use, which is a reasonable position for the authors to take. However, a short section on this might be useful since the readership would clearly be interested in this topic. For example, there is no mention of the fact that the isotope signature of the IP<sub>25</sub> produced in sea-ice is distinctively enriched in <sup>13</sup>C and this provides a valuable confirmation that sea-ice must have been present. However, the extent of enrichment seen in sea-ice is rarely matched in the underlying sediments perhaps because some production in the water column does occur after the diatoms are released from the sea-ice. Advection of this material would confound any interpretation of sea-ice location.

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A second issue is what concentrations in sediments can be reliably used as indicators of sea-ice presence. Is there a minimum value below which an inference of sea-ice presence would be unreliable? With increasingly sensitive analytical techniques analysts can chase smaller and smaller concentrations (as shown here) and yet this may not be relevant or useful in a paleo-sea study. In this regard, it would be useful to know what quantities of sediments were analyzed by the different laboratories. A recommendation about typical minimum amounts of sediments needed for analysis might be useful.

Minor comments:

Page 5265, Line 10. I suggest "even though laboratories were supplied with appropriate standards" rather than "despite provision of"

Page 5284, line 5: *Cosmochimica Acta* not *Geocosmochimica Acta*

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Interactive comment on *Clim. Past Discuss.*, 9, 5263, 2013.