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Interactive comment on “Quantification of ikaite in Antarctic sea ice” by M. Fischer et al.

Anonymous Referee #2

Received and published: 6 March 2012

Manuscript Number: tc-2012-4 Title: Quantification of ikaite in Antarctic sea ice” Authors: Fisher M., Thomas D. N., Krell A., Nehrke G., Göttlicher J., Norman L., Riaux-Gobin C., Dieckmann G. S.

The authors present the first quantitative study of calcium carbonate as ikaite within sea ice. This study is based on 2 field trips to Antarctica (on pack ice and landfast sea ice). Sea ice cores and snow were sampled and melted to determine and quantify the precipitation of CaCO_3 as ikaite. Ikaite was observed in all samples throughout the entire sea ice column with a maximum of concentration at the top layer of the ice. The authors examined its horizontal spatial distribution and speculated on the temporal evolution of the precipitation of ikaite. Finally, they discussed its importance to the carbon cycle in polar oceans.

Up to now, few studies are published on ikaite precipitation within sea ice, and this

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work presents the first estimation of this precipitation. However, the main purpose of this manuscript “quantification of ikaite” is really poorly explained, making it impossible for the reader to understand. The second purpose of this manuscript, as indicated by the authors, is to investigate the relationship between calcium carbonate and alkalinity, phosphate, and dissolved organic matter. However, the authors fail to provide any further information/discussion with this second objective. These data are partially available in the manuscript (page 512, lines 1-10 and in table 2), however, the authors must provide more detail on these relationships in the manuscript rather than simply refer the reader to the publication of Papadimitriou et al., 2009. This discussion is important and necessary as the conditions of surrounding the precipitation of calcium carbonate are not yet understood; relevant points could include the relationship between the ratio TA:DIC and the possible precipitation of CaCO_3 , the phosphate concentration impeding the precipitation of calcite and supporting the precipitation of ikaite (Bischoff et al, 1993), and so on.

Specific comments:

â€¢ Page 506, lines 1-16: Defines pCO_2 -DON-DOC and then used only the abbreviation further in the text.

â€¢ Page 508, line 2: When this field trip occurred? I guess it start in September but when did it end? What were the dates of the fieldwork?

â€¢ Page 508, lines 8-11: Although this information is already available in another publication, it would be easier for the reader if there were a short summary in this manuscript. The reader should not have to refer to four different papers to understand and interpret the data presented here. I’m thinking about salinity and temperature profiles, the brine volume, and the ice texture.

â€¢ Page 508, lines 26: “These cores were cut into 2 cm section” Which cores? The text is unclear between the line 20 and 27. Please clarify the text.

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Page 510, lines 6-12: The method used to quantify the amount of ikaite is not clear to me. Which is too bad as it is one of the main goals of this manuscript (title of the manuscript). More detail with respect to the method of ikaite quantification is necessary.

Page 510, lines 13-25: It is essential to present the brine salinity and the in-situ temperature of the brine. As I understand from the caption of table 2, this table only refers to melted ice samples. So where are the in situ brine data? Does the text here refer to the wrong samples? In table 2, where are the data from stations D1 to D10 and DSP1 to DSP25? What does the data presented in table 2 correspond to? Is it a mean of the entire ice column (for station S1-S14) or just the top 10cm? Please clarify. In addition, it is essential that you provide the sampling date if you intend on keeping the “temporal evolution” section. Is the table caption wrong, or incomplete?

Page 511, lines 20-21: Core D10 is older than the others cores and you find a higher mineral concentration. Did you see any difference in the size of the crystals? They could be less numerous but bigger as they have had more time to grow. Could you add this information? If you notice a difference, it would be really nice to show a picture/figure.

Page 512, lines 1-10: Even if these data are available in the publication of Papadimitriou et al, 2009, a quick summary including salinity and temperature profiles, and temporal evolution of each variable is necessary. In addition, Rysgaard et al 2007 suggested that the precipitation of CaCO₃ could be indicated by a ratio of Ta:DIC of 2. Geilfus 2011 showed evidence of CaCO₃ precipitation but with a ratio much more lower. It will be interesting to have this information, as they should be available from your data.

Page 512, lines 20-23; page 511 lines 9-10 It is necessary to show salinity profiles and ice texture of the ice cores. From fig 3 and 4 it is obvious that ikaite should be more likely expected at the top of the ice. The more saline layer at the top of the ice is

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interesting but you need to show salinity profile to argue that. In addition to the salinity distribution, the max concentration of ikaite could be also linked to the texture of the ice (i.e. frazil ice versus columnar ice). This makes sense as you sampled pack ice (station S1-14) and landfast sea ice (the others). In common sea ice, the top of the ice column is often made of frazil ice. This layer could be more saline due to a faster growth rate upon initial formation of the ice. The authors should show some salinity profiles and thin sections and discuss them in this context. In addition, did you sample the seawater underlying the ice? Do you think that ikaite crystals may be exported from the ice to the seawater suggesting that the bottom of the ice has lost some crystals? In addition, as your samples run from September to December, (end-of-winter to mid-summer), we can imagine that some brine flushing events could have occurred in the ice. That's another reason why it would be interesting to have the and the date of the sample (it will help to understand the figure 3 and 4) as well as the salinity – temperature profiles and to calculate the relative brine volume. Is it possible that the ice lost some crystals as the summer approached? Can you quantify that?

â€ Page 513, lines 8-11: "The occurrence of calcium carbonate in the snow.... as describes above". I don't understand this statement, could you please clarify?

â€ Page 513, lines 15-18: "concentration of CaCO₃ are on the same order of magnitude". If I go back to the page 511, lines 15-16, I read that the precipitation is on the same order of magnitude but higher. You could just say that the precipitation of CaCO₃ in landfast sea ice is more important than in pack ice and find an explanation. From the figure 3 and 4, the main difference is in the top of the ice. Maybe the ice texture, an indicator of genesis of the sea ice formation could be an explanation? See comment above.

â€ Page 513, lines 22: "reason of this heterogeneity may be salinity, temperature, texture..." It will be nice to show these data and write a real discussion about it.

â€ Page 513, lines 26: The temporal evolution could be discussed only from data of

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station D1 to D10 as they were sampled at the same location. It makes no sense to compare data from the SIPEX cruise as they were sampled in different locations (fig 1). Unfortunately, we do not have any temporal reference for stations D1 to D10. All the dates are missing. However, it will be more interesting to compare the precipitation of ikaite between the 2 different types of ice (landfast – pack ice) sampled in this study.

â€ Page 514, lines 13-25: You should be careful of doing this estimation of the contribution of the CaCO₃ precipitation to the carbon cycle. (1) You only take into account the first 10 cm of the ice. Do you know if CaCO₃ could precipitate at the bottom of the ice, as the ice forms from the freezing seawater? Ikaite is supposed to be the first salt precipitated (Assur 1958). Are you sure that you did not lose any precipitate or that ikaite could not be expelled into the underlying seawater, which could explain the low concentration of ikaite found at the bottom of the ice? In addition, as the summer draw nears, brine flushing could occur, carrying along crystals into the seawater. No quantification of either of these processes occurred in this work. (2) As you mentioned in your manuscript (page 513, lines 17-18), older sea ice may support continuously growing/evolving precipitation of CaCO₃. You only have a snapshot of this precipitation. What effect does ice age have on the amount ikaite precipitated? (3) As mentioned in your manuscript (page 513, lines 20-22) this precipitation is very heterogenic. This leads to large but un-discussed uncertainty in your estimation. (4) The authors seem to think that as soon as this precipitation occurs, it leads to an uptake of CO₂ by the ocean. In the work of Delille (2006) and Geilfus (2011) different scenarios are explained suggesting different impacts of the CO₂ flux depending on how and where this precipitation occurs.

â€ Page 515, lines 19: The importance of the precipitation of CaCO₃ within sea ice has already been suggested by Rysgaard (2007), Delille (2006, 2007), Geilfus (2011).

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