Interactive comment on "Ikaite crystals in melting sea ice – implications for p⁊CO2 and pH levels in Arctic surface waters" *by* S. Rysgaard et al.

Anonymous Referee #2

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Review of "Ikaite crystals in melting sea ice leads to low pCO2 levels and high pH in Arctic surface waters." by Rysgaard S. et al..

We acknowledge the reviewer for his comments to our manuscript. However, we do not agree that nothing new is presented. This is the first time, ikaite crystals have been identified in an actively melting offshore sea ice floe. Previous, investigations have been performed in coastal areas and in other seasons. Furthermore, it is the first x-ray measurements on single crystals from sea ice, and no dissolution experiments have been reported. Reports on pH conditions and CO_2 exchange in melting sea ice are to our knowledge absent. Finally, this is the first time anyone compare data on CO2 flux caused by pure ice melt, dissolution of ikaite and primary production from the same site.

The manuscript represents a diverse set of observations and measurements, but does not represent anything which is really new. The "Methods and Results" section describes isotope measurements for O and H but nothing about it can be found further in the Discussion (one wonders why these measurements were done and why they are needed in the manuscript).

We cannot find any description of isotope measurements of O and H in the methods and result section?

The description of the crystals is very weak and terms without a clear meaning in crystallography are used (e.g. ". . . crystallized simple single crystals ..." a crystal is by definition crystallized and what does simple mean in this context?, or "One high quality crystal . . .", what is a high quality crystal?). Than the authors describe the crystals as "simple uniform" but say that they show layered structures. This makes no sense.

When we states that "well crystallized simple single crystals"; this means that they are NOT poor crystals or crystalline aggregates. I.e. poorer crystals may show cloudiness or variable opacity, strained or variable extinction, multiple character (morphologically or optically). Our word choice accurately portrays both the attributes of *general crystalline quality* and *singularity*; and both of these attributes are in agreement with the x-ray diffraction data character.

What I have my problems with is that the authors investigated crystals by polarized light microscopy and then did x-ray studies on it, but only 4 out of 14 are identified as ikaite. How do we know that the crystals used to describe the morphology were ikaite?

The crystals identified as ikaite had a very distinct morphology, and easily recognized. As it turns out, ikaite in general is very distinctive in appearance and the only significant variation we have seen so far is: a thinner plate morphology in addition to the more common thicker rhombs. Therefore we did not analyze more as we knew for certain that they were ikaite. The reason for looking into the other crystals was purely curiosity. We have added more details on this matter in the revision.

How do the authors explain that the largest amount of crystals within the ice is not ikaite.

Most crystals were ikaite (based on x-ray and microscopic images), but we started to look for other interesting crystals, as to our best knowledge no one has ever done this before in sea ice. We are working on a new manuscript describing other interesting crystals in sea ice.

However, the biggest problem is that all conclusions are based on only one ice core and that this core was stored for some time at -18 $^{\circ}C$. How do the authors know, that the low temperature polymorph ikaite did not form during the ice core was stored at -18 $^{\circ}C$.

We know that because we could compare the morphology of ikaite crystals from photos taken in the field and in the laboratory. The x-ray was used to identify the crystals not to quantify them. All calculations on pCO2 conditions and fluxes are based on TA and TCO2 concentrations that include the ikaite crystals as they dissolve during analysis (addition of acid).

Furthermore, if crystals, other than ikite are found throughout the ice, how do they know that their location within the ice represents the location of formation (for sure minerals like feldspar and quartz do not form within the ice)?

The number of ikaite crystals is very high and by far exceed any other crystals (the reviewer must have misunderstood this fact). Any transport routes transporting this amount of unstable ikaite crystals to the interior ice is hard to imagine (the few very stable feldspar and quarts crystals most have been transported by wind). Furthermore, we now know for certain that ikaite crystals form within sea ice. And they form within hours in new ice. We have experimental evidence for this in the Sea ice Environmental Research Facility (SERF) mesocosm located at the University of Manitoba and from a recent field campaign to Northeastern Greenland. And there are millions of them even in a single 10 cm ice core section. During the present study, we did not have this information and it will form the basis for a new manuscript on conditions in freezing sea ice. However, the present study covers melting sea ice and as our calculations are based on the concentration measurements – the conclusions are still valid.

I have strong doubts that the crystals first disintegrate and then dissolve. For me what we see is typical transformation (most likely into calcite), causing the crystal to disintegrate. That the authors identified one ~20 μm fragment as ikaite was coincidence (it would

have been necessary to measure all fragments to draw a clear conclusion).

The definition for disintegrate is: "to break or separate into constituent elements, or small particles; to destroy the unity or integrity." Our images show this behavior very well; and the fact that a smaller fragment was identified (by x-ray) as ikaite demonstrates that the small-evolved particles are the same material as the starting whole. The "calcite story" is without merit.

The fact that ikaite is very unstable is well known, and represents nothing new.

The present study is the first of its kind identifying ikaite in an offshore actively melting sea ice floe. We show its distribution, its instability and provide the first estimate of its control over the air-sea CO_2 flux as well as its affect on pH in surface waters. Finally, ikaite control over the air-sea CO2 flux is compared with melting of pure ice and primary productivity. This is new.